



## WARD WHALING

(1923 – )

INTERVIEWED BY  
SHELLEY ERWIN

April – May, 1999

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### Subject area

Physics

### Abstract

An interview in four sessions, in April and May 1999, with Ward Whaling, professor of physics, emeritus, in the Division of Physics, Mathematics, and Astronomy. He recalls growing up outside Dallas and later Houston, Texas. Entered Rice University in 1941 and joined Army Signal Corps. Graduated with a BS in physics in February '44, and spent three months in the Signal Corps Officer Candidate School, Fort Monmouth, NJ, where he studied advanced radar techniques. Recalls his stint with the U.S. occupation forces in Bremen. Discharged in September 1946, he returned to Rice for graduate work, where he became a teaching assistant for William V. Houston; PhD 1947, with Thomas W. Bonner, thesis on the reactions of lithium-6 with deuterium. He recalls work with early Van de Graaff accelerators. Dr. Whaling became a research fellow at Caltech in 1949 (he joined the faculty as an assistant professor in 1952). At Rice, he had been working on energy levels of beryllium-7, which was of interest to a group at Caltech's Kellogg Radiation Laboratory. He joined the Kellogg group and helped build a magnetic spectrometer. He recalls that work and his colleagues Alvin Tollestrup, William A. Fowler, Charles C. and Thomas

Lauritsen, and later Fay Ajzenberg-Selove, Charles A. Barnes, Ralph Kavanagh, Robert King. Discusses Caltech's postwar military projects. Recalls Fred Hoyle's work on nucleogenesis at Caltech and Hoyle's interactions with Kellogg group and Caltech astronomers. Offers his recollections of social life at Caltech, and of Robert Bacher's tenure as division chairman [1948-1962]. Recalls the musical shows that J. Kent Clark [professor of English 1947-1986] and Elliot Davis put together, and the Apicians, a dining club at the Athenaeum. There is an extensive discussion of the early days of astrophysics and nucleosynthesis at Caltech. Describes his duties as secretary for the Faculty Board (a post he has held since 1984) and the work of the Academic Freedom and Tenure Committee. Discusses his unrewarding year as RA [resident associate] in Fleming House in the mid-1950s and the undergraduate culture at Caltech. Became emeritus in 1993. Reflects on how much he has enjoyed teaching at Caltech, especially the laboratories. He concludes the interview with a discussion of his work on the scanning interferometer for the McMath solar telescope at Kitt Peak to measure atomic branching ratios.

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**CALIFORNIA INSTITUTE OF TECHNOLOGY**

**ORAL HISTORY PROJECT**

**INTERVIEW WITH WARD WHALING**

**BY SHELLEY ERWIN**

**PASADENA, CALIFORNIA**

Caltech Archives, 2002

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**CALIFORNIA INSTITUTE OF TECHNOLOGY**  
**ORAL HISTORY PROJECT**

**Interview with Ward Whaling**  
**Pasadena, California**

**by Shelley Erwin**

Session 1	April 6, 1999
Session 2	April 27, 1999
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ERWIN: You're going to tell us about your family.

WHALING: I was born in Dallas, Texas, in 1923. My father was a professor of church history at SMU [Southern Methodist University], and we lived just on the edge of the campus. SMU was quite new; it was started about ten years earlier, and my father was part of the original faculty. I think there were about 300 students and thirty faculty members.

My father was actually an ordained Methodist minister. He was pastor of a church in Houston before he went to SMU. In that small faculty, practically everybody had more than one job. Another professor was the registrar; other professors were in charge of this or that; and one of my father's extra assignments was to establish a Methodist church on the campus, which he did. In addition to being a professor of church history, he established the church there, which has grown as SMU and Dallas have grown and is now, I think, one of the biggest churches in Methodism.

SMU is in University Park, a little community which is now a part of Dallas. In those days it was fifteen miles outside of town, and very rural. My parents had a cow. We lived adjacent to the campus, and the campus was mostly a big prairie. There was a vacant lot between us and the campus, and we had a cow.

ERWIN: Was this for milk, then?

WHALING: Yes.

ERWIN: So did you milk the cow?

WHALING: I didn't. I was born in 1923 and in 1926 we moved to a larger house. So we moved away from the cow.

ERWIN: Were you the first child?

WHALING: No, I was the third child. I have a sister who is eight years older than I am, and a brother who is five years older. When there got to be three of us, we needed a bigger house, so we moved into a neighborhood nearby, but with houses all around we couldn't have a cow.

It was a very nice place to grow up, with wide open spaces—very pleasant indeed. I went to school in University Park. I went to private school for my first six or seven years. And I never really stopped to think about that very much—why that happened. I never asked about it at the time. We were a very egalitarian family, and there's nothing wrong with the schools in that part of the world—certainly not at that time. But the reason was because of Anne, my older sister, who was almost a prodigy. She could read and write before she entered school. We never talked much about this in the family; I don't remember hearing much said about it. She died last June, and my wife and I went to Dallas to close up her apartment. And I found in her papers poems that she had written when she was five years old. So she was a rather unusual person. Also, she had a facial birthmark that was very prominent, and I guess other children could be quite a burden because of that. Anyway, she went to a private school and did exceedingly well. Hers was the strongest academic record they'd ever had. And when I came along, the school approached my parents and said, "Would you like to enter Ward in the school? We will give you a scholarship." So it wasn't beyond their means. Well, they were of course pleased with Anne's education there, and maybe they thought some of it would rub off on me.

