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HUGH P. TAYLOR
(1932 -)

INTERVIEWED BY
SHIRLEY K. COHEN

June – July, 2002

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Geology, geochemistry

Abstract

An interview in eight sessions in the summer of 2002 with Hugh P. Taylor, Robert P. Sharp Professor of Geology, emeritus, in the Division of Geological and Planetary Sciences. In this wide-ranging interview, Dr. Taylor recalls his upbringing in Arizona and New Mexico, where his father was an agent for the Santa Fe Railroad; his move to Southern California; and his undergraduate education at Caltech. After receiving his BS at Caltech in geochemistry in 1954 (he was one of the first two geochemistry majors to graduate from the institute), and a master's degree at Harvard, he returned to Caltech for his PhD, working on oxygen-isotope ratios with geochemist Samuel Epstein. He recalls their refinement of the separation technique and his application of $^{18}O/^{16}O$ ratios to the study of magmatic intrusions, especially Iceland's Skaergaard intrusion—studies that led to a new understanding of hydrothermal convection and the effects of meteoric groundwater (essentially, rainwater) on basaltic intrusions.

He recalls Caltech's move into geochemistry in the early 1950s under the chairmanship of Robert P. Sharp, the advent of plate tectonics in the mid-1960s, the lunar program at Caltech, and his friendship with astronaut/geologist Harrison

“Jack” Schmitt. Further recollections include the accomplishments of Gerald J. Wasserburg’s laboratory in analyzing the lunar material; Wasserburg’s feud with colleague Leon T. Silver; Silver’s reluctance to publish; Taylor’s collaboration with Silver on isotopic analysis of the Peninsula Ranges Batholith; Taylor’s collecting trip to the Skaergaard intrusion; his work with Robert Coleman of the United States Geological Survey on the Red Sea Rift Zone; his work with Bruno Turi on igneous rocks in Italy; and the discoveries made by several of his outstanding graduate students and postdocs.

The latter part of the interview amounts to a history of Caltech geology, as he describes the evolution of the division from a classical, field-oriented geology department to a first-rank division incorporating geophysics, geochemistry, and planetary sciences. Along the way, Taylor gives his assessment of the various strengths and weaknesses of the division’s chairmen: Robert P. Sharp, Clarence Allen, Eugene Shoemaker, Barclay Kamb, Peter Wyllie, Gerald Wasserburg, Peter M. Goldreich, David J. Stevenson, and Edward M. Stolper.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

Taylor, Hugh P. Interview by Shirley K. Cohen. Pasadena, California, June-July, 2002. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Taylor_H

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH HUGH P. TAYLOR

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Hugh P. Taylor **by Shirley K. Cohen**

Pasadena, California

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TAYLOR: Well, going back to my parents, my father was born in Silverton, Colorado. When he was born, in 1907, it was a major mining town. It's now a tourist town. The Durango-Silverton narrow-gauge railroad—

COHEN: I've been on that train. It's a tourist operation.

TAYLOR: Yes. They go from Durango and they eat lunch in Silverton and they come back to Durango that night. I've done that, too, so I've sort of retraced the path where my father grew up. My grandfather was an assayer in the mines. In those days, they didn't pay any attention to environmental situations, and he worked in close quarters with a lot of mercury. Basically he got mercury poisoning. When it got really bad, he had to quit the mines, and they moved down to Farmington, New Mexico.

COHEN: Were they aware that it was mercury poisoning?

TAYLOR: They knew it was some kind of a problem connected with his work. He went downhill pretty fast and died after that. And then my father started to work on the Santa Fe Railroad. He taught himself Morse code and all the things you had to do to be an operator and a telegrapher on the Santa Fe Railroad.

COHEN: Tell me about your mother.

TAYLOR: My mother grew up in Snowflake, Arizona, which is about thirty or forty miles south of Holbrook, and she went to school there—one of thirteen children, a Mormon family. My father was not Mormon. It was very unusual to have a Mormon marry outside the Mormon Church, but my mother was kind of rebellious. She went to Northern Arizona University at Flagstaff, and she also went to the University of Arizona at Tucson.

COHEN: That must have been unique at that time.

TAYLOR: Yes, for a woman, I suppose—but they valued education. However, she didn't graduate, because she met my father and they got married. They got thrown together because they were near the railroad. One of the main lines of the Santa Fe goes through Holbrook. I don't know exactly how they met.

COHEN: They were in the same place at the same time.

TAYLOR: Yes, the same place at the same time. And she did a lot of things Mormons weren't supposed to do, like smoke cigarettes, and he smoked cigarettes. [Laughter] That's one of the things I remember; it was just a terrible thing growing up. When I was a little kid, probably seven or eight, I wanted to smoke a cigarette and my father—this is a story you hear a lot, I guess—he gave me a big cigar. He lit it up and said, "OK, sit back. Let's have a smoke." And I got so sick I never wanted anything to do with cigarettes the rest of my life. I learned later that you're not supposed to inhale cigar smoke, but of course I didn't know that, so I really got sick. I did a lot of stupid things when I was a kid.

COHEN: One wonders how people survive childhood. [Laughter]

TAYLOR: Like when I was around six years old—I think it was even before I entered first grade—I got involved with a neighborhood kid who was eleven or twelve. He told me he was an operative for the FBI or something like that, OK? That was just crazy, but when you are five or six years old, who knows what's going on? So we broke into this neighbor's house that my family knew pretty well and rummaged around. I think the older kid took some stuff. I just wandered around and found all these old pint bottles of whiskey, which had little dregs of whiskey in the bottom. And I sat down and drank the dregs, and then I wandered back to my house, and I was completely drunk and wiped out. [Laughter] My mother was good friends with the sheriff in Holbrook, and so they came. They arrested the other kid. [Laughter] That was one of the traumas when I was growing up.

But anyway, my father worked on the Santa Fe Railroad. I was born in Holbrook in 1932, at the height of the Depression. He was hanging on by the skin of his teeth, as I found out later, because he was on what was called the extra board, which meant he was assigned to go wherever, to plug a gap. So, basically we traveled a lot when I was very young. I should also say that I had a twin brother. My mother gave birth to twins on December 27, 1932—we weren't identical twins. So here she had two babies to take care of, and we were traveling like nomads along the Santa Fe Railroad. We lived, if you can call it that, in boxcars, in almost every little town between Albuquerque, New Mexico, and Needles, California, on the Santa Fe main line.

COHEN: It's amazing how one survives all that.

TAYLOR: For a little kid it was wonderful; I loved it. I loved the trains; I loved everything about it then. [Laughter] There were no bad parts that I can remember. I remember the railroaders sitting on the floor around potbellied stoves while they told stories about the railroad.

COHEN: How about when you started school?

TAYLOR: I started school with my twin brother in Holbrook in 1938. And I've always been unbelievably lucky with my teachers. I had a teacher I remember to this day—Mrs. Hightower. Later I went back to Holbrook and looked through the old newspapers and found references to her. She taught me phonics—she emphasized phonics. I never went to kindergarten. I just started in the first grade in September of 1938, when I was almost six. For some reason or other, my parents had never taught me the alphabet or anything—or maybe I was just a slow learner—but I remember distinctly that when I went to first grade the students would stand up and all in unison recite something. And I didn't even know what it was, but it ended with X, Y, Z. So I started saying, with everybody else—I didn't want to act like a fool—but I started saying X, Y, Z.

COHEN: You moved backwards, the other way? [Laughter]

TAYLOR: Right. I learned the alphabet going backwards. So there certainly wasn't any kind of pushing me into anything intellectual. I knew nothing when I entered the first grade.

COHEN: It sounds like your mother had more education than your father.

TAYLOR: Yes, a more polished, formal education—right. But my father read an enormous amount. He had books all the time. He was interested in all kinds of things. When World War II broke out, he started clipping all the sections from *Life* magazine that had anything to do with the war, because he knew that *Life* was going to cover the war, and he had them all bound together.

COHEN: Do you have that? Do you know where that is?

TAYLOR: No, but I have a bunch of old *Life* magazines; I'm a collector myself. But, no, he took those with him when—later on, my parents got divorced.

So anyway, my brother and I went to the first grade in Holbrook, Arizona. I had a great teacher there who emphasized phonics and spelling, and I became an excellent speller. Now you read about different ways of teaching and so forth—to me it's silly, because phonics is so clearly something you need. You need to know how to sound out words.

COHEN: This must have been a small school. Holbrook's not a very big place.

TAYLOR: Yes, it was quite a small school. And then the next year we moved again. By this time we had more or less settled down. My father was an operator at the Holbrook depot, and it was terrific, because we lived only about 200 yards from the depot. So I could walk down there and walk back home with him from the depot. Being around the railroad all the time was terrific. I loved the big steam engines and the trains—it was a great way to grow up. [Laughter] It was hard on my parents, because they had—

COHEN: Noise and soot and dirt.

TAYLOR: And then the next year, for the first time, he got a job as an agent, and he was the main man in a very tiny railroad station in New Laguna, New Mexico, which is fifty miles west of Albuquerque and about thirty miles east of Grants. It's on an Indian reservation. So we moved there. We lived above the train depot. At first, my mother enrolled us in an Indian school, which was in Laguna Pueblo, where the students were all Indians, and we went there for only about—it couldn't have been more than a week or two.

COHEN: It didn't work?

TAYLOR: Well, my mother said, "This is ridiculous. You're not learning anything." I don't remember that much about it, but she said it was hopeless, because we were way ahead of the Indian kids, so we just weren't learning things. So she wrote back to the teachers she knew in Holbrook and got a whole bunch of books and things like that, and basically she taught us.

COHEN: So you were home schooled.

TAYLOR: Now there's a term for it, home schooling. For us it was wonderful, because we could get done with our studies in three hours in the morning and then have the whole rest of the day to play. And playing there—I mean, there was a creek with fish in it. It was great, great country. Almost nobody lived in New Laguna, but there was an Indian family that owned an old hotel there, and we became best friends with this Indian family, and also with the blacksmith in the area and so forth. And then there was a moderately wealthy person who lived in a fairly big house nearby who even had a tennis court. My mother and father used to go over and play tennis, I remember. This was a pretty idyllic existence, as I look back on it. And although we were isolated, I remember sitting around and listening to the radio—that was big; that was like television in those days—listening to *I Love a Mystery*. I remember Jack and Doc and Reggie. [Laughter] It was a great place! That lasted for just that one year—what you call home schooling.

A huge influence on me then was my grandmother, my father's mother, who by now was a widow and lived in Long Beach, California. She sent us a whole bunch of Oz books—I think all the first twelve Oz books—as a Christmas present in 1939. My mother would read these to us. So I got a lot of encouragement about reading. I don't know if you've ever read the Oz books—

COHEN: Baum?

TAYLOR: Yes, L. Frank Baum. And later on, Ruth Plumly Thompson carried on, and so forth, but the original ones were the best, and I knew them backwards and forwards. I used to dream of going to Oz. [Laughter] And then the most wonderful thing happened. In 1939 the movie came out, so it was a big production to drive into Albuquerque to see that movie. The Oz stories—later on I guess educators thought they weren't all that great for kids. But that's how I learned to read, and voraciously read. And I would read them over and over again till they were dog-eared. I still have those original books. They're all dog-eared. They were such terrific stories for kids. That guy, L. Frank Baum, had a tremendous knack for whatever it is that encourages kids to read—it seems to me anyway. I felt about the Oz stories the same way the younger generation now feels about the Harry Potter stories.

COHEN: Right. Well, as you moved up to higher grades, you must have started to go to school again somewhere.

TAYLOR: OK. That was the end of the home schooling, because—well, I should tell you about an incident that happened there, which is still very funny. I was out with my brother on the depot platform, and I had some pink papers in my hand that I had gotten from my father's desk. And I knew that the fast mail train, number eight, was coming through, and I said, "I wonder what would happen if I waved these pink papers," in the way I had seen my father do when he flags down a train, and so I did it. The darn train stopped.

COHEN: That's a federal offense, isn't it?

TAYLOR: I don't know. But the engineer thought there was something wrong at this little isolated station, so he stopped the train. They gave my father some demerits, or something like that, or he was docked in pay. They fined him for this, and I got one of the worst spankings I've ever had. [Laughter] When I look back on it now, it's kind of funny.

So we left New Laguna, and the next place we went was Grants, which was just thirty miles down the line. And this was much, much less desirable, because we were now back in a boxcar. In New Laguna we had lived in a real house, where we had kerosene lamps and no electric lighting; there was no electrical generator or anything. I remember my father had batteries, big batteries, to run the telegraph operation and so forth, but it was still a nice home. The place in Grants was living out of a boxcar.

COHEN: This was very hard on your mother, I would think.

TAYLOR: Yes, it was obviously very hard on her. And then we moved to another—I guess now my father was back on the extra board, moving around wherever they could put him, because he went to the Grand Canyon. The Santa Fe Railroad had a spur that went to the Grand Canyon, right on the rim. And there we lived in a tiny little cabin, where we had to carry in water. [Laughter] But it was fabulous, because it was right on the rim of the Grand Canyon. So every day I woke up I could see the Grand Canyon. For a few months we went to school in Grand Canyon Village—again, that was paradise for a little kid, because I got my first look at the Grand Canyon, the first look at something geological that I remember. Every day you wake up and you see this wonderful thing, and it changes from morning to night.

And then there was a big change in our lives. My father got a more-or-less permanent job in Phoenix, Arizona.

COHEN: That was already a big city.

TAYLOR: Yes—well, it was not that big.

COHEN: Compared to where you had been.

TAYLOR: Oh, yes! And there we had a real house, for the first time.

COHEN: How old were you at this point?

TAYLOR: I was in the third grade.

COHEN: It's amazing that you remember the little details of these towns.

TAYLOR: Well, it's burned into my brain. [Laughter] I've skipped over several, because, as I say, at one time or another, we lived in every little town.

COHEN: Up and down the railroad.

TAYLOR: Up and down the railroad: Gallup, New Mexico. Skull Valley, Arizona. Seligman, Arizona. Peach Springs, Arizona. Winslow. Anyway, in Phoenix we had a stable existence, more or less, for the first time since New Laguna. We weren't too far from the capitol, and I got my first bicycle and was able to ride around Phoenix, on the capitol grounds and so forth. I went to the third and fourth grade in Phoenix. And that's when we got into World War II. Pearl Harbor happened while I was in the fourth grade and then everything changed. Now there wasn't any problem about employment anymore, so our fortunes, I guess, took a rise. My father got another position as an agent, in a little town called Bellemont, which is twelve miles west of Flagstaff, right on the main line of the Santa Fe Railroad. He went up there in the summer of '42 to start that project. Now, that was somewhat like the New Laguna operation, because he was the head man in this little railroad station, and again we lived right in the depot. But now, instead of living above the depot, it was an annex to the depot where we lived. It was part of the train station, so all we had to do is walk out of our living room into the office where he worked. Again, it was idyllic, because it was like the New Laguna situation but it was a much bigger operation, because the army built a huge ordnance base right there. It was a gigantic complex. The whole time we were there, there was tremendous construction going on and all kinds of things going on with the United States Army. This was a very important stop on the Santa Fe railroad, because huge amounts of freight were coming in and out all the time, so he had about ten or twelve people working for him. In order to go to school then, we went on a bus every day to Flagstaff. I went to fifth grade in Flagstaff.

COHEN: Now, was there any integration? Were there Indians at your school or did they go to their own schools?

TAYLOR: No, in Flagstaff there weren't, but of course I went to school with a lot of Indians in Grants. Many of my best friends were Indians.

COHEN: Were these Navajos? Hopis?

TAYLOR: I don't know what tribe they were from.

COHEN: They spoke English?

TAYLOR: Yes. They were certainly very friendly. There were no blacks at all; it was all either Caucasians or Indians there.

So I went to the fifth grade in Flagstaff. And then I guess my father—I never knew exactly what happened—but I think my father had a run-in with the commanding officer at this ordnance depot in Bellemont. Anyway, all of a sudden, one day in the spring of 1943, we moved. He left the railroad, which had been his whole life, and we moved to California. I didn't understand why we were moving, but we got in our old '35 Plymouth and the whole family drove to—

COHEN: Where did you go?

TAYLOR: We ended up in South Gate, California.

COHEN: I see. Now, you had some grandparents near there somewhere.

TAYLOR: Oh, yes. I haven't mentioned them. I had this big extended family on my mother's side, the Mormon side, with *huge* numbers of cousins whom we saw a lot of. And when we were living in Phoenix, I saw a lot of them, because my maternal grandmother lived in Mesa, which is not very far from Phoenix, so we used to go over there all the time. They were all devout Mormons, and of course I wasn't. My parents didn't go to church, but my mother sent us to church, so periodically, at various times in my life, I would go to the Latter Day Saints Church. I never went to any other church. Well, I did for a little while, but basically I went to Mormon churches off and on at various times my whole life, but I didn't really pay any attention to the doctrine or anything like that. Later on, when I was a teenager, I decided it was all a bunch of nonsense. Now, my brother, my twin brother, became a very devout Mormon, so we totally diverged. Until he died in 1988, he was a very, very religious person. But I guess I was more scientific; I thought it was just a myth.

But I must say that I love everything else about the Mormon Church. I love the heritage, the pioneers. My grandmother on my mother's side used to tell me stories about when they were coming over literally in covered wagons in Arizona and Utah, and the men would have to go off and fight the Indians and so forth. The whole Mormon heritage was a huge thing in my life, except for the fact that—

COHEN: Except for the religious part.

TAYLOR: Yes. If you could just have it all without having the paraphernalia that comes along with it, I would have stayed in the Church, because I loved everything else about it. I liked the way the people behaved and how they helped each other out; and of course the history, the pioneer history, which I was always very interested in.

Anyway, in the spring of 1943 there was a big change, because we left the railroad, which I was really unhappy about. I wanted to be a railroad man. I loved the railroad, I just absolutely loved it. I still do. [Laughter] I have all kinds of books on railroads.

So it was a complete break. My brother and I went to the fifth grade in South Gate, California, and we lived in an apartment for the first time.

COHEN: I would think that anybody else would think of South Gate as just being Los Angeles.

TAYLOR: Yes, it was a suburb of Los Angeles, and it was all, basically, lower-middle-class white people.

COHEN: I think it still is.

TAYLOR: No, no—now it's all Hispanic. Huntington Park, which is just to the north, was all lower-middle-class white, basically, and now it's all Hispanic. Everything has changed in the last few years. But at that time it was a very nice place, a pleasant suburb, to grow up in. It wasn't as nice for a kid as the railroad, however.

COHEN: You were used to the open spaces.

TAYLOR: Yes. I'm really happy that until I was ten or eleven years old I was able to live almost always in a rural environment and do things that farm kids and ranch kids get to do. But then, of course, when I was a teenager I was probably better off in a situation where there was a little bit better school system and so forth. Every time I look back at my history, I see that I was lucky—just lucky, lucky, lucky.

So I started school, and my twin brother was always, of course, with me. The unfortunate thing, though, is that I was always smarter than he was. [Laughter] He was taller, bigger than I was. But it was very hard on him, because here I was—every class I ever took, I was the best. He was quite intelligent, but I think psychologically it was very, very difficult—extremely difficult—for him. He also had a lot of glandular enzyme problems when he was growing up, too. I remember he had to go to the doctor quite a bit, to get a lot of shots and things. So, almost all the things that seemed to work out just easy and simple for me, weren't so easy for him. And of course there was competition. You know, we'd get into arguments, like any two brothers would. But I didn't have the empathy and the feeling, as I look back on it now, that I should have had.

COHEN: Well, that's how kids are.

TAYLOR: Yes, that's the way kids are, but there are things I wish I had done differently. Anyway, when we got into the fifth grade in Stanford Elementary School in South Gate, we were way ahead of everybody. It was like the Indian school. I mean, the Arizona schools were way ahead of the California schools.

COHEN: That's interesting.

TAYLOR: Yes, I don't know quite why that was. So my fifth grade teacher just said, "Well, there's no point in your even taking part." She let me sit in the back of the room and read. They had a whole bunch of books, and I read through everything that was there. That was mainly what I did in the rest of the fifth grade. We got there in March of '43, and until June that's what I did. I took part in all the other things, but basically I just sat and read. [Laughter] That's when I had to get glasses for the first time. I remember now that the room was dark; I don't know whether this had anything to do with it—probably not. But reading in not a very well-lighted room, and reading constantly, probably wasn't too good for my eyes. Anyway, at that particular time I had to start using glasses, and of course kids make fun of people with glasses. And then, they skipped us a half a grade. So now, instead of being on the track we had always been on, say, to graduate in June, we were shoved into this winter class. As I look back on it, I don't know whether this was good or bad, and I don't know exactly why it was done. So that pre-ordained it for the two of us throughout junior high school and high school.

When we got out of the sixth grade, we went to South Gate Junior High School, which was quite a bit farther away. Sometimes we rode the bus, sometimes we walked, and it was three or three and a half miles to school. Most of the time we took the bus, and we'd sometimes walk home. That was for three years, that we went to South Gate Junior High School, and we were in the winter class. The good thing about being in the winter class, I guess, was that it was smaller and you would stand out more. Again, I always did well academically, but I also was able to stand out. I ran for student body president. I didn't win, but I did all those kinds of things. I was heavily into sports. We played sports all the time. That was another thing: I was always better in sports than my brother, so that was kind of hard, too. [Laughter] But he was much more my equal in sports, and it was good to have a playmate all the time. That was one good thing about having a twin brother—you always had somebody to play with.

Anyway, we graduated from junior high school in the winter of 1947 and then went to South Gate High School. Now, I remember distinctly, even though I always did extremely well and was much better than anybody else in my class, every time I'd change schools I was worried sick that I wasn't going to be able to perform. I was extremely worried that high school would be too much for me. But it wasn't. I was always, I guess, a teacher's pet. My geometry teacher, my algebra teacher, my chemistry teacher particularly—I remember all of them, and their faces. I loved them, because I guess they looked on me as a star pupil and so they gave me a lot of extra time. And I ate that up. I mean, I loved it! Later on, when I got married, they came to my wedding. I loved all my teachers. [Laughter] I had a great English teacher, a real throwback, who would make you memorize poems. She forced everybody to push beyond themselves. You had to memorize enormous numbers of poems and recite them to her, and she would sit there and listen to you.

COHEN: All these old-fashioned—

TAYLOR: Yes. I learned "The Raven" and the Gettysburg Address by heart, and all those kinds of things. I don't know how useful it was, but I thought it was very good, because I learned a tremendous amount. And she made you diagram sentences; I don't think people do that anymore. So I learned all the parts of speech. I got a very good education in general.

Some of it *wasn't* so good. I remember in twelfth grade, when I took trigonometry, this old man taught solid geometry and trigonometry. He was the head of the mathematics department, I guess because of his seniority, but he was a terrible teacher compared with the others I'd had. Here he was teaching trigonometry, and you'd get into logarithms. He taught logarithms, OK? I ran into this thing called natural logarithms, and I asked him, "What are natural logarithms?" He said, "I don't know." [Laughter] How could a mathematics teacher, the so-called head of a high school mathematics department, not know what natural logarithms are?

COHEN: You have to use them.

TAYLOR: Yes. Well, whatever it was, he just didn't know.

COHEN: But you survived it all.

TAYLOR: I guess I did. I did a lot of dumb things though.

In junior high school I first got interested in science per se, because I had a science teacher who—

COHEN: You had this tremendous affinity for the outside world and natural things.

TAYLOR: Yes, but that was my first experience in actually writing papers—term papers and things like that. I remember being kind of turned off by the fact that—and I guess this was just part of a social experiment—one of the things they emphasized was drinking and alcoholism, OK? Alcohol was one of the things you had to spend a lot of time talking about in the schools in those days. [Laughter]

COHEN: That's interesting. Well, it was before drugs.

TAYLOR: Yes, right, there weren't drugs. I never encountered drugs of any kind, all the time I was going through school. Marijuana was mentioned, but it was like the Black Death or something like that. Somebody who actually had anything to do with marijuana was almost beyond the pale. No, there was nothing about drugs, but they mentioned alcoholism.

Anyway, what I wanted to do—this was in the eighth grade—was take as much math and science as I could. So I got into this course, the only math course I could take when I was in the eighth grade, because of the progression of things. It was, I later found out, bonehead math. [Laughter] I got into this class and all we did was learn addition. We did rote addition, subtraction, division, and all those kinds of things. That was the whole thing. But I was somehow content to do it, because I could do it. [Laughter] And I must have been very naïve, because I didn't know it was bonehead math. [Laughter] I only realized that later on, that it was ridiculous to waste my time, a whole semester of math, just doing rote addition and subtraction, which I knew how to do like the back of my hand. And it was because I didn't get good advice or something—who knows?

I took chemistry, like everybody else did in eleventh grade, and at that time in Southern California they had a competition, the American Chemical Society scholarship competition for all the schools in Southern California. [Tape ends]

Begin Tape 1, Side 2

COHEN: You were going to tell me—

TAYLOR: Yes, about the American Chemical Society scholarship. Now, South Gate High School had never had anybody even take part in this before, but I had done pretty well in chemistry, and my chemistry teacher suggested I go ahead and do this, so I studied for it quite hard. Later on I found out that the kids who won it were part of little coteries—people who had really been coached and things like that—because they knew what was on the exam. For instance, students from El Monte High School almost always won it. But anyway, I didn't know any better. So we went to USC to take the exam. It was a fairly hard exam, and I didn't know what to expect at all. Then a few days later I found out I had won. I was number one in Southern California! They gave scholarships to a whole bunch of universities, and Caltech was one of them. The only place I had ever wanted to go was Caltech.

COHEN: Oh, so you had heard about Caltech already.

TAYLOR: The only reason I took the test was because one of the universities on the list was Caltech. So I won, OK? And you know, I was really pretty happy, and my school was happy, because no one from that school had ever even taken the test before. They made a big deal about it at the school. And I was only in my junior year, so I had a whole year to wait. Even longer than that, because I would graduate in the winter; and Caltech didn't take people until September, so actually I had to wait a long time after I won the scholarship to go to Caltech. Then later I also found out that the scholarship was only good if you passed the Caltech entrance examinations with a high enough score. [Laughter] But I did do OK. I finally got into Caltech in September of 1950.

COHEN: And that paid your whole way?

TAYLOR: Well, no, all it did was pay tuition. The tuition then was $600 a year. [Laughter] And later on I found out that Caltech had scholarships based on need, and I certainly was needy, so I probably would have gotten a scholarship even if I hadn't won the American Chemical Society scholarship competition. Nevertheless, it was a nice thing, because for the two years before I came to Caltech I knew I would have enough money to go to Caltech. Also I should point out that I had some good jobs during the summer; that was a time when you could make a little money in the summertime. Particularly after I got to Caltech, after the Korean War started, it was easy to get a job during the summer, so I could support myself and get quite a bit of money working in the summer.

I should also point out that besides academics in high school, I ran for student body president at South Gate High School and, again, lost [laughter] to a girl, the first girl president in the history of South Gate High School. She became a good friend. But because of that, I became quite hot—high up in the political circles in high school and so on. So I had a good time. And also I played football. I lettered in football and acquired a lifetime interest in football. My coach in high school was Charlie Hastings; he was the center on the UCLA football team in 1926. The halfback on the same team was Bert LaBrucherie, who was then the football coach at Caltech. Charlie Hastings told Bert LaBrucherie about me. I wasn't all that great in high school, but I was quite good for Caltech, so LaBrucherie knew about me before I arrived, and thus I had that entree in sports at Caltech besides.

Football was actually quite a big thing at Caltech in those days. It doesn't exist now, but it was quite a big thing. Bert LaBrucherie had been the head football coach at UCLA from 1944 until 1949. He had an undefeated UCLA team that he took to the Rose Bowl, where they got massacred by Illinois. [Laughter] And then he had a couple of bad seasons after that and got fired. But Caltech needed a coach, so [Caltech president Lee A.] DuBridge—I guess Caltech always wanted to have the best, so here was a chance to get a really outstanding football coach who had been a big-time college football coach, and they offered him the job at Caltech. Bert LaBrucherie—back in the twenties, when he was playing, UCLA played Caltech. Bob [Robert P.] Sharp was a quarterback on the Caltech football team back in those days. We actually were competitive with UCLA. So Bert remembered those times. I guess he thought he could come here and turn Caltech into a winning team, so he was really glad to get me. Even before school started, football practice started. This seems strange now, but in those days football practice always started a week or two earlier than when everybody else came to school. All the big colleges did that. [Laughter] The idea of Caltech doing it is actually pretty strange. So I arrived on campus and started playing football on the freshman football team.

COHEN: Did you live in one of the houses then?

TAYLOR: Yes, I went into Dabney House, because one of my friends from high school the previous year was in Dabney House, so I picked it. The student houses at Caltech then—talk about paradise! They were the most wonderful experience of my life. They were just so absolutely superb in every way.

COHEN: Were they throwing food in those days?

TAYLOR: No, no. Well, there was a little bit of that. In the evening at dinner, everyone dressed in a coat and tie.

COHEN: There were some manners then.

TAYLOR: Basically, the upperclassmen taught the lowerclassmen how to behave socially and otherwise. A major part of the educational experience was inculcated by the upperclassmen. And you really looked up to them; I mean, the presidents of the houses were like gods.

COHEN: I wonder when all that changed.

TAYLOR: In the sixties, it all went to… There were just so many wonderful things about the student houses then. They've lost a lot. There was a tremendous esprit de corps. They used to say that the student houses had all the best aspects of fraternities and none of the bad aspects, and I really believe that. I can't think of any bad aspects, and they certainly had a fraternal feeling, house spirit and everything else, and a tremendous social interaction. It was probably the most important part of my undergraduate experience, living in the houses and associating with my fellow students—mainly at Dabney House, but there was also a lot of competition. Benjamin Rosen [chairman of the Caltech Board of Trustees] was right next door in Fleming House; he was in my class. Fred Anson [Gilloon Professor of Chemistry, emeritus] lived in Throop Club; he was off campus. Don [Donald L.] Turcotte—I don't know if you know him or not.

COHEN: He's at Cornell, right?

TAYLOR: Yes, he's at Cornell. He was in Fleming House. We had a very strange freshman class.

COHEN: In what respect?

TAYLOR: Well, there were 180 students. In those days the freshman class was always very close to 180. So 180 freshmen, all men, came to Caltech in the fall of 1950. Now, the Korean War started that summer. Everybody had a problem with the draft one way or another. I think most people got deferments, but some people, I think, left for that reason, or other reasons. And a lot of people just left because they wasted their time and did poorly in their studies. I don't know all the reasons it happened, but I do know that when we finally graduated, four years later, there were only about 85 members of the original 180 in that class. We had a total graduating class of 90 or 95, but that was because there had been some upper-class transfers. There was a tremendous decimation, perhaps largely because of the Korean War, but I think it was also all the uncertainties of that period. A bunch of the kids didn't seem to knuckle down; they just played around and wasted a lot of time. I don't think that happens anymore; it would be awful if it did—to lose half your class. But the ones who survived were pretty good—several National Academy [of Sciences] members and people like that—Turcotte and Anson and so forth.

COHEN: Did you major in geology right away? How did that work?

TAYLOR: That's interesting. Of course, I had won the American Chemical Society scholarship.

COHEN: So you thought you should be a chemist.

TAYLOR: Well, yes. Not only that, but I had this star on my forehead, because a lot of the kids who came to Caltech had taken part in that competition and I had won. In those days, we had what was called the honor section; in the second term, they put all the so-called best students into one section, based on your grades from the first term. It was really kind of a strange thing to do. I guess they decided to isolate the so-called best students, and maybe by osmosis or something they'd become even better. I don't think it worked at all, frankly. [Laughter] Nevertheless, for the next five terms, I was with the honor section, with the same group of people. But I'll tell you what happened. For instance, the first term of my freshman year I had great teachers. They were graduate students; they taught the sections. The way it worked in those days—and it probably still does—we all sat in, say, a chemistry lecture and listened to Linus Pauling give a lecture, one or two lectures a week. And then we broke up into sections, and that's where we really did the key learning to pass the exams. And in physics [Robert Andrews] Millikan gave lectures, so I actually heard Millikan.

COHEN: So you had Pauling and Millikan. OK. Not bad.

TAYLOR: Yes, not bad, although Millikan was very old then, not very inspiring. But it was great to see him, and you could also see him walking around campus. And Pauling's lectures were a delight, there's no question about that. In math I don't think there were any lectures, it was just the sections. But anyway, the section leaders, the graduate students, were the key people in teaching. My chemistry teacher was John Waugh, who later became a professor at MIT. I don't know if you know him or not, but he's in the National Academy. He turned out to be a really super guy, and he made a tremendous impression. He was just a great teacher, even as a graduate student, so I learned a lot of chemistry. I had a great physics instructor, a graduate student named Bill Warters. And the math instructor was also superb. They were very good at teaching because they had just gone through it. So I learned a lot of chemistry and physics and math, and the other things, too. David Elliot was my history teacher. Kent Clark was my English teacher. Superb!

COHEN: They're still superb.

TAYLOR: Yes, superb teachers. Then I got a pretty good GPA [grade point average], so I was put into the honor section, so the second term, everything changed. Instead of having these great graduate students as teachers, I had Earnest Watson as my section instructor in physics. Now, I loved Earnest Watson as a human being, but he was a very old man and he just didn't—it wasn't at all the same. First of all, we had a terribly old-fashioned book, Millikan, Roller & Watson [*Mechanics, Molecular Physics, Heat and Sound*], to learn physics from. It was outdated even then. I had a great chemistry instructor, Norman Davidson. Basically what they did was they gave the honor section to professors instead of graduate students. Davidson was terrific, but Watson was not. Oh, and the math teacher—who was that? Oh yes, it was [Thomas] Apostol. Now, Apostol was much more esoteric. He was great for the guys in the class who wanted to be mathematicians, but he wasn't as good as my first-year math instructor, the graduate student, in teaching somebody who wasn't going to be a mathematician. All the guys who became mathematicians loved Apostol. I liked him, he was good, and I think he got very, very good later on, but in those days I was disappointed, frankly.

But I did quite well my freshman year. One of the bad things I started doing, though, was playing bridge a lot. I had discovered bridge, and that's part of the camaraderie in the houses, so I would stay up until real, real late at night playing bridge. And then of course I was playing sports, so I really didn't have all that much time for studies, because I had to sleep. So I just did the bare minimum. I look back on it now and I kick myself. Because I went to most of Pauling's lectures when I first started out here, but then it turned out that they were mostly cultural things; you didn't really need what Pauling was lecturing about to do well on the exams, and it turned out that I felt more like sleeping. [Laughter] So often I didn't get up and I missed his lectures. I just kick myself now, because all the things that I think of now as so important, that I wish that I had done, like that—I missed. There was just not enough time. When you are an undergraduate, there's absolutely not enough time to do all the things you want to do. And bridge was nice. Finally, though, in my junior year I decided it was ridiculous and I was wasting too much time on bridge—I must have been spending three or four or five hours a day playing bridge the first two years I was here. And I just stopped.

COHEN: Maybe that's why there were so few graduates in the class. [Laughter]

TAYLOR: Maybe that's part of it.

Now, about geology. I told you I spent part of my childhood living on the rim of the Grand Canyon. I had an outdoor type of life. And when I was growing up, there were two books that impressed me a lot. One was by Roy Chapman Andrews. I don't know if you've ever heard of him or not; he's the guy who found the dinosaur eggs in the Gobi Desert. He took American Museum of Natural History expeditions to the Gobi Desert in the twenties. I just devoured his books on exploration. Later on, I found out that he is the prototype for Indiana Jones, of the Indiana Jones movies. I picked a pretty good hero to follow. And the other was a book by Paul de Kruif called *Microbe Hunters,* which I also found out later was the inspiration for lots of other scientists. It was a terrific book, describing the scientific method and so forth. I was an avid reader. I told you that. I just read and read and read everything. But I wanted to be a scientist. And the outdoors type of thing, from Roy Chapman Andrews, inspired me also. And then when I was in high school, I never was a Boy Scout, but a good friend of mine got me into the Explorer Scouts. We took a lot of outdoor camping trips. When I was sixteen years old, the two of us hitchhiked all the way up to the Sierra Nevada and spent a month in the back country in the Sierras, just having a good time, all on our own. In other words, I felt comfortable in that kind of a situation.

But I still wanted to be a chemist. Even though I didn't go to Pauling's lectures—or didn't go to all of them—when I was a sophomore I was still planning to be a chemist. But in those days when you were a sophomore, you took geology the first term of your sophomore year, you took biology the second term, you took astronomy the third term—you know, sort of the cultural science courses. Everybody in the sophomore class took those courses. So everybody took Bob Sharp's geology course. Well, that was just a fabulous course. He was *the* best teacher. By the way, he had been written up in *Life* magazine a couple of years earlier as one of the country's best teachers. And the way the course was put together was extremely good, too. They'd give you little field projects to do, where you'd drive out—even here, you could go across the arroyo. He had a little mapping project along a roadcut and things like that, where you'd sort of do it on your own. I thought that was just terrific. It got you out of doors. And so right then I decided—

COHEN: That was it for you.

TAYLOR: Yes. And my instructor, my TA [teaching assistant], was Clarence Allen [geology and geophysics professor, emeritus]. He was a graduate student at the time. And my other TA was Lee [Leon T.] Silver [Keck Foundation Professor for Resource Geology, emeritus]. Bob Sharp was giving the lectures. You couldn't have had a better introduction to geology—Sharp's lectures and these guys as TAs, plus the fact that it was an eye-opener, because I knew nothing really about geology, nothing! So I became a geologist, and it was terrific. There were really a lot of great professors in the geology division at that time. Then it was almost all geology and geophysics; there was no geochemistry or planetary science.

COHEN: That was before the Chicago people.

TAYLOR: Yes, that was before the Chicago people. I don't know if you remember Al [Albert] Engel or Jim [James A.] Noble. It was a much smaller department, and it was all focused on geology and seismology.

COHEN: Right. Well, those were the traditional fields then.

TAYLOR: Yes, mostly classical geology. And there was a young assistant professor named Lloyd Pray, who probably was, later on, the most influential professor for me, because he taught so many of my courses, particularly some of the field geology and so forth. They were terrific teachers. Even when you were a sophomore, you felt like you were part of a family in the geology division. At that time, the graduate student body in geology was relatively small. The number of professors was also relatively small. Every graduate student practically had his own office all by himself. Some of them—maybe two—had to share an office. But there was none of this business of huge numbers of graduate students thrown into a room together. And we all went on a spring field trip every year. The entire department went—all the graduate students and all the undergraduates, along with one or two of the professors. In those days there were fifteen or so undergraduates, sometimes twenty, in each geology/geophysics class. These huge entourages spent a week out in the field with a professor. There was a terrific camaraderie.

COHEN: It's not true anymore—there are just too many people.

TAYLOR: Well, no, they don't have these things. In some ways, they do more now—they have a trip to Hawaii every year that Bob Sharp used to run and now Lee Silver runs. Things are still pretty good. [Laughter] But in those days it was much, much bigger. Well, first of all, there were lots of undergraduates then who chose geology, probably because of Bob Sharp's courses. Later on they cut that required course out. So the number of undergraduate students, at various times while I've been here, has only been two or three in geology. You know, for most of the time I've been here at Caltech, almost every undergraduate who went through here had no connection with geology whatsoever. They never took a course in geology and never saw anybody in geology. We are down at one end of the campus, and so they'd go through here and never have any contact whatsoever with the geologists, which I thought was kind of sad.

But that wasn't true in the early fifties, and it was wonderful for me. And then the

Korean War had started, so I was able to get a job at Alcoa Aluminum, in Vernon, during the summers, which paid extremely good money, so I had no real financial worries. I still had a tuition scholarship the whole time. The biggest problem came when I had to go to summer field camp my junior year, because that cut off most of the summer and I couldn't work then. In the summer field camp, we went to the Sacramento Mountains in New Mexico. Again, the summer field camps in those days were very large; we had lots of students go. They were done extremely well. Lloyd Pray was the head man, this professor I had so much interaction with, but Bob Sharp was also along, and Carel Otte was also along. Do you know Carel Otte?

COHEN: I don't know Carel Otte.

TAYLOR: He's heavily involved with the Caltech Associates and was president of the Alumni Association. Anyway, it was a beautiful education in geology, because you got such personal attention and so much close interaction with all these teachers. The one thing they didn't have was a real modern outlook, in terms of chemistry and so forth, but that all changed, because my junior year is when the whole entourage from Chicago came in. And again, here I wanted to be a chemist originally, then I became a geologist, and then the chemistry came to me. They instituted a major, and so I became a geochemist.

COHEN: That's attributed to Bob Sharp, too.

TAYLOR: Yes, right. Bob Sharp and Ian Campbell and Al Engel were the main people who went with the new trend. So almost overnight, Caltech became a major force in geochemistry as well as in geophysics and so forth. [Tape ends]

HUGH P. TAYLOR

SESSION 2

June 25, 2002

Begin Tape 2, Side 1

TAYLOR: I should have mentioned a couple of things last time. I forgot to say, in recounting my childhood education before I came to Caltech, that my mother and father got divorced in 1944. Almost immediately after we arrived in South Gate, there was a problem, and basically they split up. And my mother then, of course, was the sole support of us, the two boys.

COHEN: Your father just left?

TAYLOR: They split up, and it wasn't a very happy separation, either. She had a great deal of trouble getting him to pay child support; he only sent $40 a month. Of course, in those days $40 went a lot farther. But nevertheless, $40 a month was all he gave us, and some months that didn't come either. So it was very difficult. And my mother, of course, got a job and started to work. We went to school, but in the summer we were left on our own, which was OK; we had each other and so forth. But my mother just worked like a Trojan. I mean, she was really unbelievable. And we were fairly estranged from our father. I only saw him probably ten or twelve times after the divorce, for special events and so forth. He came to my Caltech graduation. But basically—

COHEN: He was not a part of your life.

TAYLOR: No, he was not a part of my life at all. But my mother, of course, was a key person in my life. And then the other thing I should say is that while I was attending Caltech I didn't get drafted. This was during the Korean War. And one of the reasons I didn't was because Professor Ian Campbell was the head of the draft board here in Pasadena, and he knew all the ins and outs of it. He wrote several letters for me to get a draft deferment, which worked. My brother didn't have that pull, or that clout, so he did go into the army. But it worked out extremely well for him, because he went to Europe; he didn't have to go to the fighting in Korea.

And he played football in Europe, and it really gave him tremendous confidence. He found out that he could really shine in the army. He was extremely well thought of and so forth. So when he came back, he went to UCLA as a history major and graduated in history and then got a master's degree in library science and became a librarian. For the rest of his life, he was the librarian at Cal State Long Beach; he was the head librarian there for many, many years. As I told you before, he didn't excel in grade school or high school—I think because he was in my shadow. But after we got separated, then all of a sudden, in college, he found himself and did quite well.

I was talking about my junior year at Caltech. That's when there was a tremendous change at Caltech. This was 1953. Late '52 to '53 was when Harrison Brown was hired. Caltech decided—mainly through, as I found out later, Bob Sharp and Al Engel and Dick [Richard H.] Jahns; they were the three professors, along with the geophysicists Beno Gutenberg and [Hugo] Benioff, who pushed for this—to go into geochemistry. Before that, Caltech had no expertise, really, in geochemistry, and actually very few universities did.

COHEN: I was just going to say it didn't exist in many places.

TAYLOR: Yes, very few universities had it. Harrison Brown was this bright young chemist at the University of Chicago, and they looked him over. He was very interested in the origin of the solar system and so forth. So Caltech hired him. When he was hired, he went around to various people he knew at the University of Chicago and asked them if they wanted to come out with him, because he was starting a geochemistry program at Caltech. He had a big contract from the Atomic Energy Commission, so he had a lot of financial wherewithal to get things started. He asked several people, including Sam Epstein, who was then a research fellow under Harold Urey at the University of Chicago, and Clair Patterson, who was also a research fellow at the University of Chicago, and Charles McKinney. They were the main nucleus that came out with him. So there was this so-called University of Chicago mafia that came out to Caltech and started geochemistry at Caltech. Harrison Brown was not only the head man, he was the only professor—the others started out as research fellows. But they were the scientific nucleus.

COHEN: Of course Harrison Brown didn't stay with the geology very long. He moved over to the social sciences.

TAYLOR: Yes, he moved to humanities, but actually Harrison Brown was incredibly important to the Division of Geological Sciences. He of course started the geochemistry program at Caltech, and then a decade later, because he had tremendous vision, he pushed to have planetary sciences brought here. He's the one who brought in Bruce Murray [professor of planetary science and geology, emeritus]. Harrison Brown had an unbelievable influence on the geology division at Caltech. The two major changes that took place in the last fifty years—namely, geochemistry and planetary sciences—are both heavily due to Harrison Brown. And then later on he moved to humanities. He was the foreign secretary of the National Academy, so he was by far the best known of all the people involved. But of course in terms of actual scientific productivity, he was more of a scientific statesman than a researcher. His scientific contributions weren't all that great after he came to Caltech, but he was certainly tremendously important. His importance should not be discounted in any way.

That was extremely significant in my history, because I had just chosen and gotten involved in the geology option, and all of a sudden all these geochemists came here from the University of Chicago. There was all this hustle and bustle in the basement of the Mudd building [Seeley W. Mudd Laboratory of the Geological Sciences], and I would go down and interact with them while they were building the new labs and everything else. It was very exciting, a great turn-on. As I think I mentioned last time, when you were an undergraduate in geology in those days, you were treated like a member of the family; you were treated as well as graduate students were. That's when I got to know Sam Epstein. Harrison Brown started a course—the first real geochemistry course—called The Nature and Evolution of the Earth. He gave several lectures and he had guest lecturers. Sam Epstein gave a lecture on oxygen isotopes, which was his main thing. Those of us who took it for credit had to write term papers. I wrote a paper on oxygen isotopes, and Sam Epstein was the one who looked at it and graded it. I was lucky to get in on the ground floor.