So I went to a private school. And I never thought how strange it was until I was thinking about it this morning. You know, an academic family is not loaded with money, so it

was rather odd. And I stayed there until I was ready to go to high school. At that time, my father had left SMU and gone back to being a pastor in a Methodist church. He was sent to a church in Houston at the time I was to enter high school, so I went to a public high school there.

You asked whether there were any influences that would have steered me into science there. No, I don't think there were. They might have steered me the other way. In high school I took four years of math and four years of English and four years of Latin. And physics and biology, and history and civics, and certain required courses. The course I enjoyed the most was math. It was not an outstanding course—geometry, trigonometry, and algebra, and things like that—but I enjoyed it. In physics, we played with pulleys and inclined planes and boiled some water; it was really very primitive, and the teacher was an old crone. And I guess I should have mentioned that the teacher in mathematics was an attractive, youngish woman. But this high school course certainly never would have inclined me toward physics.

ERWIN: Did you have any particular hobbies, or other things that indicated how you would like to develop?

WHALING: I thought about that, too. What did I do? When I was out of school, we played basketball and things like that. I collected stamps. I collected whiskey bottle labels. I wonder why I did that. [Laughter] I think because when prohibition was repealed, this was a very foreign something-or-other—and it was wicked. Little kids would find a bottle on the side of the road or in an alley or something and we would carefully extract the label. I had a great collection. It's like baseball cards; it was stupid, really, but we did it.

I fixed bicycles and little bits of mechanical things. I may have taken clocks apart—I don't ever remember putting one back together and seeing it run. I don't think either my schooling through high school or my hobbies would have given one any particular clue as to where I was going. But the high school I went to in Houston was in the most economically advantaged part of town. It's very near Rice University.

ERWIN: What was the name of the high school?



WHALING: Lamar High School, named after a famous early president of Texas. Practically the better half of the graduating class [went to Rice]. Rice had very high academic standards, because it had at that time no tuition. It was founded by a rich lumberman, who set it up for the white boys and girls in Texas. Now they do charge tuition, but not a lot. And it still draws a lot of students, but now more nationally. We're talking about a long time ago—this is 1940, when I graduated from high school.

ERWIN: Well, we're coming up into World War II. But I never knew that Rice was tuition-free.

WHALING: It was. It was founded about the same time as Caltech got its real impetus—when [Robert A.] Millikan came here, around 1915.

When I graduated from high school, it seemed the most natural thing in the world to go to the nearby school where my friends were going. In high school, I did my homework and I followed instructions. I don't think I was ever very much interested in anything, except maybe the math. But I had reasonably good grades, so I could qualify for admission to Rice. It was sort of like you go to the neighborhood high school and then you go to the neighborhood college. It never occurred to me to go to a school away from home.

ERWIN: Do you think this was typical of the time and the place?

WHALING: I think it was. One of my friends went to Yale, but the others all went along to Rice.

ERWIN: Did this have anything to do with your identity as a Texan? Did Texas feel like it was a place unto itself, that you didn't need contact with the rest of the world? Or was it just the custom?

WHALING: Air travel was not as common then as it is now. And it's a long way on a train across Texas. So I think it was simply, well, you go to school in your neighborhood if it's possible. And I must say that the absence of tuition was attractive. In the same way that academics are not well-to-do, neither are ministers. So the fact that it cost nothing was an attraction. And then, when you enter, the freshman curriculum is pretty much required. You had to take a science; I took physics. And you take mathematics, English, history, and chemistry.

We're now getting into the middle of 1941. My brother was already in the navy, and many kids getting out of high school who didn't go to college were getting drafted. So that became a concern, a pretty high priority. A number of branches of the service were coming to the campuses and saying—or at least were saying to engineering and science students—“If you will enlist in the navy, we will leave you here until you get your degree, and then we will give you a commission.” And that seemed a more appealing way to serve your country than some of the other options, so I tried to do that. The navy would not have me. It turned out I was colorblind. But the Army Signal Corps was not so demanding. Several of us in physics and four students in engineering joined the Signal Corps. The Signal Corps was only looking for physicists and electrical engineers and allowed us to continue our studies, which were accelerated. There was no vacation period anymore; we went to school year around.

ERWIN: There was a navy V-12 program at Caltech. It sounds like this was a similar idea.

WHALING: Yes, exactly. These were very common. Initially they told us, “You will get a commission directly when you graduate, and you will earn dollars and see the world.” But I think they found that many—or certainly at least some—of the kids they got this way made lousy officers. So by the time I graduated, in February of '44, they said, “No, we'll send you first to Officer Candidate School, which is a three-month course, and those of you who are at all capable will be commissioned then.”

ERWIN: So there was an extra step.

WHALING: And like many undergraduates, I was not exactly sure what I wanted to do. And again, of the freshman courses I took, I enjoyed the math more than anything else. But at least at Rice I was exposed to some pretty first-rate faculty. The courses were large lecture courses the freshman year, with smaller section meetings. I actually enjoyed philosophy a great deal, and I took three years of it before I graduated.