When that Chicago contingent came, the serendipity of it was just unbelievable for me, because, as I might have mentioned last time, I switched my major to geochemistry and I became, along with another person, Gordon Seele, one of the first two geochemists to graduate as undergraduate majors at Caltech.

Now, I should say another thing about the education at Caltech. It was magnificent in several ways. First of all, one of the most beautiful things about it was that they forced you to take twenty-five percent of your courses in humanities. If I had been left to my own devices, I probably wouldn't have done that, because I was into this business of taking as many math and science courses as I could. I had never had any Shakespeare before, and I was forced to take Shakespeare. I was forced to take literature courses with Kent Clark and so forth. It opened my eyes—

COHEN: A new world.

TAYLOR: Yes. I had always been somewhat interested, but it changed my focus. To this day, if I had not had that course in Shakespeare, I don't think I would have been in any way as educated as I am. I love going to Shakespearean plays and so forth, and I always have. Also, you were required to take languages; in those days, you still had to have two languages for a PhD. That's all been eliminated now, but that was extremely important, too. I had Spanish in high school, and I took German here at Caltech and I also took some Russian at Caltech. I never took any French, but I picked up enough French to pass the master's degree exam when I went to Harvard. I learned a little bit of French. But I thought a sprinkling of languages was extremely important, and now in my dotage I'm taking a French class; we have a French class that meets in our house every week. You can't really be educated unless you know a foreign language, and I think French is delightful. So that's an important part of my Caltech education.

Another part is the athletics. Nowadays I don't think athletics is very important at Caltech, but when I was an undergraduate it was *very* important. We had a big-time football coach with a big-time football program. We had good uniforms. The assistant coach, the line coach, had been an All-American at the University of Kansas. As I mentioned last time, Bert LaBrucherie had been a big-time football coach who took UCLA to the Rose Bowl just three years before he came to Caltech. Caltech, I guess, decided to go first class. We played our home games at the Rose Bowl, and the universities we played against were Occidental, Whittier, Pomona, and so forth. Some of the coaches in those schools later on—like the coach at Whittier, when I was playing—later went on to coach the Los Angeles Rams and the Washington Redskins and the Chicago Bears. Jack Kemp, who later ran for vice president with Bob Dole, played for Occidental and later starred with the Buffalo Bills in the pro league. Of course it was small-time football and we didn't have athletic scholarships—

COHEN: You also didn't have the spring training.

TAYLOR: Well, we did have spring training. LaBrucherie ran it like a big-time program, as I said. We came to school two weeks before all the other students in early September. We had spring football. We did all the things that major football schools did. We didn't go very far for road games, but we went to San Francisco, we went to Flagstaff, we went to San Luis Obispo. I would never have been able to play football at a big school. I have a lifelong love of football. And as a matter of fact, in my retirement—

COHEN: Are you going to play football again? [Laughter]

TAYLOR: No. I'm planning to write a book, though, about college football, if I stay healthy. I also played basketball. I wasn't very good at it, but I played freshman and junior varsity basketball. Fred Anson, who was in my class and is a professor here, who just retired also—he was a tremendous star.

COHEN: He has the height.

TAYLOR: Yes, but he was also an extremely good basketball player. In our senior year, we won the conference championship. Caltech actually won! They were better than Occidental, Whittier, Pomona, and all those schools.

COHEN: They don't boast about it very much.

TAYLOR: Well, it's too bad. Of course that was the last major sport conference championship that Caltech ever won. And the school spirit around the football games, when we won, and the basketball games, when we won the conference championship, was tremendous. That was a great thing about being an undergraduate here, I thought.

COHEN: They didn't have to worry about people keeping their grades up?

TAYLOR: Well, it was hard, because you had to go to practice and so forth, but we always managed. That's one other thing I should say about the education here. The other really important thing about it was that in the sophomore year you were required to take geology, biology, and astronomy. And I think those three cultural courses were extremely—of course, that's the reason why I became a geologist. The biology course was taught by Norman Horowitz, and in the lab was George Beadle! So, I mean [laughter]—

COHEN: It couldn't have been better!

TAYLOR: Yes. Just one single nine-unit course for one term lays the groundwork for all the rest of your life. I'm really sorry that those cultural science courses were eliminated later on, because year upon year of undergraduates went through Caltech without ever getting any biology, any geology, or any astronomy.

COHEN: Has that been corrected by the new core curriculum, in some sense?

TAYLOR: I think the biology course is required, which is good. There's no reason they shouldn't have one required course in those sciences for everybody. I'm really sorry that several decades of Caltech students didn't have this, but that's just the way things go.

COHEN: Now there seems to be such an emphasis on writing.

TAYLOR: There was heavy emphasis on writing then, too. We had term papers in the science courses, and we had, of course, lots of term papers in the English and history courses, the humanities courses. Later it gravitated more toward labs and problem sets, as opposed to writing. That was probably a mistake, too, because certainly learning how to write is very important.

COHEN: Of course, you didn't have all those foreign students here when you went to school.

TAYLOR: That's true, too. One other aspect of the education at Caltech that I think was really good—in those days, first of all, we took lots more courses than they take now. The typical undergraduate load then was fifty-three units or something like that. Now I think it's down around forty-five units or forty-six units. That may not sound like much, but also there were a lot of three-unit courses and six-unit courses then. There are hardly any of these kinds of things now. So in terms of numbers of courses with different instructors, you had a lot more. I would have, say, two or three three-unit courses and maybe three six-unit courses, plus the bigger courses.

There was a lot more diversity in the education then than there is now. For instance, as an undergraduate in geology I took a lot of field courses. I had to take regular surveying in the civil engineering department. I had to take plane table surveying in the civil engineering department. I had to take one full year of field geology during my junior year, and then a seven-week summer field camp geology course during the summer after my junior year. And then, if I had stayed a geology major, I would have had to have taken one full year of field geology my senior year, in which you go out every weekend. Caltech in those days had probably the most rigorous field geology program of any school in the country. One of the things emphasized in the catalog was that because of our position here in Southern California, where we had good weather even during the winter and so forth, we were able to do this. Also we have good rock exposures, because of the dry climate. So it was a paradise to learn how to do field geology. And we kept that strong, but over the years it's been peeled away little by little. Now undergraduate geology majors here only take the one year of field geology their senior year, plus a watered-down summer field camp, and that's basically it. Even so, it's still more than most schools have. But that was extremely important to me. Because I switched to geochemistry, I had to take a bunch of chemistry courses my senior year, so I started out with that advanced field geology, but it just got too much for me, with football and everything else. So I dropped out, but at least I had a few weekends out in that more advanced class. So when I left Caltech, even as a geochemist, I felt I was trained as a professional geologist. And the same thing was true using the microscope in petrography, in Ian Campbell's petrography courses. When you graduated, you felt you had really almost a professional type of training. You felt confident that you could go out and do things. They gave you tremendous confidence. Later on, when I did go elsewhere, we were way ahead of the business, just because of the training. It was superb.

All right. So here I am, in my senior year, and of course all seniors are excited—

COHEN: What do I do now?

TAYLOR: What do I do now? I kind of wanted to stay at Caltech. [Laughter] But there was very, very strong pressure. In fact, they told me I couldn't stay, that I had to go somewhere else, which was also extremely good advice. So I applied to three or four places, but the place I really wanted to go was Harvard, because a lot of my buddies in chemistry and physics were going to Harvard, including Fred Anson and a number of others. At that time, Harvard had the big name, and it also was very, very good in chemistry, physics, and geology. So a whole group of us—it must have been about eight or ten of us from that relatively small class of ninety or ninety-five—went to Harvard. [Laughter]

COHEN: Almost ten percent, you're saying.

TAYLOR: Yes, something like that. We ended up as first-year graduate students at Harvard. One was in law school; he graduated here in physics, but he went into law school. So we had a group of Caltech people and we socialized a lot together at Harvard. It was a nice thing. But the reason I picked Harvard—and also they picked me, because I had a good record; I got a fellowship—was that we had a graduate student here at Caltech who taught me petrography whose name was Fred Barker. He had been at Harvard and he knew the professors there. And there was a relatively unknown professor at Harvard named Jim [James Burleigh] Thompson. At that point he had not published anything, and very few people knew about him. Because he didn't publish, you had to actually go to Harvard and attend his lectures. And this graduate student—again, it was one of these things where graduate students are so helpful to undergraduates—told me I had to go and listen to Thompson's lectures. Well, that was the best advice I ever had, because Thompson was one of these guys who was just an absolute genius. And every lecture he gave was not just a regurgitation of textbooks but included something profound and original. So I ended up going to Harvard and taking his course and also the course of another real superstar there, Francis Birch, and some others. But I took mainly those geology courses and two physics courses, plus a thermodynamics course in the chemistry department and a calculus course in the math department. So I really didn't have that much interaction with the geology group. Most of the other geology professors at Harvard I didn't consider to be all that good. But Birch and Thompson, both of whom became National Academy members later on, were superb. And the students they both attracted—that was the other great thing about Harvard, there were so many great students. [Laughter] Actually, a lot of the professors were maybe not so great, but the students were. They had these two great superstar professors, Thompson and Birch, plus all these great students, many of whom became lifelong friends. One of them was Arden Albee [professor of geology and planetary science, emeritus], who later came here. And many of them became superstars later on. It was a great choice for me. I expanded my abilities in thermodynamics and mathematics as well as being energized by this geophysicist Birch and this petrologist/geochemist Thompson. Thompson almost didn't get tenure at Harvard because he didn't publish, but he finally did, which was good. They realized he was a genius. So that was a perfect situation. But after my experience at Harvard, I was desperate to come back. [Laughter] I mean, it rained twenty-one days straight in April, and things like that, so I didn't really enjoy—

COHEN: The weather? [Laughter]

TAYLOR: Yes, because I didn't feel free. I like to be able to do things whenever I want to do them, and you can do that here in Los Angeles. So I wanted to come back to Caltech, and of course they were willing, because I had a good record here and I had done my away time for one year—I got a master's degree at Harvard. So I applied to Caltech, specifically to work with Sam Epstein in oxygen isotopes, which was the subject of the term paper I had done there.

Now, I should digress and go back a little bit. The summer I graduated from Caltech [1954] there was another professor here, a professor of ore deposits, named James A. Noble. I don't know if you know that name, because he resigned in 1960, I think. Jim Noble was a classical geologist of the old school, OK? I don't think he was very happy about all these geochemists coming, because he didn't consider them to be—

COHEN: Real? [Laughter]

TAYLOR: Yes. It wasn't his cup of tea, so to speak. He was a nice man, and very much of a gentleman and so forth, so he never really said much. But I think he basically—there was a schism in the department between the old-school people and the younger people, who were marching in a new direction. My interactions were very good with both camps, if you want to put it that way, because when I was a senior I got involved with Jim Noble. One thing about

Noble was that because he was in ore deposits, he had all these connections with industry. In particular, he was a consultant for U.S. Steel. At that time he was in the process of trying to open up southeast Alaska for iron exploration for U.S. Steel. They knew there were some big iron ore deposits up there, but nobody knew anything much about them. U.S. Steel hired Noble as a consultant to try to figure this out. Well, to do that he needed some students, and he had a couple of graduate students who were going to do their PhD theses up there in southeast Alaska, but he also needed some other people. So he hired me, along with a new graduate student, John Ruckmick, and another undergraduate, Tom Plambeck, to go up there and stake claims and map one of these iron ore bodies in southeast Alaska. That was just a wonderful experience for me, because, first of all, we were landed on this isolated peninsula way in the wilds of southeast Alaska and we were supplied by boat every three weeks. We basically had to operate on our own and go out and map and stake the claims for this iron ore deposit. So the whole summer I spent doing that, and it was great. We did a good job for Noble, and all the stuff we did worked out pretty well.

So the next year, when I was getting ready to leave Harvard and come back to Caltech as a graduate student, again I was in a great situation, because the director of the United States Geological Survey at that time, named Bill [William T.] Pecora, a really great guy, had lots of connections at Harvard. He came by Harvard and I got to know him. He wanted me to come to work for the USGS that summer in Montana, and that would have been a great opportunity, too, because Pecora was a big name in the field. I was almost ready to do that, but then Noble called me up and said, "How would you like to come back to southeast Alaska and not be stuck on the ground but be in charge of this yacht?" [Laughter] I mean, here I am, a second-year graduate student, and he says I can be in charge of this yacht. It was a fifty-five-foot boat that people like Bing Crosby would rent out at exorbitant rates to go up there and go salmon fishing and sailing.

COHEN: So it was a luxury yacht.

TAYLOR: It was a luxury yacht. It was unbelievable.

COHEN: And you were in charge of the crew?

TAYLOR: Yes, right. So I went with another student—a friend of mine, Dick [Richard L.]

Nielsen, who was also a graduate of Caltech—but I was in charge, because I'd had the previous year's experience, which wasn't much. That whole next summer we spent cruising up and down the Inland Passage, which now people pay money to do. [Laughter] As a matter of fact, for the first time in many years, I'm going to go back this summer with my wife and my kids to take one of these week cruises. It will be interesting to see what the area looks like now. That was a spectacular summer and I got a lot of experience doing geology as well. I saw every place in southeast Alaska. This was the summer of '55; and then the next summer, the summer of '56, we also went up again, and I did the same thing again.

COHEN: Meanwhile you were back here in Pasadena during the year?

TAYLOR: Yes, I was back here. Most graduate students in a situation like that would be spending their summers doing their PhD thesis.

COHEN: But you were cruising in Alaska. [Laughter]

TAYLOR: Yes. I decided I could do my thesis on the side, so to speak, because I enjoyed doing this. Also I got some published papers out of it, because we discovered a new kind of intrusion, a differential ultramafic intrusion called a zoned intrusion. They occur in only two places on Earth—in southeast Alaska and the Ural Mountains in Russia. Noble and I wrote a paper on them for the International Geological Congress. So that was a good experience. And then there were a couple of good theses done on them here at Caltech—

COHEN: On that work. Did they ever go and mine that stuff then?

TAYLOR: No, it's still sitting back there.

COHEN: It's still pristine?

TAYLOR: What happened was—and it's not all that interesting—the iron ore had too much titanium in it for the metallurgy and so forth, so it's still sitting there. And at that time, they found these huge mines in Australia and Venezuela and places like that which were much, much easier to mine and had no metallurgical problems. So basically what happened was that other discoveries of even more giant deposits in other countries made these Alaska deposits uneconomical. Ultimately I'm sure they will be mined, because once those other deposits are gone, there's a huge amount of iron ore up there. It's just a little bit less economical to work with. The beauty of it, though, is it's right on tidewater, and so the shipping costs are very cheap. That's one of the key things, to be able to mine it and put it directly into big ships. That's very important. There's no land transportation problem there.

But anyway, so here I am, back working with Sam Epstein and starting on my thesis on oxygen isotopes. At this time, nobody knew much about what the oxygen-isotope distribution was in nature, in anything. So Sam just proceeded to analyze the oxygen-isotope ratios in almost everything in nature. I mean, waters—

COHEN: In anything he could get a hold of?

TAYLOR: Yes, in basically everything, because almost everything has some oxygen in it. And almost every process—particularly processes that take place at low temperatures, like room temperature and so forth—fractionates the oxygen isotopes, ^{18}O and ^{16}O, by easily measurable amounts. That was the key thing he had designed and formulated at the University of Chicago and brought here to Caltech—a mass spectrometer able to measure these fractionations accurately enough so that you could easily measure the isotopic variations in nature. So Sam basically analyzed everything he could get his hands on: glacier ice, waters, air in the atmosphere, carbon dioxide in the atmosphere, plants, animals. You name it—every conceivable kind of rock. I wasn't interested in plants and things, my specialty was rocks, so I focused on those, but he was doing everything. And of course when you're making measurements for the first time on something, you always discover things—that was the beauty of what he did. He had this intuitive feeling, which was certainly proved, that if you keep on analyzing new things, you're going to find something. Some things turned out to be incredibly interesting, and then he would put a student on it and pursue it—

COHEN: Then he'd go somewhere else.

TAYLOR: Yes, he was always moving around. He was a very creative man. For instance, on my trips looking for rocks I would collect trees for him, because he was interested in tree rings and things like that. And I ranged all over the world—

COHEN: So he didn't have to go. [Laughter]

TAYLOR: Yes, right, and I was happy to do that. It was a great experience working with Sam. And the other thing about it was that he allowed me… [Laughter] A lot of thesis professors might have said, "What are you doing going up to southeast Alaska every summer? Stay here, and work." He never did that. He supported me full time. You know, I could go out—and actually I made a lot of money in southeast Alaska, too, which was important to me at that time. For the first time in my life I was economically able to do things; I got a pretty good salary during the summer, and it was a lot of fun, too. He knew when I was here I was working hard. But there was this problem, because I was with Noble. [Laughter] And they did have a problem, those two professors. By this time Sam was a professor. Noble felt more and more and more isolated from the rest of the faculty, because he was being passed over, and finally he did resign from Caltech. I think he resigned in a fit of pique, because he was a big name. I mean, he used to bring lots of really good Canadian students here who wanted to come and work in ore deposits. And he must have thought, "Well, they're not treating me very well, so I'm just going to resign, and that will hurt them." But it didn't hurt. [Laughter] Well, maybe it did. Maybe we got fewer good students from Canada and so forth—but it didn't really hurt us.

And about that time Dick Jahns also resigned and went to become a dean at Penn State. And Al Engel left and went to La Jolla. During the late fifties, Caltech geology was completely transformed from a classical department that emphasized mapping and old-school stuff, plus the seismology, to a completely forward-thinking new set-up—I mean, with [Gerald J.] Wasserburg coming in and so on. And they hired Lee Silver and Clarence Allen and so forth. I was a graduate student here at the time of that great change.

The other thing was that when I came back from Harvard, besides going off to southeast Alaska during the summertime, I also was an assistant coach for LaBrucherie in football. I helped coach the freshman football team, and I was an assistant coach for the varsity football team during that time. And then the next year—I loved the student houses so much when I was an undergraduate in Dabney that I applied to be an RA, a resident associate. So I became the resident associate in Ricketts House. I didn't want to go back to Dabney, because I had already done that, and Ricketts was, in my opinion, the other really good house. During the next two years that I was a graduate student, I was the RA at Ricketts, and I was in a way reliving the wonderful things I had done when I was an undergrad. [Laughter] I really did enjoy it.

And all the time I was doing my thesis with Sam, which turned out to be a pretty important thesis. I was lucky there, too, because it was the first study of minerals in igneous rocks and involved trying to understand what oxygen isotopes had to say about igneous processes and so forth. It led to a lot of nice things, which I can talk about later on.

So I finished up. The last year of my PhD thesis, I decided I couldn't fool around anymore, so I resigned as RA and moved into the sleeping porch of the Athenaeum. That was ideal, too, because first of all, it was $12.50 a month, or $15 a month, something like that. And you had a bed. It was a nice situation. I would work until three or four in the morning, then go to bed. I would sneak in, because all these other bunks were up there. And when I got up in the morning at about ten or ten-thirty, nobody was there, so I had the whole place to myself to shower and get ready. And when I'd come back to the Athenaeum at three or four in the morning, I'd sit down in the lounge—

COHEN: Nobody was there.

TAYLOR: Right. I had my own mansion. [Laughter] I was totally focused on getting my PhD.

COHEN: So you were working all the time.

TAYLOR: Yes, I was working all the time, although it wasn't that hard, because I had this big hi-fi stereo up in my lab. I was on the roof. [Tape ends]

Begin Tape 2, Side 2

TAYLOR: Well, by this time I had met my first wife at an exchange dance at the Athenaeum. She was a student at Occidental, and she came over for an exchange dance, and we hit it off right away. She had started out as a geologist, but she was now a history major at Occidental.

COHEN: So she knew names of rocks.

TAYLOR: Yes, right. And also we just hit it off. We were going together during the two or three years before I got my PhD, and we got married at the time I got my PhD, in 1959. Now, at that time, as I said, there were all these resignations. Jim Noble resigned. Ian Campbell left to become California state geologist. Dick Jahns left to become a dean at Penn State. Jahns and Bob Sharp had both been undergraduates together here at Caltech. Jahns graduated, I think, in 1935; Sharp graduated in 1934. So they knew each other as undergraduates and then later on they were faculty members together. There was always a little bit of creative tension between Jahns and Sharp. Jahns would like to tell these stories; in fact, he was a great raconteur. And Sharp was also a good raconteur, but if you heard the same story from the two of them.... Sharp would always talk about the Jahns factor—that he was always exaggerating. [Laughter] I don't think Jahns really liked hearing about the Jahns factor.

Anyway, Bob Sharp, who was the chairman then, was going through somewhat of a crisis, because all of his professors in geology were leaving and he needed some teachers, but on a temporary basis. So he made a proposal to me when I graduated. It wasn't to stay on, because, again, they don't want you to stay on. Clarence Allen went away and then came back. Lee Silver did stay on, but he was the only one. And Barclay Kamb [Rawn Professor of Geology and Geophysics, emeritus] stayed on, but Barclay Kamb's thesis was in chemistry, so it was slightly different. But anyway, they didn't want me to stay on permanently. They weren't going to hire me permanently. They needed a teacher—

COHEN: Some body.

TAYLOR: Yes, they needed a body. And that was ideal, too. [Laughter] So they wanted me to stay on and teach on a temporary basis that next year, and I did. Also, they needed somebody to teach summer field geology. So here I am, a geochemist, but I had had this good background, and I enjoyed it so much. So the next summer, the summer of 1960, I taught summer field camp, summer field geology, in southern Arizona with a group of about twenty students. Heinz Lowenstam came as my assistant, so to speak, but I was in charge of the whole thing. And that worked out nicely, too. I really enjoyed myself. And then I stayed on and continued to do my research, because the kind of thesis I did was the kind of thesis where you just keep on doing more and more isotopic analyses. After I had done igneous rocks, I got involved in doing meteorites, because there was a big set of meteorites here at Caltech and a lot of people were interested in meteorites, including Harrison Brown. Nobody had ever analyzed oxygen isotopes in meteorites before, so we started to do that, and we found a number of very interesting things which later on turned out to be incredibly interesting. I didn’t discover them myself. Bob [Robert N.] Clayton, who was Sam Epstein’s first student and who went to the University of Chicago, was the one who really did that kind of stuff. But we nevertheless were the first ones to analyze meteorites and we led the way, so to speak. So I was doing a lot of nice things, and I had a going lab.

COHEN: And you had just gotten married, I take it.

TAYLOR: Yes, I had just gotten married. We rented a house in South Pasadena. I taught some courses and taught summer field geology. By this time, I had to think of where I was going to go permanently, because the Caltech job was a temporary job. This professor, Dick Jahns—whom I will talk about next time, because he was really an amazing individual in terms of his interactions with students and so forth, and just a terrific guy—we used to go out and play touch football together out on the athletic field. He and I got along really great, and by this time he was a dean at Penn State, in the school of mineral sciences, and they were looking for a young.... Because actually, Bob Clayton had been a professor at Penn State and had been there for a couple of years and then left and went to Chicago. Penn State was a place that had more really good people come in, work there for a while, and then leave—

COHEN: It’s in the middle of nowhere.

TAYLOR: It’s right in the middle of the state of Pennsylvania. In fact, that’s how it was spotted. It’s a land-grant university. When they decided where to put it, they drew a cross through Pennsylvania and they put it right in the middle of Pennsylvania. [Laughter] It’s crazy. But nevertheless, that was both good and bad. It was so isolated, and it was particularly isolated then, because there were no freeways nearby. It was basically four and a half hours to get to any big city: four and a half hours to Pittsburgh, four and a half hours to Washington, D.C., four and a half hours to Philadelphia, four and a half hours to New York, no matter what you did. And during the wintertime you were stuck, period—you couldn't go anywhere.

But anyway, Dick had gone there as head of the school of mineral sciences, and they were looking for a replacement in isotope geochemistry to continue to pursue the things that Bob Clayton had started when he was there. So it was a nice up-and-coming university. The vice president of the university was E. F. Osborn, who had been a Caltech PhD, so there was a big Caltech contingent there. One of the biggest names on the faculty was Wayne Burnham, who had been a student of Jim Noble's and who had also been with me in southeast Alaska, so I knew a lot of the faculty there and it seemed like a good place to go as a permanent job, particularly with Dick Jahns as the head. So I signed up that summer, so Caltech knew that I was going to be leaving at the end of the year.

January of 1961 is when I finished up at Caltech and drove back, in the middle of winter, to Penn State and started my career there, fully intending to stay there. I got an NSF [National Science Foundation] grant and started up a little bit of work, but some very strange things went on. Maybe I shouldn't have behaved the way I did—I mean, particularly to someone like Dick Jahns. But what I did was, I had gone there in January. And Sam offered me the opportunity—because I had a going lab at Caltech, I had an extraction system that was able to just churn out data. He said, "Why don't you come back here during the summer to Caltech and just continue on?" It was going to take some time to get my lab going at Penn State, so I could be much more productive at Caltech. And also I just wanted to do it. [Laughter]

So I asked Dick Jahns, "Is it OK if I go back to Caltech, where I can really make hay and really get something done real fast?" He said, "Fine!" So here I am, a professor at Penn State, spending the whole first summer back at Caltech. Again, it was very productive, because I had a working lab; it was an ideal situation. But it wasn't very happy for Penn State, because probably I should have been there getting my lab going, instead of doing this. Nevertheless, that's what I did. [Laughter] And it worked out OK. Then, in the fall we drove back East, my wife and I. We arrived literally at the kickoff of the first football game, because that was another important thing about Penn State; I used to go to—

COHEN: Real football.

TAYLOR: Yeah, real football. On a fall afternoon in University Park, Pennsylvania, at Beaver

Stadium, it was just wonderful. And we of course had season tickets. There was a group of us, including Dick Jahns and a number of the other faculty, and then we'd go to the country club afterward. It was an idyllic existence. So we just got there just in time for kickoff.

I spent the next two or three months of that fall term teaching and getting my lab going. I had an NSF grant now. But a very interesting thing happened there, which will take a few minutes to tell, but it's interesting in terms of what it's like to be at a place like Caltech versus what it's like to be at a place like Penn State. It will take a few minutes to develop this, but I think it's worthwhile telling because it does involve a lot of Caltech people.

COHEN: Go ahead.

TAYLOR: When I first went to Penn State, they had a very clumsy structure. They had a mineralogy and petrology department—that means rocks and minerals—they had a geophysics and geochemistry department, and geology was a separate department. In other words, the things Caltech had all in one department, they had in three separate departments at Penn State. And they had quite a large faculty—bigger then Caltech's.

COHEN: Well, I think they have 20,000 students or something.

TAYLOR: Yes. They had quite a large faculty in those three departments, and Dick Jahns was the head of all these three groups. Now, of those groups, the geochemistry department was the one that was really doing great. They had the young stars, and they were doing lots of really exciting work. And basically it was because of Osborn, this Caltech PhD who was now the vice president at Penn State. He was the one who galvanized the department. He got a bunch of people in there and of course my hiring was a part of that. Wayne Burnham, this other Caltech PhD, was in there, and some other good people. Now, I was hired in the geochemistry department; I was an assistant professor of geochemistry, and that was the up-and-coming department. The other two departments were somewhat moribund. They were OK. Geology was just kind of a backwater, and mineralogy and petrology had a couple of good old people, but not much. So the real action was in the geophysics and geochemistry department. By the way, the chairman of that department was also a Caltech PhD. [Laughter] There was a tremendous Caltech influence there.

Now, at that particular time they did things in a very funny way at Penn State. A lot of the hiring was done just by Osborn, the vice president, more or less all by himself. If they decided they wanted to hire somebody, they'd just go out and hire somebody; they wouldn't have a real search. But we had a faculty meeting of the geophysics and geochemistry department and in it, during the discussion, another professor of geochemistry, Rustum Roy, had proposed as a faculty appointment his assistant—who was OK, but it was really ridiculous to propose your assistant as a full-fledged faculty appointment. Nevertheless, we discussed it a little bit. And I, who was brand-new at the whole business; Wayne Burnham, who was my good friend; and another young professor, Hubert Barnes—we were the three young Turks, if you want to put it that way, and we thought maybe we should have a real faculty search. We said, "If we're going to make a new appointment in geochemistry, why don't we look around the country? Why don't we have a search?" And I remember that the chairman, Ben [Benjamin F., Jr.] Howell, said, "This is really interesting." He was kind of a funny old guy. He said, "This really is. This is the first time we've had a real discussion like this." [Laughter] Anyway, I didn't think anything more about it. And I was working late in my office that night, and Osborn—whom I knew and had talked to, but we never really said much of anything—poked his head in and asked how I was doing, and small talk like that. I didn't think anything more about it. Then the next day, Dick Jahns comes in and tells me that I have been transferred to the geology department. At the same time, Wayne Burnham and Hu Barnes were also transferred to the geology department, OK?

Now, Dick was one of these guys who never want to confront a problem head on. What a lot of people would have said—in fact, they did say—"Well, you should have resigned immediately when this happened." But he never liked to work that way; he wanted to quietly work behind the scenes. And what did I know? I was a young assistant professor. He said, "Just cool it. This will all work itself out." And I had a lot of confidence in Jahns, because I liked him a lot—he was a great friend. And I didn't care; I was just as happy to be in the geology department. It didn't make any difference to me whether I was in the geochemistry department or the geology department. I didn't know that the geology department was the dregs and the geochemistry department was much better. But Wayne Burnham and Hu Barnes went crazy. Burnham and Jahns had been so close, you see; I mean, their wives were close friends and everything else. Burnham was furious at Dick Jahns for not standing up to this. Later on I realized that this kind of abrupt transfer of faculty was almost unheard of at an academic institution. Wayne Burnham had tenure—he was a tenured associate professor. And he told me at the time, after it happened, that he went through his files to look for his papers appointing him with tenure, and they had disappeared from his files, or something like that. So some very, very funny things were going on.

In any case, Jahns tried to smooth it all over. It had all been done by Osborn and this other professor, Rustum Roy, who thought we three were trying to interfere with his department, with the hiring and so forth, trying to change everything. And the way they solved it was by shoving all three of us out. [Laughter] To me it was unbelievable. I hadn't been there that long; it didn't bother me that much, and also I'm somewhat fatalistic about things. I was happy to be called a geologist, so it didn't matter. But Wayne and Hu demanded a meeting with the head of the whole school of mineral industries, who at that time was only a temporary person who had been put in because the previous person had gotten sick with Parkinson's disease and had to resign. And he was somewhat of a nonentity. He was basically just a figurehead stuck in there by Osborn. But anyway, we had this meeting with him, and I remember it just like it was yesterday. The three of us were sitting around a table and here were Hu Barnes and Wayne Burnham and I, and Wayne and Hu were just furious. They were saying, "How could this happen?" And this poor guy, who was a coal petrologist or something, didn't really understand any of this stuff but had been told to put the lid on it, to take care of the problem. He had to keep these guys in line. And finally he lost his temper and he said, "Don't you guys understand that we're management and you're labor?" [Laughter] Then I was stunned. I couldn't believe what I had heard. Right that instant I decided, "If I can get out of here, I'm getting out of here." And then soon after that, Caltech offered me a job. Again, serendipity. I still felt I owed a lot to Dick Jahns, but I had to come back here. I kept on coming back.

COHEN: You really never left. [Laughter]

TAYLOR: Yes. In December of that year, the AGU, the American Geophysical Union, had its first West Coast conference here in Los Angeles. I was giving a paper there, so I had to fly back from State College. Now, at that time, it was touch and go leaving Penn State in the winter, because you had to go up on the plateau behind State College to go to the airport, and I just barely was able to get up there, because of the weather, the snow and so forth. It was problematic whether I could even get out; otherwise you had to try to take the train to Pittsburgh or something like that. Anyway, I came back here from State College, and it was one of those beautiful winter days in Southern California. I immediately just stuffed my acceptance in the mailbox. [Laughter] And also, Bob Sharp had been sending me postcards showing oranges growing on trees. It was a no-brainer. So here I was. I had spent nominally a year and a half at Penn State, but a good part of the time I was actually back here at Caltech. [Laughter] And by the time I had had a little time to set up a lab at Penn State, I knew I was coming back, so I didn't want to put a lot of effort into setting up a lab there, because I had already accepted the job back here. So basically I never really got started at Penn State. So I didn't do them a good service.

COHEN: Well, it sounds like they didn't do themselves a good service.

TAYLOR: But I was very productive. I got a lot of research done during that time period. So it worked out great for me. And then of course soon after that Dick Jahns resigned and became the dean of mineral sciences at Stanford. [Laughter] So it couldn't have worked out better, because if I had stayed there, Jahns would have abandoned me. I came back to Caltech, and on the way back my wife and I drove all over Canada and the U.S., because at this stage it was a question of getting lots of rock and mineral samples from different areas for my research.

COHEN: Ah, so you went collecting.

TAYLOR: Yes, we went collecting and came back loaded with samples. And my lab was here. And of course Sam Epstein was like he always was.

COHEN: Waiting for you.

TAYLOR: Yes, he was helpful and so forth, and he allowed me to move off on my own. I got a lot of good experiences out of my time at Penn State. I feel kind of guilty that I didn't give them back as much as I should have.

COHEN: OK. Well, tell us the bear story.

TAYLOR: The bear story, yes. This is in southeast Alaska, my third summer up there. We were actually on the ground near Haines, Alaska, which is right near Skagway, in the back country there. Now, on various different islands there will be only black bears or there will be grizzlies or there will be browns, OK? And the ones you have to worry about are the browns and grizzlies, particularly the grizzlies. But the black bears are not really a problem. They don't usually cause people any harm, unless there are cubs involved. But we were in this area near Haines, where the habitats of all three kinds of bears conjoined. We knew they were there. We were walking through and you'd hear them—because southeast Alaska is full of brush, dense brush, and you can't go through the brush without making a lot of noise. So everywhere we'd go we would make noise, and very often you'd *hear* noise, off in the brush. The fact that we were making so much noise, I think, scared them, because they really weren't interested in getting close to human beings. They wanted to give us a wide berth. But we had no idea how many bears were out there. Certainly you could see bear droppings everywhere, and we knew that the grizzlies and the browns were there, and none of us carried rifles, so we were totally at the mercy of these bears, but we never saw them. I saw several black bears; in fact one time we came up a dead fall and right in front of me this big black bear comes up and says *Woof!* [Laughter] Well, I just ran with another guy as fast as I could in the other direction.

COHEN: Bears run fast.

TAYLOR: But in this one place where all these dangerous bears were, I never saw a bear crashing through the underbrush, and after a while I thought maybe there weren't very many of them. But then after we'd been there several weeks, we got a helicopter to take us up to some inaccessible places. And the first time I went up in the helicopter and looked down, there were bears everywhere! [Laughter] I mean, the number of bears was just incredible! [Laughter] It was the scariest thing I can remember, because they had been there all the time that we were crashing through the underbrush.

COHEN: So they didn't bother you. Another bit of luck! [Tape ends]

HUGH P. TAYLOR

SESSION 3

June 28, 2002

Begin Tape 3, Side 1

TAYLOR: This is my last day as a professor. A couple of weeks ago, I got a letter from [Caltech president David] Baltimore saying, "You've been promoted to Robert P. Sharp Professor of Geology, emeritus. Although this carries no stipend, we hope that you will continue your associations with the institute." [Laughter]

COHEN: Don't you still get a little goody, having a chair?

TAYLOR: Oh, there are lots of goodies. I'm still called the Robert P. Sharp Professor, and I have an office, a research fund, and benefits with the Athenaeum, and all the other things.

COHEN: I see. OK. Now, will they appoint another Robert P. Sharp Professor?

TAYLOR: Yes, they will. In fact, there's enough money in the account, I think, to make two professorships. That's what [division chairman] Ed [Edward M.] Stolper would like to do. Dr. Kerry Sieh is going to be the new Robert P. Sharp Professor. That's my understanding.

COHEN: Very nice. But you've got lots of chairs over in geology.

TAYLOR: Actually we don't have very many chairs that are controlled by the division. They are institute chairs, like Barclay Kamb's—well, before he retired he had an institute chair. And Peter Goldreich had an institute chair.

COHEN: Peter has retired also.

TAYLOR: Yes, or he is going to retire in September, I think. But the number of chairs that the division controls itself, in terms of the money and everything else—there aren't very many. The money for the Robert P. Sharp professorship was raised by Gene Shoemaker and Lee Silver, professors in the division, taking wealthy donors down the Grand Canyon on a boat trip. As soon as they accumulated enough—I think in those days it was close to a million dollars—then they had enough money for a professorship, and they named it after Robert P. Sharp, who was the most important person in the division's history. And I was the first Robert P. Sharp Professor.

COHEN: Good. I know that story. I think Lee Silver told it to me. They thought of it in an airport someplace.

TAYLOR: Yes, right. He and Gene Shoemaker dreamed it up, and I'm the lucky recipient.

COHEN: Well, that's good—a good choice. You know, there's something else I wanted to ask you. I don't know whether you've thought about it or not—of course we could do it next time—I'm really interested in the Moon-rock story, the whole sociology of it; you know, why the rocks came here, the competitiveness of getting the rocks.

TAYLOR: Well, we could talk about it a little bit today, but maybe that would be worthwhile for a whole session. Because the whole division really was involved in it, and Lee Silver played a major role.

COHEN: Well, I think there's a story there to tell. We can devote some time to that. And you wanted to talk about the work you're particularly known for.

TAYLOR: Well, OK, let me start. I was extremely lucky—I keep using the word "luck"—or it was a serendipitous choice of a PhD thesis, which was entitled "$^{18}O/^{16}O$ Ratios in Coexisting Minerals of Igneous and Metamorphic Rocks" [1959]. The oxygen-isotope studies. And the reason it was such a good choice is because I basically spent the rest of my life elaborating on it—because of course Earth and the planets and the solar system and much of the universe are all in large part made of oxygen, so you can study variations in oxygen isotopic composition in numerous compounds. And also I worked a lot with hydrogen isotopes and carbon isotopes and silicon isotopes. They are all major components of the solar system, and particularly of the Earth. And this was in conjunction with what Sam Epstein was doing: He focused on a number of generalized things; I focused on rocks. And together we developed here at Caltech, I think without a doubt, the premier place for studying stable isotopes in the Earth and the planets. So we attracted a lot of students together. That was very nice, and it's still continuing. We have a young professor named John Eiler who is going to continue on in stable isotopes, and Caltech will stay strong in this field. Of course Sam was much better known than I am, but together it was a good partnership without a doubt, over the course of all these years.

So my thesis was on $^{18}O/^{16}O$ ratios in coexisting minerals, and we were able to show that significant differences in the ratio occurred in these minerals. I was the first to show that you could arrange the minerals in a sequence of increasing tendency to concentrate ^{18}O in a rock. This correlated very beautifully with crystal structure and with bond strength and a number of other chemical things. We were the first to point this out; theoretically it could have been fairly well predicted, but we were the first to show it. And the idea was that since so many minerals contain oxygen, and a typical rock will contain, say, seven or eight or nine oxygen-bearing minerals—the idea from the very start, when Bob Clayton, who was Sam's first student, did his thesis—was to apply these oxygen-isotope techniques to measure the temperatures at which rocks form, because these fractionations of the oxygen isotopes are temperature-sensitive. And if you could show that the minerals crystallized together in equilibrium, and if the oxygen-isotope distributions were frozen in at the time they crystallized and didn't change thereafter, then you could measure the temperature at which the rock formed. And each mineral pair—like, for instance, quartz and magnetite, which are very common coexisting minerals in most igneous and metamorphic rocks, will exhibit a very large isotopic fractionation that's very easily measurable and temperature-sensitive. So each mineral pair gives you one temperature.

COHEN: Did you have to vaporize this rock before you could measure it?

TAYLOR: Well, yes. The important aspect of this thing was getting a relatively simple and straightforward technique for getting the oxygen in a form that you could put into a mass spectrometer and measure its isotopic composition accurately. The way this was done was to use high-vacuum lines and take the oxygen that was in the rock and somehow or other get it into gaseous form—just oxygen, O_2. And basically this has to be done quantitatively. So, for instance, here I have a piece of quartz, which is made up of SiO_2—silicon dioxide—and if I get *all* of the oxygen out of the quartz, then I'm absolutely certain that the oxygen gas I have here in the sample tube will have exactly the same isotopic composition as the original oxygen that was in the quartz—if I get it all, you see. If I don't get it all, because of the fractionations that occurred during the process, then you can't be sure. So the important thing was to develop a technique whereby we got it all. And the only chemical you really can do this with—get all of the oxygen out, because oxygen really likes—

COHEN: It's happy there.

TAYLOR: Yes. When anything burns, of course, it combines with oxygen, and that's a very stable structure. For instance, when hydrogen burns, it makes water, which is a very stable compound. And when silicon metal burns it makes silicon dioxide, and that's also a very stable compound. But there is one chemical, fluorine, which is the most noxious chemical on Earth. It's even more reactive than oxygen. And so if you put essentially anything—any material that contains oxygen—in a vessel with pure fluorine and heat it up so that it reacts, the fluorine will react with the compound and release all the oxygen. In other words, the fluorine will essentially replace the oxygen, because essentially every element likes fluorine chemically even more than it likes oxygen.

COHEN: I'm trying to imagine this, because fluorine is a gas in its natural state.

TAYLOR: Yes. And fluorine is a very, very poisonous, awful gas that most people don't like to work with. Now, one of the nice things about it, however, is that fluorine comes in big cylinders. They use it in industrial processes. And one of the biggest problems is getting a tank with relatively pure fluorine in it, because we didn't want to have any oxygen in the fluorine itself, which would contaminate our sample.

We wanted to develop a systematic technique that you could use over and over again. The key to geochemical analyses is to develop a simple elegant technique that works [laughter], and then you just do it over and over and over again. During a lot of my work I had some very good technicians, so I didn't have to analyze all the samples myself. Of course, when I was doing my thesis I did it all myself and made sure the techniques worked. But later on, a lot of the work was done by technicians, because it was pretty much a routine procedure by then, and all you had to do was make sure everything was continuing to work all right. It was a pretty simple technique. Basically, the problem with fluorine is that it's so reactive. I was explaining the initial problem of getting a tank of pure fluorine gas. A couple of the first tanks we got had lots of impurities in them, and Sam's idea was, well, maybe if we took it off the assembly line in the middle and didn't take one of the early tanks, we'd get a cleaner product. And sure enough, that worked.

COHEN: Was there a company right here in town?

TAYLOR: Well, there have been various companies over the years. Matheson was one. One interesting thing I've learned about science is that it depends on whether or not there is some commercial use for a material as to whether or not you can plug into the pipeline and get it. And in the early days I think they used a lot of fluorine in the atomic bomb project, because the separation of uranium isotopes was done using uranium hexafluoride. But in any case, there was a big group of commercial companies, among them Matheson and Air Products, who made fluorine commercially, so all we had to do was plug into that commercially available source and get a tank of this kind of material that was pure. And the beautiful thing about it was, these tanks are big—big cylinders of fluorine; the one I used for my thesis I was still using thirty years later. Once you got it hooked up and working, it lasted virtually almost a scientific lifetime. We had to replace it once in my lifetime with another tank of fluorine. And then, later on, when we started doing the Moon samples, we got a whole separate apparatus with another tank of fluorine.

So we had the pure tank, and once you get it hooked up, it's beautiful. It had to be put on the roof of the Mudd building, and again, that's one of the nice things about living in Southern California. We could put it on the roof. That's where our lab was; it was the penthouse.

COHEN: Was that for safety reasons?

TAYLOR: Yes. If the tank leaked, fluorine is a terrible gas. It eats into everything.

COHEN: It's corrosive.

TAYLOR: Yes, it's very corrosive. So you've got to be careful with it, and that's why a lot of people initially didn't want to start using it. But Sam wasn't scared of it, and he had a student who was willing to do it, because you could see that the payoff would be great, once you got the technique down.

So the first thing was overcoming the fear of using such a noxious gas. The next thing was getting it pure enough, OK? We solved that problem. Now, in order to get it to react at high temperatures, you have to put it in a vessel. But everything reacts with fluorine, you see, so you have to have some kind of container that won't just completely react with the fluorine. A good analogy is aluminum in air: Aluminum reacts very, very fast with oxygen, but what happens in air is that a piece of aluminum will form a thin coating of aluminum oxide, and that coating of aluminum oxide stabilizes it against any further reaction. It's essentially armored, because now the outside of the aluminum is a thin layer of aluminum oxide and the air has a difficult time going through the coating, so the aluminum is perfectly stable and happy. Actually, if you heat aluminum up to a high temperature, it will burn violently in air, but as long as it's at low enough temperatures, that oxide coating prevents further reaction. Now, through a search of the literature we chemists already knew that there is another metal, nickel—pure nickel—which also behaves that same way with respect to fluorine. It will form a nice coating of nickel fluoride, which is stable up to temperatures of at least 600° Celsius. As long as you work below 600°C, after the vessel forms this coating of nickel fluoride, then it's a perfectly secure container into which you can put your sample and let it react with the rest of the fluorine. So we had the apparatus made out of solid nickel bars. We drilled them out and attached them to a vacuum line. One part of the line had to be all metal; any part that came into contact with fluorine had to be metal of some kind or other, because if fluorine comes into contact with any kind of glass, it reacts with it and makes silicon tetrafluoride. Now, at low temperatures you could use copper. At room temperature, copper also forms a nice coating of copper fluoride and then stops reacting, so all parts of the line that wouldn't be getting heated up could be made out of copper—or stainless steel would also work. But any part that was to be heated up had to be made of pure nickel.

COHEN: You had to be careful not to go over 600°.