By now, the draft and the military responsibilities were pushing us toward concentrating on physics and engineering work. And I enjoyed that, too. But that was what made up my mind

for me, whereas had there been no war I might well have gone in other directions. I think I might well have stayed in academia.

ERWIN: Maybe you would have been a philosophy professor.

WHALING: Yes, I might have been, or conceivably mathematics, but in that direction.

ERWIN: Did you keep up with any of your teachers afterward? Did you develop a continuing relationship with any of them?

WHALING: Of the undergraduate teachers, when I had occasion to visit the campus I would visit them, but it was not a close relationship beyond that. The people I knew best after leaving school were those I met in the graduate school there—at least of the faculty at Rice.

I left out one step, which maybe I should have put in. When I came to the last half of the last year of high school, I had taken all the courses the school offered. And the school didn't know what to do with us. They were going to put us in study hall for half a day or something. So we arranged that I would be excused at lunchtime instead of staying until three o'clock. But I had to go home and do home study under the supervision of my sister. Anne had graduated from SMU and gone on to Yale University as a graduate student in English, and she was always very much of an intellectual. My parents told her to assign my reading, and I had to stay inside and read until the alarm went off at three-thirty, and then I could go outside and play.

I read an awful lot of stuff—some that I enjoyed very much, some that I tried. I mean, she had me reading things like T. S. Eliot. And I tried. I knew it was very “in” at the time, very fashionable.

ERWIN: It must have been quite difficult to be by yourself, tackling works like that, where you had no real context, you had no discussion opportunities.

WHALING: That's right. And I must say I didn't get much out of it. There were other things that I read—mostly English and American literature, and some criticism—that I found interesting and perhaps educational. This was my last semester, and it was only two or two and a half hours a day. But it was a little bit unusual.

Then I eventually graduated from Rice. I majored in physics, and we were going around the clock, without a vacation period.

ERWIN: So it's really after Pearl Harbor that things start to accelerate.

WHALING: Yes, that's when it really did, and that's when we all joined the Naval Reserve or whichever service—because we were still under twenty-one; I was under eighteen. We did that so we wouldn't be drafted. We could see it coming, so we were making provisions for staying and finishing our education first.

I must say, it doesn't seem very patriotic now—and I think, after Vietnam, it seems that one could feel perhaps a little different about that—but at the time, we felt it was a good thing to do. I'm sure our parents were pushing us to postpone military service as long as we could. As I said, my brother was already in the navy and flying in the Pacific. And I think my parents worried a lot, as all parents would. They would not like to see two sons off to war.

Anyway, when I graduated, in February '44, I went to spend three months in the Signal Corps Officer Candidate School, at Fort Monmouth, in New Jersey. And while I was there, the war in Europe came to an end. I remember going up to Times Square to see the celebration of V-E Day. So we began to look in the other direction, toward the very far west, or the Pacific, as where we would be sent.

After we finished Officer Candidate School, which is a very basic sort of thing—how to salute and things like that, how to read a map, various things that are not very demanding—then we went to technical schools, also at Fort Monmouth, to learn some very advanced radar techniques, which really made use of our education.

ERWIN: And this was specific to the Signal Corps, then?

WHALING: Yes, that's right. We were using radar techniques. What we were using was a pulsed transmission for communication. It was supposed to be a very advanced communication skill. It used very high pulsed frequencies that had been developed for radar and were then being applied to communications. It started out as three months; I think it actually went on a little

longer than that, before we were considered ready to go out and operate this stuff in the field and maintain it.

So I was getting ready to go to somewhere in the Pacific to communicate from one island to another. They'd send you home for two weeks' leave and then you were to report to a port of embarkation in San Diego. But while I was home in Texas I got a telegram telling me to report to some place in South Carolina and "You're going to go to Germany in the occupation forces."

ERWIN: This was clearly before Hiroshima, then?

WHALING: No, Hiroshima happened while we were in the Signal Corps school, while I was in New Jersey. I remember seeing the headline and thinking, What have they done? I had not heard anything about the atomic bomb until it happened. It was quite a surprise to me, as I recall.

We were not sent to Japan. We were sent to occupation forces in Germany. And I ran a switchboard in Bremen, a big telephone exchange. All of the employees were German. I had had two years of German at Rice, and I could speak it a little bit and read it fairly well. But fortunately there were enough senior German engineers in the exchange that I could communicate with, and they would then transmit the orders to employees. I was the only American soldier there.

ERWIN: I would have thought that that was in the British sector.

WHALING: It was in a British enclave, you're right. But we were using it as a port, so we had shipping coming in and out of there. And somehow or other we were given maintenance of the telephone exchange. We were really quite isolated from the rest of the occupation forces.

ERWIN: The American forces, or all of them?

WHALING: Well, I don't recall any British forces there at all. There were a few American engineering troops and there were Signal Corps people there for radio and maintaining the long lines. But I don't remember any British soldiers there. We had communication with them, but I don't recall seeing British officers in our officers' club.

ERWIN: What was it like in Bremen, then?

WHALING: Oh, it was a mess. It was badly damaged in the war. And at Bremerhaven—Bremen is on the Weser River and it runs down to the North Sea, and at that point on the North Sea coast there's Bremerhaven, which was the main port. There they had big submarine pens—drydocks for submarines—but they were enclosed under concrete. And they took a tremendous battering. There wasn't much else there to shoot at, and it had all been destroyed by the time we got there. But the submarine pens were still in existence.