TAYLOR: Yes, right. Well, these were things we found out later on. It turned out that if you went up to 700°C, all your fluorine was used up, because it just reacted with the nickel. And if you kept doing it, your nickel vessel would be gone pretty soon. We were constrained therefore to work at lower temperatures. But nevertheless, most substances reacted very nicely at such temperatures before the nickel coating would deteriorate—and in particular quartz, feldspar, and almost every oxygen-bearing mineral worked very nicely. Olivine—Mg_2SiO_4—was very, very hard to get to react, however. Since olivine is a very important mineral, particularly in basaltic-type rocks, we would have to grind it up very, very, very fine in order to get it to react. In my studies I would have liked to do a lot of olivines, but I just decided, "Well, I've got all these other minerals that react easily, so I'll just concentrate on those." [Laughter]

COHEN: The path of least resistance.

TAYLOR: Yes. It wasn't until twenty or thirty years later that we really started doing olivines in a big way. It was just, you know, first things first.

I should spend a little bit of time discussing this apparatus in detail, because I used it, or some version of it, for the rest of my scientific life. First of all, the sample—like quartz or feldspar—had to be put into the nickel reaction vessel somehow or other. Now, if you did this in air, the nickel-fluoride coating reacts with water in the air and forms a hydrous material. And of course when fluorine goes in there, it reacts with that water-rich coating and makes oxygen, which ruins the samples. So you can't load the samples in ordinary laboratory air. All the samples had to be loaded in a zero-humidity dry box.

COHEN: A vacuum?

TAYLOR: No, not a vacuum, just as long as it was absolutely zero humidity. And that was fairly easy to get. Phosphorous pentoxide will suck the water out of air, and essentially bone-dry air is produced. So all we had to do was have a glove box with a bunch of petri dishes full of phosphorous pentoxide inside, and we could be certain that the air inside was bone dry. And so we put the samples in, either ground-up samples or powdered samples with different grain sizes. In some cases we can just put in a single chunk of quartz—on the order of, say, typically twenty milligrams or so. We'd load them into the nickel vessels inside the drybox, assemble the apparatus, and then take the whole thing out of the drybox and put it onto the vacuum line. And before we did this, by the way, we put fluorine into all the vessels and heated them all up even higher than we were going to do later, to make sure that we got all the extraneous oxygen out. After we had done that and taken the nickel vessels into the drybox and loaded them and put them back on the line, then sometimes we would put in fluorine at room temperature just to make sure, because we knew at room temperature it wouldn't react with the silicates. This just constituted one final check to make sure there was no contamination, because any water that got in there would react at room temperature. You'd heat it up overnight and the silicate samples would typically all react. The next morning when you came in—and by the way, we did this six samples at a time, a fairly happy number that we could do in single day. So six samples were heated up overnight and then we'd come in, and in each nickel reaction vessel we would have a mixture of oxygen, fluorine, and various fluorides, including some volatile, or gaseous, fluorides like silicon tetrafluoride and hydrogen fluoride and things like that. So now we have this mixture of all these gases, and you have to get pure oxygen out of that. Well, in such a high-vacuum apparatus it's very easy to do this, because the different gases freeze at much different temperatures. To get water out, for instance, you pass it through a dry-ice/alcohol mixture or a dry-ice/ether mixture, or something like that, which is quite cold. In the case of very volatile gases, you pass it through liquid nitrogen. So we have a whole bunch of different cold traps on the line, which the gases are forced to go through, and liquid nitrogen freezes out everything except for the oxygen and the fluorine. All of these less volatile fluorides, like silicon tetrafluoride and hydrogen fluoride, will freeze out in the liquid nitrogen and the only thing that gets through is the fluorine and the oxygen. Now, to get rid of the fluorine, we had to pass that through a nickel tube full of granular potassium bromide, because the fluorine will react to make potassium fluoride and then you get bromine out. Now, bromine at ordinary temperatures is a liquid. And so it was easy to freeze out, particularly in liquid nitrogen. So after the gases had gone through this next heated nickel tube, all of the fluorine was reacted to form bromine, and then the bromine was frozen out. And from then on, it was pure oxygen.

Now, we could have taken that pure oxygen and analyzed it directly in the mass spectrometer, but our mass spectrometer had been set up routinely to do carbon dioxide, and it was not a very big step just to transform that oxygen completely to carbon dioxide by burning it in the presence of a graphite rod. So that was the next step and it actually even cleaned up the sample a little bit more! The other nice thing about carbon dioxide is you can transfer it around very easily in the apparatus—you can just freeze it. If you've got CO_2 over here inside the high-vacuum apparatus, it's very easy to transfer it anywhere else in the apparatus just by putting a liquid nitrogen trap on the place you want to move it to. So it was very simple and elegant. And this basically was my PhD thesis.

COHEN: Working out this technique?

TAYLOR: Well, the actual technique had been pioneered earlier at the University of Chicago by Sol Silverman, while Sam Epstein was there working with Harold Urey. But working out all the techniques with many new minerals, and making sure that the whole thing worked well for all the different minerals was important to do before I could start applying it to real rocks in nature.

COHEN: And so the spectrometer gave you the—

TAYLOR: That gave me the final isotopic composition of the carbon dioxide, which was the same as the isotopic composition of the original quartz or feldspar. That was basically it. And then, to complement this, we also did hydrogen. Because many minerals contain hydrogen as well, we also could measure the deuterium-to-hydrogen ratio. The two main things I was concerned with in rocks were the deuterium-to-hydrogen ratio and the oxygen-18-to-oxygen-16 ratio, because those are the two major constituents, of course, of the water molecule. And you should be interested in the isotopic composition of both parts of the water molecule, because water is a supremely important compound on Earth.

COHEN: That's life. [Laughter]

TAYLOR: Yes, but also throughout the entire upper 200 or 300 miles of the Earth, water is an extremely important compound, particularly as you get near the surface. Water-rock interactions at all different temperatures and pressures pervade the Earth sciences. So if you're interested in water-rock interactions, by far the best technique to use—and it's still being used enormously even today—is to analyze the deuterium-to-hydrogen ratio of the substance and the oxygen-18-to-oxygen-16 ratio, because if the rock has interacted with water, then you can tell what kind of water it was, and a bunch of other things I won't start talking about.

OK, so this was the basic situation. Sam's philosophy was always just to see what's out there first. You have to make a reconnaissance, because we're in the business of doing this for the first time. Nobody knows what you're going to find, and so you have to get the background, as it were, before you start focusing on any details. So that was my PhD thesis, and it was very clear to lots of people that oxygen was going to be important. They wanted to use it for geothermometry. It didn't turn out to be very useful there, but it was a very important technique that had a lot of promise. So a lot of people around the world were interested in this. I don't know how I was so lucky to be able to choose this.

COHEN: Well, people make their luck.

TAYLOR: Yes. Well, for a variety of reasons I just happened to get in on the ground floor, so to speak. So while I was in the process of getting my PhD, I was of course thinking of looking for another job. I think I talked about this a little bit last time.

COHEN: You talked about Pennsylvania.

TAYLOR: But I should point out that I was well enough known, and Sam's projects were well enough known, that Princeton was looking at me. Also, I went to Columbia University, Lamont-Doherty, and lectured there. I also lectured at UCSD [University of California at San Diego], Scripps, the Geophysical Laboratory in Washington, DC, and the University of Wisconsin. Basically I had offers from all those places except Princeton. And the U.S. Geological Survey, I could have gone there. So I had a nice set of choices. But as I think I mentioned last time, Bob Sharp had some problems with getting the teaching done in '59 and '60 because of the resignations of several senior professors, and I jumped at his offer to stay at Caltech. So that's what I did. Then later on I went to Penn State, and then Caltech offered me a permanent job. Well, permanent in the sense that I was an assistant professor and I had to get to work to get tenure. But that wasn't that hard, because I was in such a good situation, with a working lab and everything else. Most young assistant professors have to set up a lab, but I was—

COHEN: You were all set.

TAYLOR: Yes, I was all set; just plug it in and start working. And Sam and I shared facilities and everything else. Later on, when I got a Sloan Foundation grant, I more or less gave it to him to build another mass spectrometer. It was very symbiotic relationship—almost unparalleled, I think, in the isotope community. Most of those guys are real prima donnas and they have giant egos. I never had an ego problem—because everything came so easily for me, probably. I mean, I never had to fight real hard for anything. As I say, the developments out of my thesis worked out so well and were so easy that I was promoted to tenure in 1964 without any problem, because I had published a lot and had done quite a bit of work—though my biggest and most important paper was published in 1968, well after I received tenure. It was called "The Oxygen Isotope Geochemistry of Igneous Rocks" [*Contrib. Mineral. Petrol.*, v. 19, pp. 1-71]. It was a *huge* paper; most journals won't take that big a paper. But I submitted it to a journal whose editor was a professor at Berkeley, and he knew about me. He recognized that this was worth publishing, because for the first time in one paper it laid out the basic variations in all the igneous rocks on Earth. Many of the things I discovered that were in that original paper led to what my students and I pursued in different directions over the next forty years.

COHEN: So it really was a seminal piece of work.

TAYLOR: Yes. As a matter of fact, later on I got a letter from the Science Citation Index, or something like that, saying, "This is now considered to be a citation classic. We'd like to have you write a history on how it came about." I never did that. I probably should have. I was too busy doing other things. But it was cited a lot. There was no question that it turned out to be very important, and I'm sure that the similar work preceding it was the reason I got tenure so easily. And I gave talks all over the country, even in Europe and so forth. So it was pretty well—

COHEN: Established.

TAYLOR: Yes, and we were the only ones. Bob Clayton also was starting to analyze oxygen isotopes in minerals, at the University of Chicago, but he was more of a chemist and he concentrated more on laboratory experiments and not on natural rocks and minerals. Geologically, we were the only place on the planet that was doing this kind of stuff. So we had people coming in, postdocs and graduate students from everywhere, because Caltech was the one place where you could really do this. We had people coming from France and Germany and Italy and Japan and a whole bunch of places.

COHEN: You and Sam shared all the equipment?

TAYLOR: Yes, we shared all the equipment. But of course after I came back as a young assistant professor, then I had my own papers. I still continued to publish with him, but I had my own operation with my own students. And we were off on different tacks.

COHEN: But you never built separate mass spectrometers.

TAYLOR: Yes, right. That was a good thing. We shared the mass spectrometers and mass spectrometer technicians. And that was another nice thing about it, because we therefore got twice as much bang for the buck, so to speak. We each had fairly large grants from NSF. By the way, in the early days the key thing for the geochemistry operation at Caltech was the continuing financial support from the Atomic Energy Commission contract we got every year, which Harrison Brown had brought with him when he came here from the University of Chicago. That basically supported the infrastructure of the whole lab, and you could count on it year after year after year, for a long time. As a matter of fact, all the way up until the time that NASA and the Moon samples came on board, this continuing AEC grant made it possible to do everything. It gave us so much freedom.

COHEN: You didn't have to write up proposals all the time.

TAYLOR: Well, we still had to write proposals for working grants and to support students and so forth, but as I look back on it now, from a vantage point of how hard it is to get research money now—my Lord, we were awash in money! We could do anything that we wanted to do; we had enough money to do it—largely because of the AEC grant, and then we had National Science Foundation support on top of that, plus later on the DOE [Department of Energy] grants. When the Atomic Energy Commission became the DOE, then you had to go to a separate DOE grant. But fortunately, just as things started to dry up a little bit, all this was replaced by NASA, because of the Moon samples. [Laughter] So that was another nice thing.

COHEN: Let me just put in at this point—I know those mass spectrometers, and I'll tell you why. I was teaching chemistry here in Pasadena, and Sam would let me bring in my advanced placement class. He would explain the whole thing, and he loved those kids—particularly the girls, but never mind. [Laughter]

TAYLOR: Sam just loved kids. No matter what he was doing, if somebody brought in some children, he would just stop everything.

COHEN: So that's how I know your mass spectrometers.

TAYLOR: Yes, right. Some of them were held together by sealing wax, but the point is that they worked.

COHEN: They were wonderful. It wasn't a black box; the kids could see stuff.

TAYLOR: Yes, right. You could see what was going on. And we had a technician who worked with us for a long time who just sat there and ran the samples. When I say we were awash in money, it was because we had technicians. I had my own technician. Sam had two or three technicians. We had a mass spectrometer technician. That made it so easy to do research.

COHEN: So once you had your techniques worked out, your technicians could then carry it through.

TAYLOR: Yes. All the routine grunt work from then on was done by technicians or students. For the students, they had to do everything just like I did, except actually running the samples in the mass spectrometer. We ordinarily let that be done by the mass spectrometer technician—at least for the oxygen machine. But the students had to do everything themselves, because that was part of the learning process—and the postdocs also.

Now, let me get to some of the more interesting things. First of all, we laid the groundwork for what all igneous rocks were like. Now, there's a very interesting locality called the Skaergaard—an Icelandic or Danish word. It's an igneous intrusion in eastern Greenland, in a fjord that cuts through the eastern side of the big island of Greenland, between the ice sheet and the coast. And those rocks are beautifully exposed by glaciation, because as the ice sheet retreated, it exposed the rocks. So they are completely, beautifully fresh and just recently cut by glaciers. In geology we're always looking for beautiful, fresh exposures, because in a lot of places the rock is covered by vegetation and soil, so you really have to scratch around to see what you want. In places like the Skaergaard, it's just gorgeous. Now, in 1931-1932 there was a British expedition called the British East Greenland Expedition, and on it was an Oxford professor named [Lawrence Richard] Wager. He was adventuresome; he'd actually climbed almost to the top of Mount Everest in the early days. Up to the time of the East Greenland expedition, he had been mainly interested in sedimentary rocks and stratigraphy and things like that. What he saw as this ship went through the ice pack in Greenland were all of these beautifully layered rocks on the giant outcrops along this fjord in eastern Greenland. And when he first saw them, he said, "These are sediments," because the layering looked like sediments. Then he actually landed on the outcrops and looked at them, and they turned out to be igneous rock, a gabbro, which is the same thing as basalt, only coarser-grained.

COHEN: And it solidified in layers?

TAYLOR: Yes. So it was basaltic magma—ordinary basaltic magma—but it crystallized as a completely closed system. It differentiated in place, from early crystallizing minerals to the very, very latest stages of differentiation. Now, this whole process of igneous differentiation was the most important problem studied in the science of igneous petrology for the last 200 years—to understand how the Earth differentiated from basalts into granites, basically. The main controversy was, and still to a large extent is, how did the rocks of the Earth differentiate from these basaltic-type materials into more silicic things like granites? Now, here was the first place where you had a complete closed system where you could— [Tape ends]

Begin Tape 3, Side 2

TAYLOR: … see what happened when basaltic magma that went through complete differentiation, from magnesium-rich early-crystallizing materials to absolutely pure iron at the very end stages. This intrusion had crystallized from the top down and from the bottom upward. And the last bit of magma to crystallize was very rich in iron but had no magnesium. So the iron-to-magnesium ratio was essentially infinite. All this came out later on, but the most interesting thing to Wager was that it was just so beautifully layered and the whole thing was differentiated like that. Wager, who up to that time had been a sedimentary geologist and a stratigrapher, immediately recognized how important this was, and for the rest of his life he didn't do anything but work on this intrusion. He kept on going back to it, and it turned out to be the best natural laboratory on Earth for studying basaltic magma differentiation.

COHEN: Was that the only place it was ever found?

TAYLOR: Well, no. There were similar finds, but nothing ever as beautiful and as perfect as this. This is such a perfect textbook example in every way, and a beautiful exposure of a single igneous intrusion. Now we know a lot more about the Earth, and the Skaergaard is still the best basaltic intrusion on Earth to study, because it's so perfect. I spent a large part of the rest of my life working on it also. I came to it long after Wager discovered it; it was in all the petrology and geology textbooks. In the nineteen-thirties there was a big controversy about how basalts differentiate. This was proof, at least in this kind of situation, of how it happened and that it evolved toward these iron-rich differentiates—so it had a major, major impact on geology.

When I was doing my PhD thesis in the late fifties, twenty or twenty-five years after this discovery and long after it had become famous, I of course wanted to find out how igneous rocks differentiate and what does the process of differentiation do to oxygen isotopes. I mean, here I have a new technique and I want to find out what this differentiation does to oxygen-isotope distributions, OK? Well, the classic intrusion was the Skaergaard intrusion. So I said to Sam, "Can't we get some samples of the Skaergaard intrusion?" Now, Sam had met Wager on one of his trips to Europe, so he knew him slightly. We wrote to Wager. The thing about Wager was that he was very protective of "his" Skaergaard intrusion, so he carefully guarded every sample he had collected from there. In fact, there are stories about how he would take an expedition there and everybody would go out and collect their own samples. And then he would say, "Now we'll put all these collections together," and they all came back and became part of his own private collection at Oxford. [Laughter] And he'd assign them Oxford numbers. Oxford

University still has a wonderful collection, because all these students would put together their collections for Wager. We didn't know anything about this history—about how protective he was of these samples—until later on. But anyway, the nice thing about Wager was that if it was a chemical technique he could do in England, he would do it himself. But oxygen isotopes weren't something he could do, and he wanted desperately to find out as much as he could about the Skaergaard, so when we told him about this, he was very enthusiastic about giving us some samples. Only later did I realize how lucky we had been. People would say, "What? You got samples from Wager?" because we were among the very few outside Oxford who got such samples.

He sent us some, but he didn't send us very much. He sent, I think, about ten samples, little chunks of rock. We told him we wanted the whole sequence, from the lowest in the succession to the latest differentiate—which was called the Sandwich Horizon—and some of the Marginal Border Group also, because when this big vat of basaltic magma was emplaced into the Earth's crust, there was a beautiful chilled marginal zone which he thought was the quenched original magma and he used that material as a sample of what the magma was before it went through all this differentiation. So it was very important to have a chilled marginal sample.

Anyway, I analyzed these samples. Now, I was assuming that this was the classic differentiation sequence on Earth, and it was tremendously exciting when we found out that you could align them in a nice sequence—that the very latest differentiates had significantly lower $^{18}O/^{16}O$ ratios than the original magma. By that time, we knew that all basalts we had analyzed were very uniform. I'll use the number +6, because that will come up later on. In our terminology, the basalts were 6 parts per 1,000, or 6 per mil, richer in ^{18}O than ocean water is. Another way to say that is that they were 0.6 percent "heavier"—that is, 0.6 percent richer in ^{18}O—than ocean water, which is very uniform in $^{18}O/^{16}O$ and was our standard. Basalts were basically +5.7 to +6.3 all over the Earth. These late differentiates weren't; they were down around 2.3 or 2.5, and as you went downward, or backward in the stratigraphy, they got heavier and heavier in ^{18}O, until finally the lowest ones had the normal basaltic-type value of +6. So here it was. It was a nice simple sequence, starting out with normal basaltic magma and differentiating and getting these lower and lower ^{18}O values, and so I thought we had a simple differentiation sequence and the late differentiates were low in ^{18}O. As a matter of fact, you could design a theoretical model such that the crystals that were forming were a little bit richer in

^{18}O than the magma. And as you pulled them out steadily, then the magma would be lower and lower and lower in ^{18}O. So we had a beautiful model based on these four or five samples, and we published it, as one of the papers from my thesis. [H. P. Taylor, Jr. and S. Epstein, "$^{18}O/^{16}O$ Ratios in Rocks and Coexisting Minerals of the Skaergaard Intrusion, East Greenland," *J. Petrol.* 4 (1), pp. 51-74 (1963).] Of course, every igneous petrologist on earth reads it, because the intrusion itself is so famous and this was the first $^{18}O/^{16}O$ paper on it. But in the same paper we analyzed the chilled margin, which was supposed to be the original material, and that was also very low in ^{18}O. It wasn't at all like the +6 value of the normal basaltic magma. So something funny had happened to the chilled margin—it wasn't what it was supposed to be. And not only that, but when I analyzed the coexisting minerals from it, they were twisted around. See, in my thesis I had proved that in igneous rocks you always saw this sequence: quartz heaviest in ^{18}O, then feldspar, then pyroxene, then olivine, and then magnetite, and so forth. And here in this chilled-margin sample they were twisted around. Feldspar was lower in ^{18}O than the coexisting pyroxene! I knew that something funny had happened to that rock. This was just the outer two feet of the intrusion—of a huge intrusion, ten kilometers across. It was only the outer two feet, and I interpreted that part as having interacted with groundwater from the outside of the intrusion, which we already knew was low in ^{18}O. The idea was that these low ^{18}O waters entered the intrusion and reacted at high temperatures with the chilled marginal gabbro, reacting faster with the feldspar than with the pyroxene and pulling it down lower in ^{18}O. In other words, I explained that this funny business with the chilled marginal gabbro was because of influx of water, but only on a tiny scale, just affecting two or three feet of the inside of the intrusion. It crossed my mind that maybe the low ^{18}O values I had seen in the differentiation sequence—that maybe that might have something to do with this low ^{18}O water also, but we ignored that possibility, because it fit the theoretical model so well, and it would have required such enormous amounts of water that no petrologist at that time would have believed it!

So in this paper, which was coauthored with Sam because it was part of my PhD thesis work done under his direction, I had these two kinds of low ^{18}O rocks: one set that occurred late in the differentiation sequence, which I thought was due to the differentiation, and one in the chilled margin, which affected just a tiny amount of the rock and which I attributed to influx of meteoric groundwater—that is, rainwater that had filtered down into the cracks and pores of the country rocks and that we knew would have been low in ^{18}O.

After I came back from Penn State and started to pursue this a little bit more in my Caltech laboratory, I analyzed a bunch of samples from the Scottish Hebrides, which were also classic igneous localities—the Isle of Skye and the Isle of Mull. And amazingly enough, I found many low ^{18}O rocks there, and these weren't late differentiates, these were the original undifferentiated magmatic bodies. And also I found oxygen-isotope reversals like I had found in the chilled marginal rock at the Skaergaard intrusion, but on a much bigger scale. So all of a sudden the light dawned—*boom!*—that these rocks had to have interacted with meteoric groundwaters on a gigantic scale at high temperatures, because huge volumes of rock were affected. Then I started looking in detail at descriptions of these rocks, and all the early descriptions of these rocks included lots of mineralogical evidence for a water-rich alteration in the rock at very high temperatures. But everybody had always assumed that this water was magmatic water, not meteoric groundwater. In fact, up to this time, all of these igneous rocks that were described in which you had abundant hydrous-alteration minerals—chlorite and hornblende and things like that, hydrous minerals replacing the original igneous minerals—they had all been ascribed to magmatic water.

COHEN: Now, when you say "magmatic water"—

TAYLOR: That means water that was originally dissolved in the magma coming from the depths of the Earth; then as the silicate minerals crystallize, the magmatic water boils off and reacts with the crystalline rock. Magmatic water has this special isotopic composition near +6 mils, and because of equilibrium at high temperatures it can't cause these kinds of changes in the $^{18}O/^{16}O$ of the rock. But I was seeing huge—by "huge" I mean three, four, five per mil, sometimes seven or eight per mil—depletions in ^{18}O. Now, that was impossible with magmatic water. So the point of the story is that in 1961 and 1962 I found out that meteoric water was interacting with igneous rocks at very high temperatures on a huge scale in the Scottish Hebrides and that all the descriptions of these hydrous minerals that geologists had thought were due to magmatic water were in fact the result of influx of meteoric groundwaters. By the way, "meteoric" means "from the sky;" it's just rainwater that has percolated down into the rocks. It turns the world of igneous petrology upside down. Instead of the water coming from the magma and moving out into the surrounding rocks, the water's actually going in and reacting with the intrusion itself. So it was a major discovery.

COHEN: Did people believe you at first?

TAYLOR: No. No, absolutely not. As a matter of fact, I'm not even sure that I was that certain myself. [Laughter] But I had analyzed enough rocks by now that I was really pretty much certain how things were. And I knew that these very low ^{18}O results had to have a special explanation. And the only thing on Earth that has such low ^{18}O values is rainwater. So I put the whole thing together. The lucky thing was that *I* discovered this instead of someone else, because now I realized that in the Skaergaard intrusion those so-called low ^{18}O late differentiates weren't a result of differentiation at all. In fact, it became clear that this classic igneous intrusion that was thought to represent a beautiful closed-system differentiation of basaltic magma wasn't a closed system at all. These huge amounts of water had come in from the outside in the upper parts of this intrusion, and the only reason I didn't really see this is because I didn't have enough samples, because Wager was so stingy with the samples. All I had was a couple of samples in sequence, and the lowering of ^{18}O just happened to be in this same sequence—because as you go upward in the intrusion, that is where you see the biggest influx of meteoric water. It was just an accident of lack of samples, plus not understanding this new phenomenon. The influx I originally had seen in the chilled margin was also true, but in addition, and much more important, it happened on a gigantic scale that changed the whole upper part of the intrusion. So instead of having a simple classic differentiation sequence—*that* was still true, but in the late stages huge amounts of meteoric water were also coming in and changing the chemistry. I don't think many scientists believed this at the time, but I was convinced, and that was part of this citation-classic paper of 1968. So in other words I had made a terrible error, but since I myself was the one who discovered it and corrected it, I felt very lucky. I was able to recover, although it was an eye-opener to find out that I could have been so wrong. [Laughter]

By that time, I had collected samples from several other of these differentiated intrusions around the world. We had gone to this locality in Labrador that has another intrusion like this, and I analyzed it, and it just was bang-on the +6 per mil value. The latest differentiates showed no change in ^{18}O at all.

COHEN: Now, is that because there was no meteoric water there?

TAYLOR: Yes, no meteoric water; that intrusion was emplaced at a much greater depth. So everything was falling into place, and in that 1968 paper that was the citation classic, all this stuff was laid out. In fact, the basic concepts of almost everything we now know about oxygen isotopes in igneous rocks was in that paper, in at least a primitive form.

As soon as I realized what was happening, I started thinking about why it had not been recognized before and what the processes are that I should look for to expand on these concepts. The whole idea here is that every intrusion emplaced into the upper part of the crust—by the upper part of the crust I mean, say, the upper ten kilometers—any intrusion that comes into that environment is very hot, at least 700º C and some as high as 1,100° C if it's basalt. So it's a huge thermal anomaly. Now, if it comes into rocks where the cracks and pores are full of groundwater, that groundwater clearly is going to be heated up, and when it's heated it wants to move upward in the Earth's gravitational field, because hot water is more buoyant. Hot water is less dense than cold water, so the hot water moves upward. Now, as long as the groundwater is all interconnected, through fractures in the rocks and so forth, then cold water has to move in radially toward the heat anomaly—that is, the magma body. In other words, every time you have an intrusion, it's like a hot spot, like a frying pan on a stove. Convection causes radial influx of cold water toward that hot spot, and the water in the vicinity of the hot intrusion buoyantly moves upward. So essentially you can think of the process as a giant pump. Gravity just sucks water in radially toward the intrusion. This made all kinds of sense, but nobody had ever really thought about it before, and certainly not on any kind of a scale where after the intrusion had crystallized and fractured, huge amounts of this groundwater were getting in and interacting with the hot intrusion. In order to test this idea, therefore, I concluded that the biggest isotope effects should be seen in the places where the country rocks were most permeable.

Rocks differ vastly in their permeabilities. Permeability is a measure of how fast and how easily the groundwater can move through the rocks. Now, there had been a lot of previous scientific work on groundwater and permeability, because groundwater is very important to people. So the variations in permeability of most rocks were already fairly well understood, and it was well known that some of the most permeable rocks on Earth are young, fractured basalts, because when a volcano erupts, the lava cools and breaks up into columnar joints. You've probably seen this in a lot of places, like the Devil's Postpile, near Mammoth. The solid rock is less dense than the magma, and so when it crystallizes it makes all these cooling fractures. And then you have lava tubes and things like that, so you can actually have underground rivers of meteoric groundwater. A young sequence of volcanic flows, particularly basalts, that has recently poured out represents some of the most permeable material on Earth. Now, with time, as groundwater moves through it, this kind of formation is self-sealing, because you get solution from one place and deposition in another place, so a lot of the fractures and openings get sealed up by deposition of minerals. But at least initially it's very permeable.

So I said, "OK, I'm going to look for places where there are permeable country rocks and also where the descriptions of the rocks show evidence for the same kinds of features that I saw in Scotland which led me initially to this concept." I started going through the literature, and one of the first places I found was a set of samples that had been worked on by a very famous petrologist at Princeton whom I knew very well named Arthur F. Buddington. I liked him a lot. He had given me a lot of inspiration when I was younger, when I visited Princeton, and also when I was at Penn State, since he had retired there. He was just a nice old guy who had done a lot of work in the old days. I first ran into his name when I was doing geology in southeast Alaska. He worked a lot there, and he had also described a series of shallow intrusions in the Cascade Mountains in Oregon that trended north through the entire length of the state, from the California border to the Columbia River. These intrusions are all about 10 million years old, and they were intruded into this sequence of lavas in the Western Cascades. It was perfect. All of his descriptions of the minerals and everything and the geologic setting was ideal to test this concept.

So I wrote him about this and asked him what he thought. He was a classical geologist. He couldn't believe that something like this had happened. But he said, "You really ought to go there," and he gave me a little bit more information. So I drove up to collect these rocks. As a matter of fact, my wife and I combined it with our vacation. I sampled all of these intrusions the whole length of the state of Oregon. Nobody had looked at them in a serious way since Buddington's original paper—back in 1928, I think. And then I came back to Caltech and analyzed them, and *bam!*

COHEN: Right on the money.

TAYLOR: Right on the money. And not only that, I could map the distribution of ^{18}O in them, and it just came out beautifully. This paper I rushed into print, so it came out very soon after the big paper. [H. P. Taylor, "Oxygen isotope evidence for large-scale interaction between meteoric ground waters and Tertiary granodiorite intrusions, Western Cascade Range, Oregon," *Jour. Geophys. Res.*, v. 76, pp. 7855-7874 (1971).] That paper also made a big splash, and now people were really starting to believe in these new ideas.

There's another reason I was lucky. I was a young professor when two of the greatest things in the history of Earth sciences happened—namely, the birth of plate tectonics and seafloor spreading, and the lunar program, both of which occurred in the sixties. I had now shown that basically every igneous intrusion that comes into the shallow part of the crust, into permeable country rocks, is going to produce this enormous convective disturbance in the adjacent groundwaters. It's a giant heat engine, which is going to cause big hydrothermal convection systems, with the heated groundwater interacting with the igneous rocks. And seafloor spreading had just been discovered, so people immediately took this idea and said that all along the midocean ridges you had exactly the same kind of environment! Igneous intrusions were coming up. It wasn't too much longer after that that these black smokers were discovered on the midocean ridges. The idea was that hot, igneous intrusions were coming into the central ridge and causing hydrothermal convection—not of meteoric groundwater but of ocean water that had permeated the fractures in the basalt. In other words, all along the entire midocean ridge system this process that I had discovered was happening.

COHEN: So you really started a whole new thing.

TAYLOR: It had a huge impact. A lot of what I did later on was just to ask, "How big and how widespread is this phenomenon?" Because what I looked at in Oregon were small intrusions, only a few kilometers across and in a very favorable environment to have this kind of thing happen. The question was, "How common is this process?" Well, later on we found out that it takes place on an even more gigantic scale. In many cases, you don't see any petrographic evidence for it at all—the only evidence in a lot of these places is the ^{18}O levels, because a lot of this happens at such high temperatures, particularly in basaltic intrusions, that it doesn't leave any hydrous mineral imprint. So you look at the rocks in thin section, and you have no idea that so much water has passed through this rock at a high temperature, because it leaves the rock looking just like it was before the process started. The only difference is that all the oxygen isotopes have been shifted to different values.

Again, looking back on it, it's almost obvious that these kinds of massive convective systems should have been anticipated. You wonder why it took so long—well, maybe I shouldn't say it was that obvious, because the process depends on the permeabilities and how much water is available in the rocks. It turns out that given the lifetime of these intrusions—a typical small intrusion takes at least 10,000 years to crystallize and a little bit longer to cool down; some of the big ones, like the Skaergaard intrusion, take 100,000 years to crystallize and a million years to cool down—it's a question of how much water can be pumped through the rocks and over what distances within these time constraints. And until we really had a good feeling for what the actual permeabilities were, you couldn't have been that certain. So it wasn't that obvious. But it's obvious that here you have a hot spot, and it's clearly going to start to cause these fluids to undergo convective circulation. And if a rock fractures, the water is going to migrate through the rock. But nobody would have known about this process without these oxygen-isotope analyses.

I should point out that this was only possible because Sam and others had previously shown that rainwaters were so different in oxygen and hydrogen isotopic compositions from, say, ocean water. All rainwater on Earth is depleted in ^{18}O, and depleted in deuterium, relative to ocean water, because of what happens in the meteorological cycle. When water evaporates from the ocean, it fractionates its isotopes. So when an air mass, for instance, produces precipitation, the rain or the snow is richer in ^{18}O and deuterium than the cloud mass it left behind. As one of these cloud masses moves, say, from west to east across North America, the first stuff to precipitate looks not too different from ocean water. But as the air mass moves across the Sierra Nevada the snow and the rain is successively lower and lower and lower in ^{18}O and deuterium. For instance, the water in Alaskan rivers and lakes is much lower in ^{18}O and deuterium than the water we drink here in California. As you move across North America to higher and higher latitudes, or even to Greenland, the snow and the rain and everything else get lower and lower in ^{18}O. Now, these systematics in the geography of rainwater opened up a new way of looking at things. Later on, when I had some students and postdoctoral fellows come to work with me, we started to work on these systematics in various rocks across North America. For example, porphyry copper ore deposits, which are a major class of copper deposits. There's a whole bunch of them in western North America, notably Arizona. The biggest one is in Utah—the huge Bingham pit, west of Salt Lake City. It's the biggest man-made object; you can see it from outer space almost—just a big hole in the ground. And Butte, Montana, also has one of these things. There's a whole string of them going all the way up from northern Mexico to Alaska. They're copper concentrations in a typical shallow intrusive body. Now, people had all along thought that these ore fluids were magmatic water, for all the reasons I mentioned. Hydrous minerals are associated with these copper deposits and they'd all been explained as being due to magmatic water. In fact, these intrusions are very rich in magmatic water, OK? So here was a situation where later on, as we found out, you not only had lots of magmatic water but you also had influx of meteoric water from the outside.

So I and some of my students from the early seventies got together with a former student at Caltech, Dick Nielsen, who by that time had started to work for Kennecott Copper Corporation in Utah—he actually was the best man at my wedding, and I knew him real well because he had been my assistant in southeast Alaska. This friendship provided the possibilty of getting well-documented samples from these porphyry copper deposits, because Dick had worked on many of them himself. Here was the idea: We were pretty certain that magmatic water was important in these deposits, but there was also a good chance that meteoric groundwaters were also involved. So the idea was to try to see whether the systematic changes in ^{18}O and in deuterium of rainwater as you go north could be seen in the waters that interacted with these deposits—which had formed at various times 20 million or 30 million or 40 million years ago. We knew by this time that North American rainwater even back 30 or 40 million years ago was much lower in ^{18}O in Alaska and in British Columbia than it was in Arizona. So here was the framework. All we had to do was look at the same kind of geologic situation—namely, something like these porphyry copper intrusions, which all formed in the same way across a broad distance all the way from Mexico to Alaska. And we could be sure that the magmatic water, since that's determined by what's going on in the depths, would probably be the same throughout. But the meteoric water in Arizona was very different from the meteoric water in Montana or Utah, and even more so in northern British Columbia and Alaska: The ancient meteoric groundwaters were expected to change in isotopic composition, both in deuterium and in oxygen, as we went north. This worked out perfectly, and it was written up in a couple of papers that I did with Simon M. F. Sheppard and Dick Nielsen [S. M. F. Sheppard, R. L. Nielsen, and H. P. Taylor Jr. "Oxygen and hydrogen isotope ratios of clay minerals from porphyry copper deposits," *Econ. Geol.*, v. 64, pp. 755-777 (1969); S. M. F. Sheppard, R. L. Nielsen, and H. P. Taylor Jr., "Hydrogen and oxygen isotope ratios in minerals from porphyry copper deposits," *Econ. Geol.*, v. 66, pp. 515-542 (1971)]. First of all, we showed that the early high-temperature alteration in these deposits was due to magmatic water: It had the right deuterium value and the right ^{18}O value and it was the same in Arizona as it was in British Columbia and Montana. But at a later stage, some of the big alteration zones associated with these deposits, which most ore geologists thought were also due to magmatic water, turned out to be due to influx of meteoric groundwater.

By this time, I had gotten heavily involved with a bunch of exploration and research geologists who were working for the Anaconda company. The two big copper companies in those days were Anaconda and Kennecott, and the geologists of these two companies didn't speak to each other, because they were competing. However, because I was an academic geologist I could communicate with both groups. I knew Kennecott geologists through Nielsen, but I also knew a lot of the geologists in Anaconda. As a matter of fact, Charles Meyer, a professor at Berkeley I got to know real well, really a great guy, was the major consultant for Anaconda. Meyer was very interested in these kinds of studies, so I got to go down into the mine at Butte, which is an Anaconda mine, and Simon Sheppard and I did a major study in Butte that elaborated on this process. [S. M. F. Sheppard and H. P. Taylor Jr., "Hydrogen and oxygen isotope evidence for the origins of water in the Boulder Batholith and the Butte ore deposits," *Econ. Geol*., v. 69, pp. 926-946 (1974).] The Anaconda geologists, in fact, had already determined, secretly in their own company—they didn't tell anybody, because they used it as an exploration tool—that there was indeed an outside, or meteoric, hydrothermal system and an inside, or magmatic, hydrothermal system in these porphyry copper deposits. On the basis of their own studies, not having anything to do with isotopes, they had come up with similar concepts, but they didn't tell anybody outside Anaconda. Simon and I came along with our isotope techniques and sort of reinvented the wheel, as far as they were concerned, because they already knew this.

The Anaconda geologists developed their concepts solely on the basis of geologic evidence for different pressures. The outside system, the meteoric water, is under hydrostatic pressure, because it's just a column of water that goes up to the surface of the Earth, whereas the inside system is under lithostatic pressure, about three times the pressure on the outside system. They had used a lithostatic-hydrostatic pressure analysis, whereas we used the low ^{18}O versus higher ^{18}O picture. But then we published the aforementioned 1974 paper and I published a more elaborate one at the same time that really nailed this conclusion down. [H. P. Taylor Jr., "The application of oxygen and hydrogen isotope studies to problems of hydrothermal alteration and ore deposition," *Econ. Geol*., v. 69 pp. 843-883 (1974).] And it turned out to be an important exploration tool, because the main copper-ore bodies were right at the interface between these two systems. That's what they had been keeping secret. But we were the ones to publish it; they didn't. So we got the published credit for it, even though, as it turns out, they already knew it and they also knew that this interface was where most of the copper was. [Tape ends]

HUGH P. TAYLOR

SESSION 4

July 5, 2002

Begin Tape 4, Side 1

COHEN: Would you talk about the Moon program a little bit?

TAYLOR: OK. To start off, I should go back a little bit. I had been in Dabney House as an undergraduate, and when I was a junior, a freshman entered our house and became a very good friend—Harrison Schmitt, who later became one of the astronauts who went to the Moon on Apollo 17. When I graduated, he was only a sophomore, but he was part of the program that Jim Noble put together with U.S. Steel in southeast Alaska, which I described earlier. So he was up in southeast Alaska with me for one summer, and I got to know him really well. Then later, when I came back as a graduate student, he was a senior. And at that time his father was a famous consulting ore-deposits geologist in Silver City, New Mexico. So we convinced Jim Noble, our ore-deposit geologist—who was good friends with his father; they were kind of in competition with each other, but they were good friends—to take a giant field trip around the entire Southwest for all the people interested in ore deposits. This trip lasted almost a month, and we toured all the famous ore deposits all around the Southwest. The trip was very important to me, because it was my first introduction to all these ore deposits, and later on I worked on almost all of them with stable isotopes. I was the first, with some of my students and postdocs, to do the deuterium and hydrogen and $^{18}O/^{16}O$ studies of all these ore deposits. One of the big ore deposits was near Silver City, so we went to Jack Schmitt's home in Silver City—Harrison Schmitt's nickname was Jack—and I got to know his family.

I say this only because later he was an astronaut and became one of our more famous alumni because of that. At the time, he was thinking about where he should go for graduate school. I had been to Harvard—I told you about this fellow Jim Thompson—and I advised Jack Schmitt to go to Harvard. He ended up going to Harvard and doing his PhD there under Jim Thompson. Then he went away on a Fulbright to Norway to study eclogites. And then, after he finished his PhD, he went to work for the United States Geological Survey. At that time, Gene Shoemaker, who later became division chairman here and was also a Caltech graduate—I think he got a bachelor's degree and a master's degree here, and a PhD at Princeton—Gene was the first geologist to really get interested in the Moon per se. In fact, Gene desperately wanted to be an astronaut himself. That was his *raison d'être*. And he carved out his career by first working on Meteor Crater, in Arizona; he did some of the definitive work on Meteor Crater. With another fellow at the USGS, a mineralogist, he made the first discovery of the mineral cocsite, which is the high-pressure form of silica in the shocked quartz sandstone, the Coconino Sandstone, that surrounded Meteor Crater; this was a very famous discovery. Anyway, Gene became quite a famous scientist, and he oriented his life toward the Moon—and impact craters on Earth, because you could obviously see that craters were important on the Moon. He founded the Astrogeology Branch at the USGS in Flagstaff, and Jack Schmitt went to work there after he got his PhD at Harvard. Now, about this time—I can't remember the exact dates—Gene was diagnosed with Addison's disease, which I don't know much about but which kept him from ever being an astronaut, because if you have that, I think you black out at various times. So it was obvious that he was finished as an astronaut. But here was this young kid, Jack Schmitt, who was in the astrogeology program, he was the right age and everything else, adventuresome and so forth. So Gene convinced Jack Schmitt to apply for the scientist-astronaut program as it got started in subsequent years. And since Jack was a real good friend of mine, I kept up the relationship.

COHEN: What year would that have been?

TAYLOR: I can't remember when it all started, but it was in the early sixties. Jack Kennedy is the president who said in 1961, I think, or '62, "We will put a man on the Moon before this decade is out." Jack Schmitt didn't become a part of the astronaut program until much later. But because I knew Gene Shoemaker so well and knew Jack Schmitt so well and they had started this, I obviously got interested in it. And of course when Kennedy made this announcement, we took it at face value. We started thinking about what we would do. The planning was very rough for some time. I don't think the really important stuff got started until about 1966. There were several scientific conferences on what to do, which I attended and got heavily interested in, but it was obvious that Caltech would have a major part in this, because by this time our geochemistry facilities were really state of the art. Gerry Wasserburg's lab and Lee Silver's lab and Sam Epstein's lab and my lab—it was very clear we were going to play a major role in the Apollo program, so we started getting tooled up for it. Then NASA started a program of funding, because they knew they were going to bring samples back and they had to have people to—

COHEN: Did JPL [the Jet Propulsion Laboratory] have an early part in this?

TAYLOR: I don't think JPL had much of a play in this. It was basically in our Division of Geological and Planetary Sciences. Fortunately by that time our planetary sciences program had taken off full blast, so we had a lot of people interested in planetary sciences. In 1969 Gene Shoemaker became our division chairman, so that brought tremendous interest at that time.

Even though Jack had gone to Harvard and gotten his PhD there, his intellectual home was really still Caltech, because of his associations with Lee Silver and me and so forth. So when he became a member of the astronaut program and started learning how to fly jets—this was after the Apollo program was well along—he decided that since there was no certainty that a scientist was going to go to the Moon, even himself, he wanted to make sure that at least the astronauts who went had some capability in geology. And although Harvard had a good program, the person he looked to was Lee Silver. Jack got a hold of Lee and asked him if he'd be willing to do this. You couldn't have picked a better person, because Silver's whole method of imparting knowledge was just exactly perfect for the astronauts. These were bright guys, and he got them out in the field, none of this lecture stuff that just turns people off and puts them to sleep. Jack Schmitt had a great deal of trouble convincing NASA that this was important, because the astronauts had tremendous restrictions on their time. Their program, from the time they woke up until they went to sleep, was extremely proscribed, and the idea that they would take time off to go away for geology field trips—that really took a lot of pushing.

Nevertheless, they were able to do it. I think Gene Shoemaker had a lot to do with it, along with Jack Schmitt. A couple of the astronauts—I think Jim Lovell was one—went along on one of Silver's field trips and came back just raving about how great this was. So they got the whole program started. Apollo 13 was the first group—that was Jim Lovell and Fred Haise—to have taken part in a lot of Silver's exercises. What Silver would do is get them all out in a geological area he knew pretty well and say, "OK, you guys, go and collect me a suite of rocks."

And they'd say, "What? A suite of rocks?" And what he meant by that was that they should go out and identify different rocks by color, texture, whatever you think you can do, but get the whole panoply of different kinds of rocks that you see in this particular area. And he chose areas in which you could do this. So they would go out, and they were bright enough that they could pick nine or ten rocks that gave them a good feel for the rocks in that area. Then the idea was to put the rocks together in some kind of structural context—in terms of which ones were overlying the others, which ones might have been intruded into the others, and so forth. And just letting them work it out for themselves—these bright, enthusiastic guys, who didn't waste any time, either. They really worked on it. And then of course they camped out at night, when they would discuss rocks and geology with Silver and Jack Schmitt. So it was really a terrific project and it gave Caltech a real entree into the lunar program, particularly with the astronauts. And it was all because of Jack Schmitt's association with Caltech and the fact that his intellectual home was really here at Caltech—with us, rather than anywhere else.