ERWIN: Were they full of disabled submarines?

WHALING: There were wrecks in the harbor. Certainly there were lots of parts of submarines lying around on the ground. It was pretty desolated. The city itself was a very old city and shipping center, and it had suffered considerable damage. But the cathedral was still OK, and a lot of the women who worked at our switchboards sang in the choir at the cathedral. They invited me to attend their services, and I did.

ERWIN: This is Luther's territory.

WHALING: Yes, that's right. They don't call it a cathedral, but it's the main church in town.

We didn't get to know the Germans very well. Another Signal Corps officer and I were billeted in a house which had been liberated—occupied—by US forces. But the woman who owned the house lived in the basement. She wasn't supposed to, but she did. And if we left our dirty clothes on the floor in the morning, they would get washed and appear all neatly folded when we came back at night. [Laughter] So we didn't look very hard at what she did. And I almost never spoke to her. She kept out of our sight. But we were gone—we'd go to work in the morning and come back in the evening.

ERWIN: When you say she wasn't supposed to live in the basement, she was supposed to have been evacuated?

WHALING: She was supposed to have been evicted from this house. The house had been—  
“liberated,” I think, is the word they would have used. Occupied forces will commandeer certain  
buildings for their offices and for billeting their officers.

ERWIN: So the population was just supposed to fend for itself and these buildings were taken for  
military use?

WHALING: Yes.

ERWIN: But she just hung around?

WHALING: Yes. And I guess when we left, she moved back upstairs.

ERWIN: But in exchange for her residency, she did little favors for you.

WHALING: Yes. The house was always kept neatly swept and clean. And even the laundry got  
done—not cleaning and pressing uniforms, but underwear and socks and stuff like that got done  
for us. I hope she....I guess she wasn't too happy about this, but anyway it came to a satisfactory  
conclusion.

One of the most memorable parts of my overseas duty was going over there on the boat.  
There were maybe a dozen Signal Corps people and 300 nurses. That was an adventure! The  
trip was very, very rough. What had been a transatlantic passenger ship had been taken over as a  
troop carrier. We arrived on Christmas Day, 1945. I came back in September of '46, and that  
was the extent of my military service. Nobody shot at me, and it was a very easy way to fight  
the war.

ERWIN: Then you received a discharge? You were finished with what you owed to the service?

WHALING: Yes, that's correct. I had applied to graduate school at Rice while I was still over  
there, and they said OK. I think I arrived a little bit late, not on the first day of school, but they  
said, “That’s all right, we'll make an exception and you can catch up as you go along.” So I got  
back to Houston.

ERWIN: Obviously you wanted to go to graduate school all along.

WHALING: That I sort of assumed. I mean, my sister and brother had both done that; my brother went to law school, actually. My father, of course, had done graduate work.

ERWIN: Were you thinking of an academic career?

WHALING: Yes. I'd spent an awful lot of time around the university when I was little, so I'd always leaned in that direction. So I went back to Rice as a graduate student. And again, it was possible to do it there, as you could here, now, with very little cost. Not only was there no tuition but you could have an assistantship, which would cover your living expenses—so there was no financial burden.

ERWIN: Now is this the GI Bill? Or even beyond that?

WHALING: I never thought about the GI Bill... There was no tuition, and my stipend came from a graduate assistantship. Maybe Rice recovered some costs from federal sources—I don't know.

ERWIN: That benefited so many.

WHALING: Yes. But I also had what we have here now—you know, a GRA [graduate research assistantship] or a GTA [graduate teaching assistantship]. I was entering the graduate school, and I remember doing teaching assistant work. With a GRA, it's hard to tell whether they're working on their thesis or working for somebody. In many ways, the GRA is very much like a scholarship, where you're paid, but you're working on your thesis and you'd be doing the same work whether you were being paid or not, or you were being paid by an outside source. The fact that there was no tuition may have meant that there was no GI Bill. Frankly, I do not now recall the financial arrangement—whether the GI Bill just paid tuition or whether it also contributed to a living expense, but I know I had a stipend from Rice in addition. So it was easy to support yourself and be fed and clothed without any burden on your family's resources.

ERWIN: And you were still in physics?



WHALING: And I was working in physics. The courses that I took as a graduate student were almost all in physics—oh, there was some math, but nothing more broadening.

ERWIN: Did you start to define your direction in physics at this time?

WHALING: As an entering first-year graduate student, you're filling in a lot of gaps in various areas. The first time I ever took a course in quantum mechanics was as a graduate student. Nowadays, of course, we do it for the undergraduates, but we didn't in those days. So we took courses, but they were not a very major part of our program; it was the thesis research that one did. One teaching assistantship I had was grading papers for the president of Rice, who was a physicist named William V. Houston, who had been at Caltech and had gone to Rice. [Became president of Rice in April 1947—ed.] [Tape ends]

### **Begin Tape 1, Side 2**

WHALING: I do remember working in the labs, just helping kids who got stuck. But my last year there, I remember grading papers for Houston and his advanced course—a course that he taught here at Caltech initially, and then he wrote the book [*Principles of Mathematical Physics*], and he went to Rice and taught it again there.

ERWIN: It's interesting that he was both the president of the university and also actively teaching. Was that typical in those days?

WHALING: The president who preceded him was Edgar Odell Lovett, who had been there for many, many years. He did not teach. But I think Bill Houston may have said, when they approached him, "Well, I've enjoyed teaching; I've taught all my life. I would like to continue doing it." And he did. He taught at least a couple of different courses from time to time there—using books he had written, courses that he had taught here at Caltech and developed here. And I helped him with one of the courses and got to know him pretty well. Certainly that was one of the factors in pushing me to come to Caltech when I finished at Rice.

My thesis work was a study of the reactions of lithium-6 with deuterium. This was of some interest at the time, because that reaction liberates an immense amount of energy. Those

are easy things to react—deuterium, which has charge 1, and lithium, charge 3—because they have low charges and they can be brought together closely with fairly low energy. And the idea of using lithium and deuterium as a fuel in some sort of reactor, like a tokamak or any sort of fusion reactor, was of some interest. And there was even talk about using it as a booster in a weapon of some sort. I imagine that if that ever happened, it was being studied in the government laboratories, but that work would have been secret.

It was a somewhat unique reaction, and it was difficult to study, because lithium-6 is a rare isotope—seven-percent abundance—of lithium, and lithium-7, which is the abundant isotope, is present of course and also reacts with deuterium—readily, because it has the same charge. So it was very difficult to study the reactions of lithium-6 in the presence of all the lithium-7, which is ten times as abundant and confuses the results.

So I was given the assignment of building an isotope separator, essentially. Basically, all it is is a mass spectrograph. The idea was that instead of just measuring masses with it, you accelerate the mass-6 and mass-7 isotopes and then separate them magnetically, and then collect the mass-6 isotope. And we would make targets of lithium-6, which I could then bombard in an accelerator and measure how lithium-6 reacted with the bombarding deuterons.

So I worked on that. We had some success; we could separate the two masses. But the trouble was that the impurities in our vacuum system would be deposited on the targets along with lithium-6; we'd get a lot of carbon, from the vacuum-pump oil vapor. We got rid of the trouble with lithium-7, but we were confronted with so much carbon that that was the great problem—trying to get rid of oil contamination in the background and the oil vapor in what was supposed to be a good vacuum. It was a good vacuum, by those days' standards.

I was struggling mightily with this problem when the AEC [Atomic Energy Commission] released some quantities of the lithium isotopes they had separated in the big mass isotope separators at Oak Ridge. The idea was that they would be used in medical or biological research. The AEC would make small quantities of many, many isotopes available, and on the list was lithium-6. I remember we ordered a tiny amount, not much bigger than a few grains of salt, of lithium-6 isotope in some salt—I've forgotten what compound, maybe lithium fluoride. It was quite adequate for a target for nuclear bombardment; you just need a tiny amount. So we carefully practiced with ordinary lithium fluoride and learned how to make targets. And then we put our magic lithium-6 fluoride in, and it was a success. I was able to go ahead and complete

my work in fairly short order, but with great help from the Atomic Energy Commission. I'm much indebted to them.

ERWIN: Now this accelerator that you used, it predated the war?

WHALING: Yes. The first of the so-called Van de Graaff accelerators was built at MIT, by a member of their faculty, William Van de Graaff. But it was in an airplane hangar; it was out in open air, because you're generating very high DC voltages and you would make sparks. That's why they put it in a huge building, and it was built on high insulated columns.

Then Ray Herb, at Wisconsin, shortly before the war, figured out that you could build one of these things in a pressure vessel and increase the pressure sufficiently so that you would suppress the sparks; that is, the dielectric strength of air increases as the pressure increases, and at very high pressure it is a very good insulator. So the physics people at Rice built a pressurized Van de Graaff generator. At the same time, they built one at Caltech, in [the W. K.] Kellogg [Radiation Laboratory].

ERWIN: Is this the one that Tommy Lauritsen [professor of physics 1946-1973] worked on?

WHALING: Yes. It was built, but before they really used it, the electric generator machinery and the accelerator tube were taken out of the pressure tank. The tank was used to decompress divers who got the bends from coming up too fast.

ERWIN: That was during the war? Military divers, you're talking about?

WHALING: Yes. Or shipbuilding divers anyway. So the pressure vessel was used for that purpose; it was not used for nuclear physics during the war. Similarly, the one at Rice was built just before the war, but I don't think it was used much during the war; I think everybody was away. And both the one here at Kellogg and the one at Rice were based on the one built in Wisconsin by Ray Herb. Actually, the one at Rice was a closer copy of the Herb machine than the one here. The Wisconsin and Rice machines were both horizontal; the one here was vertical.

So I became quite familiar with the operation of and maintenance of and use of Van de Graaff generators, such as were used here. When I finished my work at Rice—when I got my degree there—

ERWIN: Are we talking about the PhD now? The lithium work.