Unfortunately Apollo 13, which was the first somewhat geologically trained set of astronauts, was a disaster, and they had to come home without even getting to land on the Moon. Apollo 14 was Alan Shepard, I think, and he showed little or no interest in the geology program. He was one of the original astronauts, and I guess they had other fish to fry. One of the things he did was hit a golf ball on the Moon. But starting with Apollo 15 and Apollo 16 and Apollo 17—of course, Apollo 17 was Jack Schmitt himself. Those three missions, the last three missions, were the most important, because by that time they had the lunar rovers and they could take extended excursions away from the lunar module on the Moon's surface and really collect rocks. And there's no question that it paid off. For instance, Dave Scott and Jim Irwin were on Apollo 15, and they had been very enthusiastic workers with Lee Silver in the field geology program he put together. And Silver had emphasized an important rock that people were pretty sure they would find on the Moon—a white rock full of feldspar that was fairly easy to recognize, even for a novice geologist, called anorthosite. And one of the great things was when David Scott, on one of his early excursions on the Apollo 15 site, got out and said, "Hey! I think I found it," because Silver had emphasized the fact that anorthosite was probably going to be one of the oldest rocks they would see, because within the original magma oceans that formed the Moon, plagioclase crystals would float and could accumulate to form this rock. So David Scott said, "I think Lee Silver's going to be excited, because I think we've got some anorthosite." And sure enough, when he brought it back, it was anorthosite.

COHEN: And if they hadn't had this training, they never would have known that.

TAYLOR: Well, they might have picked it up by accident, but the idea was that they knew what to look for. And the cheering among the geologists back in Houston when they heard that David Scott had found anorthosite was really marvelous. So Silver really deserves a lot of the credit.

One of the great experiences I had was going to Florida when Apollo 17 lifted off, to see this friend of mine, Jack Schmitt, being shot off to the Moon. Of course Silver was at the launch and a lot of other people—Arden Albee and so forth. It was a terrific experience. Jack invited several of his friends—people he went up to southeast Alaska with—to watch the liftoff. Of course by this time we were heavily involved working on these Moon samples in our own lab.

COHEN: But this wasn't the only lab that got the lunar samples?

TAYLOR: No, no. Several other labs, in the United States and in foreign countries, were involved in the program. As far as stable isotopes are concerned, I should mention, there really were only three or four other labs. Our only real competitor was Bob Clayton, Sam's first student, who was then a professor at the University of Chicago.

COHEN: All the same club.

TAYLOR: Yes, basically the same club. The nice thing about it was that in the mid-sixties, when NASA started funding this work, there was a big pot of money available. These were the glory days, when we had lots of money both from the AEC and the NSF, and then from NASA. It was a tremendous advantage to have the NASA program and the lunar program, which was a big source of funding, come along and replace those AEC grants. I wouldn't say we were awash in money, but we had plenty.

In fact, in my career I really never had to worry about funding. I never had a proposal turned down. I was unbelievably lucky to be a scientist at a time when the federal government supported science in a big way. We were basically able to do whatever we wanted to. I mean, it was paradise.

Well, we actually didn't know how big the samples were going to be, but we were pretty certain they weren't going to be really big, and they were going to be really, really valuable. So we started designing our equipment to do the smallest samples possible. Everybody did, around the country.

COHEN: Do you remember that idea for a while that everything had to be quarantined for six or eight months?

TAYLOR: Oh, yes. Well, it was, but that was not a real problem. I can't remember when we did the very first samples, but there was a lunar science conference in 1970. The first landing was July of 1969, and the first results were published in *Science* in January of 1970. So we got the samples very, very quickly, within just a few months after arrival back. And of course we were all tooled up. We had been practicing; everybody had been practicing on terrestrial samples, so when we got them we were ready to go. Everybody was going to do everything they could to be at that first lunar conference, which was truly exciting. To hear the very first scientific results on the lunar samples was in every way truly wonderful!

As far as getting the operation started, since we knew that the samples were going to be small and valuable.... I described to you earlier how we treated silicate minerals, like quartz and feldspar, which is what these Moon rocks were: We would react them with fluorine. All we wanted was the oxygen, because we wanted to measure the oxygen's isotopic composition. So all we needed to do was get the oxygen out of the silicate; we would throw away the silicon tetrafluoride, which was just a by-product. Silicon tetrafluoride is a gas that comes off during this reaction, and we would just throw it away.

Well, we decided that now these lunar samples were going to be really valuable, so we should do something special. Nobody else was going to do this; we were the only ones that decided to retool to do both smaller samples and also samples in which we preserved the silicon tetrafluoride and analyzed the isotopic composition of the silicon. Silicon has three isotopes: ^{30}Si, ^{29}Si, and ^{28}Si. And there was almost nothing known about silicon isotopes at all in nature, although I should mention a little aside here.

There's a very interesting story. My freshman physics lab partner, and also a member of my house, Dabney, was David Tilles. After he had been a couple of years at Caltech, for some reason or another, I can't remember exactly why, he transferred up to UC Berkeley. Maybe it was his junior year. But anyway, for whatever reason, he went up to Berkeley and then stayed on for graduate school there in physics. And he started working on his PhD thesis under a scientist I later got to know pretty well, and whom Gerry Wasserburg knows pretty well—John Reynolds. The thesis Tilles chose to do was on silicon isotopes, [laughter] which was really remarkable: The only work on silicon isotopes in rocks in the entire world gets done by this friend of mine who did his PhD thesis on it at Berkeley. So in other words, that was the bible, but Tilles had just barely scratched the surface. Unfortunately, he never lived to see the lunar program, because he was climbing with his son on the coast of Oregon and a boulder fell down and hit him on the head and killed him.

Nevertheless his pioneering work on silicon isotopes was the only guide we had for doing these things. So we were set up to save all the lunar silicon tetrafluoride. NASA gave us the funds. We built a mass spectrometer dedicated specifically to silicon isotopes, and it wasn't easy, because silicon tetrafluoride is a funny kind of chemical and we had to have an all-metal line—I won't go into all the details, but it was a major project. It turned out that silicon isotopes don't show much variation in nature. They are not like oxygen and deuterium and carbon and things like that; there are only very, very small silicon-isotope fractionations, as it turned out. Of course we didn't know that at the time—although Tilles's work showed that there was not much variation, but he had just scratched the surface. Later on we found out that there were indeed only very small variations in most terrestrial rocks. The hope was that if we had found something, it would have opened up a whole new isotope system for analysis. But there were still many things to do, because the whole universe is still full of oxygen and carbon and hydrogen. We weren't running out of things to analyze at all.

Now, Clayton's lab in Chicago analyzed the oxygen isotopes as well, and basically got the same results. It was very, very interesting, because the lunar samples don't show any of the kind of retrograde exchange during cooling that we saw in almost every terrestrial igneous rock. That's one thing we saw right away: Because the moon was so dry and lacked water, this re-equilibration that happened to all coexisting minerals in rocks that formed from crystallization of basaltic magma at high temperatures—the kind of thing I talked about in the Skaergaard, for instance; that during cooling they continued to exchange because there was water around which acted as a medium to help them exchange—on the Moon, this didn't happen. So we had the first gabbros, which had preserved their original—

COHEN: What's gabbro?

TAYLOR: Gabbro is a basalt that crystallizes at depth and therefore crystallizes into a rock with coarse-grained minerals instead of fine-grained minerals like a basalt. Lunar gabbros and anorthosites were different from Earth rocks in a very profound way—namely, that the minerals, once formed, preserved their oxygen perfectly. The oxygen atoms froze in and they didn't continue to exchange as they cooled down, as they always do on Earth. And one of the things that struck me right away with the lunar samples was how beautiful the crystals look. If you look at a feldspar crystal in a terrestrial gabbro, it always looks milky white. Part of the reason is that fluid inclusions are impregnated in it during this exchange process. You almost never get these beautiful, clear, gemlike crystals; in fact, you just don't, in a typical terrestrial gabbro. But on the Moon, virtually every crystal was gemlike. You could actually tell by looking at it. If it was pristine, it had not exchanged, so it all fit together as a very nice story.

Now, the other thing that was extremely interesting with the lunar samples was the lunar soil—the so-called lunar regolith. This is the fine-grained Moon dust that covers the entire moon. There were outcroppings of rocks and breccias and things like that—

COHEN: This was the dust that they were going to sink into?

TAYLOR: Yes, that was Tommy Gold, who was a professor at Cornell, you know—

COHEN: Right. We know the dust story.

TAYLOR: Yes. He said, "Everybody's going to disappear into this dust."

COHEN: He later claimed he never said that.

TAYLOR: Yes, but that's not true. [Laughter] But, that's another story. But this lunar dust was very interesting. Of course we analyzed it. You couldn't separate minerals from it, but you could analyze it. And also we'd get fairly big samples of it, so we analyzed the deuterium-to-hydrogen ratio, the carbon-13/carbon-12 ratio, the oxygen-18/oxygen-16 ratio, and the silicon-30/silicon-28 ratio, so we had all the important stable isotopes covered. And we were the only lab that had them all, OK? When we did, we found that the lunar soil, the regolith, had slightly higher silicon-30/silicon-28 ratios and higher $^{18}O/^{16}O$ ratios than the solid crystalline rocks that we were analyzing. It wasn't much, but it was clearly a measurable amount. So we decided to do what turned out to be a really brilliant thing.

COHEN: When you say "we," who are you working with?

TAYLOR: Well, I'm talking about Sam Epstein. See, this was another nice thing. When I came back to Caltech as a young assistant professor, I was basically on my own. I had my own group and my own students and postdocs and things like that. But the lunar program got us back together, so it was like a rebirth of the graduate student-professor relationship, except now we were—

COHEN: More equal.

TAYLOR: Yes. We were somewhat equal, but I always deferred to Sam. [Laughter] It was terrific to be able to work side by side. We considered the samples so valuable that we hardly let our technicians do any of it; we wanted to do it all ourselves. And Sam was galvanized. It was like a rebirth for him, too. So it was like we were two graduate students working side by side in the lab together. It was a very exciting time, one of the happiest times of my life, because we'd rush to get to the lab every day and we were working side by side. And he had a certain facility with things; for instance, he could blow glass beautifully. I couldn't, but I could write up the results better than he could. It was a beautiful symbiotic relationship that flowered again in the lunar program.

Anyway, we decided to do what we call stripping the lunar soil. That means we put a large amount of sample in the reaction vessel, these nickel reaction vessels, and just put in a little bit of fluorine so it was just a partial reaction. We were just trying to see what the surface of the grains would give us, and then we would analyze that. Then we'd put in a little bit more and analyze that, and put in a little bit more and analyze that, and so forth. In this crude way, we stripped off the outside of the grains. Well, the first results were just unbelievable, because we found the oxygen-isotopic fractionations were like 50 per mil. We had never seen anything like it. It was far bigger than any ^{18}O value we had ever found in rocks on Earth. And not only that, but the silicon isotopes differed from terrestrial rocks by 25 to 30 per mil. I told you we could hardly even measure any silicon-isotope variations of more than 1 per mil among the different minerals or rocks on Earth. Here we had this gigantic effect in the first little bit of the lunar soil. It was just unbelievable! We found that the effect got less and less and less as we stripped more and more away. We observed a beautiful systematic change going back into the lunar grains until finally, after we had stripped off all this surface material that was so rich in ^{18}O and so rich in ^{30}Si, we got back to the original stuff that the moon was made of. So this was a really major discovery.

COHEN: You must have been using microtechniques. I mean, how do you strip—

TAYLOR: The way we did it, as I say—we put in a whole gram of the lunar soil, and then we just put in a little bit of fluorine, you see, not enough to react the whole sample. That first little bit, with which we were essentially stripping the grains, turned out to be this *huge* isotopic effect for both oxygen and silicon. After the first discovery, it wasn't so interesting, because we found that all the lunar soils showed these effects, and it was very clear what had happened. The lunar soil was made during lunar bombardment and microbombardment. Tiny little particles as well as big particles were constantly hitting the lunar surface, and they would partly vaporize the material and it would recondense. This was the ideal situation for producing big isotopic fractionations; in fact, that's how you produce large physical isotopic fractionations, because basically the light isotope, particularly in the lunar gravity, can actually escape from the Moon's atmosphere. So once this material was vaporized, the ^{16}O-rich material and the ^{28}Si-rich material would preferentially leave the lunar surface, would actually escape from the Moon, leaving behind this much more enriched material, which would accumulate as the coating. So this proved, if you needed proof, that you had this micrometeorite bombardment of the Moon. But the main thing was that it produced this huge effect; the biggest isotopic fractionation of silicon we have ever found in nature is in the lunar dust.

So that was a very nice discovery. All the lunar samples showed it as we continued to do Apollo 12 and 14 and 15 and 16 and 17. We did them all. We continued to do this whole program through all of the returned lunar samples, and we found a lot of interesting things. But after the first discoveries, as far as the kinds of things that Sam and I did, it became somewhat routine because we didn't make any more what I would consider monumental discoveries. By then, we just were cleaning up and showing in detail the kinds of things that were going on.

COHEN: Now, when you were doing that, what were the other people, like Albee and Wasserburg, doing?

TAYLOR: Oh, they were doing other things, like radiogenic isotopes. Albee was doing petrography and microprobe work on the lunar samples, and Wasserburg was doing radioactive elements, like uranium, and trying to date the lunar rocks. I couldn't even begin to describe all the interesting things that Gerry Wasserburg's lab did on lunar samples. They did a tremendous number of things. In fact, the lunar program had a tremendous, unbelievable impact on terrestrial geochemistry, isotope geochemistry, because people were tooling up to do things they had never been able to do. The whole neodymium-samarium isotope thing, which is another set of radioactive parents and daughters, all got started with the lunar program. Gunter Lugmair was the first one to do it, and then Wasserburg immediately jumped on it, and one of his students, Don [Donald J.] DePaolo, did some of the more definitive work on the neodymium isotopes. It blossomed into a whole new facet of isotope geochemistry on terrestrial samples, but it really had its birth on the lunar samples. And that was true of many, many things. Wasserburg's whole laboratory got enormously better in terms of precision and everything else and the fact that they would do much smaller samples and so forth, because of the way he tooled up the lunar program. So the lunar program had a great impact on terrestrial geochemistry. Just the mere fact that it introduced a whole bunch of new procedures and techniques and ideas which then, after the lunar program went off and most of the major discoveries had been made with the lunar samples, they started to apply all these techniques to terrestrial rocks, and it's still going on. It set the stage. Many, many labs were built around the world.

COHEN: Just to do this?

TAYLOR: Yes, just to continue to do neodymium isotopes and so forth, which had gotten started with the lunar program.

Now, the other thing that Sam and I did: It had been very well understood from the astrophysicists that the solar wind would be deuterium-free. Nobody had ever measured it, but theoretically all the astrophysicists agreed that the solar wind would be deuterium-free, because it was known that helium was burned up in the nuclear processes that take place in the Sun. The Moon is a big receptacle for solar wind—the solar wind that migrates out from the Sun is collected on the lunar soil. So one of the things we set up to do was analyze how much hydrogen was present in the lunar soil and what its isotopic composition was. As I say, we fully expected to find very, very low deuterium values. Ocean water on Earth contains about 157 parts per million deuterium—that is, for every million atoms of hydrogen in ocean water, 157 of them are deuterium. But on the Moon it was less than one.

COHEN: So there really was none.

TAYLOR: It was way, way less than one, but still it wasn't completely deuterium-free. So that led to a quandary, which is still somewhat open-ended. You see, when the astronauts move around on the lunar surface, the water they expire gets vented out through their backpacks.

COHEN: Ah-ha! So you were measuring that.

TAYLOR: Yes, right. And that's terrestrial water, which has a much, much larger amount of deuterium. We kept on trying to get down to the least contaminated samples, but we couldn't find any lunar soil sample that didn't have a little tiny bit of deuterium in it, and we finally concluded that it was just contamination from the astronauts' backpacks, because the samples were carefully preserved on the way back. And after they got to Houston, we don't think it was added, although you can never be sure, because it doesn't take much contamination to put a little bit of deuterium in there.

But in any case, it was very nice to be able to show that the lunar soil samples were essentially pure solar wind—and there's a lot of hydrogen in the lunar samples. People who want to go back and set up a Moon base have picked up on that, because there's enough hydrogen there to actually use as a fuel. All you'd need to do is heat it, and of course you'd use the Sun's rays to heat it. You could have solar mirrors up there which would heat it up using the Sun itself, and that drives off the hydrogen. Then you can burn it and get energy from it. And also when you burn it you get water, so you have a source of water on the Moon. In other words, there's enough hydrogen in the lunar soil that if you process enough of it you could undoubtedly sometime in the future—if and when we do set up a Moon base—use this lunar hydrogen we measured for both energy and water. There's no question about it, because there's a significant quantity of hydrogen up there.

As I say, we continued for the next several years. The last flight was December '72. So basically by '73 or '74 we had finished analyzing essentially everything. We kept all the samples in a safe. We had a special lab with an alarm on it and everything like that. My involvement in it tapered off, because I was still much more interested in terrestrial things. Sam continued with it—not really doing much in the way of lunar samples but with meteorites and things like that. So he continued to get NASA grants all the way up to even after his retirement. [Tape ends]

Begin Tape 4, Side 2

TAYLOR: I'll jump ahead. When Sam retired in 1990 he was one of the last professors that had to retire under mandatory retirement. I don't think he would have retired otherwise, because he kept working in the lab up until the day he died, literally. That was his life, working in the lab and getting new data, and he did stay active. But it was interesting—in those days, emeritus professors couldn't be PIs [principal investigators] on grants. So after he retired, I became the PI on his NASA grant. Even though I had left the NASA grant program several years before, I just did that, so that he would have enough money to keep his program going. Later on they relaxed that rule and now emeritus professors can be PIs. There were a few other nice things that were done with the lunar samples, but all during this time I continued to work on terrestrial things with my students.

COHEN: Did you have any graduate students who got their degree from working on the lunar soil?

TAYLOR: Oh, no, that was always just Sam and me. We had several Epstein and Taylor papers and Taylor and Epstein papers. We just kept grinding them out and going to the lunar science conferences. I stopped going to the lunar science conferences in 1975 or something like that. I was doing other things. Sam kept going. Even though there were no new samples coming back, people continued to do other things and compare things with the lunar samples, so the lunar science conferences are still going on. But I lost interest. The truly exciting lunar science conferences were the first and second and third. That plus the plate tectonics revolution of the mid-sixties were the two greatest things in science I've ever been associated with. And I was lucky enough to be a young scientist at the time they both happened.

COHEN: So, you were done with your part, and as far as Caltech went, it was just Sam continuing to work on it.

TAYLOR: Well, of course Wasserburg continued. His students still go to lunar science conferences—I think they do, anyway. And then later on, Ed Stolper came, so we still had a major presence in the lunar science conferences—Stolper and his students and Wasserburg and his students. Epstein continued with it. Silver dropped out.

Silver of course has this interesting business that he doesn't publish much. Silver has more unpublished data in his file cabinet than you can believe; it's just a tragic situation that a lot of it may never see the light of day in a publication, I'm afraid, but that's just one of his idiosyncrasies. He's a great scientist and he's done a lot of great things. But I'm one of the few people that know it, because I'm so interested in what he's doing and I talk to him all the time. He's a fantastic source of knowledge and data. Certainly he's the greatest geologist I've ever been in contact with, and except for Sam Epstein he's certainly the biggest influence on my career. He just has this problem of not being able to seem to get it down on paper—although later in his life, because his students needed it, I think, he became a little bit better at it. But the fact is that he had some funding problems later in his life. I know that NASA finally cut him off.

COHEN: Because he didn't publish enough?

TAYLOR: Yes. He had two or three papers, maybe, early on, that they apparently almost had to pull teeth to get out of him. That's a sad aspect of the whole thing, but he contributed in so many other important ways to the lunar program and to Caltech.

COHEN: Oh, there's no question about that.

TAYLOR: He was such a tremendously important person for me—to have somebody who was so knowledgeable about so many aspects of geology and petrology!

COHEN: He loves Caltech, too.

TAYLOR: Yes, just as much or more than I do. We went on lots of field trips together, to Labrador and all kinds of places. I've probably spent more time in the field with Lee Silver than I have with anybody else, and I've learned more from him than I have from anyone else by far. Also, I've done quite a bit in the way of scientific studies with him, although a lot of the work I've done with him has not yet been published. He made some great discoveries about granitic batholiths, these huge masses of granite you find all around the Pacific rim. He spent a lot of his life working on these things, particularly the one here in Southern California, the one that stretches from Los Angeles all the way south into Baja, California. It's essentially made up of a whole bunch of different plutons, bodies of different kinds of granite, which we can call granodiorite, true granite, and tonalite or quartz diorite. Silver started working here on what he called the Peninsular Ranges Batholith in the nineteen-fifties. The Peninsular Ranges are the ranges that include Baja California, because it's a peninsula; in California it's called the Southern California Batholith. "Batholith" just means a huge mass of granitic intrusions.

COHEN: So it's just these mountains that come up from Baja.

TAYLOR: Yes. If you drive inland on Interstate 15 to San Diego, you go right through it—all the hills you see there are part of the Southern California Batholith, or the Peninsular Ranges Batholith, which continues on from Riverside all the way south almost to the tip of Baja. And the Sierra Nevada Batholith is also just like this. And the Coast Range Batholith of British Columbia is like this. They wrap around the edge of the Pacific Ocean. You can find them in Japan and Kamchatka and all the way around the Pacific and then down in the Andes. Now, they all formed in somewhat the same way. These batholiths have their origin as a result, somehow or other—nobody really understands for sure—of the process of subduction, as the oceanic plate gets pulled down into the upper mantle. After the material goes down about 100 kilometers, something—either the water comes off it and partially melts the overlying mantle, or you're actually partially melting some of the slab that's being carried down, or both—you know, we're still arguing about exactly what goes on, but there's no doubt that something happens at a depth of about 100 kilometers in the slab that's being subducted and this causes a great number of masses of magma to come up. And that is what has produced all these granite plutons of various types that you find all around the Pacific, including the Sierra Nevada and the Peninsular Ranges.

The thing that Lee Silver did that nobody else was doing at the time, back in the late fifties and sixties, is he invented a way of dating rocks of this age using the mineral zircon. I won't go into all the details of how he did it, but it was very clever. He had to use different grain sizes and different colors and different radioactivity contents of the zircon, so it required some interesting separations of minerals. His operation was set up to take large amounts of rock, grind them up—I'm talking about 100 or 200 pounds of rock—he ground it all up, separated out the zircons, which make up only a small fraction of the rock, then separated these zircon crystals, which are beautiful little red and orange crystals in the rock, into different size fractions and color fractions and things like this and then analyzed the different fractions, and then plotted them up on a graph, and extrapolated the correct age. This is a beautiful discovery that he and some other people made early on. But at the time he was first doing this, no one else was doing this on such young rocks—these rocks that were only about 100 million years old that make up this particular batholith. Most of the batholiths that are all around the Pacific are in the range, say, of 150 million years old to maybe 30 or 40 million years old. But he could date them. And not only that, but he understood the petrology and everything else. Also this thing was right in his backyard. He just loved these rocks. He had gotten interested in them way back when he was a graduate student. First of all, he dated all the plutons. He found a beautiful systematic range of ages in which the eastern half was all younger than the western half. But in the Peninsular Ranges Batholith he found that the total range of ages was only from about 130 million years to about 85 million years. Now, that's a 45-million-year age range. But it was beautifully asymmetrical. The east side was all young, about 100 million to 85 million; the west side was all older. That was a great discovery, because there were also some other differences between the west side and the east side which he found because of the differences in petrology. Then he started doing strontium isotopes, which is one of the things that Wasserburg also did. And in fact, I don't know if you've heard about the Wasserburg-Silver feud. [Laughter]

COHEN: I know there's not a lot of love lost.

TAYLOR: Yes. Part of it was because they were doing similar things. I don't know all the ins and outs of it, but some of it had to do with a paper on which they were coauthors many years ago. But the point is they were doing similar things; in other words, they had basically similar instruments. One was in the basement of North Mudd and the other one was on the second floor of Arms [Charles Arms Laboratory of the Geological Sciences]. So here we had these two labs that were somewhat in competition with each other, and they were in competition almost as if they were at different universities. As far as the Caltech Division of Geological and Planetary Sciences is concerned, it was not all that happy an arrangement. And frankly, I worked with both of them, although I certainly worked more with Silver. I coauthored papers with Wasserburg and they've turned out to be some very nice studies. And I've certainly had a lot of associations with Silver. The beautiful thing about Wasserburg is that everything you worked on with him got published. [Laughter] You didn't have to worry about that. A lot of the things I've worked on with Silver have still not seen the light of day. Nevertheless, there's no question that I've been much more intimately associated with Silver than with Wasserburg, and Silver has clearly contributed much more to our understanding of the origin of granites. Of course I never had to really choose sides, although if I did have to choose sides, I would choose Lee, just because I've been so much more closely associated with Lee all these years. But that's neither here nor there, that's just a facet of our division during my tenure here that you had to live with.

COHEN: Who had to deal with this? Bob Sharp?

TAYLOR: I suppose Bob—and Gene Shoemaker and Barclay Kamb—had some headaches along these lines, but I guess they worked out a *modus vivendi*. If you want anecdotes, I'll tell you one.

COHEN: Sure. [Laughter]

TAYLOR: There are a lot of these kinds of things, but this is one of the more interesting ones. It's somewhat ironic. Actually, a lot of the stuff that went on was—if it weren't so tragic in some sense, it would be just really funny. OK, here's the story. Wasserburg and Dimitri Papanastassiou—

COHEN: I haven't heard that name in a long time.

TAYLOR: Yes. Dimitri was very good at designing and building state-of-the-art mass spectrometers. So when the lunar science program first got started and Wasserburg was tooling up to do smaller and smaller samples, he and Dimitri designed, with the help of Victor Nenow and Curtis Bauman—Curtis Bauman was our machinist, a great guy. I should mention them, as part of the infrastructure of the Caltech geochemistry program. Professors and even technicians can't do it all; you need to have people who can really do these kinds of things, and you had to have an electronics person. That was Victor Nenow. He was just magnificent. Later on he did a lot of work on the Hubble Space Telescope with Jim [James A.] Westphal. Jim Westphal depended on Vic Nenow tremendously, because a lot of that Hubble telescope design was Victor Nenow's. Victor Nenow was just wonderful; he didn't know a lot of theory about electronics, but he was just a magician with his hands. And not only that, but we had all these older mass spectrometers; and he could keep them all running. So he played an enormously important role. Curtis Bauman was the machinist, and really a fine fellow and quite a good machinist, I think—although he was certainly not as accomplished in his field as Victor Nenow. Victor Nenow was irreplaceable; fortunately he didn't retire until two years ago, so essentially I had him my entire career here at Caltech; otherwise I couldn't have done any of the kinds of things I'm talking about here without his support and without Curtis Bauman's support. And Sam would have said the same thing; in fact, they all said the same thing.

So Wasserburg had the money and had Dimitri to do the designing and Victor Nenow and Curtis Bauman to build it. They built this state-of-the-art mass spectrometer with NASA money which was, at that time, I think, the state-of-the-art mass spectrometer in the world. I mean, they were right on the cutting edge—there was certainly no instrument any better than that. And it was put together here at Caltech, a homegrown instrument built in this particular fashion, and it worked beautifully. Then Silver got involved with the NASA program, and he had these old mass spectrometers that he and Clair Patterson had been using. But because NASA had so much money to dole out to people in the early days, Silver applied for money to build a mass spectrometer, and of course he wanted a mass spectrometer that was state-of-the-art, like the one Wasserburg had, because he wanted to do similar things with it. They had different interests—I should point that out—but the kind of mass spectrometer they needed was basically the same. And of course Curtis Bauman and Victor Nenow were division employees, and because they had built it, they knew everything about the Wasserburg mass spectrometer, which, by the way, Wasserburg called the Lunatic. He called his whole operation the Lunatic Asylum, and he called this mass spectrometer the Lunatic.

So Silver got the money to build a mass spectrometer. And who was going to build it? Vic Nenow and Curtis Bauman. I don't know all the details about exactly how it was decided, but it was obvious that if they were going to build a mass spectrometer, they were going to use their knowledge of the Wasserburg machine to build it. And I think Gerry was perfectly happy with that—well, maybe not perfectly happy. [Laughter] So as they were building it, somebody stuck up a sign—I don't know who it was—saying "Lunatic II." All the time it was being built, it was called Lunatic II, just sitting there like that. Lunatic I was Wasserburg's machine and this was basically a copy. I don't know exactly how much of a copy it was, but it was a natural thing, this kind of cooperation in research. This is something that everybody should do; we're all in the same business. So finally the mass spectrometer was done. I don't know when that happened—I'm not good on dates; probably about '69—anyway, about the time that samples were going to be brought back. And Silver wanted to have a party celebrating the completion of his mass spectrometer. So he had this party, and I looked up and the Lunatic II sign was no longer there. Instead Lee made a speech and said, "I want to give this a name." I think he mentioned Lunatic II or something like this, but he didn't want to call it "II." And about that time *Man of La Mancha* was a big hit, the musical about Don Quixote. And Lee decided he wanted to call his mass spectrometer "Dulcinea." [Laughter] So he had this sign reading "Dulcinea," which he brought out and hung on his mass spectrometer. As soon as Wasserburg heard this, he just apparently exploded, because I think he was happy as long as it was clear that this was Lunatic II and a copy of his machine. Fair enough, OK? [Laughter] And perhaps Silver should have been more tactful. Or maybe—I don't know.

COHEN: Maybe he did it on purpose.

TAYLOR: Who knows? But anyway, Dulcinea? Lunatic II was an obvious name, particularly since the sign had been there the whole time the thing was being built. In fact, for all I know, Wasserburg told Vic or somebody, "This is OK with me as long as you put a sign up there saying

'Lunatic II' on it." I don't know; I have absolutely no idea. All I know is that they had this party, the mass spectrometer was renamed Dulcinea, and then all hell broke loose. [Laughter] And after a week or so, maybe involving the division chairman—who knows, I don't know—the name "Dulcinea" was removed and they put back "Lunatic II."

COHEN: So Gerry won that one.

TAYLOR: If you can say that. [Laughter] I guess it was like children playing in a sandbox: "This is mine! This is mine!" [Laughter] I'm sorry, but that's the way it looked to people outside. But of course Gerry had a case on his side.

COHEN: Why didn't they both use the same one?

TAYLOR: Oh, that would have…. [Laughter] Excuse me, but that would have been absolute.... In fact, that was one of the problems. All the rest of the geochemists in the world, of course, knew about the feud and the fact that we had duplicate facilities for these two professors, one in the basement of North Mudd and the other on the second floor of Arms. Of course Silver's operation, even though it was quite productive, never was as productive as Wasserburg's. Wasserburg, in terms of quantity of output and different kinds of things, just dwarfed anything that Silver could do. Wasserburg had huge numbers of people working for him. Silver always had a much smaller operation. Silver was working in a somewhat different style, and of course Silver rarely published. [Laughter] Wasserburg continued to get lots of funding and Silver's funding dried up to a certain extent. So it had all the elements of pathos about it, but it was a major aspect of the geochemistry operation in our division. It probably belongs in some kind of a history. It probably should be discussed by somebody more knowledgeable than me, because little anecdotes like this—I don't know all the details of what happened.

COHEN: I think we acquired some reputation for people not getting along very well.

TAYLOR: Yes. From the outside, yes. And the students from each group didn't talk much to each other. That's kind of sad, particularly since some of the others of us were reasonably happy to work with both groups. There was another interesting thing. There's this area out in the

Mojave Desert called the Marble Mountains, which is a famous locality because it's one of the oldest places where you can find early Cambrian rocks, the oldest fossiliferous strata in the world. The big explosion of life happened in the Cambrian period, and this is one of the places where the lowermost Cambrian is sitting directly on older rocks. I can't remember, again, exactly when or why this happened, but I know that Wasserburg and Albee and their group were out there at exactly the same time that Silver and his group were, OK? [Laughter] So here were two somewhat—I wouldn't call them expeditions, because it was probably just two vehicles in each group or something like that—

COHEN: A field trip?

TAYLOR: Yes, it was a field trip. They were both out there collecting the same rocks on the same weekend. And they never talked to one another. Later on, each blamed the other for jumping on their turf, so to speak, because that happens in geology. If you start working in a certain area doing something, you sort of take it—

COHEN: That's not unique to geology. The astronomers are after the stars.

TAYLOR: Yes, right, exactly. You don't want somebody coming in at the last minute trying to scoop up and do something that you've been spending a lot of time working on. So there was a big controversy about this locality—about who was there first and who wanted to do these kinds of things. Things like that went on periodically.

COHEN: Well, they probably could do that. They all had as much money as they wanted. As you were saying, these were the golden years.

TAYLOR: Yes, that's right. That was an aspect of it, too, until Silver's funding began drying up. Of course then Wasserburg really shot out in front, in terms of productivity.

Let me get back to Silver's contribution, the Peninsular Ranges Batholith, OK? Silver dated much of it, which was a prodigious effort. Even to date a single pluton is a prodigious effort. He must have had forty or fifty plutons dated on the east side and the west side. That's how he was able to show that there was this asymmetry. Even though these were all separate plutons, the group on the west side had come up at a clearly older time than the ones on the east side. Nevertheless, they were all within this 40-million-year period, between about 125 million years old and 85 million. At about this time, people were getting age dates on some of the other batholiths. For instance, in the Sierra Nevada you could find rocks that were as old as 200 million years or 175 million and things like that—so, in other words, you found much older plutons and much, much younger plutons. In fact, in the Sierra Nevada you can find plutons that are very, very young. To make a long story short, in most of these other batholiths, including all the other ones in North America and extending into South America, and around into Japan, each one of these other batholiths has had a whole series of plutons coming up into it that in many cases started 200 million years ago, or even earlier, 220 million years ago, and it's continued on. These overlapping plutons keep coming up, keep coming up, keep coming up, all the way up to 20 million years ago or even 5 million years ago in the Sierra Nevada. With Silver there was luck, serendipity, whatever you want to call it, in his backyard. With the Peninsular Ranges Batholith, he didn't have any of that complication. He showed, for the first time, that there weren't any early plutons; the earliest one was about 128 million years old. And there weren't any late plutons; the latest one was about 85 million years old. And not only were they circumscribed in this very short time period but there was this beautiful asymmetry from west to east. You could divide the batholith right down the middle, and the west side was older than the east side. There was essentially no overlap. So he knew that, before he even started the strontium-isotope project.

Then he started the strontium-isotope project, which is one of the things that Wasserburg also did. Now, the initial strontium tells you something about the kind of rock the pluton was formed from, so it's key to understanding this process that I was talking about, in which the magmas are coming off the subduction zone down about 100 kilometers deep or so. And here was the unbelievable thing: Silver showed that there was a beautiful and systematic change in the strontium-isotope ratio across the batholith. These are separate plutons, and each pluton had its own characteristic strontium-isotopic composition. You could actually draw contours, or isopleths, down the batholith and show that the strontium-isotopic composition of the source material these plutons came from changed drastically from west to east. On the west side, they looked just like midocean ridge basalts. They had the same strontium-isotopic composition as the basalt coming up at the midocean ridges. But as you went farther east, they got more and more and more radiogenic, showing that clearly they come from a different source that had a more radiogenic parent material in it, which can be found only in continental types of material. But the main thing was how beautiful and systematic it was. He found the asymmetry here in Southern California, and they traced it all the way down halfway through Baja, California, and found the same thing. This is when I first got involved; I can't remember the date exactly.

So he had all these samples, OK? And I was tooled up to do oxygen isotopes, so my involvement was a natural thing. I started analyzing the same samples in which he had found this beautiful strontium-isotope correlation and age correlation. I analyzed these things and, oh, it was just unbelievable! I found the same beautiful asymmetry in oxygen isotopes. Not only did I find that the rock was getting higher and higher in ^{18}O as you went from west to east but the west side—the extreme west side, which had the strontium isotopes that looked just like a midocean ridge basalt—also had ^{18}O just exactly like a midocean ridge basalt. So it looked as though it came from the same reservoir as the midocean ridge basalts. And as you go east, they got heavier and heavier and heavier in ^{18}O until finally, on the east side, I got the heaviest ^{18}O values I had seen in any granite anywhere on Earth. So not only was there a great variation, but it fit perfectly with the strontium and it was some of the biggest variations. And it was telling us something really profound about the rocks that these plutons came from—something you could not tell in any other way, because if you just looked at them in terms of petrography, the rocks on the west side look the same as the rocks on the east side. You have tonalites on the west side and tonalites on the east side. And the other interesting thing was that it didn't matter whether it was tonalite or granodiorite or granite. The important thing was where you were geographically. The kind of rock type, which you'd think would matter, didn't matter at all. The only important thing was position and age, as it turned out.

So this was a truly monumental discovery, which we published as an extended abstract. Now, Silver always presented his new results at meetings, so he has lots of abstracts, but many scientists don't consider abstracts to be a real publication. But the important discovery paper of this phenomenon I'm telling you about, which I consider one of the great discoveries in igneous petrology, and many other people do, too, appears only in this extended abstract [H. P. Taylor and L. T. Silver, "Oxygen and strontium isotope relationships in plutonic igneous rocks of the Peninsular Ranges Batholith, Southern and Baja California," *Short Papers of the Fourth International Conference, Geochronology, Cosmochronology, Isotope Geology*, R. E. Zartman, ed., U.S. Geol. Survey Open-File Report 78-701, pp. 423-426 (1978)] and in a guidebook we coauthored for a meeting here. [L. T. Silver, H. P. Taylor Jr., and B. Chappell, "Some petrological geochemical and geochronological observations near the international border of the U.S.A. and Mexico," Guidebook to Field Trip, San Diego, Geol. Soc. Amer. meeting. *Mesozoic Crystalline Rocks: Peninsular Ranges Batholith and Pegmatites*, Dept. of Geol. Sci., San Diego State Univ, pp. 83-110 (1979).] It appears as a paper, and those are the only places you can actually see it, except in a review chapter in a book I published a few years later. ["Igneous rocks: II. Isotopic case studies of circumpacific magmatism," in *Stable Isotopes in High Temperature Geological Processes,* J. W. Valley, H. P. Taylor, and J. R. O'Neil, eds., *Min. Soc. Am. Rev. in Mineralogy*, v.16, pp. 273-317 (1986).]

I was kind of worried about this. This was really hot stuff when I first found it, because for the first time you had this beautiful linear correlation between ^{18}O / ^{16}O and ^{87}Sr / ^{86}Sr. It was the first time this had ever been shown so beautifully. And I should also say, to add to it, not only did we find this systematic west-to-east change in the Peninsular Ranges Batholith but I found that right down the middle of the batholith there was an abrupt jump in ^{18}O. The strontium isotopes didn't really show this. It was a little bit crude, but you could actually show a discontinuity in ^{18}O that clearly separated the east side of the batholith from the west side.

There was an international geochemical meeting in Aspen, in 1977 or '78—probably 1978, because I started working on this in the mid-seventies. Now, ordinarily you'd just give an abstract, which is a one-paragraph thing, the kind of thing that Lee does. But for this meeting they started what they—well, the lunar science conference, actually, had started these extended abstracts. Now, an extended abstract was like a two-page or three-page paper which you actually put two or three figures in and maybe even a table of data. So I went to Lee and said, "Well, let's give this paper at the Aspen meeting," the correlation between ^{18}O and strontium, which was a great discovery, I thought, and he did, too. And then we published a figure. The figure is showing the correlation between ^{18}O and strontium, which was the key thing, with all the data points on it. No real tables, but actually a figure showing the data points and also a graph showing what these ^{18}O contours were. Once this was out and could be referred to, then it was there, and it's now been referred to many, many times. But it's kind of funny, because here's this extended abstract and this guidebook, and then this review article; and they're the only places where you can find these profoundly important data about granite batholiths.

I mentioned to you the fact that Silver had shown that these plutons were very well constrained in age. Now, that made this batholith the simplest one to study, of all the ones all around the Pacific rim. For some plate tectonic reason, which I won't even go into, the batholith was born in the Cretaceous period 125 million or 128 million years ago, and it was over in the Cretaceous. It's all Cretaceous, and there's nothing before 128 million years ago and nothing after 85 million years ago. That's not true of any of these other batholiths. It's simple in its history. It's much easier to see what's going on. And that's probably one of the reasons why the asymmetry here is so beautiful—because you're actually able to separate out the unique aspects of this batholith. You don't have to worry about all these older plutons cutting through, because every time you complicate the picture with a whole set of younger plutons and older plutons, you're less able to see exactly what's going on. As a matter of fact, the whole history of my career in geochemistry is to try to get the simplest geological situation for any kind of geological process that I'm interested in, and I've gone all around the world searching for these simple examples that also have excellent rock exposures. That's one of the reasons I fixated on the Skaergaard. But this one was in my own backyard—my backyard as well as Silver's. And not only that, but Silver had laid all the groundwork out for me so beautifully that it just—

COHEN: That you just went on and did it.

TAYLOR: Yes. Then I started looking in a serious way at batholiths all around the world, but always it was going back with a comparison to the Peninsular Ranges Batholith. That became the so-called type locality to which everything else is referred. [Tape ends]

HUGH P. TAYLOR

SESSION 5

July 11, 2002

Begin Tape 5, Side 1

TAYLOR: Well, last time I was talking mainly about the lunar program and a few things that arose out of the lunar program. But at the same time this was going on, I returned to my initial interest in the Skaergaard intrusion in eastern Greenland, which I mentioned before. I had always had in the back of my mind that I would have liked to have done more samples on the Skaergaard intrusion, because in the interim I had basically been chasing this problem of how widespread these meteoric-hydrothermal systems associated with igneous intrusions were. How deep do they go in the crust, and how big are the intrusions that are affected, and so forth. I was just trying to understand this whole process in greater detail, since it was the major discovery of my scientific career and I was in a position to pursue it. So today I'm going to talk about the amplification of that basic situation, involving both meteoric waters on land and ocean waters in oceanic environments.

The discussion about the Skaergaard started in 1969 or 1970, I can't remember exactly. A professor at the University of Oregon, Alexander McBirney, who was a volcanologist I had gotten to know pretty well, came to Caltech to give a lecture. He was a really nice fellow. We hit it off very well. He's a little bit older than I. He graduated from West Point, and I've always been interested in military problems and so forth, so I started talking to him about the battles of Napoleon and things like this. We had a mutual interest in this. I didn't know that he had graduated from West Point. He became friends of the president of Nicaragua, and after he left West Point he went down and operated a coffee plantation in Nicaragua. He made money at it, but it wasn't that great a thing. He was a smart guy, and he was looking around for something else to do, and a professor of geology at UC Berkeley, Howell Williams, came down and was doing geology in Nicaragua. And McBirney started going around with him in the field. He got interested in volcanology and then decided to chuck the coffee business. He went back to school at a later date, studying with Howell Williams. Even though he got his PhD at the age of thirty-five—I think; it was very, very late—he immediately brought a lot of pizzazz to the field of volcanology. Originally he was at UC San Diego, Scripps. He had a falling out with Al Engel, who had been a professor at Caltech before he moved down to La Jolla. Al was very difficult to get along with. If we ever just talk about personalities in the division, Al Engel and his wife, Celeste, who is basically his analyst, make a very interesting—

COHEN: She was his psychiatric analyst?

TAYLOR: No, no, his chemical analyst. He and Celeste did a lot of analytical chemistry on rocks and minerals. There's a very interesting story there, which I don't know if anybody else has ever told, about the early history of this division. But for right now I'll just mention that McBirney left UC San Diego largely because of a clash with Al Engel. I knew Al very well, and Al always treated me just fine—actually, Al Engel was the first member of the geology division that I came in contact with when I was a sophomore. When I decided to switch to geology, Al Engel taught sophomore mineralogy and he was the one I approached about doing a research project and a reading project. He wasn't all that helpful about it, but nevertheless he was the guy I first mentioned this to.

As a matter of fact, I might as well tell a little anecdote about Al when I was an undergraduate here at Caltech. Al used to have his office in the place where Wasserburg now has his big laboratory complex. Wasserburg originally had this lab built for all the students in the division to carry out chemical studies; however, soon after it was built, Wasserburg took it over and made it part of his own complex. Well, that was the original mineralogy laboratory that students worked in in the old days, back in the early fifties, when Al Engel was a professor of mineralogy, and Al had his office right next to that lab. The way the mineralogy lab worked was that a lot of the students worked late. In fact, there were a lot more, I think, working really late at night than during the day. That was always my own modus operandi when I was an undergraduate and a graduate—to work late at night. I usually worked until two or three in the morning—

COHEN: That was sort of normal for Caltech.

TAYLOR: Yes, particularly in those days. Anyway, a bunch of us were in there one night making a lot of raucous noise, telling stories of various kinds, and Al was working that evening in his office right next door. All of a sudden, he opens the door and comes into the mineralogy lab and says, "OK, you guys. I'll make a deal with you. I won't make any noise if you don't make any noise." [Laughter] And I of course looked at him, and I was perfectly happy not to make any more noise. But a fellow undergraduate, a senior at that time—and this sort of illustrates the attitude of Caltech undergraduates vis-à-vis their professors, particularly in the geology division—Dick Knapp, who was a senior at the time and not all that great a student, either, but he had no fear of professors, blurted out, "Al, you just go ahead and make all the damn noise you want." [Laughter] And Al walked away; that was the end. But that kind of camaraderie and informal discussion—calling professors by their first names—was the hallmark of the geology division when I first came into it, and one of the things that most appealed to me about the geology division. Dick Jahns, for instance, refused to have anybody call him Professor or Dr. Jahns. Everybody had to call him Dick.

Anyway, getting back to McBirney. He left UCSD and founded the Center for Volcanology at the University of Oregon. Eugene, Oregon, is a beautiful place and a very desirable place for geologists to go, with the Cascade Mountains and volcanoes in the background and so forth. So it was an ideal situation. He set up a really nice operation there, and he became a world-famous volcanologist, even though he had gotten his PhD so late. Well, Caltech invited him down to give a lecture. I hit it off with him and we went out drinking after his lecture and I mentioned to him about how I had always sort of wanted to go to the Skaergaard. And he says, "God. I've always wanted to go to the Skaergaard, too." Now, the thing you have to realize about this is that up to that time it was a very hard place to get into, because you had to go through the Arctic ice pack. There were no helicopters and certainly no roads. But you had to go through the ice pack, so you had to have a ship that was able to go through the ice pack. And then, unless you were going to stay there and winter over, you had to get back out through the ice pack, and there were only about forty-five days, or at most two months, in July and August, when you could do this. You had to get in as early as you could, maybe early in July, and get out before the end of August, otherwise you were stuck for the whole winter. It was not an easy place to get into. No one other than British geologists had ever been there before, because you had to mount a real expedition to get there. And it never even occurred to me to try to go there. But McBirney, a West Point graduate who could well have become a general, I guess, was a man of action. He said, "Let's just go." So there in a bar here in Pasadena we cooked up this idea for a joint University of Oregon-Caltech expedition to the

Skaergaard intrusion.