WHALING: Yes, the machine was used to accelerate the deuteron beams to a million volts, or 2 million volts, to study the rate at which energy would be produced when lithium-6 reacts with a deuteron and breaks apart into two alpha particles; you get a huge energy release. The accelerator was used to accelerate the deuterium beam. So in 1949, when I finished that work, I was looking for a place to go. Even then, and I guess for quite a long time, people who were thinking about an academic career would take some postdoctoral time, and often they would do it in Europe, just to complete or further polish their training. About the time I was getting ready to graduate, I applied for a National Research Council Fellowship. You've probably run into those before in these histories, because they preceded the National Science Foundation as the main source of funding for postdoctoral study. So I went from Rice to Caltech. In some ways, it was just a natural, following the family footsteps, not genealogically, because my thesis supervisor at Rice was Tom Bonner and he had spent a year in Kellogg—or maybe two, actually—as a National Research Council fellow before the war. He worked alongside Willy [William A.] Fowler [professor of physics 1939-1982], who was a graduate student at Caltech then. And he knew Tommy [Lauritsen] and of course Charlie [C. C. Lauritsen, professor of physics 1930-1962]. And when I wanted to continue my work in nuclear physics, he said, “Well, go to Caltech.”

There was another connection: the head of the Physics Department at Rice was also a nuclear physicist, and the one who had built the accelerator. He was an Englishman named H. A. Wilson, who had been trained at the Cavendish and then had come to Rice—I think in the original faculty in 1916, when Rice was founded. He was an old man when I knew him. But he and [Robert] Millikan had worked on the same problem of measuring the charge on the electron. Wilson did it—or he tried to do it and succeeded in getting a value—by using a cloud chamber, where you introduce a cloud of little droplets. Wilson's idea was to watch them fall and then apply a horizontal electric field and see them change directions as they fell. The droplets would

move sideways when they acquired a charge. It was the same idea that Millikan used, but Millikan had refined it by using a *single* drop rather than a cloud of drops that one sees in a cloud chamber.

So they were working at the same time on the same problem, and I'm sure they were quite aware of each other's work. And maybe that's why Wilson had urged Tom Bonner, who got his degree at Rice, to go to Caltech as a postdoctoral fellow.

And then, as I mentioned earlier, there was my friendship with Bill Houston. All this suggested that I come here rather than any other place I could think of to go. And the National Research Council Fellowship would provide the funding. It doesn't cost Caltech anything, so they welcome visitors on that basis.

I first drove out in June with Mrs. Houston and their daughter, Anne. I remember driving out just after commencement. Mrs. Houston brought me to the campus and introduced me to Carl Anderson [professor of physics 1933-1976]. I remember we had dinner with Carl Anderson—I think it was actually at the Huntington Hotel and not at the Athenaeum. The Andersons and the Houstons were very good friends. I guess Carl was a bachelor in those days; he married very late.

ERWIN: He already had a Nobel Prize.

WHALING: Oh yes, he got that very early [1936].

I remember coming to the campus, but it was just after commencement and I don't really remember meeting anybody in Kellogg at that time. I did arrange for an apartment to stay in when I arrived here. Really, my main function had just been to serve as a chauffeur for the two women, who wanted a ride out here. For the rest of the summer of 1949 I worked in the labs at Rice, and then I came here in the fall; my appointment started with the school year here.

It turned out that the work I did during the summer was of considerable interest to the people here—rather more so than my thesis work—because the group in Kellogg was studying the energy levels of light nuclei and looking very hard to find some regular pattern that might give them the sort of clue that the pattern of levels of the hydrogen atom gave to the model of the Bohr atom and eventually led to quantum mechanics. I mean, that's the only precedent they had to go by, so they were doing sort of the same idea: "Let's measure the energy levels, and maybe

we'll find some regularity of spacing that will help us understand." The nucleus whose energy levels they had studied most extensively in the Kellogg Laboratory was the lithium-7 nucleus. It doesn't have very many levels at the low energies accessible to them, but the Kellogg group had done the most precise and accurate work on the levels they could excite.

When I went back to Rice for the rest of the summer after my graduation—I had, I guess three more months there—I was doing two things. One was getting my thesis in the form for publication, and then I was working with a graduate student who was studying the energy levels in beryllium-7, making use of the lithium-6 targets that I had acquired from the AEC. And lo and behold, we were able to find in beryllium-7 a level that corresponded very closely with one of the levels in lithium-7. And this, then, gave them a clue: that is, "Let's look for the so-called mirror nuclei"—nuclei that differ only by exchanging a neutron for a proton. Beryllium has four protons, three neutrons, and lithium has three protons, four neutrons, so they're very similar and they turned out to have corresponding levels. And this was one of the first observations of this symmetry, and it fit very nicely into what was going on in Kellogg. They found our beryllium-7 of great interest.

ERWIN: So you brought this with you.

WHALING: Yes, it was an unexpected bonus. I did not know of Kellogg's interest in beryllium-7 when I first visited the lab, but when I came the second time, I brought the raw data. And it showed nice evidence of a particular level that the Kellogg group had been searching for; it worked out very nicely.

When I arrived, I didn't really know exactly what I wanted to do. We did not have the lithium-6 here to work with; we could have gotten it, I guess, if we'd wanted it. But another problem the Kellogg Laboratory was working on—and Alvin Tollestrup [graduate student, later professor of physics 1962-1977] was doing a thesis on the subject—was trying to measure nuclear masses very accurately by measuring nuclear reaction energies. As you know, in high-energy physics they measure the mass of particles in MeV [million electron volts] or GeV [giga-electron volts]. The equivalence of mass and energy is the mother's milk of high-energy physics, but it had not been much used in low-energy nuclear physics at that time. But they were trying to do that in the Kellogg Lab, and Alvin wrote a thesis on the subject. ["Precision

determination of the energy released in nuclear reactions in the light elements." (1950)] And as an approach to that, the plan was to build a large magnetic spectrometer to measure the energy of high-energy particles that are emitted in a nuclear reaction. And by building a chain of reactions, starting with the proton, one can measure very accurately the mass of the neutron by the energy released in the decay of a neutron to a proton. By measuring subsequent mass differences as you move up the scale, you can tie the nuclear masses to any reference mass you choose. Initially the chemists chose oxygen. Then they discovered that that wasn't good enough—there are two kinds of oxygen. So they said, "We'll use oxygen-16." Now the most precise measurements are in terms of carbon-12. That's the basis: when they talk about a mass 1, mass 2, it means 1/12 of carbon-12, and so on.

Well, these can be measured through nuclear reactions that relate how much energy is released as one nucleus is transformed into another. And that was the purpose of building this magnetic spectrometer that I was put to work on. It had been designed by Sylvan Rubin, who had gotten his PhD [1947] a couple of years before I arrived. The components had been ordered. But when I got here, there was the job of getting it assembled and putting the instrumentation and the controls and so on. So the first problem I worked on was getting this magnetic spectrometer ready to go. It took about a year to get it assembled and build counters, the vacuum system, the target chambers, and get the leaks out of it, and so on.

The first problem we were set to work on was to measure the energy released when lithium-7 is bombarded with a proton and makes two alpha particles. This reaction formed a link which enables one to relate the mass of the proton and the mass of the alpha particle and the mass of lithium-7. I can remember Willy Fowler was very much interested in this, and every morning he would come to school early, because he taught an eight o'clock class; I don't think anybody has done that in years, but he did. And he would come over even earlier than that, fill up all the blackboards in the room on the third floor of Bridge [Norman Bridge Laboratory of Physics] that he used for his classes. So he got there quite early—before I got to school, I assure you. But when he got out of his class at nine, he wanted me to come and show him the results we got the night before. The postdocs and graduate students tended to work at night, and we had to be there by nine in the morning to show him what we had learned the night before. He really wanted to see the raw data as it was coming out, but he didn't want to stay at night to see it.

ERWIN: What was his official position at this time? Was he the director [of the Kellogg Laboratory]?

WHALING: No. Charlie [Lauritsen] was always in those days the director, but Charlie wasn't as people-oriented. He took an interest and he always was there, but Willy wanted to look over your shoulder and check your arithmetic and see what was going on. Charlie was willing to wait until you finished and then see what you'd done; he wasn't as immediately keeping close tabs on it. But I can remember Willy—if we had something really exciting we'd wait for him to come out of class, but otherwise we'd wait down in our office. And the phone would ring, and he'd say, "Why don't you bring your stuff up," and we'd go up and show him what we'd done the night before.

ERWIN: I take it you found this stimulating?

WHALING: Yes, I did, very much so. He had a remarkable facility for keeping all the constants of nature in his head. He used a slide rule. He'd sit there and work out stuff and check your work very quickly. He didn't have to look up any conversion factors or constants; he knew them all. It was quite impressive.

That work turned out to be successful. But I've left out a very important part. When I arrived on the scene, the pieces of the magnet, as I said, had been designed by Sylvan Rubin. And before I arrived, there was a young Chinese graduate student, C. W. [Cheng Wu] Li. And after Rubin left [1948], Li was told to put the pieces together. But his English was very poor and he did not communicate well with the engineers in the central shop or with the stockroom people, if he had to go and get something. The project was going very, very slowly. So Willy asked me to step in and so-to-speak help Li.

Well, it turned out that I sort of took charge of the construction project. We worked together, and Li used the first experiments performed with the spectrometer for his thesis ["Nuclear Reactions with a Proton Magnetic Spectrograph" (1951)]. The work was successful, and we published a paper with four names on it—C. W. Li, Ward Whaling, William A. Fowler, and C. C. Lauritsen ["Masses of Light Nuclei from Nuclear Disintegration Energies." *Phys. Rev.* 83:3 512-518 (1951)]. Lauritsen had only a supervisory role, but I was pleased that he chose to



associate his name with this work and that he gave it wide publicity. Charlie was president of the American Physical Society in 1951, and the outgoing president gives a major address at the annual meeting of the society, held in New York City. In his address Charlie talked about our experiment and the fact that we had been able to derive the masses of all the light nuclei—including radioactive ones—using only nuclear reaction energies as input. This was the first time that nuclear masses had been measured without the use of a mass spectrometer. Charlie was as proud of this work as Li and I were.

It was appropriate that Li's name was the first on that paper, because he used the experimental measurements he and I made with the spectrometer to derive the nuclear masses. The energy released when a nucleus decays, or when two nuclei react, is a measure of the mass difference between the particle or particles in the initial state—that is, before the reaction—and the mass of the particle or particles in the final state, after the reaction. A large number of the reactions that measure the energy difference between neighboring nuclei in the periodic table are equivalent to a set of simultaneous equations, and these equations can be solved to find each nuclear mass that appears in the set of equations in terms of the mass of one nuclear-mass standard—oxygen-16 in our experiment.