McBirney certainly carried the ball on it. He got in touch with some geologists from Scotland who also wanted to go, because there was competition between the Scottish universities and the English universities. So we hired a Norwegian sealer, a very, very strong ship that could go through the pack ice. And we decided to go the next summer, in 1971. I took along a graduate student of mine, Richard Forester, and McBirney took along two other colleagues of his and two or three students from the University of Oregon. So the American group plus this six-member British group got on this Norwegian sealer manned by a Norwegian crew in Bergen, Norway, in the early summer of 1971 and set off. McBirney didn't want to fly. He never wanted to fly. So we originally went to England on the SS *France*. It had never even occurred to me to take a big ship, and that was just a couple of years before the *France* stopped doing transatlantic runs, so I'm very glad I did it.

COHEN: Who was paying for all this?

TAYLOR: Well, we got an NSF grant. It was done on the cheap, because we were combining with the British, and the Norwegian sealer was not as expensive as a bigger ship, and so forth—so it was done on an NSF grant and shared equally with the money from the Scottish group. I don't remember exactly how much it cost, but NSF certainly supported our end. And they realized that no one other than British geologists had ever been there, and American universities had a lot more pizzazz as far as geochemistry and modern techniques were concerned than the British at that time, so it was a natural thing to fund. And it certainly paid off. It paid off enormously.

And I also should say this was the first time I had ever been on such a ship in such bad weather. The North Sea was terrible. It was just terrible, and everybody was sick. The British had taken the nicer bunks up on the topside of the vessel, and the Americans were all shoved down into the fo'c'sle, but it turned out that we were in a better position than they were, because the ones up above—see, this was not built to be a passenger ship, it was built to be a sealer and all the bunks of the British contingent got completely demolished on the first night out on the North Sea. Everybody was sick. People were saying, "I want to die." [Laughter] My student was just—it was awful. And there were rumors floating through the ship that we were going to put into the Faroes, which are little islands just east of Iceland, because the storm was so bad. But of course that was just nonsense; the Norwegians were perfectly happy to keep on going. [Laughter] But thank God, the thing only lasted for two days, and we got near Iceland and got quiet water, and from then on everything was fine. We made our way through the pack ice.

Of course it's 100-percent daylight, there's no night, because we were up there near the first of July. And so my first sight of the Skaergaard—I mean, the excitement! To actually get on the outcrop of this famous intrusion that I had already started to work on—it was really magical. And that feeling about those kinds of rocks has never left. I collect these kinds of intrusions, but this one was the best; it was just wonderful. Well, we went ashore and set up camp, and then we had the whole six weeks just to collect rocks and work on this intrusion. Of course we all had different things to do; various people did different things. But I and my student, Richard Forester, were mainly there to collect a complete suite of samples across the whole intrusion, not just those tiny little sets of samples that Lawrence Wager originally sent me. We had a little skiff with an outboard motor on it which would take us around.

COHEN: So you didn't actually have to get on the ice pack and climb?

TAYLOR: Well, I should say that this is on the edge of the ice sheet, and it was all rock. There were glaciers, so we had to walk on glaciers. Mainly we walked. Sometimes we would take a boat ride if we had to go across to an island, but basically it was a combination of riding in a little skiff with an outboard motor and mostly walking and climbing around. The Scottish group was taken to another locale. They weren't going to the Skaergaard, they were going to another place in Greenland a little bit farther up the coast. But the Americans, the group from Oregon and the group from Caltech, were there on the Skaergaard, because our focus was on the Skaergaard.

So what we did was gather by far the biggest collection of non-Oxford samples yet collected there. Wager had mainly focused on samples that were along the main fjord, but I wanted to collect from the entire intrusion. So we got, from this really, really important intrusion, which is certainly still the most important igneous intrusion on Earth by far—we got this huge collection of rocks, thousands of pounds of rocks, covering the entire intrusion. We brought it back on board the Norwegian sealer, but we had to leave before the end of the third week in August, otherwise we weren't going to be able to get back through the ice pack.

So we headed back. We were remembering the really bad weather and how awful it was in the North Sea, so we were just dreading the trip after we got past Iceland, but the trip back was perfect.

COHEN: You had all this ballast, all these tons of rocks. [Laughter]

TAYLOR: I don't know what it was. No, it was just better weather. Anyway, so we landed in England at West Hartlepool, offloaded our valuable samples, had them shipped back to Caltech, the Oregon group took their stuff and we took ours, and then for the next two or three years that's what I spent my time doing. Now, my student, Richard Forester, who helped me collect the samples, was doing his PhD thesis on some other area. He had followed up my earlier work in the Scottish Hebrides, the Isle of Skye and the Isle of Mull. That was his PhD thesis—and also another one of these kinds of low ^{18}O intrusions in the San Juan Mountains in Colorado. So he wasn't really part of this effort. I did it myself with the help of my technician. But when we finally wrote the papers up, he was a coauthor, because he had helped me, of course, collect the samples and so forth. [See, for example, H. P. Taylor Jr. and R. W. Forester, "Oxygen and hydrogen isotope study of the Skaergaard intrusion and its country rocks—Description of a 55-million-year-old fossil hydrothermal system," *J. Petrol.* 20 (3), pp. 355-419 (1979)].

I'll stick on the Skaergaard story more or less to its completion right now, because it certainly dominated my research. It is the dominant thing that I've done as a scientist, without question, because I started on it with my thesis and I came back into it with this wonderful opportunity to be part of the first non-British expedition into the Skaergaard intrusion. And then I had all these samples, which were incredibly valuable. Because a lot of people now wanted them, and I was perfectly happy to give them—

COHEN: Did you send Wager any of it? [Laughter]

TAYLOR: Well, no. By this time Wager had died. I had gotten to know him a little bit and talked to him, because he came to California while I was still a student and when I was a young assistant professor. But we were only the second expedition to the Skaergaard since he died.

Anyway, so I had all these, geologically speaking, incredibly valuable samples, and I was doing the oxygen and hydrogen isotopes on them. And it turned out to be a remarkable story, even more remarkable than I had realized before, because up to then, with Forester's thesis and a number of other things I mentioned earlier, we had mapped out the general distribution of $^{18}O/^{16}O$ ratios in and around these igneous intrusions, mapping the extent of the meteoric hydrothermal system that heated waters and interacted with the rocks. But the beautiful thing about the Skaergaard intrusion was—I mentioned earlier that it was the classic intrusion demonstrating igneous differentiation. And so you knew exactly what the last liquid was to crystallize way up high in the intrusion, called the Sandwich Horizon. In other words, here was an unsurpassed opportunity to put the magmatic structure together with the hydrothermal structure and show beyond any doubt that the hydrothermal system operated only after the crystallization—that there was no influx of meteoric water into the liquid intrusion, the liquid magma. You had to wait until the rocks fractured, and as soon as they were solid enough to fracture, the outside waters could make their way inward. But the way the Skaergaard intrusion crystallized—it didn't crystallize mainly from the walls inward, like many classic granitic intrusions. It crystallized mainly from the bottom upward and to a lesser degree from the top downward. So the last liquid to crystallize was a sheet of liquid way up high in the intrusion. And it turned out that some of the rocks underneath the sheet of liquid were fracturing and the water from the outside meteoric system was penetrating into the solidified intrusion *underneath* this sheet of magma liquid. Basically, we could map the entire hydrothermal system with relationship to the remaining liquid magma at each stage in the crystallization history and show that the liquid magma was essentially impermeable to these outside influxes of water. And then finally, as soon as the last little bit of liquid crystallized, then the whole thing fractured. Now the hydrothermal system could penetrate, and we had a big hydrothermal convection system which affected the entire intrusion. It was just a beautiful story.

At about this time—well, I know exactly when it was. It was in 1978 when I was approached to take up the Crosby Professorship at MIT and go there for a semester and teach a course if I wanted to.

COHEN: This was a visiting—?

TAYLOR: Yes, a visiting professorship at MIT. So in the fall of 1978 I went to MIT. I gave a course somewhat similar to the courses I was teaching at Caltech. Brian Wernicke [Chandler Family Professor of Geology] was one of my students. He came over from Harvard—Harvard students could come over. So I had quite a large class of MIT and Harvard students. But also I had quite a bit of free time, so it was a perfect opportunity to write up all of the Skaergaard stuff and get it more or less in hand.

What I was able to do as far as these hydrothermal systems were concerned was to map out the geometry and the timing of the influx of water into these crystalline igneous intrusions while they were still hot, and show that the water was getting in underneath this sheet of liquid magma at very, very high temperatures—800º, 850º, 900° C. No one had ever suspected before that you could have these meteoric hydrothermal systems getting into rocks that hot. Of course, then the water gets heated up by the hot rocks, and the system goes on for as long as it can, until finally all the original heat is gone and then the whole system just sort of decays off and dies. So these hydrothermal systems are like living phenomena, in a way, and that's why I call them fossil hydrothermal systems—because of course when I look at them, they're long dead. They're born when the igneous intrusion comes in, the energy source. They live for a while, and they go through a whole series of stages, and then they die, just like a living organism. And the way you were able to discern all these things was by mapping the distribution of oxygen isotopes, because as I mentioned earlier, rainwater is the only thing on Earth that has low enough $^{18}O/^{16}O$ ratios so that when it encounters these rocks—all of which start out pretty uniform, like the Moon; in our scale, about 6 per mil, or six parts per thousand, heavier than ocean water—they are pulled down to values of -2, -3, -4. Easily measurable, because we can measure down to a tenth per mil.

So basically what I did was map the distribution of $^{18}O/^{16}O$ ratios in the Skaergaard intrusion and from that was able to infer what was going on in terms of the timing and geometry of the hydrothermal system. At about this time, at the University of Arizona, there was another scientist I hit it off real well with. That's the interesting thing about scientific cooperation: I mentioned the fact that if McBirney and I had not hit it off so well personally, none of this would have happened. Well, there was a professor at the University of Arizona named Denis Norton whom I had met a long time before, because he was interested in ore deposits. He had worked at Kennecott with Dick Nielsen, who was with me in southeast Alaska and with whom I wrote those papers on ore deposits. Denis Norton was working there, but he left Kennecott and became a professor at the University of Arizona. And while he was at Kennecott, working with another scientist named Larry Cathles, they developed a computer code; computer modeling was just starting to come into geology at that particular time. I'm not into the computer business or the theory or the modeling stuff at all. But they had done this, and Denis Norton had set up a group at the University of Arizona in which he was modeling on a computer these hydrothermal systems, the exact same things that I was looking at in real life in the field. And, again, we hit it off real well. So I said, "Here, I can give you everything you need to know, all the information you need to know about the hydrothermal system around the most important intrusion on Earth—namely, the Skaergaard intrusion. Why don't we do a joint study modeling this thing, and see if you can't mimic with a computer my $^{18}O/^{16}O$ distribution in the Skaergaard intrusion?" And he said, "Well, we can do that."

So I spent a lot of time going back and forth to Tucson before I went to MIT, working with him. The code he had developed told us everything about how the water would move, if we defined the permeability of a system. It was a two-dimensional thing, and of course my work was three-dimensional. So we had to first of all transform my data onto a two-dimensional section and work on that two-dimensional section. But once we had done that, then all I wanted to see his computer modeling do was somehow or other mimic my measured $^{18}O/^{16}O$ distributions. Well, he could model the entire distribution with time, following the hydrothermal system from the time the intrusion first enters until it cools off completely. And there were a number of parameters that I won't go into in any great detail, but the most important parameter was permeability. And the actual permeabilities of rocks on cubic-kilometer-size scales were just not known at all. Permeabilities are typically measured by petroleum geologists, because of course in oil fields geologists are extremely interested in permeability; it's key in understanding how the petroleum gets from one place to another and also how easy it is to extract. So they have lots of ways for measuring permeability *in situ* and also on cores that they bring up. But that gives you only the permeability of a hand-specimen-size piece of rock. Well, that means nothing in fractured rocks, because a single major fracture, like a major fault, can produce an enormous permeability through rocks. So if you have interconnected fractures, you cannot get any idea of what the permeability is by measuring a hand specimen. As a matter of fact, if you measure a hand specimen of these kinds of rocks, you'd find that they're essentially impermeable, but in fact they're quite permeable because of the fractures. So the only real way to get permeabilities is to make field tests—say, to put a radioactive tracer down one hole and then a kilometer away have a probe that tells you how long it takes the fluid to get from that place to this other place. And then you're actually measuring permeabilities over kilometer-size masses of rocks. But that had never been done before. So the other aspect of this project was that since permeability was the key thing in determining how this whole thing was going to evolve on the computer, we were able for the first time to measure permeabilities of large volumes of these kinds of rocks *in situ*.

As a matter of fact, the whole thing just worked out unbelievably like a charm. Denis Norton would run his—and by the way, in those days this was very expensive computing, because it was a tremendously complicated code. So what we did was simplify it first and run it through. That was the other problem with this thing: You had to run the whole thing through. In other words, you had to develop the computerized hydrothermal system, let it begin, start running—and it ran for 500,000 to 700,000 years. He had to let it run—in computer time translated into real time—for about 700,000 or 800,000 years and then see what the result was. And if that didn't work, then you had to start the whole thing over again. [Laughter] So it was kind of expensive.

So what we had to do was zero in on the right permeability. We'd try a permeability, and it didn't even come close to giving the right answer. Then we'd change the permeability and finally we zeroed in on it and decided we were close enough to do the real thing. We ran the whole process through in a real detailed calculation. It came very close. It reproduced the actual isotopic values and the geometry of my $^{18}O/^{16}O$ distributions very, very close to my measurements. But it wasn't quite good enough. [Laughter] So we changed the permeability by a factor of ten. Every time we did it, we'd change the permeability of the intrusion by a factor of ten. Oh, I should say another thing about the Skaergaard intrusion, which turned out to be just unbelievable. I impressed upon you how important it was because of how it was differentiated, but the other thing about it was that it was intruded into essentially a very simple geological environment. There was a major unconformity in which about ten kilometers of flood basalts were sitting on top of Precambrian gneissic basement, OK? So what you had was a thick section of Precambrian gneiss, all of which can be considered essentially one kind of rock, and then above that ten kilometers of flood basalts, all of which also can be considered another kind of rock. So essentially, for our computer modeling problem the situation couldn't have been any simpler. We had essentially a three-rock problem. We had the ten-kilometer-thick section of flood basalts—and they weren't folded or anything, they were just flat-lying flood basalts sitting on top of Precambrian crystalline rock. And then this intrusion, about 55 million years ago, was emplaced into these rocks. This intrusion is about ten kilometers by eight kilometers by five or six kilometers thick—something like that, so it's kind of an equant-shaped massive intrusion which somehow or other made space for itself and sat there and crystallized, surrounded by, in a simple geometry, these two other rock types. So all we had to do was assign one permeability to the basalts, which are very permeable; another permeability to the crystalline gabbro; and another permeability to the gneiss, which was essentially impermeable, as it turned out. I already knew that the gneiss was essentially impermeable, because my hydrothermal system didn't go down into the gneiss. All of the +6 per mil, normal ^{18}O part of the Skaergaard intrusion was below the projection of this unconformity. So in other words, that simple geometry was part of the gift that I gave Denis Norton in this package. But the point of the thing is that by just assigning a single permeability to the gneiss, a single permeability to the crystalline Skaergaard intrusion, and another single permeability to the basalts, we were able to finally mimic the exact distribution of ^{18}O I had measured. I mean, it was a tremendous—

COHEN: So you're saying now you could quantify what you were doing.

TAYLOR: Yes, right. I was able to quantify it, and to measure the actual permeabilities of these giant-size masses of rock, which nobody had ever been able to do before. These are the same kinds of rocks that people were encountering in the oceanic crust, so it had great implications in terms of what was going on in the midocean ridges, too, which I'll get to. It couldn't have happened more beautifully. I was writing up my part at MIT, and Denis Norton was putting together the computer part, of which we were coauthors.

In the meantime, McBirney had finally gotten his part of the Skaergaard thing written up, and so we worked out a deal with the *Journal of Petrology*, which was the most prestigious igneous-rock journal, to put all three—and they were giant papers—all three big papers together in one edition of the *Journal of Petrology* [20(3): 1979]. They all came out together, back to back to back, and it made a tremendous splash. Well, it was the biggest thing of my career really, because it just laid the whole thing out. It just nailed it. [Laughter] And soon after that I was elected to the National Academy and a bunch of other things. Everything just came together. I became the Robert P. Sharp professor. I got a lot of job offers.

COHEN: So what do you do for an encore?

TAYLOR: Yes, I suppose you could say that. But it worked out just super. And those papers are extremely heavily cited, because they laid the groundwork for the whole thing. So much serendipity was involved in that every step along the way—sitting in the bar with McBirney, just discussing this thing, actually going to the Skaergaard. Soon after that, the Skaergaard turned out to be such a bonanza that people started going every year in hordes. And McBirney went back time after time after time. I never went back again. I had other fish to fry, and I also had enough samples. I got some extra samples from some of the other expeditions that went later on. But now you can get up there very easily. There's an airstrip there. You can actually fly in. There are other areas up there; it's literally a gold mine for geologists. Also, they recently found an important platinum and gold horizon in the Skaergaard intrusion, so they're actually maybe even going to mine it in the future. Even though it's in this quite inaccessible place, it's right on the ocean, so you can get the material out.

COHEN: How long did you stay at MIT?

TAYLOR: Oh, I was just there for a semester. I taught the fall term of 1978 and then came back.

COHEN: And you were glad to come back, of course.

TAYLOR: Well, actually, that's when I got divorced. My wife was a librarian and we were separated during that time physically. She came out for a couple of weeks, but then we just decided to split up. So the next year, 1979, is when I got divorced. I had been married twenty years, but we didn't have any children, so it was a very amicable divorce.

COHEN: It sounds like you were away a lot anyway.

TAYLOR: Yes, I was away a lot. I went to Labrador and lots of places that I haven't even talked about. Although she went with me on my Africa trip. I don't know if I mentioned my Africa trip, where I went all over Africa and ended up in South Africa collecting another one of these big intrusions, the Bushveld intrusion. Part of that trip I went to the Seychelles Islands out in the Indian Ocean. That was very interesting. [Tape ends]

Begin Tape 5, Side 2

TAYLOR: I was going to tell you about the Seychelles Islands because it sort of illustrates the way I worked. When I was doing this major project that ended up in my first important single-authored paper, "The Oxygen Isotope Geochemistry of Igneous Rocks" [*Contrib. Mineral. Petrol.*, v. 19, pp.1-71 (1968)]—when I was a young assistant professor—since I was making these analyses for the first time, I would go through the Caltech collection; and if I saw an interesting rock I would just analyze it. And there was a rock in there that had been collected by Harmon Craig. I think it was given to Gerry Wasserburg, and then Wasserburg put it in the Caltech collection. It was from the Seychelles Islands. It was a beautiful granite. I analyzed this sample, just to do it, and *bang!* Unbelievable, because it had a very low $^{18}O/^{16}O$ ratio, which at that time was unique among granites. And so I just put that in the back of my mind. And later on, when I took this big trip to Africa that was mainly a project to go to South Africa to collect this other important intrusion…. There are two really important intrusions on Earth, the Skaergaard and this one in South Africa called the Bushveld. It's much bigger and older than the Skaergaard intrusion, and just as much a classic in its own way.

So, again, I spent a whole summer in South Africa collecting the Bushveld intrusion. But on the way back, I flew from Nairobi to the Seychelles Islands. At that time the airport in the Seychelles had just opened. I think this was in 1973. If I had tried to go there before that, I would have had to go by ship, which is pretty time-consuming. But they just opened this airport at the time I was going to Africa, so I extended my trip a little bit and went out to the Seychelles Islands for a week. [Laughter] To be a geologist is just—I mean, people pay huge amounts of money to go to the Seychelles Islands on holiday. [Laughter] So here I was, going out there not only to collect rocks but I had a great time. It's the only oceanic island that's made of granite. All the other ones are made of volcanic rocks. But this is made of granite, sitting out in the middle of the Indian Ocean, so the beaches are completely different, because they are granite beaches. You have palm trees and tropical foliage with a granite background instead of volcanic. It's a glorious, glorious place, and at the time almost completely untouched. Very few tourists went there.

This one sample that I had gotten from the Caltech collection, that had this unique low $^{18}O/^{16}O$ ratio, was the reason I went there. So I collected everything I could get. I rented a car and drove around the island and collected the big island, and then I took a light airplane trip over to one of the other islands, Praslin. There are two main islands that I collected, Mahe, the big island, and Praslin.

Well, all those rocks came out fantastic, too, because it was a huge area—it was the first really large mass of what I call low ^{18}O magmas. Remember I said that in the Skaergaard the liquid magma was impermeable to this low ^{18}O water that was present in the rocks outside the intrusion. The water just didn't get in there. The liquid magma stayed with its original oxygen-isotopic composition. There are some liquid magmas that we can analyze: For instance, on Iceland, where you have an eruption yesterday, you can go out and analyze it, and some of those have low $^{18}O/^{16}O$ ratios—so you know that the liquid magma sometimes gets low on ^{18}O. The question is how does it do that? Well, that happened later on in my career. But the Seychelles was the first large plutonic example of a low ^{18}O magma. It was absolutely clear-cut—the whole thing, the entire island of Mahe, which is about twenty miles long—

COHEN: So that had to be formed in a completely different way than all the other islands.

TAYLOR: Well, than essentially most other granites on Earth, for that matter. [Laughter] It's still virtually unique, as far as I can see. And I've never really written that stuff up. That's one of the things I still have sitting on my desk—to write up the final monograph on the Seychelles Islands study. Little bits have come out, but the complete story I hope to get to.

OK. We finished up the modeling study with Denis Norton. The publications came out and made a big impact. The next big thing we got involved with also goes back.... I've done lots of cooperative projects; I like that way of doing science. But they have to be people I really get along with. So the next stage was as follows. It was an obvious thing to get involved in the midocean-ridge problem, because I think I mentioned to you that 1965 was the birth of plate tectonics and seafloor spreading. Before that, nobody had any real idea that that's what's going on. But after 1965, '66, '67, essentially that was a revolution in Earth sciences that happened while I was a young professor. Caltech didn't play much of a role in it; it was mainly Harry

Hess, at Princeton. But nevertheless, it dominated our thinking. And so in the seventies, it was obvious that one of the things you wanted to do was apply oxygen isotopes and hydrogen isotopes to the same kinds of problems that I was looking at on land, with rainwater, to submarine systems involving ocean water. It was obvious that the same thing was going on, that you had big hydrothermal systems—because a midocean ridge is just a big mass of magma coming up into permeable rocks. The only difference is that it's ocean water instead of meteoric rainwater.

Now, the problem as far as oxygen isotopes are concerned, though—rainwater is very low in ^{18}O. It goes down to -15 or -20. When -15 water penetrates and interacts with +6 rock, it's a huge difference until the rock gets pulled way down in ^{18}O to values like -3, -4, the kinds of things I talked about at the Skaergaard. Now, with ocean water the same thing is going on, but the signal-to-noise ratio is much less favorable, because now the situation is as follows. Ocean water is near zero on our scale. That's because it's our standard, OK? So here you have zero per mil water entering +6 rock. It's only a 6-per-mil difference instead of 20 per mil, so the effects now are not going to be anywhere near as great. And if the temperature is low enough—I won't go into all the details—there won't be any lowering of ^{18}O at all. The only lowering happens if the temperatures are above 300° C. If the temperatures are below 200° C, you get an enrichment in ^{18}O instead. And if the temperatures are just right, like around 250° C, you could have a whole ocean go through the rocks and you won't see any change in ^{18}O at all. It would be just like nothing. In terms of ^{18}O nothing would happen. Therefore submarine environments are much more tricky because, in fact, a lot of the water does go through at about 250°. But nevertheless, it was obvious that this needed to be looked at.

I can't emphasize enough what a revolution plate tectonics was geologically. Before that, when I was an undergraduate certainly and also as a graduate student, nobody believed in continental drift. If you talked about continental drift in the United States you were laughed at. In fact, I wrote a term paper once for a course taught by Clarence Allen on continental drift, and it was all marked up. [Laughter]

COHEN: He didn't believe it?

TAYLOR: He didn't believe it, even though Clarence Allen was one of the major proponents of it, because he got involved in these major faults, like the San Andreas Fault, which had these giant displacements. Nobody could figure out how you could have a fault with 300 kilometers displacement. But plate tectonics solved all this. So, all the problems that people were facing all of a sudden cleared up. I would say that before 1964 essentially nobody believed in it, except maybe three or four strange ducks. And after 1967 or '68 everybody believed in it. [Laughter]

COHEN: That's a quick conversion.

TAYLOR: Oh, it was just *boom, boom, boom*—so many things fell into place so quickly. The key paper was a paper that I actually looked at in a seminar when I was a student here at Caltech. These researchers down at Scripps went out on a ship and measured magnetic anomalies, and they found these very strange magnetic anomalies, the strange reversals in magnetism, that formed linear arrays in the ocean floor. And people thought they were basaltic dikes. Nobody had any idea what was causing these linear magnetic anomalies in the ocean floor. And at the same time, on land they had just discovered—and I'll mention this because of a person named Allan Cox, who later became the dean of Earth sciences at Stanford and tried to hire me up at Stanford, was one of the main discoverers of this, of the fact that the Earth's magnetic field periodically reversed, just flipped over. But here was one group working on the flip-flop of the Earth's magnetic field and another group working on the ocean and seeing these linear magnetic anomalies. And then this graduate student—or I guess he was a young postdoc—in Britain called Fred [Frederick J.] Vine happened to be looking at the same paper that I had looked at and wrestled about when I was a student, these linear magnetic anomalies, and he noticed that if you put a line down the middle of them, they were symmetrical. One side was the mirror image of the other side. After it had been pointed out, it was obvious that they were mirror images. [Laughter] And the mirror plane was at the midocean ridge. And the widths of the anomalies were proportional to the age on land, the reversals that Allan Cox and his cohorts had found. So in other words, it all came together. And in fact, what it was was that ocean crust was forming at the midocean ridge and moving outward, and as it moved outward it had the magnetic signal of the Earth at that particular time. And then, when the magnetic signal reversed, the whole thing flipped. So these linear magnetic anomalies could be translated directly into the age sequence, the chronology of the reversals on land that Allan Cox had discovered. Fred Vine saw this from data that had been sitting in the literature for almost ten years, from the Scripps scientists, who didn't know how to interpret it. And it probably couldn't have been put together until you had the reversal chronology, because up until then nobody even was sure that the Earth's magnetic field was reversing.

But, you see, it all came together. And the main proponent of it was Harry Hess at Princeton, who had written this paper on seafloor spreading ["History of Ocean Basins"], not thinking in terms of magnetic anomalies and not thinking in terms of magnetic reversals, just thinking in terms of trying to put together a reasonable picture of how you can explain the ocean basins and the trenches and everything else. And so he had this paper which was published in 1962. Then you had the magnetic reversals and the linear magnetic anomalies—the Vine interpretation—plastered together with Harry Hess's model. All of a sudden, people, including myself, thought—as a matter of fact, I can tell you exactly the time that this came to my attention. I was at a Geological Society of America conference, and I went up to Robert Hargraves, who was a paleomagnetics guy who does the same kind of thing that Joe Kirschvink does here at Caltech now—he was a professor at Princeton, where Harry Hess was. I asked Hargraves, "Is anything interesting happening at Princeton?" And he said, "Well, as a matter of fact, there is. There's this fellow, Fred Vine, who's visiting us from England." And a week or two before that is when Vine, putting his ideas together with the Hess model, had discovered plate tectonics. And here I said, "Is anything interesting happening at Princeton?" and he said, "Well, as a matter of fact, there is." [Laughter]

And then, a month later, it was just everywhere. I mean, it just permeated the field. If the Internet had been around then, it would have been even faster. It was magical. Everything fell into place. The so-called explanation of the 300-kilometer displacement of the San Andreas Fault—I mean, *everything* fell into place so perfectly that when you look back on it now, how could we have been so stupid?

COHEN: Well, hindsight is always fantastic.

TAYLOR: Just the idea that the continents link up across the Atlantic Ocean. I mean, that was really pretty obvious. [Laughter] It just shows how conservative geologists are, particularly American geologists. That brings me to my story here.

OK, I'll get back on track now. There are these things called ophiolite complexes. Now, they've been known for a long time in Europe. What they are is a sequence of rocks. Sometimes it's called the holy trinity, because they have pillow lavas underneath deep-sea sediments, what we call pelagic sediments, sitting on top of gabbros and peridotites—these same kinds of rocks that I talked to you about in the Skaergaard intrusion. So you have this trinity of cherts and pelagic sediments—siliceous sediments—sitting on top of pillow lavas, which in turn sit on top of gabbros and peridotites. The sequence is consistent. And that so-called holy trinity had been recognized in a lot of places—in the Alps and so forth. It had never been recognized in the United States. All the time that I was an undergraduate and a graduate student, not one mention of ophiolites in North America. First of all, nobody understood what it meant. But as soon as plate tectonics happened—as a matter of fact, a Caltech undergraduate named Eldridge Moores, who got his PhD at Princeton with Harry Hess, became one of the main figures in this breakthrough.

That's an interesting story. When I was a senior graduate student, I think the last year before I got my PhD—I think I mentioned that we were short of teachers here at Caltech and that's why I stayed on as a young professor, because so many senior professors resigned or left at that particular time, like Dick Jahns and Jim Noble. The shortage was there even before that, because Al Engel had left and he's the one who used to teach metamorphic petrology to undergrads. And we had a lot of courses to teach. So as a graduate student I taught metamorphic petrology, and my teaching career actually started when I was a graduate student. So it was natural for me to stay on as a professor even after I got my PhD, and that's what I did. I taught some other courses plus summer field geology. And one of my students was this fellow Eldridge Moores. He was also in my house, Dabney House, and I liked him a lot. We hit it off real well. He's now a professor at UC Davis. And John McPhee—do you know this writer?

COHEN: Sure.

TAYLOR: OK. Well, John McPhee has written a book about Eldridge [*Assembling California* (New York: Farrar, Straus & Giroux, 1993); revised and reprinted in McPhee, *Annals of the Former World* (New York: Farrar, Straus & Giroux, 1998)] because he and Eldridge—as a matter of fact, John McPhee learned a lot of his geology from Eldridge. Well, when Eldridge did his PhD thesis at Princeton, it was on one of the ophiolite complexes. Now, he started before plate tectonics, and so he did a good job mapping everything, but he didn't really understand what he had. This was a place in Greece, one of these ophiolite complexes in Greece, and he described it but he didn't really know what it was, because this was pre-plate tectonics. As soon as plate tectonics happened, he realized that all it is is oceanic crust. Every ophiolite complex then, after plate tectonics, was interpreted as oceanic crust that somehow or other had been shoved up on land. We didn't need to drill a hole—the classic Moho that was talked about back in the sixties—to see what the oceanic crust was like. All over the world, these ophiolite complexes existed, and they are just oceanic crust, chunks of oceanic crust. So Eldridge realized this—that what he had done for his thesis was a chunk of oceanic crust, and he immediately went to Cyprus and described another one, along with Fred Vine. He got very close to Fred Vine, the discoverer of plate tectonics, and they worked on this together. And then later on he started doing these things here. Now, the interesting story here is that up and down the Coast Range of California, starting right here in our backyard and going all the way up to the Oregon border, there is an entity, this sequence: cherts sitting on pillow lavas sitting on peridotites. It goes the entire length of California. It's called the Coast Range Ophiolite, and it's sitting there. The rocks were known, but they weren't called ophiolites until after plate tectonics.

It just shows you that if you don't really know what you're looking for, you won't see it, because as soon as plate tectonics happened, the people who were mapping the Coast Range immediately said, "Well, that's what this is. This is a chunk of oceanic crust, and it goes all the way up the entire coast." These complexes were just chunks of oceanic crust, and if you were mapping them, or walking on them, you were actually going down twenty miles into the upper mantle of the Earth. That was extremely powerful. And again, in the back of my mind it was obvious that we should apply stable isotopes to these kinds of phenomena. Instead of trying to drill deep holes through modern oceanic crust, which is difficult—and actually which was starting at that time—you could go out and collect samples from these ophiolites.

At about this time, another one of these fellows—as a matter of fact, a person who I probably hit it off the best with out of all the people from other universities that I've been involved with—his name is Bob [Robert G.] Coleman. He was at the U.S. Geological Survey and I first got involved with him back in the early sixties. I was interested in very high-pressure metamorphic rocks, because again, the question was, Are these high-pressure metamorphic rocks formed at such and such a temperature or not? And oxygen isotopes were originally, as I told you, thought to be most useful as a geothermometer. So it was natural for me to approach Bob Coleman, who was working on such rocks up in Northern California in the California Coast Ranges. They're called blueschists. They are metamorphic rocks, and they're blue because of the mineral glaucophane in them. That term, blueschists, was just starting to be formulated, and the idea that they were unusually high-pressure and relatively low-temperature rocks. We needed to get some idea what the temperatures were, and oxygen isotopes were the thing. So I approached Bob Coleman when I was a young assistant professor, and we hit it off immediately, and he gave me all of his mineral samples and I analyzed them. Again, it came out beautifully. They were the right temperatures. They were all real high pressure—we knew they were high pressure—and they all formed at about 300° C, relatively low temperatures.

And the other thing about it was, I'm very interested in college football. I told you that I helped coach, and I played at Caltech for Bert LaBrucherie. I kept my interest. Well, Bob Coleman had played football for Oregon State back in the early forties, before he went into the army. So I just loved talking to him about old college football from Oregon State. That was a Rose Bowl team that never played in the Rose Bowl, because World War II had just started and they were afraid to have the game here in Pasadena, so they moved it to Durham, North Carolina. It's the only Rose Bowl game that's ever been played outside of Pasadena, and Oregon State went to that. He knew all the guys on that team. Anyway, we hit it off great, and I kept up my association with him off and on after our paper together on the blueschists. [H. P. Taylor Jr. and R. G. Coleman, "O^{18}/O^{16} ratios of coexisting minerals in glaucophane-bearing metamorphic rocks," *Geol. Soc. Am. Bull.*, 79 (12), pp. 1727& (1968)] As a matter of fact, one of my favorite people, Jim [James R.] O'Neil, who was Bob Clayton's first student, came to Caltech to work with me and Sam Epstein. And he did some projects with me—mainly with me and also some with Sam—and after his tenure as a postdoc here, he was looking for a job. I suggested he look at the USGS. At that time Bob Coleman was the head of the Geochemistry and Petrology Branch at the USGS, so Jim and I went up there and looked around. They finally did offer Jim O'Neil a job and he spent the whole first part of his scientific career at the USGS, in Menlo Park. It was Bob Coleman who put this whole package together. And since I was so close to Jim O'Neil—the liaison back and forth between the Menlo Park lab and down here—we did a lot of nice things together over the years, and it all started because of this association with Bob

Coleman.

So in the middle seventies, Coleman switched his…. He was always interested in the problem of blueschists, which are formed when one of these plates gets subducted down one of the oceanic trenches, like the Marianas Trench off the coast of South America—these deep trenches where the plate is being shoved down into the upper mantle. That's how these high-pressure rocks called blueschists are formed. Later on, some of them sometimes bounce back up from great depths and we are able to look at them in surface outcrops. So he'd always been interested in this problem. Of course when plate tectonics came along, he became really interested in the oceanic crust, because he had looked at it in a lot of places. He didn't know then that it was oceanic crust, but it became clear after the plate tectonics revolution that it was. So what he wanted to do then, as a member of the U.S. Geological Survey—he got together with a professor at UC Santa Barbara and they put together a plan. Because the trouble was that the USGS couldn't work overseas; it's the United States Geological Survey, so they really have to work in the United States. But Coleman wanted to work in the best places he could work. This was always my feeling; I wanted to work on the best geological example I could find anywhere on Earth. I was able to do this, because I was free as a bird, because I was in an academic institution. He was not free, because he was at the USGS; he could work in the United States, but he couldn't go overseas very easily. So he had to get somebody else to put in for a grant to NSF if he wanted to work overseas, and he had to get special dispensation from the USGS to do it. Anyway, with the help of this professor, Cliff Hopson, at UC Santa Barbara, they put together an NSF grant in the mid-1970s to go to what Bob Coleman knew was the best ophiolite on Earth. It's in Oman, in the southeast corner of the Arabian Peninsula, at the end of the Persian Gulf. Essentially the entire country of Oman is one of these ophiolites.

COHEN: It's a desert, isn't it?

TAYLOR: Yes, it's a desert. Well, that's another nice thing. Deserts are nice for geologists because the outcrops are so good. You don't have a lot of vegetation. I mean, there are other big ophiolites on Earth, like in Papua, New Guinea, but they're all covered by jungle. You can't see what's going on. First of all, Oman is the biggest, best, most glorious, certainly the best outcropping of one of these ophiolites on the face of the Earth. Nothing even comes close to it.

COHEN: And this was known?

TAYLOR: Well, it was sort of known, because oil company geologists had written a report on it. So, yes, the general idea was known, but nobody had ever done any scientific research on it up to this point. It was known to be one of these fragments of oceanic crust, but it was not just a fragment of oceanic crust, it was a giant slab 300 miles long, about 30 miles thick, and 40 or 50 miles wide. It's a chunk of the oceanic crust that got shoved up onto the Arabian Peninsula about 100 million years ago and was preserved. See, that's the trouble; the fate of most oceanic crust is to get subducted—shoved down. It spreads away from the midocean ridge and then when it reaches a trench, it goes down. So it takes some weird geological accident to somehow or other shove it up onto a continent, and once it gets up onto a continental slab, it's preserved. Well, Oman was the greatest, most wonderful geological accident in the whole history of the Earth, because this sample of 100-million-year-old oceanic crust formed at one of these midocean ridge spreading centers, got shoved up onto the Arabian continent and was preserved there. So now you can go and look at it. You can actually go there and walk thirty miles down into the upper mantle. It's mind-boggling to think that you can do this. You don't need a drill hole, it's all sitting right there out in front of you. So Coleman recognized this as an important thing to study, and he put together this program to do it. Since I knew him well, I said, "Gee, why can't I get involved in this project and do the oxygen isotopes on it?" He said, "Fine." [Laughter]

Actually, I was pretty busy at this time, so it was very hard for me to get away when they finally got the expedition all put together. They did get the funding for it. But I had a PhD student named Bob [Robert T.] Gregory, who was just starting out and was very interested in this kind of project; so I sent him instead. The only reason I was able to do so is because I had my own NSF grant. So I sent Bob Gregory along and he worked out perfectly. He and Bob Coleman got along great. Gregory didn't just collect samples; he actually participated in the mapping. He's a very good mapper. They mapped a whole swath all the way from Musqat, Oman, which is the capital, built right on the peridotite, going all the way across the entire mass of ophiolite—a swath of about seventy kilometers across the entire ophiolite. They mapped this and collected samples from it. And then Gregory came back, and for his PhD thesis that's what he did. We published this. [R. T. Gregory and H. P. Taylor Jr. "An oxygen isotope profile in a section of Cretaceous oceanic crust, Samail ophiolite, Oman: Evidence for ^{18}O buffering of the oceans by deep (>5 km.) seawater-hydrothermal circulation at midocean ridges." *Jour. Geophys. Res.*, v. 86, pp. 2737-2755 (1981)] It was the first time that we had a complete suite of samples from the entire mass of oceanic crust. Well, it was just beautiful, because the picture came out almost beyond belief. After he had gotten all the data together, it turned out that the entire lower part of the ophiolite, where the gabbros were—which are petrographically like the Skaergaard rocks—they were low in ^{18}O. The upper part, the pillow lavas, was high in ^{18}O.

Of course, I should have pointed out that typically between the pillow lavas and the gabbros in these ophiolites there is also a thing called sheeted dikes, which pre-plate tectonics nobody could understand, because it's 100-percent dikes, and of course that's just the dikes that form at the midocean ridge. Nobody could understand when you saw an outcrop that was 100-percent dikes, how it could form, because the only way a dike of basalt can come in is to extend outward and move the crust out. At a midocean ridge, that's exactly what you'd expect, but nobody could understand how you could have dikes that went on and on and on forever. Again, this is one of the things that plate tectonics explained beautifully—now the sheeted-dike complexes became very explainable. So as you'd go down through the pillow lavas, they were high in ^{18}O but the sheeted dikes were essentially not changed at all. And you'd go down into the gabbros and they were lower in ^{18}O. And you put together the whole picture and made a mass balance of the entire oceanic crust. There was essentially no change in ^{18}O. If you added up the rocks that were enriched in ^{18}O and the rocks that were depleted in ^{18}O, they balanced each other out almost perfectly, so that the entire section had not been changed in ^{18}O at all.

By the way, I should point out that all of this had to do with one of these hydrothermal systems: We showed for the first time that the marine hydrothermal system was penetrating at least eight kilometers down into the oceanic crust, down to the Moho. The Moho is the boundary between the oceanic crust and the upper mantle. And what we found was that these ^{18}O effects, which can be explained only by hydrothermal interactions with ocean water, were going essentially all the way down to the Moho. So that meant the entire oceanic crust was hydrothermally altered, even though nobody ever would have even thought of that before. But not only was it hydrothermally altered, there was a huge mass of water that went through it at high temperatures, but there was no overall change in the ^{18}O of the crust. And the only way this can happen is, it's either coincidence or it's cause and effect—namely, that the ocean water's isotopic composition is being controlled by this process. Now, this had been suggested before. But this nailed it, because we had the absolute demonstration that the entire oceanic crust had been drastically changed in ^{18}O. The upper part had been drastically enriched in ^{18}O, the lower part depleted in ^{18}O. But overall, nothing had happened. So what it meant was that this process of hydrothermal interaction at the spreading center was controlling the isotopic composition of ocean water.

So Gregory, who was a good theoretician also, and because it was a really important thing to do, made a theoretical model of this process. He was able to show that given different kinds of spreading rates around the world, it would take only.... If I started the oceans with any arbitrary value—say, God made the oceans +8 per mil, it would take only about 40 million years to get it down to zero again, by this process. So for the first time we absolutely knew for sure that the hydrothermal interactions at the midocean ridge spreading centers were controlling the isotopic composition of ocean water. That's the reason ocean water is 6 per mil lower than basalt. There are other processes that affect ocean water, too, but this is by far the dominant process.

Well, that's basically it. We continued to refine this. The samples were so valuable that when we got back here—there was one paper with Wasserburg, because Wasserburg and one of his students did the strontium isotopes and the neodymium isotopes on these same samples. [M. T. McCulloch, R. T. Gregory, G. J. Wasserburg, and H. P. Taylor Jr., "Neodymium, strontium, and oxygen isotopic study of the Cretaceous Samail Ophiolite and implications for the petrogenesis and seawater-hydrothermal alteration of oceanic crust," *Earth Planet. Sci. Lett.*, 46 (2), pp. 201-211 (1980)] And that turned out to be a beautiful study, too, because they were able to show that the strontium isotopes—see, ocean water has a certain amount of strontium in it. So you could actually see how far down the isotopic composition of strontium had been changed by this process, whereas the neodymium had not been changed at all. And the reason for that is that there is essentially no neodymium in seawater. So in other words, it put together the picture for neodymium isotopes, strontium isotopes, oxygen isotopes, and hydrogen isotopes. It just laid it all out for such a very important part of the Earth—namely, oceanic crust.

The next interesting thing that happened in my life was mainly in the nineties. Up until about the eighties, I had nothing but men graduate students and postdocs, but after 1986 or something like that, it was almost all females. [Laughter] Near the end of my career, so to speak, I thought nothing new would come. I had a postdoc from Israel named Mordeckai Magaritz, with whom I did a lot of work. Unfortunately he died quite young. He probably would have ended up being the most important scientist that was associated with me, but he died too young. He was the head of a whole department at the Weizmann Institute before he died [1993]. Another person who worked with me as a postdoc, Bruno Turi, was a professor at the University of Rome, so I had several major projects on Italian rocks, and I love to go to Italy, so I should talk a little bit about Italy. [Tape ends]

HUGH P. TAYLOR

SESSION 6

July 16, 2002

Begin Tape 6, Side 1

TAYLOR: Well, I was talking about the oxygen and hydrogen isotope distribution in the oceanic crust, which is fundamental to all Earth processes, because, first of all, it's the major process of formation of seventy-five percent of the Earth's crust in the oceans, which takes place at the midocean ridges and which involves hydrothermal interactions with seawater. And I was lucky enough to get in on the ground floor of the oxygen isotope work on this phenomenon. If you're interested in the interaction of water with rocks, then a study of hydrogen isotopes and oxygen isotopes, the two elements of the water molecule, is the way to proceed. So we were skimming the cream of this business of finding out what was happening in terms of hydrogen- and oxygen-isotope changes in all the rocks of Earth, but in particular in this giant reservoir of rocks in the oceanic crust. First I need to tell you a little bit about the oceanic crust. The oceanic crust goes back only to about 150 million years ago, because the oceanic crust that formed before that, in the entire rest of geologic history, has disappeared. It's gone down subduction zones and back into the mantle. And then what happens to it is another story. But what turns out to be very important—in terms of all kinds of geochemical parameters, but in particular hydrogen and oxygen isotopes—is when the oceanic crust is originally formed, it looks just like the basaltic crust of the Moon. The Earth and the Moon look like they come from the same basic $^{18}O/^{16}O$ reservoir.

COHEN: And formed at the same time also?

TAYLOR: Well, the Moon may have split apart from the Earth very early in the Earth's history. But basically they were formed from the same reservoir of material, and oxygen isotopes make that very clear. So the stuff that's coming up as basalt from the upper mantle and making the oceanic crust initially looks basically just like the Moon. It has this value of +6 that I've been talking about—6 per mil heavier in ^{18}O than ocean water, which is our standard. But the term "oceanic crust" refers just to the rocks down to the Moho, which is the boundary between the crust and the mantle. The ocean crust is typically about eight kilometers, or five miles, thick, all over the entire oceans. And what we were able to show—by looking at these ophiolite complexes, which are fragments of old ocean crust that somehow or other has been saved from subduction by being shoved up on continents—was that the entire thickness of oceanic crust has been changed from its original value to new values of ^{18}O by these hydrothermal interactions. In other words, the entire eight-kilometer-thick section of oceanic crust has been totally reconstituted. The upper parts have become very rich in ^{18}O and the lower parts have become depleted in ^{18}O.

Now, that has great implications, because most of this material of course goes down into the mantle, and whereas the mantle is all about +6, the stuff that's going down into it has been modified in this way and some parts of it are very rich in ^{18}O, up to +15 or so, and some parts are very low in ^{18}O, down to +2 or something like that. The basaltic magma that comes up at the midocean ridges is very uniform at about +6, but the solid rock that goes back down is very heterogeneous in ^{18}O, because of the seawater-hydrothermal interactions that take place near the midocean ridges. And the only way you can demonstrate this, really, is with oxygen isotopes.

We were lucky, in the sense that without the development of the concept of seafloor spreading and plate tectonics, which happened in 1965, along with a coming together of stable-isotope geochemistry—Sam Epstein's and Harold Urey's putting together of the techniques for doing these kinds of things.... I still look back on it and think how remarkable it was to have this all come together at the same time, because if either one had been ten or fifteen years out of sync, it wouldn't have played out in the same way at all. Instead, as soon as we had the tools for making the measurements, we had the concept, which led us to the correct rocks, and everything just fell into place. And in those days we had plenty of research money! So we could go to these faraway places and get samples and so forth. Nowadays it would not be so easy—and before World War II it would not have been possible, either. As far as my career is concerned, the paradise, in terms of funding, was in the fifties and sixties and seventies. Since then it's been downhill. The right place at the right time has just been the story of my life. [Laughter]

So we went to the biggest and best ophiolite on Earth. First you get the concept of what kind of rocks you want to look at and then you find the place that represents the biggest and best example of what you're trying to look at—because most of the Earth is fairly well understood, in terms of mapping—and also you want to study an area that has nice outcrops so that you can actually get well-documented samples. Well, as I said, the Oman Ophiolite, this huge rock mass in the southeast corner of the Arabian Peninsula, was ten times or a hundred times better to study than any other ophiolite on Earth. So the idea of being able to get together with Bob Coleman of the U.S. Geological Survey and go over there and do this study was just marvelous.

COHEN: This was the late seventies?

TAYLOR: Yes, this was the late seventies. Coleman was also interested in other things. He was then also very interested in the opening of the Red Sea. Now, the Red Sea started as a rift zone. It is true oceanic crust. There, over the last 15 million to 20 million years, a continent has split apart and made a new ocean basin, a very narrow ocean basin. Bob had gone over there in the sixties and gotten a bunch of samples that were associated with the original opening of the Red Sea, and because of my association with him, I asked him if I could work on these samples, so we collaborated on this project. It turned out that what he had there was basically an ophiolite-like complex, with sheeted dikes and rifting clearly associated with the opening of the Red Sea. What I was able to show was that the hydrothermal fluids in this case were not ocean water: In other words, when the Red Sea originally opened, the overlying waters were all meteoric in origin, as they should have been, because this was a splitting apart of dry land—of a continent. So this was the first example of an ophiolite complex formed on land at the time of the splitting apart of the continents. From then on, I really started concentrating on these rift zones, because a rift zone is an ideal place to observe the kinds of oxygen and hydrogen isotope phenomena I was vitally interested in. What is needed—in order to have one of these big hydrothermal systems, whether it's oceanic beneath the sea or meteoric on land—is a giant heat engine. That basically means having a lot of magma coming up. And there also needs to be lots of fractures, to allow the surface waters to go down and interact with the hot rocks. Rift zones are the ideal kind of situation for both these phenomena, and they exist in various places around the Earth and they have formed over a wide range of times in Earth's history. So a lot of the rest of my career was spent looking for all the different kinds of rift zones I could find, and going to them. And it never failed. Every one of them had this—

COHEN: Pay dirt?

TAYLOR: This pay dirt! That's a good way of putting it. And the other beautiful thing about it was.... You see, geology can be complicated. The kinds of things I've been talking about are relatively simple things, where something happened in a certain set of circumstances: For instance, you had an intrusion, you had an opening of the Red Sea, and you had the formation of one of these hydrothermal systems, and from then on very little else happened to it. You're looking at it without much of any later geologic complications. But in many other, older localities, there's another geologic event after that, or even several events, particularly if you're looking way back in Earth's history. In other words, everything is all messed up. You take these things that originally formed this way and then you fold them and you fault them, you break them and you massacre them, and then a present-day geologist has to try to look back through that mess to see what was going on at the time the original phenomenon was happening. This is often very, very difficult. Again, a remarkable thing about oxygen isotopes is that these hydrothermal processes transform the $^{18}O/^{16}O$ ratios of huge masses of crust. Cubic-mile-size masses of crust get their isotopic composition changed from what it was when they started to these new values, which can either be low or high depending on the isotopic compositions of the different kinds of waters as well as on the different temperatures. Huge masses of the crust have had their $^{18}O/^{16}O$ ratios transformed permanently, and all of these later events—folding, faulting, and disruptions—won't change this. Once the crust has had its oxygen-isotopic composition changed drastically, it's preserved throughout all these other complicated later events. So you can look back and see the original event through all this structural mess, and that's another thing I was very interested in. One example of this is one of the interesting kinds of rocks that geologists have known about for a long time, called eclogites. These are rocks, basically, that have exactly the same chemical composition as basalt. In fact, they are basalts, but they've been heated up and raised to very high pressures, so the mineralogy is no longer the mineralogy of basalt. Instead of having feldspar in them, which is a low-density mineral, they have garnet in them. They are garnet-pyroxene rocks rather than plagioclase-pyroxene rocks, basically; the plagioclase is gone because plagioclase isn't stable at real high pressures. So when one of these basalts gets subducted down into mantle, it gets transformed into eclogite.

Now, the interesting thing about this is that many of these eclogites can be found at various places around the Earth. And we started analyzing these eclogites, and it turned out that these eclogites weren't +6. They were basalt in composition, but instead of being like fresh basalt, they had ^{18}O values that ranged from +2 to +12—exactly the values we see in hydrothermally altered oceanic crust. It was very clear that these eclogites were in fact subducted oceanic crust—which was then metamorphosed deep in the mantle, in some cases twenty or thirty or forty miles deep into the mantle, and then some geological process brought them back up to the surface, where we could collect samples from their eroded outcrops. Now, one way they get brought back up is in explosive eruptions, such as diamond pipes; diamonds are also formed deep in the mantle at very high pressures. That's the only way you can form diamonds, which are a form of carbon. At normal pressures it's graphite, but deep in the mantle at real high pressures, carbon is transformed into diamonds. So we know that these materials came from deep in the mantle, because they contain diamonds. They also contain these eclogite fragments, and these eclogites were found to have this strange set of ^{18}O values, which proved that all they were was oceanic crust that had once been up on the surface of the Earth, had been hydrothermally altered, and had been taken back down thirty or forty miles, before being erupted back up to the surface, along with the diamonds. So we can see that whole process as having taken place. Essentially, most of the eclogites you see are just metamorphosed oceanic crust that's gone through this original hydrothermal process near the surface of the Earth.

Now, one of the other interesting things about this is that from a lifetime of looking at oxygen isotopes in rocks—which is basically my whole *raison d'être* [laughter]—I came to a somewhat outrageous hypothesis. Because there is no way to get large changes in ^{18}O at high temperatures, the only way you can get large changes in ^{18}O is to have a geologic process that occurs at low temperatures, because that's the only way the fractionations are large enough. So therefore, after twenty or thirty years in this business and after looking at rocks all around the world, I finally realized that every rock we look at that's significantly different from the primordial value of +6 has to, at one time or another—either that material or the parent of that material or the grandparent of that material—had to reside on or near the surface of the Earth. So if we see a granite which is +9 in $^{18}O/^{16}O$, then some part of that granite, which is roughly fifty percent oxygen, must have originally resided on the surface of the Earth, because that's the only place where it could have gotten that high ^{18}O value. And this, I think, even though it is somewhat of an outrageous hypothesis, has held up even over the last twenty years or so since I made it.

See, the Moon has no water, and the Moon is very homogeneous in ^{18}O, whereas the

Earth is very heterogeneous in ^{18}O. A remarkable difference between the Earth and the Moon is that during most of its history the Moon has had almost no water. The reason there's so much ^{18}O heterogeneity on the Earth is basically because we have a lot of water along with the rocks, and the water and the rocks interact with each other over a whole range of temperatures, particularly at low temperatures. For instance, all sedimentary rocks with clay minerals in them form during weathering at low temperatures. Well, that process produces very ^{18}O-rich materials. That's the only way to make such very ^{18}O-rich materials—to have something happen at low temperatures interacting with an oxygen-rich fluid, like water. And that happens on the Earth but it doesn't happen on the Moon, so the Moon is very homogeneous in ^{18}O. And it's clear that the oxygen isotope inhomogeneity on the Earth is because of the range of temperatures, from room temperature or lower all the way up to very high temperatures, plus the existence of this huge mass of the hydrosphere: the waters—ocean water, rainwater—that interact with the rocks and change them. So that was a really important thing, because it meant that ultimately any rock that has an $^{18}O/^{16}O$ composition either higher than +7 or lower than +5 has had to have something funny happen to it in its history; it couldn't just have come directly from the primordial mantle. So if we see a high ^{18}O granite, we know that it's either melted sediments or sometime in its dim, dark past its parents were sedimentary rocks or altered volcanic rocks of some kind or other. And this is going to be investigated more and more. I am sure that future scientists will continue to look at these kinds of things.

So that idea of the interaction between magmas and crustal rocks then pervaded my research for the next twenty years or so, because it was clear that oxygen isotopes, particularly when you combine them with strontium isotopes and some of the other isotopic systems, really can say something. It's a beautifully robust way of finding out how original magmas from the mantle have interacted with other rocks, because the rocks come from the mantle with a certain isotopic value and then they interact with rocks that have much different values. And the mixing process that takes place changes the rocks. So you can get some idea of how these magmas, as they come up through the crust, interact with the crust.

I'll back up a little bit. Most of my work has been going out, collecting rocks, analyzing them, coming back and interpreting them. But because this was such an important process, I started doing my only real theoretical contribution to geology, because I was trying to understand this process in more detail. A magma comes up into rocks that have a certain different isotopic composition. It starts to mix with these rocks, interact with these rocks. The rocks break off and fall into the magma and get dissolved by the magma and so forth. What is that process? Now, before that, most geologists and geochemists had just treated this process as simple mixing. You take the one rock, the liquid magma, and you take the other rock, the country rock being incorporated into the magma, and you just mix them together. There are theoretical mixing curves for these kinds of things. I'm sort of a history buff—including the history of geology—so I started looking at some of the real old classic papers by one of the giants of petrology, Norman Levi Bowen. He is the father of igneous petrology, and he was a genius. He worked at the Carnegie Institution of Washington in the Geophysical Laboratory for many years, and he wrote this great book in 1928 called *The Evolution of the Igneous Rocks*. So much has been done since 1928, but nevertheless—

COHEN: It's still relevant?

TAYLOR: Yes, it's still extremely relevant. And in that book, which most people had just sort of forgotten about, he made it very clear—because he has a whole chapter on this process of assimilation—that this process is not just one of mixing. Because at the same time the material is coming into the magma and mixing with it—usually, for one thing, it comes in colder than the magma, so essentially some of the heat from the magma is used up in heating the rocks falling into it. And also it depends on whether the material is being dissolved and how much heat is involved and so forth. The heat of fusion of the melt becomes an important part of this process. When material is incorporated into magma, it's not just simple mixing, because the process forces the magma to crystallize. I mean, it's so obvious and such common sense that when material is being mixed into the magma, particularly if it has a different temperature than the magma, the magma is forced to crystallize. And the magma just proceeds basically along the same liquid line of descent. It's crystallizing the same minerals that it was crystallizing before these new rocks fell in, with slight modifications. So instead of being just simple mixing, it's mixing plus crystallization. And this process, which I called combined assimilation and fractional crystallization, or AFC—it later became AFC, when I wrote the first paper on this subject in 1980 ["The effects of assimilation of country rocks by magmas on $^{18}O/^{16}O$ and $^{87}Sr/^{86}Sr$ systematics in igneous rocks," *Earth Planet. Sci. Lett.*, 47(2), pp. 243-254 (1980)]—a study of how this process of combined assimilation and fractional crystallization affected isotopic systems, both strontium and ^{18}O. Don DePaolo, who was a student of Wasserburg's, picked up on it and wrote another paper on AFC, in which he brought in other isotope systems like neodymium and so forth. And from then on, for the last twenty-two years or so, everybody talks about AFC.

But the main point was that instead of just being mixing, now you had combinations. And it changed all the curves. So if you'd plot ^{18}O against $^{87}Sr/^{86}Sr$ and things like that, you'd get completely different kinds of curves, because the whole trajectories change drastically when you incorporate both processes. So I applied this to a number of places, as many other scientists also did, and now there are whole schools of scientists that investigate these AFC isotopic and chemical trajectories.

Actually, I have to go back a little bit, because in the sixties I had a postdoctoral fellow from the University of Rome, Bruno Turi. When he arrived in Pasadena he could hardly speak English at all. We had trouble communicating, but he picked up English quite rapidly. He wanted a project to work on, so I gave him this process of what happens to a granite pluton around its edges when it interacts with its surrounding rocks, and we had a beautiful granite pluton right out here, not far from Hemet, right in our backyard. The original magma that came in was about +7 and the country rocks around it were extremely ^{18}O-rich, like +20. A huge contrast in ^{18}O. So we made several traverses across the contact. This was the first time that anybody had ever demonstrated how far the effects from the country rock go into the pluton. We found that all around the outside of the pluton you could trace these high ^{18}O effects inward for at least 100 to 200 or 300 meters. Now, this was long before the AFC process was formulated, but it was probably an AFC process.

Well, he did a couple of these kinds of projects with me here in California. He returned to Italy, and I had always been interested in the rocks in Italy. Everybody knows about them—Vesuvius erupted last in 1944, and everybody knows about Pompeii, and so forth. So I communicated with Bruno Turi about maybe having a joint project on these Italian rocks, which I knew nothing really about. He'd already started to work on them, and it turned out that they were much more remarkable than I had any reason to believe, because Vesuvius is just one of a series of these kinds of volcanoes that stretch all the length of the Italian peninsula, from way down at Mount Etna in Sicily all the way up into northern Italy. They're all very young; they all have been active in the last million years or so. And they are very strange igneous rocks. This has been known for a long time. Most of them don't have feldspar in them; they have very funny minerals. They are so-called peralkaline igneous rocks, very low silica. So what you have here is very strange rocks, and nobody really understands how they formed. But they clearly come from the mantle with these very, very low silica compositions, and they come up into rocks of the continental crust of Italy that are full of silica, full of quartz. So it's an ideal place to study this interaction between rocks of different kinds.

And the other nice thing about it is that in the northern part of Italy there's another province just of granites, which are clearly melted crust. We found very early on that these are very high ^{18}O granites, like the Isle of Elba. This is the famous island where Napoleon was exiled, and it is all made up of granite. I analyzed some of that granite, which Bruno Turi sent me; it's a very high ^{18}O granite, all of those rocks from that area in Tuscany—an area west of Florence and south and east of Elba—where the rocks are all very high in ^{18}O. And they are all very young; they formed in the last 5 million years or so. But they are true granite. They have quartz in them and everything else, so they are just melted crust. In fact it's very clear from the ^{18}O evidence and the strontium-isotope evidence and so forth that these are melted sedimentary rocks. Even major-element chemistry tells you that they are melted sedimentary rocks. And their high ^{18}O values therefore were not a surprise, because if they are melted sedimentary rocks, they would be expected to have these very high values. So here you have this nice geological situation in which the crust is being melted in the last 5 million years, and then cutting right across it is this line of very funny volcanoes stretching from Vesuvius all the way north through the Alban Hills, through Sabatini, through Vulsini, and so forth, going north into the region where these magmas interact with this quartz-rich and ^{18}O-rich granite. So it was an unsurpassed opportunity, because both magma systems, which were completely different from one another, were interacting with one another over the last million years—which is very young, geologically speaking. Strange things happen when these two magma systems overlap, chemically and particularly isotopically. So Bruno Turi and I spent a lot of time working out this process.

COHEN: I'm surprised that there wouldn't be a lot of Italian geologists, given the variety of stuff they have to look at.

TAYLOR: Oh, there are a lot of Italian geologists.

COHEN: But they weren't doing that.

TAYLOR: Well, that's because there weren't any labs. Bruno Turi had the only isotope laboratory in Italy that was looking at igneous rocks.

COHEN: Ah, which he learned from you.

TAYLOR: Yes. He came here to Caltech and learned. There was also no such laboratory in France until some young scientists from France, Claude Allegre and Marc Javoy came over to Caltech and learned our techniques. Then they went back and set up labs of their own. Now they've become world famous, particularly Allegre, who worked with Wasserburg.

What I really haven't touched on is that Caltech, as far as silicate rocks are concerned, was the place to come to. All the people who started the stable-isotope laboratories in Germany came here to work with Sam or me. The people in Finland came here to work with Sam; the people in France came here to work with Sam and me; Italy, Israel, and so on. Every place that wanted to work on rocks came to Caltech to learn the techniques and then went back and set up their laboratories. Bob Clayton's lab at Chicago was our only competitor, and he wasn't very interested in rocks. So we had a tremendous influence. Of course it all goes back to the original Chicago group of Urey—that was the real pioneering thing. But as far as rocks are concerned, there were a lot of places doing stable isotopes, but the only places that were doing rocks in a big way was here and Clayton's lab.

Now, also at this particular time, in 1976, the Geological Society of London invited me to become the William Smith lecturer. William Smith is the father of geology, and they have this annual lecture with a symposium around it. So I laid out all this stuff in this lecture, about how granites form and interactions of water with granites. And that made another big splash. Several universities then tried to hire me. So in the late seventies, part of which I spent at MIT as a visiting professor—

COHEN: Right, you've spoken about that.

TAYLOR: But apparently I was on the list for the National Academy, because Allan Cox, who was in the National Academy and who was the dean of Earth sciences at Stanford, made a big play for me. He really wanted me to come to Stanford. And Stanford is a great university. I certainly was interested in Stanford, but I wasn't about to break my ties with Caltech. So we worked out something. I would come up to Stanford for six months and see how I liked it—a visiting professorship up there for six months. They gave me a place to live right on campus. This was right after my divorce, so I was just starting to go with my present wife, Candi, whom I met at just about this time. She would come up and we'd visit around. We even looked for a house up there in Palo Alto. We were seriously considering Stanford, particularly because Bob Coleman had retired from the U.S. Geological Survey and was now a professor at Stanford. And I knew a lot of other people there, so I was very seriously considering Stanford. At the same time, Texas A&M offered me a big prestigious named professorship. Then the state of Arizona, which is my home state, started getting into the bidding. It was kind of interesting going through this, because I was born in Arizona. At Arizona State, Paul Knauth, a former student of Sam's, was now chairman of the geology department, and the one thing that Arizona State lacked was anybody in the National Academy. Sam had been in the National Academy for four or five years before me, and then I was elected, so Arizona State decided to go after both of us. Sam was interested, too, so we actually visited Arizona State together and interviewed. They wined and dined us.

COHEN: Now, Arizona State is in what city?

TAYLOR: That's in Tempe. And they showed us the big offices and labs we'd have so we could stay together. We had this good team relationship and so on, and it was somewhat appealing. I don't know whether Sam ever really was thinking about it, but I gave it some serious thought. And then what happened was.... See, the University of Arizona has always considered itself *the* university in Arizona. I mean, Arizona State is an upstart. And I had been going back and forth to the University of Arizona previously, because that's where Denis Norton was, who worked with me on the computer modeling study I talked about. So they decided they weren't going to let Arizona State pull the rug out from under them, so the University of Arizona offered me a job, too. So here were the two Arizona universities plus Stanford fighting over me.

And I was considering it, because I was at a stage where if I was ever going to make a move, it had to be then. So I certainly was interested. Barclay Kamb was the chairman here at the time, and I guess he realized this. Maybe Lee Silver told him that I was considering these offers. So when the first endowed professorship in the division came about—this Grand Canyon Professorship, whose money was donated from trips down the Colorado River led by Silver and Shoemaker—somehow or other they decided to give it to me. And I think it was because of the timing. Here I was, ready maybe to jump off to Stanford or someplace else, and an endowed professorship is a nice thing to make you stay put. I think Barclay and Lee decided that the first Robert P. Sharp professorship should be given to me. Well, it was nice. Unquestionably, although I never talked to Wasserburg about it, I think he was very upset about it. Because he certainly was a far more prestigious scientist than I was, I mean *far* more. He had won more medals; he had been in the National Academy for a long time. And to have the first endowed professorship in our division not go to him…. So anyway, the division had a big party for this, not particularly to honor me but just the idea of the first division professorship, which really was to honor Lee Silver and Gene Shoemaker, who had been the ones to put the whole thing together. Wasserburg didn't show up. And then soon after that, Wasserburg got his own endowed professorship, the John D. MacArthur professorship, and they had a big party for *him*.

COHEN: Did everybody go? [Laughter]

TAYLOR: Yes. Well, I went. And I could see Wasserburg's point, because it was just a question of timing. This was a carrot to keep me at Caltech. But as it turned out, I really wasn't going to leave Caltech anyway, because I just love this place. [Laughter] And Sam really wasn't. Diane [Epstein] certainly wasn't interested in going to Arizona, I don't think. At about the same time, I was elected to the American Academy of Arts and Sciences, so all these things happened more or less simultaneously. And I decided to stay at Caltech.

But then the opportunity came at this same time to go to Arabia to work on the opening of the Red Sea, because before I had just been working on samples that Bob Coleman had given me. The U.S. Geological Survey had a big program in Saudi Arabia then, something I didn't really know much about. There were lots of American geologists over there mapping Saudi Arabia, and it was all paid for by Saudi oil money, so the USGS didn't have to put any of their own resources into it other than the human beings that were over there. Everything was paid for by the Saudi Arabian government, and it was supported extremely lavishly. Every field party had a helicopter and a crew chief to maintain the helicopter. They would all have a camp set up with a cook and people to take care of things. It was an unsurpassed opportunity to go over and work in a very interesting geological area and do it in style, so to speak. Because Bob Coleman was involved in this, I got a temporary appointment as a geologist with the U.S. Geological Survey. And along with a student of mine, Bob Gregory, who by now was at Arizona State as an assistant professor, we went over to Arabia. It was a really great experience. First of all, I was able to get a lot more samples from this area I was working on at the opening of the Red Sea. And then also we went into the hinterland in Saudi Arabia and started working on a lot of these volcanic eruptions that had brought up these big nodules, these eclogites and other things I've been talking to you about. And then after we finished two months in Arabia, we went around Oman together to do some more collecting on the Oman Ophiolite.

By this time Bob Gregory had finished up his work with me. He continued to work on Oman, but he had more or less finished up his work with me on Oman. But at MIT I had met this woman postdoc named Debra Stakes, and she was a very adventuresome young woman and very interested in oceanic crust. I gave lectures on Oman, and she got very interested; she wanted to go to Oman to work with me. So I invited her. When I came back to Caltech from MIT, she joined me as a postdoc. My NSF grants had up to that time all been with Earth sciences, but she had an entrée into the oceanographic group, so there was another source of funding. She actually wrote a proposal and got funding to work in Oman. [Tape ends]

Begin Tape 6, Side 2

TAYLOR: She was my first woman postdoc. And this is amazing, because in Arabia the women really have a difficult time: They can't drive cars; they have to go around with their faces covered. I thought Oman would be difficult for her, but she was just amazing. She went out and got huge numbers of samples all by herself, with some help from a British group of geologists. My previous set of samples with Gregory had been in the southern part of Oman, near the capital, Musqat. She did a whole series—a much more elaborate set of studies—in northern Oman. She got all the samples back to Caltech and we started analyzing them. So with Gregory's work and her work, we put together the entire ^{18}O distribution of the Oman Ophiolite.

She found a lot of very interesting things that I won't go into in any great detail.

COHEN: Where is she now?

TAYLOR: Oh, she's now at MBARI, the Monterey Bay Aquarium Research Institute, in Monterey. It's funded by the Packard Foundation. For some time before that, she was a professor at the University of South Carolina, after she left Caltech. But while she was doing this job as a postdoc with me at Caltech, there was a married couple we brought in. The husband was a postdoc who worked with Fred Anson, and his wife, Teresa Bowers, was a geochemist, the student of a professor at Berkeley I knew very well, a good friend of mine. She just wandered into my office one day. She came down here with her husband, basically, and had no job herself. And Teresa wondered if she could catch on, or something like that. So I tried to think of what we could do. And we did have this grant to work on the oceanic crust with Debra Stakes, so I was able to finagle around to get support for her. So she did, basically, another computer-type study. In a way, what she did on the oceanic crust was similar to what Denis Norton and I had done on the Skaergaard intrusion, because her expertise was in theory and in putting together the computer modeling involving the chemistry as well as the isotopes. She put this all together in terms of trying to understand the alteration of the oceanic crust from a theoretical point of view. That worked out pretty well. She put a very nice set of studies together and then went back to MIT to continue this work with an oceanographic—

COHEN: Oh, so she was still a student at this time.

TAYLOR: No, no, she was a postdoc. She went back for another postdoc. Now she's on her own as a consultant in the Boston area. Those were my first two women collaborators, and they were both quite successful. And it was just a spur-of-the-moment thing.

COHEN: They walked in on you.

TAYLOR: Well, one was just because she happened to be at MIT at the same time I was and wanted to work on ophiolites. And the other one just walked into my office because she didn't have a job and her husband was here at Caltech. And they were both postdoctoral fellows. So it wasn't any doing on my part, going out and trying purposely to find women. But from then on most of the people who worked with me were women. They were the first two.

COHEN: Why do you think that was so, because more women were coming in?

TAYLOR: I think we were just getting more women; that's probably the main reason. It's somewhat interesting, because the whole climate has changed. Of course when I first came to Caltech it was all men, and now I'd say at least half of the students are women—at least in geology, anyway. In the last decade, most of my students have been women. Things have totally changed in that way.

COHEN: Why are they more attracted to what you're doing?

TAYLOR: I don't know. It could be just the statistics of small numbers—you know, I haven't had that many students.

Starting in the late eighties, I had a student, Emelia Burt, who came to me and wanted to work on sedimentary rocks, and she did a PhD thesis on sedimentary rocks in the Appalachian Mountains and other places—basically a reconnaissance. She was not a very strong student, but she did a workmanlike job, and we filled in some big holes, because we still needed to know what the $^{18}O/^{16}O$ distribution was like in sandstones and shales and all the different kinds of important sedimentary rock groups in different areas of North America.

And then another student, Diane Clemens, who is the daughter of a paleontologist at Berkeley—so she was brought up in the field, so to speak—came to Caltech from UCLA. She was interested in the Sierra Nevada at first, and so she started working with Jason Saleeby. But she also wanted to do stable-isotope work and she wanted to look at the deep crust. Now, you can look at the deep crust in the southern Sierra Nevada. In fact, that's where she started to work. But she came into my office wanting to do this, and, as you might surmise, I said, "Let's go to the biggest and best." There's a famous area in northern Italy called the Ivrea Zone. It had been more or less worked out over the previous ten years that the deepest part of the continental crust had been shoved up in this southern part of the Alps and exposed the continental crust-mantle boundary. We had already worked on the crust-mantle boundary in the oceans; the Ivrea Zone was a place where you could look at the crust-mantle boundary in a continent. So I said, "Well, instead of fooling around with the southern Sierras, why don't you go to the biggest and the best?" So she thought about that and three days later she came back and said, "OK." [Laughter] I was flexible enough with my NSF grant that I had enough money to fund her field work, so she put the whole thing together and went over to the Ivrea Zone.

The thing I've been struck by with these women students is how adventuresome they are. They just go off and do these things by themselves. I worry about them—that they should take an assistant along or something like that when they go up into the wilds. But with Diane I suppose I shouldn't have been so surprised, particularly since her father was a paleontologist and so forth. In any case, it was no problem at all. She went over, she did all the work by herself, came back and analyzed the samples, and she's now a professor at Cal State Fullerton.

And then the most interesting thing of all—first of all, I should say I had another student, this fellow Peter Larson, who's now the chairman of the geology department at Washington State. He did a complete isotope study on one of these big ash-flow tuff caldera eruptions. What these are are eruptions that in some cases can be as big as 3,000 cubic kilometers of magma, rhyolite magma, that are erupted within a day or two. For comparison, the 1980 Mount St. Helens' eruption was only a quarter of a cubic kilometer. So these are 10,000 times as much as was erupted at Mount St. Helens, and in virtually the same length of time. They are unbelievably catastrophic events, among the most catastrophic events in geologic history. When one goes off, it's a major event—the cloud of ash goes all around the world, and you can still find the layer of ash from some of these things thousands of miles away from where the original eruption was. They are huge things, and when this happens it leaves a big hole in the crust and you get a collapse. That makes a caldera, and these calderas are circular depressions, which are then almost immediately filled by water and make a lake, like—

COHEN: Crater Lake?

TAYLOR: Like Crater Lake, which is a very small one. But some of them are huge. The calderas can be forty or fifty kilometers across, depending on the size of the eruption. The bigger the eruption, the bigger the caldera. And then after the eruption they always go through the same sequence of events. You get the giant eruption, the caldera collapse, and then resurgence, because the magma that's still down there pushes up in the center and makes a dome in the center of the caldera, which sticks up.

COHEN: You mean an island?

TAYLOR: Yes, an island in the middle of a lake. In the ring fracture, which is a zone of weakness, magma comes up along the ring fractures, too. And then, of course, it starts to be eroded away. What Peter Larson did with me was to investigate one of the best exposures of one of these in Colorado—the Lake City Caldera, which had been mapped by Peter Lipman, who is the father of calderas, one of the major gurus of calderas at the U.S. Geological Survey. And he had done a nice job with it, so it was a perfect place to do these things. We found more or less what we expected to see. By now things were getting pretty cut and dried; we knew what to look for. We found a big hydrothermal system around the resurgent dome in the center and big hydrothermal systems around the ring intrusions in the caldera. It was a very nice study, working out the geometry relative to the intrusion; it didn't really break new ground, but it was a nice, elegant study that needed to be done.

Now, that leads up to a story, because along about 1988 or 1989, Carey Gazis, another female graduate student, who was very good, very smart—she had been in the Peace Corps in Kenya, so she was quite adventuresome also, and a very good field mapper, very independent—came to me and was interested in stable isotopes. At about this same time, a group of American geologists—including Peter Lipman, the guy I mentioned from the U.S. Geological Survey—was invited by the Soviet Academy of Sciences to go to the Caucasus Mountains, these very high mountain ranges in southern Russia. They go up over 15,000 feet and they've been uplifted very rapidly. And while they were there, Peter Lipman, who is the guru of calderas, saw this incredible, beautifully exposed caldera. It had formed only about a million years ago, so you'd expect to be able to see rocks near the original surface. But the erosion has been so great in the Caucasus because of the tremendous uplift that now half of the caldera has been eroded away, so you can see way down into the original interior of the system. It's a beautiful young caldera, and you can actually get down and get samples of the thing and see what's going on. Well, Lipman came back to the U.S. and gave a couple of lectures on it and pronounced it to be one of the biggest calderas, and certainly the best-exposed caldera, he had ever seen.

The interesting thing about it though, from my point of view, is—again, it's the biggest and the best, but we've already done a really good one in detail, namely, Lake City. What more are we going to find? But Carey Gazis was interested in this, and the opportunity was there. We had the support for it, the infrastructure. There was a cooperative agreement between the Soviet government, the Soviet Academy of Sciences, and our Academy of Sciences, so she wasn't going to be all by herself. She would have a group of geologists with her. But I fully expected her to find more or less what we had found before. So she goes over there the first summer, collects the samples, comes back, and analyzes them. And she immediately found something utterly unique, which had not been discovered in the previous caldera system.

COHEN: A graduate student's dream.

TAYLOR: Yes, right, except we were totally confused, because we couldn't figure out what was going on. But then, I guess it was in 1989—I don't know the exact date—the Soviet Union collapsed. That was her second summer of collecting. She got out of the country just about the same time as all this happened. But of course it meant that we couldn't go back; that was the end of the Soviet Union collaboration. But she had enough samples to do quite a nice study and it worked out all right.

Now, what she had found was.... Always before in these things, when we see a hydrothermal alteration, the water goes into the rocks and exchanges thoroughly with the feldspars, which are the minerals the most susceptible to exchange with hot hydrothermal fluids. So if a rock is going to get pulled down in ^{18}O by low ^{18}O waters, the feldspar shows it first and the rest of the rock, particularly the pyroxenes and some of the other minerals, is usually left alone. But what she found was that, first of all, the ^{18}O lowering was a caldera-wide event. The upper part of the caldera is the part of the eruption that comes out last—the liquid magma—and then a lot of it falls back into the caldera, falls right back into the depression, but now it's upside down. So this is what she was looking at. She found out that the lower part of the caldera was normal in ^{18}O. And then, as she went up into the upper part—the upper 500 meters or so, a half-kilometer-thick section—it was all low. The whole rock, of which a lot was volcanic glass, was pulled down in ^{18}O, but the feldspars were all normal. [Laughter] I mean, we were just wracking our brains. In other words, some process or other seemed to have lowered the entire mass of liquid magma but had left the feldspars alone. But this was done in a completely strange way, because when these materials erupt and fall back down, they come back in inverted from their original position in the magma chamber. So the upper part of the magma chamber, which is where you'd expect to see near-surface water-magma interaction, if anything like this was happening, was actually normal in ^{18}O, and that just made no sense at all. What she had discovered was a unique phenomenon which didn't show up in the caldera studied earlier by Peter Lipman.

Actually, it turns out that many of these calderas probably show this new phenomenon, if you know what to look for. So the upper part of this system—and by the way, I should say that these eruptions form what are called welded tuffs. A tuff is just a fragmented mass of granitic magma that falls back into the caldera. But when it falls back in, it's still very hot. In fact, they call these things glowing avalanches, because when they form they're incandescent. They're a sediment, basically. When they form this sediment, they're extremely hot, so they weld together—all the fragments of rock and pumice and everything else weld themselves together, and that's why they're called welded tuffs. The welding is significant, because it tells you that the material was extremely hot and that it all happened very, very quickly.

Now, it was the welded tuff portion of this thing that showed this phenomenon. So we wrestled with this for a long time, trying to figure out some weird way of making this. The trouble was that there was no historical example that had really been looked at. The closest thing was the Valley of Ten Thousand Smokes, in Alaska. This was an eruption that happened in 1912, in a wilderness part of the Alaskan peninsula, in southwestern Alaska. The ash fallout from it covered Kodiak Island, southeast of the Alaskan peninsula, so people knew that something impressive had happened back in the interior. But in those days there was no way of immediately getting back in there, so nobody went in and looked at it until four years later, when the National Geographic Society put together an expedition and went in. A fellow named [Robert F.] Griggs, who led the expedition, went up to this thing and looked down on it, and what he saw was a million fumaroles. First of all, he saw a plain; this valley had been completely covered by one of these ash-flow sheets, and there were literally a million fumaroles, steam vents. That's why he called it the Valley of Ten Thousand Smokes—it was really a valley of a million smokes. And they were scared to go down into this smoking valley. The first summer they just looked down on it, and the next summer they actually crept out on it, and they were very, very careful. By the way, I should say that at this time they didn't know what they were looking at; they thought they had found another Yellowstone or something like that. And they had it made into a national park, because they thought it was going to last in this form forever—like Yellowstone, basically. Yellowstone was originally seen back in the late 1800s, and then it was made into a national park, and it's still more or less going today. But the Valley of Ten Thousand Smokes was all over by the mid-nineteen-twenties—this thing only lasted from 1912 to around 1923. Nobody saw it from 1912 to 1916; then when they first saw it in 1916 it was just unbelievable. The entire valley was covered with steam vents, with very hot steam coming out, measured at 650º C locally—really, really hot steam. And then it was virtually all over by 1923.

Well, I knew in the back of my mind about the existence of the Valley of Ten Thousand Smokes. I didn't know anything about it—didn't know about the temperatures, didn't know exactly what had happened and so forth. But it became very clear that whatever was going on there had to be thought of in terms of what Carey Gazis had found in this welded ash-flow tuff in the Caucasus Mountains in the Soviet Union. One idea we had entertained was that somehow or other these fluids had circulated in the upper part of the tuff. We knew that if it was a long-lasting hydrothermal system—like 10,000, 15,000, 20,000 years, or something like that—the feldspars would have had to have been pulled down in ^{18}O, because the feldspars are always pulled down in such circumstances. But they weren't. So whatever process was going on there had to be over in ten or twenty years. If a large amount of water were pumped through the rocks but lasted for only ten or twenty years, nothing would happen to the feldspars. So, she already understood the stringent time constraints, and then she had in the back of her mind that this was the only way she could explain this. And then, all of a sudden, we see in the Valley of Ten Thousand Smokes that it *was* indeed over in fifteen years. I mean, it was an unbelievable coming together!

The thing I need to emphasize here is that this welded tuff in the Caucasus is a huge mass of rock: It's 700 meters thick and it covered the entire caldera. From the surface all the way down to 700 meters, almost a half mile, all of the rock was depleted in ^{18}O but the feldspars were left alone. So that meant all of this change had to have happened in ten or fifteen years. Well, that's exactly what happened in the Valley of Ten Thousand Smokes. Right after this eruption in the Caucasus there was a Valley-of-Ten-Thousand-Smokes occurrence, an incredibly vigorous hydrothermal system that affected the upper part of the system for a very short period of time and then was suddenly over.

Now, the other part of the story is that Elizabeth Holt, another female graduate student, was just starting out at this particular time. She was a Caltech undergraduate and then she left to go to Stanford to work with one of the volcanologists there. But she was kind of unhappy there, and also she had a boyfriend down here at Caltech. So she applied to come back to graduate school here, and she wanted to work on stable isotopes. Carey Gazis was just finishing her thesis on the Russian example [1995]. What Carey had been working on was the material inside the caldera; she knew next to nothing about what was going on outside the caldera. The next natural thing to do was to look at the outflow, at the part of the tuff that goes outside the caldera, and see if the same thing could happen. So Elizabeth said, "Why don't we go and look at the Bishop Tuff?" Now, the Bishop Tuff is this welded tuff that was erupted from the Long Valley Caldera, Lake Crowley, in the eastern Sierra Nevada. It's just north of the town of Bishop. You go up this very steep grade. Well, that steep grade is the surface of the Bishop Tuff, formed 760,000 years ago when one of these eruptions happened and made Long Valley Caldera. And it's still hot near Mammoth Lakes—

COHEN: Sure. It's still smoking there.

TAYLOR: Casa Diablo Hot Springs—so there's still a little bit of heat there. So 760,000 years ago one of those things happened there and made this beautiful outflow called the Bishop Tuff, which Owens River now cut through, in this deep gorge. It's an absolutely ideal situation. If I were looking for the biggest and best place to look for an outflow of tuff, I can't think of anywhere else in the world better than the Bishop Tuff, which is right here. Also, Elizabeth was a rock climber, and in order to get samples on that sheer cliff of Owens River Gorge, you have to rappel down in some places to get to the rocks. So she wanted to do it, to see whether or not she could find the same kinds of things as Carey Gazis saw but in the Bishop Tuff outflow. Well, to make a long story short, she found everything and it all fit perfectly. It's an exact analogy to the Valley of Ten Thousand Smokes. And later on she did collect a few samples from the Valley of Ten Thousand Smokes. She went up there and got those and found the same thing. But not only was she able to show that the upper part of this welded ash-flow sheet of the Bishop Tuff had gone through the same process as that discovered by Carey, but that it had happened in just about ten years, again. So everything again just fell beautifully into place. And she could map these structures, because you can actually see these tubes and fumaroles where the hydrothermal steam vents had been forced through; and those are the exact places where the ^{18}O was lowered the most. I had seen many, many hydrothermal systems before, but they were all formed over a long period of time, tens of thousands or hundreds of thousands of years. But here were these things in which large volumes of rock had been transformed in ^{18}O and they had formed in just ten to twenty years. So in the sunset of my career I discovered, with these two women students, something really, really remarkable—in my experience anyway. It sort of topped it all off.

In the Valley of Ten Thousand Smokes it seemed obvious that the hydrothermal system was meteoric water. However, the person who had mapped and had written the definitive paper on the Bishop Tuff thought all those fumarole structures had been created by magmatic water.

COHEN: So that was wrong.

TAYLOR: It was wrong, yes. It was quite wrong. [Laughter]

COHEN: And this is your work of the last ten years.

TAYLOR: Yes, this is wrapping it up.

Well, let me continue a little bit more about the rifting study, because that also concerns Carey, in a way. I had a friend, Ron Oxburgh, who was chairman of the department at Cambridge University, and he had visited over here. I think he got his PhD at Princeton, so we knew him quite well over here. He wrote me and told me he had a very good student who was working on his PhD in the Pyrenees and had done the straightforward classical mapping and petrology and so forth and wanted to learn stable isotopes. And I said, "OK, send him over." This was Steve [Stephen M.] Wickham, and he was really an enthusiastic, wonderful fellow, charming and very open to talking to people and very interested in lots of things. He had started working on these rocks in the Pyrenees, which are examples of an orogenic, or mountain-building, event that covered all of Europe about 350 million years ago to 280 million years ago, so a lot older than the kinds of things we have been talking about. Most of western Europe underwent this event and one of the places where you can really see it is in the Pyrenees. After he came here and did the stable isotopes in my lab, he was able to show that ocean water was involved with the metamorphism of those rocks at the time the igneous intrusions were brought in—down to much, much deeper in the crust than we had ever thought before. So he reinterpreted the whole orogenic event in the Pyrenees as one of these rifting events, because again, rifting—an extensional event—is the only way you can get this kind of thing done. Some people had suggested that maybe rifting was involved instead of compression. In tectonics it's either rifting or compression or some kind of in-between. And with both kinds of events you can have heating and metamorphism. Well, oxygen isotopes is one of the ways that you can tell the difference. He was able to put this down pretty well.

And then, as an extension of this, I decided.... I had this postdoctoral fellow from Israel named Mordeckai Magaritz. He worked with me on a number of projects, but after he went back to Israel, he was off on his own. He wanted to work on a classic area. Well, the classic book on the formation and melting of granites in the deep crust, which are called migmatites, had been written by a guy who worked in the Black Forest in Germany. This was *the* classic area in migmatites, because the classic book had been written on it. So he went to the migmatite area in the Black Forest and collected a bunch of samples and then he came to my lab to analyze them. One of the things about Magaritz is that he constantly wanted to keep coming back to the United States. [Laughter] His first son was born here. He just loved to come back to the United States; Israelis like to do that. So, instead of finding normal granite values, he found all these very low ^{18}O values in the Black Forest, and I thought that was very interesting. So we decided to drop the other things and concentrate on this: Why was the so-called classic area of migmatites and melting of granites so full of these very low ^{18}O values? At that time I wasn't thinking of rift zones or anything like that. Well, again, to make a long story short, we went back, sampled the Black Forest in this one area, got a lot more samples, and found that all the very low ^{18}O values were concentrated along a narrow zone that cuts right across the southern Black Forest and had been interpreted before as a thrust fault, a compressional feature. We realized that what it had to be was a rift zone, because the only thing that makes for low ^{18}O, particularly along some kind of a linear belt, is rifting. You pull apart the crust and you have magmas coming in. But it was a rift zone that had later been squeezed and compressed. So when all the action happened to produce the ^{18}O effects, it was a rift zone; and then later on it transformed itself into a compression zone and made a thrust fault. It's one of these places where we were looking back through a complicated series of tectonic events. And nobody had realized that it was a rift zone before.

Now, the other part of the story is that that's the same tectonic event that Wickham later on was working on with me in the Pyrenees, and we realized these were the same kinds of things. So in other words, all over western Europe—we now think that this event happened about 330 million years ago—there was a series of rifting events, with a whole bunch of different rift zones. The Vosges Mountains are another one of these things, right across from the Black Forest. You go across the Rhine into the Vosges, in France, and you can project this rift zone that I was talking about right across the Rhine River. Carey Gazis went over into the Vosges Mountains and found the same kinds of things. It all fit together. And we had just barely scratched the surface by the time I retired. I think using oxygen isotopes to understand these kinds of complex tectonic events is going to be a major thing in the future.

My last field student, Greg Holk, who is now a professor at Cal State Long Beach, went up to look at the best examples of detachment faults in North America. Brian Wernicke, who came here to Caltech along with his wife, Joann Stock, is very interested in detachment faults, which are rift faults associated with a tremendous amount of deformation and uplift. They're extensional events, but they are not just normal rift zones. They are followed by a large number of complications. You can study them all over western North America, but in terms of trying to understand them from an oxygen-isotope point of view you want to go as far north as possible, because the farther north you go, the lower in ^{18}O the meteoric waters were, and so therefore you have the biggest signal-to-noise ratio. Holk made a very nice set of studies in which he mapped out the ^{18}O effects along these detachment faults. Every time you see one of those elongate, north-south-trending lakes in British Columbia, you know you're straddling one of these detachment faults. If you analyze rocks along one of these zones, they're all low in ^{18}O. So we were able to show that this whole area has been permeated by this phenomenon.

And then my last student, Edwin Schauble, was totally different. He started out working on granites in Nevada and more or less finished that study, but then he got very interested in theory, and he decided to do the main part of his PhD thesis with me and George Rossman on the theoretical calculation of iron-isotope fractionations in nature. Nobody had really measured much in the way of iron-isotope fractionations in nature—or of chromium-isotope fractionations in nature and chlorine-isotope fractionations. So the main part of his thesis is a theoretical study of those kinds of things. And what he was able to predict is that the isotope effects should be easily measurable, large enough from the point of view of theory, and systematic enough, and probably also important enough, because iron is a very important element that occurs in many different minerals. The biggest problem is that we wondered whether or not the fractionations in nature were going to be big enough. Well, these theoretical studies showed now that they are.

COHEN: Has he finished his work?

TAYLOR: Yes, he just finished. He has a job offer from UCLA, so that's where he's probably going to end up. [Tape ends]

HUGH P. TAYLOR

SESSION 7

July 18, 2002

Begin Tape 7, Side 1

COHEN: We spoke after we were done with the tape last time. You did talk about your work habits, which I thought were a little bit—

TAYLOR: A little strange. [Laughter]

COHEN: Well, out of the ordinary.

TAYLOR: OK. Well, I guess I should start back when I was a student as an undergraduate here at Caltech. I think I mentioned some of these things before. I was in Dabney House, and in those days it was very nice socially—a tremendous social atmosphere. I almost never got up for breakfast, and there was no socializing at breakfast anyway, but at lunch in the student houses we all sat together. We were waited on by waiters. After lunch, the house president and the officers would conduct house business and so forth, and different cliques would sit together and socialize. And the same was true at dinner. At dinner you had to wear a coat and tie. And we had house coats. There was a tremendous amount of house spirit, in inter-house competition in sports, for instance—and I was heavily involved in all of those activities. Plus I played on the varsity football team and the freshman and JV basketball team. I had a terrific time with all the extracurricular activities here at Caltech, and I was able to keep my studies up and my grades up quite well, even though I spent the entire evening after dinner usually socializing. Then I got interested in bridge and I started playing cards. I can't remember exactly when that happened, but there were always at least one or two groups playing bridge in the lounge after dinner. So we would while away the hours in the lounge, and it was very nice. I think people who go through the student houses now can't appreciate what a warm, homey atmosphere it was in those days.

And also we had in those days barn dances. I think they don't have them anymore. But at the barn dances there were two things, one of which was called a crew race, where you drink beer real fast. You had ten-man teams. And then there were what were called flamers, where they would turn out the lights and take a shot glass full of liquor and light it, and then the person would drink it. And with the lights turned off, it was very spectacular actually.

COHEN: These were dances?

TAYLOR: Yes.

COHEN: Where would the girls come from?

TAYLOR: From Occidental or Scripps. There was a tremendous social life then, because every weekend we had exchange dances with the local colleges. Caltech students typically, even in those days when the freeways weren't really there, would drive forty or fifty miles to pick up their date and come back. So a tremendous number of Occidental women and Scripps and Pomona women married Caltech undergraduates in those days. That partly stopped when women were admitted to Caltech, and during that period of time the social life was very bad. Before that, there had been lots of interactions with women students from other colleges, but I think the women who were here at Caltech didn't like that, so they stopped it. I don't know all the details about that, but I do know that the social life in the houses when I was a young faculty member here did not appear to be as good. People, I think, were unhappy. The undergraduates were less happy—certainly less happy than I was, because I was very happy.

Oh, at these barn dances the flamers got very, very strange, because at first it started with just one shot glass held next to your lips, but then later on they would do a waterfall, where they'd hold it up about ten inches above their lips, and then you'd see a streaming flame coming down. And then there would be double waterfalls and backward waterfalls. I mean, it was very, very elaborate. And people would practice all the time—practice, practice, practice. Caltech students did this kind of thing a lot in those days. And in the drinking beer—I didn't do the flamers, but I did drink beer very fast. [Laughter] And you would line up in ten-man teams and drink a beer, and then the next person would be tapped, and so forth. When I arrived in Dabney House, Dabney House was the best crew team among all the four houses at that particular time. And as a freshman, for some reason or other, I got inserted. I just had a natural talent. So I became the best, fastest beer drinker on the team, even though I was only a freshman. In those days, they drank out of bottles. The reason Dabney was so good was because they had discovered a technique of tipping the bottle up real fast and then the beer would foam up and just sort of shoot out of the bottle; the other houses didn't know how to do this. And the ten-man race was all over in like twenty-five seconds. If you just pour it out of the bottle, it comes out quite slowly—gurgle, gurgle, gurgle. Ricketts House was our big competitor. When I was a freshman, Ricketts had just discovered this technique and Dabney got beat. And soon after that, the various houses decided that doing this with bottles was too difficult to judge, so they changed to mugs. And with mugs it turned out I was even more of a star. [Laughter] With mugs it was much faster. I could drink a full twelve-ounce bottle of beer out of a mug in something like one and a half seconds. We'd practice with drinking water in the alley lavatories. We'd just go in there and practice, practice, practice until we were really, really good.

So during my undergraduate days, these were the kinds of things—playing on the interhouse athletic teams, going to the barn dances—a tremendous amount of fun. At the end of the evening, typically, after I had wasted all this time, then I would be faced with having to do my term papers or my problem sets or whatever it was. And I ended up staying up very late working away.

COHEN: That's customary, isn't it?

TAYLOR: Well, it probably is, but particularly for me it was customary. I mean, some other students would go to the library to get away from the noise. The student houses were very noisy in the evenings, too, so it wasn't that easy to study. There were lots of distractions even if you were in your room. But I never went to the library. I just waited until everything was quiet and that's when I did everything. So my most productive times when I was an undergraduate were from about eleven in the evening until three in the morning. And then I would sleep late. Then I'd kick myself later on, because I had to be very judicious about what classes I went to. I only went to the classes that I absolutely had to go to to make my grades. So for instance, when Linus Pauling was lecturing—these were more or less cultural lectures in chemistry—I went to very few of them. They were of course entertaining, but you didn't need them. There was nothing in the lectures you needed on the exams, so I made a choice to sleep in instead.

As I look back on it now, those were stupid decisions. I wish I had recordings of all those Pauling classes. And later on, when Feynman was giving his lectures, then I was a young assistant professor and I didn't have time either. And I wish I had gone. But fortunately in the case of Feynman, now you can get all these things on tape, and I listen to them over and over again. They're just wonderful. I wish I had the Pauling tapes. That would be fantastic, because he really was a great lecturer, and he always made things extremely clear and entertaining. But anyway, that was my modus operandi when I was an undergraduate.

Then when I came back here as a graduate student, I was ensconced all by myself in my laboratory up in the penthouse of what is now North Mudd, and that's basically where I did my PhD thesis. It was quite isolated. I put my big hi-fi set up there, and I would work until three or four in the morning. I was RA, resident associate, of Ricketts House for two years. So essentially I was living the life of an undergraduate again, but in a new house.

COHEN: Did you help students much? I mean, would you go around and see if there were any problems?

TAYLOR: Yes. Periodically things would come up. One thing that Ricketts had was the brake-drum riots. There were a couple times when kids got injured and you had to take them to the emergency room. So it wasn't all fun. But I have to admit I probably wasn't all that much of a role model. For instance, we had poker games in my room lots of nights and so forth. I basically had a good time. I think I was pretty responsible, but I also had a good time and I enjoyed it. And also I was assistant coach on the freshman football team. And then of course I mentioned that every summer I went up to southeast Alaska, which had nothing to do with my thesis. So I was really burning the candle at both ends as far as my thesis was concerned. But I would work until three or four in the morning in the lab. I could get a lot done. At about that time, particularly when I was writing my thesis, I would go to Bob's Big Boy coffee shop and sit down there and spend most of the evening until they closed, drinking coffee and writing.

COHEN: This was up on Colorado?

TAYLOR: Yes. The first one was the Bob's Big Boy that lots of students used to go to, right next to PCC [Pasadena City College]. And then, when I came back as a faculty member, I still continued to do that. Even though I was married then, I actually worked a lot. A lot of young faculty people did this. They would come back to Caltech in the evening after dinner and work in the lab. Lee Silver was the most clear example. He worked tremendous numbers of hours. He would go home for dinner and then come back and work until very late in the evening, and I would also. Often when I got home my wife was asleep. And she was working, too. I would be working in the lab if I had something to do in the lab, but if I had something to write I would go to the coffee shop and sit and drink.

COHEN: Did you have a special table?

TAYLOR: No, no. Well, I used to sit at the counter. The only bad thing about it is that people were smoking next to me, and I didn't smoke. So that bothered me. But later on that all was gone. [Laughter] As I got older and older, things got better. And then later on I started not sitting at the counter. In fact, they eliminated the counter at almost all these places. Now they just had tables, and that was even more convenient, because I could spread out. When it started, I was typically going there late at night and working, but in the sixties, as a young assistant professor and then associate professor, I had a lot more writing to do. And by that time I was more or less fixated about my work habits. I wanted a place where the phones didn't ring, where I wouldn't be disturbed by people coming into my office and talking to me, or hearing something outside and wanting to go out and find out what was going on. I found that if I isolated myself in a restaurant from all that distraction—even though people might think a restaurant is noisy, I could just totally block it out. As a matter of fact, I didn't even know the noise existed. It was a perfect situation. I drank black coffee, which wasn't fattening. I have a weight problem, so it was a nice atmosphere to write in. All of my papers were written basically at Bob's. And when Bob's finally closed, it became Coco's. And now I go to Coco's.

COHEN: You're still doing this?

TAYLOR: Oh, yes, absolutely.

COHEN: At Coco's up on Lake Avenue?

TAYLOR: Yes. I think in the sixties or early seventies—I can't remember when it was—I started doing it in the mornings, because I had technicians and graduate students to do most of the actual nitty-gritty work. So what I would typically do is start off my day in the coffee shop, again, because my wife would go off to work and I didn't want to cook breakfast for myself, so I'd go have a small breakfast, toast and eggs, and then sit there and do my manuscripts. All my own papers, as well as the PhD theses done under me, I would read over and annotate in the coffee shop. And often, if I had some free time in the afternoon, I would go over there and write. [Laughter] So I was away from campus, because basically it turned out that when I was in my office I was either doing something that I had to do, telephoning or something that had to be done in the office, but all of the creative work and the thinking and everything else was done in these coffee shops.

COHEN: You don't give them credit in your papers for that, do you?

TAYLOR: No. I probably should have, because, first of all, the PCC Bob's closed. And then we were living over in western Pasadena, so I started going to the one in Eagle Rock, on York Boulevard. And when we finally moved to eastern Pasadena, I started going to the one on eastern Colorado, across from where Fedco used to be. And often I would take my son Michael there, because his school was right there, and we'd have breakfast together and then he'd walk off to school. But basically my entire professorial existence here at Caltech for the last thirty or thirty-five years has been to spend my mornings at the coffee shop doing my thing and then coming over and doing all the kinds of things like supervising and going around the lab and seeing what was going on, and so forth.

COHEN: And you always did your teaching in the afternoon?

TAYLOR: Well, no. I tried to get my classes to be at ten or eleven o'clock, so that I could go over and do my stuff and then come over to the campus. As a matter of fact, I forgot to mention that: That's where I did all my preparation for my lectures. So that would be the typical schedule. On a day when I had teaching—say, I had a ten o'clock class—I would get to the coffee shop at eight and go over all my notes and then drive to Caltech and give my lecture. For both teaching and all my writing it was ideal. And I absolutely should have given them credit. [Laughter] But it got to be a situation where I really couldn't function very well unless I was in that kind of environment, or comfort zone, if you want to think about it that way. It's really remarkable how much of a creature of habit I turned out to be. And the great thing about Caltech is they don't care about things like that. All they care about is if you are producing. They don't care what you do or where you do it. And I certainly was able to function very well. And I still do it. [Laughter] Now I'm retired, but my day hasn't really changed. I go off and spend the morning at a coffee shop. That's where I was this morning.

COHEN: But you have a cell phone now.

TAYLOR: Well, no, that's the one thing I hate. I don't like talking on the telephone, actually. It's always bothered me when I've been in my office, talking to a student or somebody, and the phone rings. And you're forced to answer it. I hate being interrupted by the telephone. Another one of my idiosyncrasies, if you want to put it that way, is that I was often very hard to reach. I figured that if somebody really wanted to reach me, they would.

COHEN: How many people knew about this coffee shop?

TAYLOR: Oh, I think everybody at Caltech knew. It was kind of a running joke here. As a matter of fact, when Bob's Big Boy on Lake closed, the *Pasadena Star-News* came in with a reporter, and they took my photograph and published my picture in the *Star-News*. [Laughter]

COHEN: When you went to a strange place, like MIT, did you find a coffee shop?

TAYLOR: Well, at Penn State there was a diner I used to go to. I managed. But the East was not conducive—particularly when I was in the East at those particular times, they didn't like to give refills on coffee. [Laughter] I thought they were really behind the times. I think that's not so true anymore, but in those days you really had to fight for a refill, so it wasn't that convenient. I never found anything as nice as Bob's Big Boy, and now Coco's.

Well, that's how I started going to Bob's, because when I was an undergraduate, often after we'd spent the evening playing bridge or something, we'd go up to Bob's Big Boy for a milkshake or something like that. That topped off the evening.

COHEN: But you didn't flunk out. [Laughter]

TAYLOR: Oh, no. I could have done better, clearly, but my grades were quite good.

COHEN: But what you're saying is you could have learned more by going to more classes.

TAYLOR: I could have learned a lot more. But I would spend long, long hours—the labs in geology were very nice in that way, because you could do them on your own time, so I would go in and do them all late at night. Most labs in chemistry you're pretty much constrained to do during the actual lab time, when the lab instructor is there. But geology labs weren't that way. You could do them mostly on your own. There were trays of rocks or trays of minerals or something like that, or you were looking through a microscope and so forth, and I found that it was far more productive to do this very late at night than to do it during class time. So I almost never went to the class hours. And I didn't really need all that much help, so I just would operate in my labs late at night. I pick up things very fast, so I learned to identify essentially every mineral there is. George Rossman is a superstar at this right now and certainly far better than I ever was; but at the time I was by far the best at identifying minerals and rocks.

COHEN: Now, something else. You have not taken to using a computer, a word processor in your work.

TAYLOR: Well, no. I resisted that for a long time. My modus operandi was I would write out my manuscripts in longhand at the coffee shop, bring them in in the afternoon, give them to my secretary, she would type in the afternoon and then I would get a double-spaced or triple-spaced clean copy. The next morning I'd go back and do the changes. So there were lots of iterations. I'm a perfectionist as far as writing is concerned, so it bothered me tremendously if the grammar wasn't exactly correct. And my spelling was always perfect. Everything had to be perfect. [Laughter] So most of my papers went through seven or eight or ten drafts. And nowadays in the case of word processors you can do all that moving around and transferring yourself, but in those days I would scribble and draw circles around things and put arrows. My manuscripts were famous for saying "insert this here" and things like that. It was a mess.

COHEN: Do you use a word processor now?

TAYLOR: Well, now I do, and I find it's not all that easy—and also my productivity in terms of papers has fallen off in the last few years. The way I've been doing it lately is mainly through students. So they play the role of secretary, because they all have word processors. As a matter of fact, I have a hard copy right now that I'm working on. I scribble on it and give it back to them—or send it to them; in this case the student is in Bakersfield—and then they'll go through it and redo it. I do very little of this maneuvering around on manuscripts myself. What I do, though, is e-mails, in the last four or five years, because it's silly to take up a secretary's time. And also I knew that when I became emeritus I wouldn't have a full-time secretary anymore, so I started operating on this correspondence myself, and I found out it wasn't that hard. [Laughter] But it is very hard to teach an old dog a new trick.

As I look back on my Caltech undergraduate experience, I focused totally on what I had to do to get grades, because that's the key thing. In geology there was a little bit more than that, because if I got interested in something like minerals, then I would really go at it and spend a lot of time at it. But basically I did just enough to get A's. [Laughter] I think my final overall GPA was 3.7 or 3.8, something like that. But anyway, it was plenty good enough to get a fellowship to Harvard when I went to graduate school and also to get a fellowship at Caltech when I came back. I had an NSF fellowship, so my grades were fine in that sense, and also in graduate school they were fine.

But I didn't learn as much as I would have wanted to, and particularly when I was taking sophomore physics. I think I mentioned before that I was in the honor section. For freshman physics I had Dean [Earnest] Watson. He was not a very inspiring or great teacher, because he was really a classical-type physicist from the old days, but he was a nice person. And for sophomore physics I had Victor Neher, and he was the guy who did [Robert A.] Millikan's cosmic-ray work, so he did a lot of important stuff on cosmic rays. Millikan had this idea about cosmic rays that apparently later on turned out to be wrong. But nevertheless, Neher was able to make some nice discoveries, because they would be traveling all over the world. They wanted to go close to the North Pole, where the incoming cosmic rays were much more intense. But I didn't know any of that stuff when I was an undergraduate; all I knew was that he was teaching electricity and magnetism. The book was not very good, and Neher, frankly, was not a very good teacher for sophomores. I think I mentioned that I was much happier when I had a TA, a little bit closer to the action.

I remember very clearly that I just didn't like electricity and magnetism, and I don't think I got a very good grade in it. And that's one of the reasons why I jumped to geology, because I thought physics was just not for me. But then, about 1975 or something like that—it was at least twenty years ago.... I had always been interested in relativity. Everyone who is a scientist is interested in relativity. And I thought I knew something about relativity and Einstein and stuff like that—the fact that when things really speed up they become more massive. I thought I knew this. My favorite recreation is going to old used-book stores. So I went to this used-book store, and I always go to the science section. I found this book on relativity written by an MIT professor, and I bought it and I thumbed through it. He had an explanation of relativity in terms of electricity and magnetism that just completely blew me away. As a matter of fact, scientifically it was like a religious experience, almost. I had always been brought up learning that the only time that relativity is important is when the speeds are approaching the velocity of light, and then you have this tremendous increase in mass, and the lengths shorten and everything else like that. What was happening was that you were essentially replacing Newton's laws with Einstein's new formulation of them, but it really didn't have anything to do with electricity and magnetism. It was mechanics. The way the whole thing was put together was a revolution in mechanics. But this book—

COHEN: Do you remember who wrote it?

TAYLOR: I think his name was French; it was a paperback book [A. P. French. *Special Relativity*: MIT Introductory Physics Series (Nelson Thornes, 1971)]. But anyway, I had always been interested—not interested enough to really go into it myself, until I got this book. But I had indeed wondered why Einstein's original paper was called "On the Electrodynamics of Moving Bodies." Electrodynamics, so electricity was clearly.... I mean, Einstein put it in the title of his paper, and I wondered why it was called that. Well, this book made it absolutely clear why. The basic reason that Einstein formulated relativity, I found out at this late date in my scientific life, was because he realized before anyone else that there was a tremendous incompatibility between Maxwell's equations in electricity and magnetism and Newton's laws, and that either Maxwell's equations were wrong or Newton's equations were wrong or they were both wrong. It was impossible for Maxwell's equations to be right and for Newton's equations to be right. They were totally incompatible with each other. And this book explained this in a beautiful way—that magnetism is a relativistic effect. When you have a moving charge, an accelerating charge, it creates a magnetic field. If you accelerate a charge, it creates a magnetic field. It was so amazing to me. It's interesting from the point of view that I had supposedly had the premier scientific education that you can probably get in the United States, or maybe in the world, right? I was a Caltech undergraduate and I did very well, but there was this tremendous hole in my fundamental understanding of physics. Here you take a moving charge, OK? It all has to do with coordinate systems. If you put your coordinate system with the moving charge, then you just need to deal with electrostatics and solve Coulomb's law. So you solve Coulomb's law with a stationary charge, and then you apply the Lorentz transformation to it and go backward to when it's moving, and then all of a sudden the magnetic field appears, because in Coulomb's law there is only an electrostatic field. And so every current through a wire that produces a magnetic field is a relativistic effect. Magnetism is a phenomenon of relativity. And all of this, every working physicist knew, including Neher. But they didn't teach it in the undergraduate curriculum at Caltech. What I thought at the time this happened, in the 1970s, was, "God, this is wonderful! This is beautiful stuff, and so powerful and so interesting!" And yet I never got it when I was an undergraduate; I got it much later. So then I put down everything for a few months and went back and relearned electricity and magnetism, because I was so interested in it. [Laughter] And also I was really kind of irritated—

COHEN: That you hadn't learned it when you were here.

TAYLOR: Well, and I blamed the way it was taught. Classical electromagnetism, in those days, was taught in a terrible way. Later on, I found out that [Edward M.] Purcell, a Nobel Prize winner in physics at Harvard, had written an elementary textbook [*Electricity and Magnetism*] that did present it this way. It's part of the Berkeley Physics Series. Apparently it wasn't all that successful; although it appealed to me, it may not have been that great a pedagogic way to teach electricity and magnetism. But I thought it was wonderful. It gave me a tremendous deep feeling for physics that I hadn't had before, and for electricity and magnetism. So since then I have spent a lot of my time relearning physics, partly out of enjoyment. That's one of the reasons I spend so much time listening to these Feynman tapes, because he was such a wonder.

And I also kick myself for not contacting him personally here at Caltech, but I don't think he was all that approachable. I never said more than a few words to him, just "Hi" and things like that. Nevertheless, his whole personality comes across on these tapes, and boy, what a great way to learn physics!

COHEN: You have to be quite motivated to learn that.

TAYLOR: Yes, but I am motivated. At this stage in my life I still want to learn everything I can. I mean, I want to learn everything about everything. That's why I spend a lot of my time just reading.

COHEN: You said that one of your favorite occupations is going to bookstores.

TAYLOR: Well, I would travel a lot to meetings and go to new cities; the first thing I did when I'd go to Toronto or Vancouver or New York or Washington, DC, would be to look in the Yellow Pages and find old bookstores. I'd always buy seven or eight books every trip. My entire house is lined with bookcases, and the books are stacked in some cases—well, they're all double-stacked, books in front of books, so I don't even know what's behind the books. And in some cases they are triple-stacked. And I also have a whole bunch of bookcases out in the garage. My wife, Candi, is really upset. [Laughter] Well, she has a lot of books herself, cookbooks. And then in my office here at Caltech—

COHEN: Do you know what you have?

TAYLOR: Oh, no, but it's enough to fill a bookstore, without a doubt.

COHEN: You may have some valuable stuff there.

TAYLOR: Oh, probably. To me the important thing was the content. I don't care about whether it's a first edition or any of this kind of stuff. A reprint is just as good. All I care about is the content.

COHEN: Once you've read the book do you go back to it?

TAYLOR: Oh, absolutely, all the time. When I think of something, I like to be able to go and read about it immediately. So that's why I like to have my books. I know more or less what they are, and I've got them arranged by category, so when I want something I'm able to go and find something, or certainly find the category and then refresh my knowledge about things. Particularly for the last ten years, I've gotten interested in a number of topics and just exhaustively pursued them. I have the kind of job where I can do that, particularly now that I'm retired—

COHEN: But you've only been retired for one week. [Laughter]

TAYLOR: But I've been preparing for it. And it's wonderful to be able to get interested in something and just pursue it. My life has been full of serendipity. The greatest serendipity of all is, here I am in this position. I'm still reasonably healthy. And the Internet came along. So actually I don't go to used-book stores much anymore, because if I want information about something, I immediately now go to the Internet. You can find information about anything you could possibly want. So here I am at this stage in my life where I'm somewhat immobile, and all of a sudden this great technological invention has been produced which allows me to do what I always envisioned doing without having to go off to all these libraries or used-book stores.

I still love to go to used-book stores, but first of all, my house is really inundated with books. I have no place to put them anymore. And also I find that the amount of information on the Internet is just staggering. For instance, I got interested in the bombing of Japan during World War II—I mean, *really* interested in it, so I spent an enormous amount of effort on it. I have every book ever written about the subject, plus a lot of stuff off the Internet now. It's just enormous. I'm able to pursue something almost to the point where there's almost no information left.

COHEN: At one point you became the executive officer for geology—before Wasserburg came in or afterward? How did all that happen? You were going to talk about the department next.

TAYLOR: OK. Why don't I just go through some of the historical things in the department and lead up to this, just to put it in chronological order? [Tape ends]

Begin Tape 7, Side 2

TAYLOR: Well, first of all, when I came to Caltech as an undergraduate, the geology department was very small. There were only about ten or twelve professors in it, and there was only one member of the National Academy, Chester Stock, who died when I was a freshman [1950]. I remember I was out at football practice and I heard that Chester Stock died, and he was the chairman of the department then. So when I first entered the geology department, they basically were a classical, field-oriented geology department with very little in the way of modern laboratory facilities of any kind. It was not particularly distinguished internationally. It was very distinguished in terms of seismology, but the seismologists were off campus, and they really were a separate entity. In fact, in all of the papers out of geophysics the address used was the Seismological Laboratory, not the Division of Geological Sciences. So the people I came into contact with were in this classical field-geology department—Bob Sharp, Lloyd Pray, Dick Jahns, Al Engel, Jim Noble, Ian Campbell, and so forth. Bob Sharp was a genius of an administrator—and a genius of a teacher! He is the most important person in the entire history of geology at Caltech; this is so obvious that it goes without saying. And he, together with Al Engel—and Dick Jahns to a certain extent, but mainly it was Bob Sharp and Al Engel—decided they were going to modernize the division. It seemed a natural thing, because Caltech was very strong in chemistry and physics, and we were already very strong in geophysics. We had one of the best seismological groups in the entire world, with Gutenberg and Benioff and [Charles] Richter. So geophysics was in good shape, but the chemistry side of the geology division was just hopeless. In fact it didn’t really exist, because the only people who were at all chemically inclined were Dick Jahns and Al Engel, and neither was very well trained in chemistry.

So Caltech decided to go into geochemistry, and they brought in Harrison Brown from Chicago. And Harrison Brown invited Sam Epstein and Chuck McKinney and Clair Patterson, who had just measured the age of the Earth—a pretty important thing—to come to Caltech. And they also came with Heinz Lowenstam in paleontology. Chester Stock was a vertebrate paleontologist, and when he died that was basically the end of vertebrate paleontology at Caltech. It had been big and important. He was a very distinguished professor. He was in the National Academy. But that was the end of vertebrate paleontology at Caltech, and also invertebrate paleontology, until Lowenstam came and brought his own kind of expertise on coral reefs and so forth.

That was a tremendous transformation of the department, which I of course was able to get in on the ground floor of, and it just blossomed from then on. A couple of years later, Gerry Wasserburg was brought in, also from Chicago, as a young assistant professor. He came two or three years after that first influx of geochemists. And now we had this nucleus of geochemists which later became world famous. Almost everybody got into the National Academy. It became a world famous department of geochemistry, all happening basically because of the leadership of Bob Sharp.

Now, soon after that, Al Engel decided to leave. I don't know exactly why. He said it was because of the smog, and the smog was indeed quite bad in those days. He moved to UC San Diego's geology department, which was just starting up then, but he never cut his ties with Caltech. He took a leave of absence. And Bob Sharp wanted desperately to keep Al, I think. So a lot of deals were made. It was very unusual, because Al Engel was down at La Jolla for at least five or seven years and still had his position here at Caltech; he could have come back at any time. And I think Sharp was thinking he might, but he never did. I think it was probably good for the department that he didn't come back, because he'd basically burned his bridges here. He was an interesting character whom I could talk about a lot more, but I think I'll just leave it there. And his wife, Celeste Engel, was a very important personality around the department. She and Al and Bob Sharp were very close. I think she would have been glad to come back, but Al, either because of the smog or whatever, decided to stay down at UCSD. And finally they terminated his appointment here. I was looking through some old catalogs and I noticed that he was on the list of faculty for a long time after he left the campus.

Then Beno Gutenberg was getting old, and he was the other real star of the department in those days, a world-famous seismologist. I don't know exactly when it happened, but Bob Sharp was looking around, and the other geophysicists were looking around, for the brightest young geophysicist they could find, who would be groomed to take over the directorship of the Seismological Laboratory when Gutenberg finally had to step down, and they picked a student of Maurice Ewing's at Columbia named Frank Press. So Frank was brought here. I'm sure the offer was "You will become the director of the Seismo Lab when Gutenberg steps down." Anyway, I don't know that for a fact, but I think that's what happened. So Frank was brought in, and that was a tremendous thing for Caltech, because the Seismo Lab with Benioff and with Press as director didn't lose any ground at all. In fact, it expanded—a tremendous group of graduate students. I think it was the best group of geophysics graduate students in the world at that time. They came to work with Frank Press when he became the director, and it was a tremendously exciting time in geophysics—but again, all happening at the off-campus laboratory. So geophysics and geochemistry were just world class now. Seismology before had been quite good, but now it was quite good with a bang, because Frank was elected to the National Academy, Benioff was elected to the National Academy, and all of the geochemists were sooner or later elected to the National Academy. So we went from a situation where our only faculty members in the National Academy were Chester Stock and Beno Gutenberg to a situation where almost everybody on the geological sciences faculty was in the National Academy. And so very soon after that, in the sixties, our department became ranked number one. Sharp became chairman in the early fifties and he was chairman for sixteen years [1952-1968], longer than any other person except maybe [John P.] Buwalda [geology division chairman 1926-1947—ed.]. And during that time we basically were transformed from just an ordinary department—certainly a very good classical field-geology department—to a world-class geochemistry, geophysics, and geology department. It was just astonishing. I was a young assistant professor during that period and received tenure at the culmination of it.

Now, about this time there were a lot of personality clashes, because the older group of faculty, the classical geologists, some of them didn't really integrate all that well with all this new stuff that was going on. Now, one of them was Jim Noble. The interesting part is that I was extremely close to both camps. In fact, I was probably the person who most had a foot in both camps, because my main scientific efforts were in the geochemistry group with Sam and all the others and so forth, but in the other camp I spent three summers in southeast Alaska with Jim Noble, and I was always very interested in ore deposits. Noble basically isolated himself from the rest of the division, and he could do that because he had his office on the second floor of North Mudd and he had his laboratory where he taught his course—I think he taught a couple of courses in ore deposits—right next door. Every afternoon at three-thirty or four he would have coffee in there for his group, and his wife would bake cookies and so on. It was one of the best social hours that we had in the division. He operated essentially separately from everybody else in the division, and he could do that, because you can do that as a professor. But he never really got along with the geochemists, particularly Wasserburg. I think Wasserburg didn't much care for the way Jim Noble involved himself with graduate students.

Noble played a very important role in the early history of the division, because first of all in those days government money wasn't all that easy to come by. There wasn't the huge mass of government money that later on became available through NSF and AEC and so forth. And a lot of the support in those days for graduate students came from mining companies and oil companies, ore-deposit exploration groups—or U.S. Steel in my case—and places like that. So the only professors who had access to quite a bit of funding to support graduate students for theses—well, the Seismo Lab always had their own money, so they were in good shape. But in the geology part, the only people who had lots of funding were the people who were consultants for companies, like Jim Noble. Noble had a very good reputation as far as classical field geology was concerned. In Canada at that particular time, almost everybody who went into Earth sciences and geology was interested in ore deposits. I can't begin to emphasize how important ore deposits were in that era as far as students and so forth. At virtually every university in the country there was a professor of ore deposits; it was very important to have a professor of ore deposits in order to have a viable Earth sciences department. And in Canada in particular, because of so much mineral activity, most of the good students went into ore deposits and many of them came to the United States to go to Harvard or Princeton or Caltech. Noble was a great attraction—in part because he had money—and we got a tremendous number of really good students from Canada. We had nobody else here that was attracting Canadian students, but he was attracting them, and also a lot of good American students interested in ore deposits.

So he had this whole group of students that he could support during the summers to do their theses and so forth. In southeast Alaska, three PhD theses were done under his auspices and paid for by U.S. Steel. I think Wasserburg hated that. I may be making too strong a case, but I think he thought, basically, that Noble was buying students. Noble had support for students paid for by companies, and there was always the problem, when you were working for a company, as to how much of it was completely open to be published, and whether or not something would be proprietary, and whether or not you'd have to wait maybe a year before you could publish, and stuff like that. All those were indeed important academic considerations, and I think Wasserburg made a big issue of this kind of thing, and he wanted to make absolutely certain he put every roadblock in the way. Unless there was absolute complete freedom to publish and so forth, he wasn't going to let Noble get away with anything. And Noble was a true gentleman; he just basically withdrew from these clashes. Wasserburg was really—he could say outrageous things. I never heard Noble say a bad word about anybody in my entire life. They're just different personalities. So Noble withdrew from interaction with the department, but there was always this connection with me, because I was in both camps.

Dick Jahns, who originally had been very supportive of the new geochemistry operation and had joined Bob Sharp in promoting it, started having second thoughts about the whole thing. Dick was basically a field-oriented person, and the kinds of things he was interested in in geochemistry weren't the kinds of things we were doing. He really didn't understand it. And again, I got along great with Dick Jahns. We were just total buddies, because I liked the way he operated. We'd always go out in the afternoon and play touch football over in the athletic field and stuff like that. As a matter of fact, at the time I was a graduate student I was even thinking of maybe not working for Sam, because I liked Dick so much. I was thinking of doing a thesis with him. So here I was, caught in between. I loved the classical people, particularly Jahns and Noble, because I got along with them so well, but I could also see clearly that the future was over here with Epstein and Wasserburg and not with Jahns and Noble. That was a backwater. Personalities and friendships aside, the only important thing is science and discovering new things!

Anyway, so that's the way it worked out. Noble then resigned from Caltech. At this particular time I finished my PhD, and I think I mentioned before that Sharp had a real serious problem then, because he didn't have enough faculty to teach the courses. He needed some teachers. So he hired Chuck Helsley, Charles Helsley, and Manny [Manuel N.] Bass, who had been students at Caltech, to teach courses. They should have understood that they were temporary, because I certainly knew that I was temporary when I was first hired in the same way after I got my PhD. I mean, it was very clear that I was going to teach summer field camp and do these other things, but I knew it was a temporary job, and that's why I was going off to these other places, and I finally went to Penn State. But somehow or other maybe it wasn't clear for them. So finally when they were let go, Helsley and Bass, I think there was a lot of bad feeling, because I guess they figured that they weren't treated fairly. They were basically only here for a few years and they were gone, and they weren't given any chance to get tenure. Who knows what exactly happened? But they were hired as assistant professors. I don't think we could do that now; I don't think we could hire an assistant professor and then just throw them out after two years. Things were done a little differently in those days. Sharp had things he had to do and he did them, whatever it took. And Noble had his own advanced classes, which were really over the hill, no question about it. It was starting to become a moribund thing, and the number of applications from Canadian students was being cut down.

I think the last big crisis involved a student named Marvin Lanphere, who started out trying to work under Jim Noble. Lanphere later went to the U.S. Geological Survey up in Menlo Park. He was a good friend of mine and a nice, smart student. And Wasserburg at that time didn't have many students, and I think he really wanted Lanphere. So he made a huge stink about the fact that Lanphere was going to work with Noble only because Noble had money to pay him. I think there was a faculty meeting at which there was some kind of clash. All of a sudden—see, Lanphere was a fellow graduate student of mine at the time, and Noble just wrote him a note saying, "This project is cancelled. I can't have anything to do with you," or something like that. In other words, Noble cut Lanphere loose, because he just didn't want any of this controversy about how graduate students were funded or anything else. So Lanphere did go to work for Wasserburg and got his PhD doing potassium/argon work. I'm sure he's much more successful because of his association with Wasserburg, so it was a very good career move for him. But the point is, Lanphere was devastated when Noble cut him off. Noble just couldn't stand controversy. And Wasserburg—it didn't bother him at all. He could deal with it. So, soon after that, Noble resigned from Caltech. I think he thought his resignation would hurt Caltech; in fact it didn't hurt Caltech at all.

Before that, Noble had just taught his advanced courses. Bob Sharp, as chairman, went to him. He needed teachers, and field geology was a very important class at Caltech, and he had nobody to teach advanced field geology, and Noble was a field geologist. So he went to Jim Noble and asked him to teach field geology, which Noble had never done. I know Noble didn't like having to do it, but he did it. I don't know if you could refuse a chairman in those days. I think he did it reluctantly, but he went out there. We had this mapping area in Tick Canyon, out in the northern San Fernando Valley. Tick Canyon was a very famous field area that Caltech used; it was quite a difficult area. Now it's all built up with houses, but in those days it was a classic field area. Carel Otte mapped there. All the early students, including myself, who went through Caltech geology did their Tick Canyon tour. Noble went out there, and his expertise was in detailed mapping in mines, so he looked at it from a fresh point of view. He mapped the whole area himself, because he wanted to know what it was like, and he completely reinterpreted the whole geology. So here was this area that had been used as a field-mapping exercise for Caltech students for fifteen years, and Noble goes out there and in a week or two teaching the course completely reinterprets the entire geology. [Laughter] And he was so proud of this.

COHEN: Was he right?

TAYLOR: I don't know who was right. It's so complicated that I don't know. [Laughter] The basic question was whether these rock units were intrusive or extrusive—you know, it's pretty fundamental. Are these magmas poured out on the Earth's surface or did they crystallize at depth? Well, he interpreted them as intrusives. I think he might well be right. Now it's all covered by houses, so I guess we'll never know. But I remember how gleeful he was that he had gone out there to this so-called classical area and reinterpreted everything this way. [Laughter] And then, soon after that, he resigned. He had a big house just two houses down from the Athenaeum, on the north side of California Boulevard—one of those big, mansion-type houses. So he never moved away. He stayed here in Pasadena, and he would often come over to the division and use the library and stuff like that, so I continued to see him. And I used to go over and talk to him all the time. But it was clear that he thought that he was hurting Caltech when he resigned. He was angry, I think, in his own way. And he certainly didn't need the Caltech salary.

At about the same time, Ian Campbell also left. Campbell was also a very important part of the division, an extremely important part—not a great scientist—but the glue that socially held the division together, and a great administrator. He was the acting chairman before Bob Sharp, and so forth. He had a tremendous influence on Bob Sharp's early career, and Dick Jahns's also. Ian Campbell resigned to become the state geologist of California. Again, I think these classically trained professors could see the handwriting on the wall—that they were getting older and things were changing. This wasn't going to be the kind of department it had been when they were younger—this became obvious. So in one fell swoop in the early sixties—'60, '61, something like that—

COHEN: All these classical people left.

TAYLOR: Yes. Well, Engel had left earlier. There was a really great assistant professor here at Caltech when I was a student, named Lloyd Pray. He was in charge of summer field camp. He didn't get tenure. Our division has had very few assistant professors who have been denied tenure; you can count them on the fingers of one hand. Of all these changes, I think the biggest mistake that Caltech made in this respect was to let Pray go. I think they treated him somewhat unfairly, because first of all I think he was a very good scientist. And he went to the University of Wisconsin and carved out a good career. He never became world famous, he wasn't elected to the National Academy or anything, but he was a great teacher and he ran a great summer field camp. *The* most important course I took as an undergraduate at Caltech was summer field geology in New Mexico with Lloyd Pray. In fact, a lot of my undergraduate courses were with Lloyd Pray. I think they exploited him here, because he had a tremendous teaching load and he also had been a student here; he got his master's degree and PhD here at Caltech. So basically he just stayed on. He did all this coursework and ran a terrific summer field camp. It made me very sad when Lloyd Pray was denied tenure, because they gave him so much work to do. And he and Jahns were very close. So here, all of a sudden in the early sixties, the entire classical group basically was wiped out. Now, this didn't hurt us in field geology that much, because Lee Silver by that time had come on board and Arden Albee had been hired, and Arden was also a very good field geologist. So we were basically replacing the classical field geology group with a more modern-looking group of field geologists.

Another big crisis came when I was at Penn State, in 1961 or so. Lee Silver had been an assistant professor. He was hired in 1955 as an assistant professor. Now, Silver has this terrible problem that I alluded to before—that he doesn't publish much. He's a great scientist and knows more about geology than anybody I've ever come into contact with. And certainly in terms of getting information by osmosis, just by sitting and talking, he's the most important person in my life as a scientist. Very little of it's written down, so the only way you can get it is by talking to him. [Laughter] Although I'm exaggerating a little, you could say that half the knowledge of the western United States was in Lee Silver's head, and the other half was written down by other people. But Silver had access to all of it, because he had everything that was in his own head plus he had everything that was written down, whereas everybody else just had the half that was written down. [Laughter] So he always was ahead of everyone else. But if you do so much important stuff and you don't write it down, then the rest of the world doesn't know about it.

He wouldn't get upset about this. I mean, if you talk about his problem in publishing, it bothered him, but it was much more important for him to go out into the field and get more new data. He was the greatest accumulator of data I've ever seen, and the ratio of his data to his publication is probably the greatest in the whole history of geology. But in that case it's kind of sad; now a lot of it is probably not very useful anymore. A lot of it has been superseded by new techniques and so forth. The other thing that happens in geology is that you stake out an area for yourself: "Silver's working here," OK? Well, that makes it off limits to other people. So for a long period of time people didn't feel as though they could go in, because Lee was working there.

All this has over the years sorted itself out, but that was part and parcel of the whole business. When I was at Penn State, Lee was coming up for tenure. Now, I don't know all the details about it, but Lee visited me at Penn State at the time, and I know that he was going through this agony. And that's about the time that Caltech offered me the job, to come back. And I think one of the reasons was because there was an ambiguity about whether Lee Silver was going to get tenure, because he just had not published enough. Again, I don't know the full story on this; I've just heard several rumors. But at that time Barclay Kamb was a professor at Caltech and had a lot of clout. I've heard that they were getting set to not promote Lee but he had published one quite important paper. So he did have this one paper that came out, and Barclay Kamb—I think because he realized how important Lee was to Caltech—put his foot down, with maybe a couple of other people, and said, "We just can't afford to lose this guy." So Lee got tenure, maybe just by a hair. Thank God he did! And later on—it wasn't too long—he did publish a couple more very important papers. He had published lots of abstracts, so everybody knew how good he was; certainly people here at Caltech must have known how good he was.

I can't remember when, but he got elected to the National Academy three or four years before I did, or something like that. So the scientific community finally recognized him. He had lots of good connections with the U.S. Geological Survey and a lot of the National Academy members in geology at that time were in the USGS. This is a kind of situation where people vote their friends in. So people in the USGS who knew Lee and knew how good he was—like the chief geologist of the USGS, Charlie Anderson, and Jim Gilluly, both of whom were in the National Academy—they knew how good Lee was, and so that plus enough important publications got him in. And that was great for Caltech, because I think he's just been marvelous for our division in every way. I mean, you can't have everything. It would have been great if all of his stuff had been published, but you take the good with the bad. And as far as I'm concerned, he and Bob Sharp and Sam Epstein were the most important people in the division over the years.

Now, I was talking about Frank Press. Frank had visions of greater things also. That was very clear. He was very ambitious. And I can't remember exactly when Frank left to go to MIT [Press left in 1965—ed.], but anyway, before that happened I think there was some friction between him and Bob Sharp. I think Bob didn't like the fact that the Seismo Lab was so isolated from the rest of the campus. By that time, the Seismo Lab had two laboratories over there across the arroyo. We had this big division-wide party there. The whole thing was run by Bob Sharp, and it was like Frank Press wasn't even there. I thought this was very strange, because the director of the Seismo Lab was being left out of the whole thing. It was almost like some kind of a power play. It seemed to me at the time that Bob wanted to reassert that the Seismo Lab was really part of the division, whereas the Seismo Lab had always resisted and wanted to be its own separate entity, because they figured they were somewhat superior to the rest of the division.

Then the other thing that happened at about this time was that Frank Press wanted us to go into oceanography, and we had a major evening faculty meeting in which we discussed this idea. This was just before the plate tectonics revolution, so it was very perspicacious of Frank to do this, because it's possible that if we had done that, we might have gotten in on the ground floor of plate tectonics, which we didn't. Caltech basically missed the bus on seafloor spreading and plate tectonics to a large extent. Who knows what would have happened anyway? But the point is that we basically decided not to go into oceanography, because Caltech wasn't on the ocean, and so forth—we're not like Scripps, we're not like Woods Hole. So we just decided we had better not go down that road, even though three-quarters of the globe is ocean. And also at this time Harrison Brown had just brought Bruce Murray into the division as a research associate and was expanding into planetary sciences. I don't know all the ins and outs of exactly how this happened, but basically it was decided to move into planetary sciences rather than into oceanography. I think Frank was upset about all this, and that's perhaps one of the reasons he left. Also he had this offer to be chairman of the department at MIT, and he would be able to constitute the department in the way he wanted it. So Frank Press left, and that was a major blow to geophysics at Caltech. After he left, we hired two of his most recent students, Don Anderson and Stewart Smith. Frank was a very prestigious person, no question about it. He was really a star, so that was a big loss to Caltech. But it was clearly the right decision, because I don't think we could have done both oceanography and planetary sciences, and by concentrating on planetary sciences and building up planetary sciences in the way we did—look, Bruce Murray grew from being a research associate to a professor and then director of JPL. And then we started hiring the other people in planetary sciences, including Peter Goldreich and Andy Ingersoll and so forth. And it turned out to be one of the best groups, if not *the* best group, in planetary sciences in the United States.

So that was a very important decision. Harrison Brown was very important in that, because he was the guy who really sparked the whole thing. By this time, Harrison Brown was not really active in geochemistry per se; he had been moving into other fields. But he was the guy who got us started in planetary sciences. And once it had been well started, he decided to move into the humanities division. He was a professor in humanities for a long time. Harrison would do his thing and then move on. But he played a critical, major role in the start of geochemistry and also in planetary sciences, the two major revolutions that happened in our division. So he deserves a lot of the credit for its present stature, along with Bob Sharp, although there's no question that Sharp was the key person.

I can say just a few words about the other chairmen, I guess. After Sharp stepped down as chairman, we were looking around for a new chairman. I won't go into all the details, but we decided to look outside. We finally decided on Gene Shoemaker, and he took the job. Gene was very good as a colleague and everything else, but I guess he wasn't that responsible as chairman. Some of the faculty became upset because we came close to not getting the South Mudd building. Getting the money and getting the Caltech administration to decide to let us have that—that was all happening at the time Gene Shoemaker was chairman. I remember that Sharp was upset, even though he was no longer chairman. [Laughter] He could see the handwriting on the wall—that unless Gene's feet were kept to the fire it wasn't going to happen. I guess that Gene would let a lot slip through the cracks, because he was interested in so many different things, whereas Sharp had really paid attention to the job of being chairman. So Gene didn't stay in the job too long. It wasn't really his cup of tea. He kept his ties with Caltech; he became a part-time professor. He moved back to Flagstaff, to the Astrogeology Branch of the USGS, which was his true love. This was at the start of the Moon program. Well, that was his true love; the chairmanship at Caltech—he probably just didn't put the effort into it. Although as I look back on it now, nothing serious happened and we did get the building. But I think that was basically because Sharp, even though he was no longer chairman, still kept working at things. Thank God for Bob Sharp, because that was very important, to bring the Seismo Lab to campus and also to have a place for the planetary sciences and so forth.

After Gene stepped down, we decided we had better not go outside anymore, so we decided to put Barclay Kamb in as chairman. And Barclay Kamb was chairman during the time when all the nice things were happening to me—the endowed professorship and everything else like that—so I owe a lot to Barclay. He was chairman for eleven years [1972-1983] and then stepped down. And then we decided that we'd look outside again. So we looked at a number of people, and finally Peter Wyllie's name came up. I remember we had a lot of discussions about it. Peter Wyllie had been a chairman at Chicago; he was a very well-respected person in the field. It didn't look like there was anybody here at Caltech who could really take care of this group of wildcats on our faculty that were so difficult to deal with. We figured we'd give Peter a shot. We had gone through several other names. And Wasserburg says, "I think Wyllie is going to come." And I said, "No, I don't think he'll take it." Wasserburg said, "I'll bet you a bottle of really, really good wine that Wyllie will take the job." I said, "OK, you're on." [Laughter] So I lost the bet, because Wyllie did accept. But again, coming in from the outside—at least Gene had been at Caltech for some time. So here we were, for thirty years we'd had nothing but chairmen who'd had previous experience at Caltech. And the one who had been brought in from the outside, Gene Shoemaker, had not really been that successful. Barclay's tenure as chairman had been by far the most successful since Bob Sharp. Bob Sharp and Barclay Kamb were really, really important for our division.

I should tell a story about when Barclay was chairman. We had this search committee for a petrologist. There were three names on the list. One was a young petrologist from Johns Hopkins named Bruce Marsh who had been a student of Ian Carmichael's at Berkeley and whom I had gotten to know very well over the years—a brilliant young guy who had done a lot of really nice things in petrology. Nobody here knew him very well, although we did have him out for a few lectures. Another was Tim Grove, at MIT, who had been here as a visiting postdoc with Arden Albee. Arden was a great proponent of Tim Grove, because Tim was great with the electron probe and experimental measurements of that type. And there was Ed Stolper, who had applied to Caltech as a graduate student about four or five years earlier and we had wanted him tremendously as a graduate student at the time. It was the best application I think I've ever seen, the best recommendations I've ever seen, from people we really knew, like Jim Thompson of Harvard. But Ed decided not to come to Caltech for his PhD. He went off to the University of Edinburgh for a year, and then he went back to Harvard and got his PhD there—and then he surfaces again as a prospective faculty member. So here was this great student, who was like a second Barclay Kamb or something like that, so brilliant just in terms of his scholarship and his original graduate application that we had desperately wanted him as a PhD student, and then he surfaces as a possible assistant professor. He was an experimental petrologist, which was also very good, because at the time we didn't have anybody in experimental petrology. So the three candidates were all in petrology but in three different fields. All of them were very good.

Wasserburg was chairman of the search committee, and I was on the search committee, and we spent a lot of time agonizing over this. One thing Wasserburg does whenever he gets a task is to do an organized, bang-up job. He does everything in a really systematic way, so it was extremely organized. We had big loose-leaf folders full of all the people. We looked at the entire grouping of possible candidates across the country, or even the world. But also with Wasserburg there's a problem that nobody's ever really good enough. [Laughter] It's very, very hard to get somebody that's good enough. They all came out to give talks and so forth and so on. To me it was absolutely clear that Stolper was the smartest and the best, but the two other guys had proponents, too, and both of them had been in the field longer. Here were three finalists out of a long list, all of them were good, and it was kind of a free-for-all.

In those days we had evening meetings to discuss appointments. In fact, we had lots of evening meetings in Barclay's house. It was a very homey enjoyable place. We all sat around as a group. Our division always worked in that particular way. Important decisions like this were always done by the entire group. All the professors came, including the assistant professors, untenured professors, everybody, and we all discussed the thing together. That's the way Barclay ran things. As far as I'm concerned it was a terrific way to do it, because it was conducive to making a lot of good decisions.

So at Barclay's house we went through this whole thing, and there was no resolution after many hours of discussion. But Ed Stolper was in a situation where in the next two or three days we had to let him know what was going to go on or he was going to take a job somewhere else. I can't remember where, Columbia or someplace like that. And I was unbelievably upset. Anybody with any sense knew how brilliant Stolper was. But somehow or other, particularly Wasserburg.... I guess to Wasserburg, nobody was ever as brilliant as Wasserburg. To me it was so obvious that it would be stupid not to get Ed Stolper. The other two guys were fine, but, my God—I just couldn't understand it, OK? Well, Barclay, at the end of this meeting when it was clear that the faculty was not going to come to any kind of clear resolution, decided to make the decision himself. See, the Caltech chairmen have a lot of power. Bob Sharp, when he was chairman, hired these young professors just on his own, and I think [Linus] Pauling, who had been chairman of the division of chemistry, did similar things. And I guess Barclay knew this, because Pauling was his father-in-law. [Laughter] And so Barclay at the end of the long evening, close to midnight, stands up in front of his fireplace and says, "OK. Well, here's what I'm going to do. We'll make an offer to Stolper." And then there was, I think, not quite a gasp, but this was something that more or less had never been done before—a chairman all of a sudden just deciding this thing. [Laughter] But of course I was happy, because I got the person I wanted. And then the meeting broke up.

The next day there was a lot of static, because of the fact that it had seemed so heavy-handed and wasn't done democratically and so forth. I know Wasserburg was really upset about it. Barclay then went around to various people and touched base to make sure that he did have support for this in general. And certainly I gave him support. Because of the time constraints, it was a real difficult decision that Barclay just felt he had to make. And as chairman he had the power to do it, and he did it. And thank God he did, because getting Stolper here was a key decision. The others all carved out very good careers. Tim Grove is a professor at MIT. Bruce Marsh was chairman of the department at Johns Hopkins and has done a great job. But neither one is in the National Academy, whereas Ed Stolper was elected several years ago. So there was no question, as far as I'm concerned, that we made the right choice, and not just from the point of view of the science but in terms of sheer intellectual and scholarship abilities. I'm in awe of Stolper, not just because he's smart but because he has a great feel for what's going on in science, plus he later became our chairman. Except for Bob Sharp, I think Ed Stolper is the key person of all the division chairmen in my tenure here at Caltech, because all of those young assistant professors from the fifties and sixties who were so successful retired during his chairmanship, and they all had to be replaced. So the division has been completely remade, largely during Ed Stolper's tenure as chairman, and mainly with young people, which is the right way to do it. And it looks like we're off to a really, really good start. Of course only time will tell, but the prognosis looks good; and we've gone off into a lot of interesting new areas, which is always important. You can't stand still; you've got to keep moving. So Ed Stolper was offered the job here; he came, and he's been extremely successful. [Tape ends]

HUGH P. TAYLOR

SESSION 8

July 23, 2002

Begin Tape 8, Side 1

TAYLOR: Bob Sharp hired several young assistant professors in the mid-fifties, including Clarence Allen, a professor of structural geology who had done his PhD thesis here at Caltech [1954]; he worked on the San Andreas fault. Bob also hired Lee Silver, who did his PhD thesis [1955] in southern Arizona and was interested in igneous petrology. And he hired Barclay Kamb, who did his PhD thesis [1956] in the chemistry department with his father-in-law, Linus Pauling. And he hired Gerry Wasserburg, who came in as an assistant professor of geochemistry and did a lot of important work on isotope geochemistry. Three of those were Caltech PhDs and one was a Chicago PhD; and they formed the nucleus, all in the mid-fifties, that Bob Sharp hired while he was chairman. Along with Sam Epstein and Clair Patterson, who had earlier come from Chicago, and Heinz Lowenstam, also from Chicago. Now, the interesting thing about all of those appointments was that every single one of them became a member of the National Academy of Sciences.

COHEN: A distinguished group.

TAYLOR: That's batting 100 percent; it's pretty hard to do better than that. And the other interesting thing was that three of them were Caltech PhDs, which is somewhat frowned upon—to hire one of your own PhDs. But all of them had first gone elsewhere. Clarence had, I think, been a year at the University of Minnesota, and Lee had done a lot of work with the U.S. Geological Survey and earlier had been away at the University of New Mexico for a master's degree. Barclay Kamb had been in the chemistry department, so he wasn't really homegrown in the sense of being strictly in the department. And then, just a few years later, while Bob was still chairman, I was hired, a Caltech PhD, and Don Anderson was also hired, also a Caltech PhD. And we both later became members of the National Academy. All those young appointments, hired as assistant professors within a space of about six years, formed the nucleus of this group, along with the older people, Epstein and Patterson and Lowenstam, all of whom were very distinguished. So clearly Bob Sharp—

COHEN: Had a good eye.

TAYLOR: Yes. And the faculty around him made tremendously insightful appointments. It would be hard to conceive of doing any better. And then, when Bob stepped down after sixteen years, Clarence Allen became the interim chairman, and we were looking around for somebody to replace Bob Sharp. Of course, how do you replace Bob Sharp? Clarence, I think, would have been a very good chairman, but I don't think he really wanted to do it. So anyway, we decided to look outside. And we went through a lot of names of outside people. And we finally came up with Gene Shoemaker, who had a lot of ties to Caltech. And since we were at that time moving into planetary sciences, it seemed like a natural thing to do also, and everybody liked Gene Shoemaker. So he became chairman, and he was certainly an outstanding faculty member, but I don't think he paid enough attention to details day to day. So when the next chairman search came around, we pretty much decided that we wouldn't go outside, because we decided we needed somebody who knew how the division operated.

Barclay Kamb seemed like a natural choice. He was the right age and everybody respected Barclay. He was considered the brightest young person around. And I can tell a little story about that: Barclay was two years ahead of me as an undergraduate. Although I didn't know him when I first came on campus, everybody pointed him out and said he was the brightest, the most brilliant undergraduate at Caltech. He took something like eighty units a term. He took geology courses, astronomy courses, everything he could take, even though he was a physics major. In those days they didn't give A+s; he just got a straight-A average. He was the intellectual god among the undergraduates at the time. Later on when I was a junior, he and the president of my house, Dabney House, Ron [Ronald L.] Shreve, were both first-year graduate students in physics. They had bachelor's degrees in physics and stayed on for graduate school in physics. But they both always had an interest in geology, and they were great friends. After one year in the physics department—I think in 1954 or something like that—both of them left the physics department and came into the geology department. And everybody in geology was patting themselves on the back and everything, because we got these two really bright physics students. Because we were trying to build up physics and chemistry, and to get two outstanding physics graduate students, including *the* outstanding person—namely, Barclay Kamb—both to come to geology at the same time was a real coup. And then Barclay went along to summer field camp with me in the Sacramento Mountains in New Mexico with Lloyd Pray, so I got to know him pretty well there as a fellow student. Later on, after he finished his PhD, he was hired as one of these young assistant professors. So by the early seventies we decided to ask him if he would be chairman, and he took on the job. And he understood how we operated, so he basically continued in the Bob Sharp mode. I think he was a truly excellent chairman.

Here's one interesting little vignette. While Clarence Allen had been acting chairman, we had this really great administrative aide named James McGaha. He was just terrific. He was an expediter. All the time that Bob Sharp was chairman the administrative day-to-day routine things were taken care of by Jim McGaha, because he was so good at it. If you needed something, you'd go to Jim. He was an action man, and he would do it. So when Clarence became acting chairman, he was trying to figure exactly what he had to do, so he started going through the accounts and the books and he started finding funny things going on. And it turned out that this guy who was just great, who was just a terrific guy that we all loved and thought was wonderful because he made life easier for all of us, had been embezzling. And he was doing it in very creative ways. As a matter of fact, one of them involved myself, as I realized later on. I had a Sloan Foundation grant, but I had just put it aside. I didn't need the money at the time, so I just put it aside until one day we were building a new mass spectrometer and I said, "Well, I have this money, Sam, so why don't I get the Sloan money and we'll use that to help build the mass spectrometer." So I went and found there was nothing in the account. And I went to Jim McGaha and I said, "Gee, what's wrong? I haven't used this money." He got kind of red in the face and said, "It's OK. I'll figure it out." And within a day or so the money was back. To me, in retrospect, it was clear that he had just been robbing Peter to pay Paul. He had been juggling all of these accounts, moving money around—

COHEN: We call that cooking the books now.

TAYLOR: Yes, right. Well, it was a famous case at Caltech. The business services people changed their entire way of doing things, because one of his favorite tricks was to have a reimbursement check for a visiting speaker or something like that sent to a Caltech address. And apparently Jim would just get the check and endorse it and cash it. And a lot of the money he gave to his church. So it was a very strange thing.

COHEN: It was small sums, evidently.

TAYLOR: Yes, right. But it was a great scandal.

COHEN: Did they prosecute him?

TAYLOR: Yes, they did, and he went to prison; I don't know for how long. But it's just one of those things. It wasn't discovered by Bob Sharp, because everything was going smoothly. It was only discovered when Clarence Allen was trying to educate himself about being chairman. Fortunately Gene Shoemaker and Barclay Kamb didn't have to worry about these kinds of things.

When Barclay was chairman, one of the things he tried to do that was not a success was trying to get somebody here in resource geology. We looked at people on the petroleum side of the field, on the ore-deposit side of the field, and it just didn't work out.

COHEN: Do we have anybody in that area now?

TAYLOR: No. It just collapsed. It probably turned out that it's good it didn't go forward, because we were trying to hire a senior established person, and somehow or other that never seems to have been good for our division. Our best appointments have been young assistant professors, like the ones I just mentioned, who grow into the job here.

Now, after Barclay finished his eleven-year term as chairman, again we were in the position of looking for our next chairman. And we decided to go outside again, because there was no obvious internal person that could rule the roost so well. So we looked at a lot of outside people, and Peter Wyllie was hired. Peter took the job. It was clear right away—first of all, it's very hard to come into a place like Caltech that you don't know and be off and running without knowing the whole history and everything else. And with the hornets' nests that we have in our division—you know, they're so critical that I think Peter had a tough time almost from the word go. There was a very short honeymoon; he had been chairman for three or three and a half years and then there was a movement to replace him. They just didn't think the division was moving fast enough. And I have to tell you that I thought this was unfair, first of all because it's only a five-year term, so he only had a year to go. I suppose he wasn't dynamic enough in the view of some of the important faculty members like Wasserburg, who always had a big say. I think even Stolper, who was on the same floor with Wyllie, had some problems with him at that time; Stolper was kind of a young Turk, who wanted to get things moving, and that was the biggest argument against Peter. The leadership, or the vision, so-called, just didn't seem to be there. The other chairmen who had really been successful, Bob Sharp and Barclay Kamb, clearly had vision. I think partly it was because Peter was just trying to get his feet together.

But in any case, there was a lot of negative feeling. Also at that time the provost was Robbie [Rochus E.] Vogt. Now, Robbie Vogt—I don't know if you've heard a lot of stories about Robbie Vogt, but as long as I'm talking about Caltech history—he was a really strange person to have as provost at that time. I'll give you some examples. Robbie had these dinners, to which he would invite a whole bunch of faculty members from the different divisions. Supposedly it was to have a meeting of the minds so that he could get information from a lot of different people, and that would allow him to do his job as provost with more information. But in fact I went to two or three of these things, and they were all just monologues by Robbie Vogt.

COHEN: Now, these weren't social events, in that you brought wives or anything?

TAYLOR: Oh, no, no, it was all faculty. It was a dinner, but basically it was—

COHEN: A meeting.

TAYLOR: Yes. I mean, it was partly a social occasion; first, you get all the people together. But then, instead of having some back-and-forth, in which you get different ideas, basically it was all a monologue by Robbie Vogt. And one of the things that apparently came out of this, maybe because of talking with other people in our division who were unhappy, Robbie Vogt more or less implied that in order to move forward we had to get rid of Peter Wyllie—not in so many words, but he made it clear that that was what he wanted. The provost is very important in terms of how the divisions operate. So whatever was going on, it seems clear that Peter was not very effective with Robbie Vogt. But of course it turned out, I think, that *nobody* was very effective with Robbie Vogt, because Murph [Marvin L.] Goldberger [Caltech president 1978-1987] couldn't get along with him either, and later on Robbie was replaced by Barclay Kamb as provost. So I think a lot of the reason why Peter Wyllie was eased out [1987] is because of some kind of a collaborative effort between Robbie Vogt and some of the people who were unhappy in our division. I don't know. But in any case, it led to a difficult time for our division, because when that happened, we had to have a new chairman. And absolutely we weren't going to go to the outside again, because—

COHEN: You had learned your lesson.

TAYLOR: Yes, because the first outside person we'd gone to, Gene Shoemaker, had been a fine faculty member but really wasn't all that attuned to being a chairman and certainly wasn't as successful as the internal people. And then our next step, Peter Wyllie, who also was just a perfect gentleman—I mean, Peter is an absolute treasure as far as I'm concerned, just as a human being, but it was difficult for him to handle. I don't know what I would have done if I had been in that situation with Robbie Vogt. I think that was very difficult for the institute at that particular time, because Vogt was so difficult. He's dynamic, but he basically had his own ideas and nobody else's ideas seemed to matter.

Well, right about this time, George Brimhall, who was a professor of geology at Berkeley, and actually turned out to be a distant relative of mine, was here as a visiting professor. We became very close to the Brimhalls when they were down here for a year. I had known him before, because, as I told you, I had worked in ore deposits and he was a professor of ore deposits. And his wife and my wife, Candi, got along real well, so we used to often have dinner together. And I remember we were talking about where we were from, where our family grew up, and he said, "Well, my father grew up in a small town in Arizona. You've never heard of it." I said, "Well, tell me. Maybe I have." And it turned out that his father is a brother of my uncle by marriage. My family grew up in Snowflake, Arizona, and his family grew up in Taylor, Arizona, and the two towns are only about five miles apart. [Laughter] Not only that, but we are both from Mormon backgrounds but neither of us are Mormons now. Mormons tend to write their family histories, so he brought out his family history. And I said, "Well, I have my own family history," because my grandmother wrote it down. So I brought out my family history and we compared them and found the Brimhalls mentioned by my grandmother! It was all very strange. [Laughter]

Anyway, Berkeley was in some trouble at this particular time and they didn't have anything going as far as modern geochemistry was concerned. So Brimhall's idea, since he was going back to be chairman of the department, was to hire Gerry Wasserburg. And in one fell swoop Berkeley would be put on the map in terms of isotope geochemistry. Gerry was young enough and energetic enough back in the late 1980s to do something like that. So they put together a package for Wasserburg that was just unbelievable: He was going to be a professor of physics, a professor of chemistry, and a professor of geology, and he was going to have all kinds of money. It was really an outstanding package, and there were all kinds of Nobel Prize winners up there to hobnob with.

So anyway, at this time we needed a chairman and it certainly had to be somebody internal. And I must say that I was one of the proponents, because I knew Wasserburg very well. I came into the division the same time he did, and we had grown old together and so forth. And I thought, "Well, Wasserburg has won every medal there is, he's done everything there is, he certainly is energetic and hardworking, and he commands the respect of the administration. He could be a great voice for us in the administration. And all of his wonderful hardworking attributes and energy can be focused for the good of the division, because now he's got no other dreams of grandeur. There's nothing else he could accomplish." He had won the Crafoord medal and every other medal you could get. So it seemed like a reasonably good possibility. Enough other people believed that it would happen—and I think they also believed that if this worked out, it would be maybe the only way to keep him here; and it was important to Caltech to keep Wasserburg here, because he was our scientific superstar. So anyway, we decided to offer the division chairmanship to Wasserburg. And I think he wrestled with it and wrestled with it, but nevertheless he took the job. We had great expectations that not only would we be better off—and by the way, by this time Barclay was provost, so now we really had clout.

COHEN: People in high places. [Laughter]

TAYLOR: Right. People in high places. So the division was poised to go on to great things. But the first thing Wasserburg did was change everything. All along, all this time that he had been a member of the division, it turned out—and I should have realized it, because of some of the other things I mentioned to you—that he didn't think the division was being run right. [Laughter] I mean, we had had all this great success! I don't think he even thought that Bob Sharp was a very good chairman—I don't know that, but I have a feeling. He's so critical! And so all along, all the time that he was growing up in the division, as near as I can make out, he was churning inside because he didn't think it was being run right, and now finally he's in charge, he can fix it up to run the way he always thought it should have run.

Now, by this time our faculty is about thirty or thirty-one professors. It's quite a bit larger than it was when I first got here. But still, every decision we ever made was made with the whole group getting together democratically and speaking as a committee of the whole. Certainly we had committees, but basically every important decision was made as a group, and usually in these evening meetings. Well, Wasserburg didn't like the evening meetings; he didn't think they were businesslike enough. So from then on, every important faculty meeting had to be held during the day. Well, now, holding faculty meetings during the day is very difficult, because of all the different things that are going on during the day. Also, it's not as conducive to leisurely discussion and a meeting of the minds. In any case, it wasn't the way we had done things. So frankly, I thought those daytime meetings were a step backward, and a lot of other faculty did, too.

The other thing he did was, he thought the division was a disorganized mess because geology and geochemistry weren't separate and so forth. He wanted to organize the division in a systematic way. Now, there were some good aspects to this, because what he did was say, "OK. We're going to have four separate entities in the division. We're going to have planetary sciences, geophysics, geochemistry, and geology. And they've all got to be separate options with separate people in charge and separate groups of students, and they all have to have a syllabus on how they're going to be run…," and everything else. Everything had to be organized.

Of course, planetary sciences was already basically separate, and geophysics, the Seismo Lab, was also separate. But geology and geochemistry had always been just sort of hit or miss. In fact, you'd ask a lot of the PhDs in geochemistry and geology, "Do you want to get your degree in geochemistry or geology? More or less, you can take your pick," because the requirements for both were so nebulous. Wasserburg didn't like this. Geochemistry had to be separate from geology. So one of the first things he did was institute this set of executive officers. We had never had executive officers, and certainly not different executive officers for different options before. So he decided to appoint—well, first of all, he had to get this idea approved by the administration and the faculty, because this was now going to be in the catalog and everything. [Tape ends]

Begin Tape 8, Side 2

TAYLOR: These executive officers were going to be actual formal appointments. So then Wasserburg asked for the advice of different people. I told him that for executive officer for geology, as long as he was going to do this, he should pick Lee Silver, because he was our senior field geologist. But I don't think Wasserburg would have been very happy working closely with Silver, although I think he certainly did consider it. Anyway, for whatever reasons, he picked me to be his executive officer in geology. And he picked Rob Clayton to be his executive officer in geophysics, even though this was very difficult, because Don Anderson, as director of the Seismo Lab, was the de-facto leader of the geophysics group. But another thing about Wasserburg was that he had always hated—"hated" may be too strong a word—but disliked the fact that the Seismo Lab was a separate entity and had its own source of funding and its budget and always acted as if it were separate and above the rest of the division. I don't think he liked that. As far as I can see, one of the reasons he did this was to undercut the director of the Seismo Lab, because now the director of the Seismo Lab was just the director of the Seismo Lab and had nothing to do with the students or the running of the geophysics program at Caltech. That was now in the hands of the executive officer, Rob Clayton. And the executive officer of planetary sciences was Andy Ingersoll, which was fine. And then the question was, Who is going to be the executive officer for geochemistry? There was nobody in geochemistry that Wasserburg thought was up to the job, so Wasserburg named himself executive officer for geochemistry. [Laughter] In other words, he was wearing two hats.

Now, one of the problems with this, as I said, was that geology and geochemistry were inextricably interlinked. I was partly a geologist and partly a geochemist. For many years I taught field geology here at Caltech, but I was also in geochemistry. Now I was executive officer for geology, but I also was in the geochemistry option; I had many geochemistry students. So now when there were meetings of the geology option, I had to preside as executive officer of geology, but I also had to go to the meetings for the geochemistry option. So now there were lots more meetings. But there was no separate officer for geochemistry, which in a way was good, because it was already top-heavy. It was top-heavy with administrators. We only had a faculty of thirty, and now we had a chairman and four executive officers underneath the chairman for this small division. [Laughter] You can joke about it, but it was really.... It had its good aspects. We did focus and look at all of our programs, and the geochemistry program was in fact strengthened because Wasserburg now was in charge of it. So a lot of the things that were not being handled very well started to get handled much better.

But it made everything more difficult in terms of time, because before every division meeting, usually at lunchtime, we'd have to have an executive officer meeting in which we'd go through, for an hour and a half or two hours, the whole agenda of what Wasserburg wanted to accomplish, and then we had to do it again at the faculty meeting. Now again, this is not all bad, because there's a lot of exchange of ideas. And he certainly would listen to us when we'd speak and so forth. So nominally good things came of it, but we all had other things to do, and time was probably the most important commodity for all of us, and it was being used up. I have to say that the bottom line of Wasserburg's tenure as chairman was that he made everything more time-consuming. Everything before that used to be done in a very simple way, and that's one of the great things about Caltech, in my history—there's been so little bureaucracy. It really has gotten worse, because it used to be if you needed to buy something, it was very easy to do. Now it's harder to get things done. There are a lot more bureaucratic procedures. Probably a lot of them were put in because of problems like Jim McGaha, and the growth of the campus, and so forth. But this was a shock in the late eighties, when Wasserburg was chairman, because not only did he make us all executive officers but he said, "Now, if you guys don't have time"—maybe he was thinking that this was taking too much of our time—he said, "We've got to have academic officers as well." So each of us had to pick somebody in our option and name them academic officers. So now we had a board, we had the chairman, four executive officers, and four academic officers. [Laughter] So, that means that a third of the division was part of the administration—nine of the thirty professors. As I look back on it now, it's almost laughable. But this was Wasserburg's somewhat military idea of how things should operate. All along he thought there should be more systematic ways of running things—that the old nebulous way of doing things was no good.

And the other thing was that Ed Stolper, of course, was in the geochemistry option, and he started to be at loggerheads with Wasserburg over a variety of things. When Stolper first came here, they were great friends, but with time I think Stolper saw how Wasserburg operated. You know, Ed believes that everybody should be treated fairly and as nearly as possible in exactly the same way, whereas Wasserburg thinks people should be treated according to their merits—or his idea of their merits. One of the things he did with executive officers is he set up an administrative account for all of us, a slush fund we could use—you know, extra money. He got the institute to give us a few thousand dollars a year that we could use for whatever we wanted. He thought as chairman you had that kind of thing, so as executive officers we should have the same. Well, that's a nice perk, but we didn't really need it, and I don't think it was a good use of division funds. Nevertheless, he did that. So with time there was more and more of a clash with Ed Stolper. And maybe Stolper and Wasserburg just should have tried to figure out how to work it out with each other, because they had to live together. But I think Wasserburg figured that being chairman, he didn't really have to listen to Stolper's views when they clashed with his own.

Now, at about this time, the late eighties, Ed had, as you might expect, made a big scientific splash, and Harvard was making a big play to hire him. And things were getting bad enough with Wasserburg that it looked like Ed might go. Also, at this time we had a very good geophysics professor, Brad Hager, and he had had a big offer from MIT and he was struggling about whether or not to take it. And the one thing a chairman is supposed to do is make sure we don't lose any good faculty and that we get good new faculty. And here was one of the first times we were going to lose somebody to MIT who a lot of people thought was a very good faculty member, because they were offering this fancy job. And for whatever reason, Wasserburg didn't go to bat strong enough to keep him here. As a matter of fact, there was a big swath of money just coming into geophysics, and instead of using that as a plum to try to hold Brad Hager here, Wasserburg never even mentioned it to him. But finally, when Brad came in and told Wasserburg he was definitely leaving, Wasserburg said, "Oh, that's too bad, because we just got this big sum of money. You could have had all this." [Laughter] When I heard that, I thought, "Oh, this is crazy."

Anyway, that's the story. There's probably another side to it. And Brad did go to MIT. He was a good friend of Stolper. Stolper, I think, was unhappy about that. So here we are. We had one professor leave because he had such a great offer from MIT. Stolper had this great offer from Harvard, and that's where he came from and all his family was back there. Even though he really had done well at Caltech and clearly loved Caltech, this thing with Wasserburg was getting bad.

And then there was the Kirschvink affair, which I think you probably know quite a bit about; I don't know all the details about it. Joe [Joseph L.] Kirschvink was a former Caltech undergraduate. I had taught him in summer field camp, way back when he was an undergraduate, and he was obviously a bright young kid—he graduated with a degree in biology as well as in geology. Then he went to Princeton and worked with Al [Alfred G.] Fischer and also did an independent PhD thesis, and then we hired him back here as a geobiologist. He was our paleontologist-geobiologist, which was a field we didn't have—we had Heinz Lowenstam and Kirschvink, and Kirschvink was more or less a protégé of Lowenstam's after he came back. Joe was very interested in paleomagnetism and the magnetism of insects and animals of various kinds. I think he also wanted to see whether or not he could find magnetite in various animals, because Heinz Lowenstam was the one who first discovered biomineralization, magnetite, in chiton teeth, and it was a great discovery. Joe Kirschvink continued this work in an even more powerful way by bringing in actual paleomagnetic measurements on these kinds of things. His research was focused not just on geological problems but on biological problems and how magnetism affected animals that navigate in the Earth's magnetic field.

I don't know all the details exactly, but I know that Joe wanted to work on human subjects, and he had a faculty member in biology that he was going to work with—because apparently, I think, you have to be an MD to work on human subjects. But in any case, he had these funding proposals to do these kinds of things, to work on things that may not have been geological problems but were certainly important scientific problems. The chairman has to sign every proposal, and Wasserburg thought it was not appropriate research to be taking place in our division, because it wasn't geology. I don't really understand why he behaved this way, because I know for a fact that if Wasserburg had wanted to work on a physics problem when he was a professor, if any chairman had tried to stop him from working on something that was just pure physics rather than geology, he would have raised a ruckus.

So it was another example, I think, of probably the biggest problem that Gerry has, which is that he can't look down on himself from above and see his own actions vis-à-vis somebody else's. It's so clear that as a professor he would demand independence, and particularly at a place like Caltech. How could a professor not do research on any subject that's intellectually rewarding, in any field? I mean, if you can't do that at Caltech, you can't do that anywhere. And to have a chairman—who certainly has the authority, because he has to sign the proposal—to have a chairman say, "You can't do this research because it's not appropriate in our division," this made no sense to anybody. Kirschvink appealed it to the Academic Freedom and Tenure Committee and went up through the administration; and somehow or other Wasserburg's wrist was slapped and he was told that he had to sign the proposal, so it finally did go through. But then a year later or something like that, the same thing came up again, and again he refused to sign Joe's proposal. By this time Joe was furious. He and his good friend Jim Westphal started to raise a big ruckus. And the people they went to—because Wasserburg had set up this organization—were the division's executive officers. There were only three of us—obviously we didn't invite Wasserburg—to discuss this problem. And it was serious enough that a lot of the faculty were on Joe Kirschvink's side. Also there were these other problems: Stolper was thinking of leaving; other people were tired of some of Wasserburg's dictatorial ways of doing things. And they *were* somewhat dictatorial—although none of them was as dictatorial as the one thing Barclay did in hiring Stolper, which was good. [Laughter]

COHEN: Sometimes it's good to be dictatorial.

TAYLOR: Sometimes it's good to be dictatorial. But anyway, when Wasserburg was dictatorial, things didn't seem to work out very well. So the three executive officers had a couple of meetings, and then we got the whole division faculty, and they voted almost unanimously that something had to be done about Wasserburg. So now, as executive officers, we go to the provost, Barclay Kamb. This is what made it difficult, because Barclay had been a provost only for a year or two, and now he was faced with a crisis, because we had just deposed Wyllie as chairman and now here we were again. It was like a joke: What's the matter with you people in geology? You just destroy chairmen. You chew them up and spit them out. [Laughter] First you get rid of Wyllie and now just a couple of years later you get rid of Wasserburg. But in any case, Barclay had to deal with it.

Now, we made it clear what the division's wants were and that it couldn't go on—that

Stolper was almost certainly going to leave. Kirschvink was up in arms, and rightly so, because it just didn't seem right to anyone who really objectively looked at it. And I still don't understand why Wasserburg did it. For the life of me, I don't know why he did this, other than I think he probably felt it was right. Inside he felt he was right and the rest of the world was wrong, and he stuck to his guns, which is somewhat admirable, I guess, but in this case it was going to destroy the division, because Kirschvink was in a turmoil and the most important person, Stolper, I think would have left. We made this clear to Barclay, and Barclay also was the reason Stolper was here in the first place, so he wasn't very happy about the idea of Stolper leaving. So whatever went on with Barclay and [Thomas E.] Everhart—Everhart was Caltech's president then; and Wasserburg, all of a sudden one day near the end of the academic year, at a noon faculty meeting, Wasserburg came in and said he'd resigned. We knew what had actually happened, but Wasserburg never.... He just said he'd had it with these guys. [Laughter] But from that point on, it was clear that he blamed me and Sam and a bunch of others. He really was upset about it, and I suppose rightly so. And he was furious at Barclay also. It didn't do Barclay's situation any good because it made it look like the whole division was a mess. I'm sure to the rest of the administration the geology division didn't look like it was in very good shape, because it was making all these peculiar decisions.

Peter Goldreich was made acting chairman. That worked out quite well, because he had a lot of clout and very good antennae out through the whole institute. There's an interesting story about Clair Patterson: When Barclay was chairman, Pat made it somewhat known that he was unhappy not being a professor. He had always been a senior research fellow and he had been more or less happy, but somehow or other he had gotten to a point where he really wasn't happy with not being a professor. So Barclay, as chairman, got his appointment through as a full professor. It wasn't all that easy, either, because Pat had these idiosyncratic behaviors and didn't really want to teach, and things like that. So there were a lot of problems, but he certainly had the stature to be a full professor at Caltech. Anyway, Barclay got the appointment through, and then he went to Pat. And again, I don't know all the details, but he went to Pat and said, "OK. Here. We got it through." And Pat said, "Well, I don't think I can accept the responsibility that would come with all the teaching." [Laughter]

COHEN: He turned it down?

TAYLOR: Yes, he turned it down. I mean, it was unbelievable. We just said, "Oh, that's Pat." But then when Peter Goldreich was chairman, it came up again. Now, I think it was because Pat was thinking it would be better for his future if he did have a faculty appointment. So Peter got Pat's appointment through as a full professor and this time he accepted it. Very soon after, at about the same time, Pat got elected to the National Academy, and later on he won the Tyler Prize. So a lot of nice things happened to Pat. He was very deserving of these things in his old age.

But Peter Goldreich didn't want to be a permanent chairman. We would have been, I think, very happy to have him, but he had a joint appointment in physics. He had all these meetings. I mean, talk about somebody who had to go to a lot of meetings—that's Peter Goldreich. So Dave [David J.] Stevenson became chairman. He was a good choice, because he's very mature, very solid, and did things in a very systematic way. Now, he kept the executive officers in place. So we all stayed as executive officers during Stevenson's administration, but we never met. [Laughter] You can look in the catalogs and see on the books that we had executive officers during this whole time, but they were just on paper. Then finally, when Stevenson's term was up, we got Ed Stolper as our chairman, and he did away with the whole thing. [Laughter]

COHEN: So you're back at square one.

TAYLOR: Well, now we have just one academic officer, George Rossman. I mean, it was silly to have that many administrative people. It was so crazy, as I look back on it now. [Laughter] And as I said, Stolper has now had to be responsible for replacing all of the old professors who've retired in the last decade. Of course I'm the most recent one, just retired on July 1st.

COHEN: So that's been a huge change.

TAYLOR: Oh, an unbelievable change. But now we're in atmospheric chemistry with Paul Wennberg; we're in oceanographic chemistry with Jess Adkins; we're in a lot of new fields. We're in biology with Dianne Newman. They all look very impressive—young people.

COHEN: So you think the department's in good shape now.

TAYLOR: Yes, I do. It's an amazingly lucky department because, starting back when Wasserburg was chairman, we've looked at lots of appointments in geochemistry. In fact, we made several junior and senior appointments in geochemistry, and for one reason or another they turned us down, and we kept on seeming to be losing out. And finally we hit on Ken [Kenneth A.] Farley. It was just lucky. And he decided to come. He's turned out to be just fabulous. All the other ones, the previous appointments we made, who turned us down, have been moderately good but nothing like Ken Farley. Ken Farley did a whole bunch of really great things right away. Yes, I think we're in good shape. We're a young department with lots of assistant professors, which is a good way to go—not a lot of old fogies and so on; they've all retired. Stolper has at least three more years; he will have served the full ten years. During my tenure there have only been three chairmen who lasted longer than five years—Sharp, Kamb, and now Stolper. And those are clearly the three top people. They've done very well.

You have to want to wield power. Wasserburg certainly wanted to wield power, but he wasn't very good at it, I don't think. But Sharp wielded power very well. Barclay obviously wielded power very well when he had to. And Stolper wields power very well. So you have to want to do these things. I'm lucky I never had to be in a situation where I had to do that, because I don't like to wield power. I don't get upset about a lot of things. I would have gotten very upset if Stolper hadn't been hired or if Stolper had left Caltech. I think I would have then made it clear how unhappy I was about that, because it's so obvious how gifted Stolper is. But you know, I guess it wasn't that obvious to Wasserburg; in fact I think Wasserburg may have been somewhat jealous of the fact that there was this other budding superstar competing for attention, or something like that.

But to my way of thinking in the latter part of my career, the essential things were getting Stolper and keeping Stolper. And now he is the chairman at a time when the whole division is being revamped in a massive way, and hopefully it will be good. He certainly pays attention. One of the great things about Stolper is that he's really thinking about the future of the division. One of the projects he started a few years ago concerns all the people who have endowed professorships. They each have a fund; their salary money is offset and goes into this research fund that they can use. But it just sits there and doesn't gather interest. He wanted each of us to make that money part of the division's endowment; he'd still allow us to tap it if we ever needed to, but it would be part of the division's endowment now. He's an endowed professor, and he's done this also, with his own research fund. I think he's built up a fund of something like two million dollars. The division is financially enormously better off now than it was when he took over as chairman. Apparently he's the only division chairman who's thought of doing this kind of thing. He's creative about the way he deals with problems, and creative in the sense that he knows it's important for the division to have a source of money it can tap into when it needs to. And it's not for himself; it's for the future of the division after he steps down. That's the kind of vision you want to have, the kind of vision that Bob Sharp had. When I was chairman of the division's space committee, the only person who would ever willingly give up space was Bob Sharp himself. [Laughter] I mean, to unselfishly give up something for the good of the division or for the future of the division, that's the true test.

COHEN: How many people pass that?

TAYLOR: Well, it's one thing that Wasserburg absolutely didn't do. He didn't give up anything. We thought he would make the *division* greater, but instead he used the chairmanship to increase his own group's size and strength.

COHEN: What do you plan to do now that you've retired? Just what you've always done?

TAYLOR: Well, I have a lot of papers I have to finish writing up. But I'm really interested in a lot of things. Although maybe it's a pipe dream, I would like to write a book about college football. I have a number of really strong interests, and college football happens to be one of them. I'm a great sponge for information about a number of things, like movies. I just love movies and plays. As a matter of fact, if I see a movie on television, I only need to see a couple of scenes in it and I immediately know what it is. Also I'm very interested in World War I and World War II. I may try to write some books.

COHEN: Will you continue any of your geology projects?

TAYLOR: Oh, yes, I'll continue all that. As I say, I have a lot of papers to write, and I still have an NSF grant to finish up some of these papers with former students.

COHEN: So you don't envision leaving Pasadena or anything like that?

TAYLOR: Oh, no. We're going to stay here. Well, I wouldn't mind leaving Pasadena, but I couldn't stand to be away from Caltech. [Laughter] No, I wouldn't want to move.

COHEN: Well, good. Anything else you want to add for the record—personal observations?

TAYLOR: Well, I could mention something else. Before the planetary sciences group came here, we had been called the Division of Geological Sciences, and after they were an entity here, the Division of Geological Sciences just didn't seem right to them as a title for our division, because "geology" applies only to the Earth. So there was a big debate about what we should call our division, to make the umbrella big enough for planetary sciences. And there were a lot of ideas. Lee Silver wanted to call us the Division for the Study of Earth and Planets. [Laughter] I just hated that. See, I'm a great believer in tradition. If something's poor, then get rid of it, but if something is good, like our division name, then don't get rid of it. And to make it the Division for the Study of Earth and Planets—I just hated that. As much as I love Lee, I just hated that idea. So I *really* spoke up against that. And other people wanted to call it the Division of Earth and Planetary Sciences, or something like that. But again, I felt strongly that it should stay the Division of Geological Sciences, and then maybe amend that. And I *really* spoke up, and I can be very loud. [Laughter] Finally enough people agreed. I was worried that we might have to say "Division of Geological, Geophysical, and Planetary Sciences," which is too cumbersome. But Don Anderson said, "We don't have to worry about that. The geophysics group is happy to be under 'Geological'." So we settled on Division of Geological and Planetary Sciences. No other entity in the world, I think, has that specific title. There are lots of divisions or departments of Earth and Planetary Sciences, but I don't think there's another Division of Geological and Planetary Sciences. And I like it, first of all, because it's a tie to our past. That was the original name of our department, which Buwalda started. I think that's great.

Also, you wanted to know which one of these medals meant the most to me.

COHEN: Ah, yes!

TAYLOR: And it certainly is the [Arthur L.] Day Medal. First of all, it's given by the Geological

Society of America, and I consider myself a geologist. As a matter of fact, one of the things I worry about most about our division is that it might get too far into these peripheral fields and leave geology and rocks behind. I hope that doesn't happen, but fewer people in our division go to the Geological Society of America meetings now than they used to. Lots of our faculty members have been presidents or high up in the Geological Society of America. The Geological Society of America is the premier geological body in the United States, and I hope Caltech will always have a presence there, but it's less now than it was.

One medal they give is the Arthur Day Medal, which is given for the application of physics and chemistry to geology. It was started in 1948, when I was in high school. There have been fifty-four of them given. I was the recipient of the Day Medal in 1993, so about ten years ago. At the time, I was looking through the list of Day medalists, and it was just incredible. First of all, most of them I knew and had associated with. And one of the reasons I knew so many of them was because so many had been associated with Caltech. An amazing number of Day medalists had been either undergraduates, graduate students, or faculty members at Caltech, all of which I was, or visiting professors or visiting scientists at Caltech—including Frank Press, Sam Epstein, Gene Shoemaker, Don Anderson, and Gerry Wasserburg. Many students, including one from my own class of 1954, that class I told you about that had an original 180 members and finally graduated with only about 90. [Laughter] One of those members was a Day medalist—Don Turcotte, who is at Cornell. It was remarkable to me when I went through that list. And a lot of them, the ones that weren't specifically associated with Caltech, were people I got to know at Harvard as either graduate students or faculty members, like Jim Thompson and Francis Birch. So in my acceptance speech I said, "I'm just the latest in a long, long line of Caltech people to receive the Day Medal," which was natural for Caltech, because the Day Medal is given for the application of physics and chemistry to the study of the Earth.

COHEN: Sure. That doesn't exist in some places.

TAYLOR: Yes, it makes sense. But the remarkable thing is that Caltech has totally dominated the list of Day medalists. Of the fifty-four recipients it's amazing how many were associated with Caltech. So I said, "I'm just the latest in a long line of Caltech recipients, and I'm sure there'll be several after me," and there have been. In 1995 there was Tom [Thomas J.] Ahrens [professor of geophysics]. Don DePaolo, who was a student at Caltech when I was a professor, got the Day Medal. Rick O'Connell, who was a student at Caltech, just got it last year. It's like the Caltech medal, as far as geology is concerned.

COHEN: Well, the department here has been number one for a long time.

You haven't had too much to do with any of the presidents. You haven't mentioned any of them at all.

TAYLOR: Well, I've interacted with them socially. When I came here, most assistant professors got start-up funds and labs. But I already had my lab, so I never got anything, and I never asked for anything either. I haven't been a pest. [Laughter] I've never had a problem with the administration. They've all been really good to me. Of course, I knew [Lee A.] DuBridge as a godlike figure when I was an undergraduate. And I knew Millikan from the point of view of seeing him walk around campus—and he gave us a lecture once. Harold Brown was the only president who came around and actually looked at my lab and wanted to know what I was doing. None of the others have done that. I got to know Murph Goldberger pretty well; he was one of the most easy to get to know of all of them. I liked him. I also liked Tom Everhart. [David] Baltimore is kind of distant and cold. But as I said, I'm happy to have been in a place where they let you do your own thing and operate as an individual and—

COHEN: And just go your own way.

TAYLOR: Yes. They make it so easy for you to do science here. If you can't do science here, boy, you can't do it anywhere! I could not possibly have done as well anywhere else. It just would have been impossible. This is the place where I felt comfortable. Caltech made it possible for me to do things. I owe them everything. [Laughter]

COHEN: Well, that's a good note to stop on. [Tape ends]