In our experiment, this required the solution of forty-three simultaneous equations to find the mass of twenty-five nuclei between the neutron and fluorine-20, all expressed in terms of the atomic mass unit, or one-sixteenth of the mass, of oxygen-16. That would be an easy problem with modern computers, but in 1950 we had to use an approximate method called the relaxation method, which involved many, many iterations. Li did that computation by himself with an abacus and a Monroe mechanical calculator; all I did was check his results.

ERWIN: But it came out right?

WHALING: Oh, yes, it did. It was very nice work, and I think Charlie [Lauritsen] was quite proud of it. He designed the experiment—he said, “Well, let's do this”—but the actual day-to-day operations of it he did not follow. Willy followed it very closely. But the guys who hunted for the leaks in the vacuum system and turned on the beam and bombarded the target and measured the reaction energies were Li and I. Li solved the simultaneous equations by himself. That project went well.

ERWIN: When did that happen? Was that your first year?

WHALING: I think the first year was pretty well taken up in building and assembling the magnetic spectrometer. The big magnet stood about four feet high, though the radius of curvature was only sixteen inches. But with all the coils and cooling and the vacuum chamber, it was a great big thing that was not easy to move around the building.

ERWIN: Had you ever done work with equipment of that size?

WHALING: It was in some respect very much like the mass spectrometer I used at Rice to separate mass 6 and 7. In that case, we knew the energy of the particles and we were measuring the mass and collecting the nuclei of mass 6. In the spectrometer at Caltech, we knew the mass of the particles but we were measuring their energy. But it was the same idea of deflecting them in a magnetic field at high velocity in a vacuum. It was probably the first paper I did here.

ERWIN: How many papers had you done before that?

WHALING: Well, my thesis was my first publication [Ward Whaling and T. W. Bonner, "Disintegration of Lithium-6 by Deuterons," *Phys. Rev.* 79:2, pp. 258-61 (1950)]. And I think there was a letter or two in the *Physical Review*.

ERWIN: Who was Li's advisor? Fowler?

WHALING: Yes.

ERWIN: I was wondering if Li turned up on other papers with Fowler. The name sounds familiar to me. Did he stay at Caltech?

WHALING: No, he went back to mainland China. It was about that time that Caltech was having problems with the guy in the wind tunnel—[Hsue-shen] Tsien, who [was deported].

ERWIN: This is starting to be a part of the McCarthy era—essentially the Red Scare era, around 1950. And I don't know the date precisely for Tsien, but he was detained for a while and finally then was sent back. [Tsien was deported in 1955.—ed.]

WHALING: We had another postdoc from mainland China, named C. Y. Chao, who had worked in Kellogg. I think he had worked in Kellogg before I came. He had acquired parts to build an accelerator, a small one, when he returned to China. The insulators and vacuum tube were stored on the third floor of Kellogg for years, because he was not allowed to take them to China, so we just kept them in crates up there. Eventually I think they did get back to him, but whether he was ever able to build an accelerator I don't know. He was an older man, whereas Li was just a student.

Li went back to mainland China and became a prominent director of an institute in an inland province. He has since visited us once. He's considered a senior person there—not in the government but in their science establishment. He's supported by the government, and he works on reactors and things like that.

We had a competitor in this research. At the same time that we were building a magnet spectrometer, so was Bill Buechner at MIT, and with the same ideas in mind. There were some differences: he used photographic plates as detectors, as the mass spectrometer folk used to do; we always used electronic counters instead. We used a fixed radius of curvature and varied the magnetic field. And MIT used a constant magnetic field and measured the radius very accurately.

But we were back and forth, measuring the same quantities. And we really got pretty good at it. These were techniques that had not been used before, and we were very cautious in assigning uncertainties to our measurements. Initially our uncertainties were—well, I can remember Willy, after we gained more experience, going back and saying, “Look, look, that measurement is better than we said it was.” And he would be willing to reduce the uncertainties on an experiment long after we had done it. We would calculate the uncertainties as best we could, but when we began to be able to repeat our measurements with very good reproducibility, he gained a lot of confidence. So we were always competing with MIT in trying to get smaller uncertainties. We would often measure the same quantity. It was interesting to see Willy and Buechner battling it out; it was a very friendly and useful competition. Later I published a long

paper with one of Buechner's team members [D. M. Van Patter and Ward Whaling, "Nuclear Disintegration Energies," *Rev. Mod. Phys.* 26:4, pp. 402-43 (1954); and "Nuclear Disintegration Energies II," *Rev. Mod. Phys.* 29:4, pp. 757-66 (1957)—ed.]. I never said anything about it, but I was slightly amused to see Willy occasionally reduce the uncertainty on an old Kellogg measurement when MIT would later publish a value in excellent agreement with ours.

ERWIN: Now, Tommy Lauritsen was at some time publishing reaction data and working with—what I understand by that—working on certain reactions or keeping the data to a point where he felt that it was accurate. And then he would publish it, or he would record it or share it? How did that whole process work?

WHALING: Tommy's main contribution was to collect data from many different laboratories and make a critical synthesis, or analysis. When they differed, he tried to figure out which one was best or how to average the two—perhaps using different experimental methods in measuring the same quantity—[deciding] what weight you give to two different methods. Periodically he would publish his results.

ERWIN: I see. So in a way, it becomes a critique, in effect.

WHALING: Yes. It was very, very useful. Everyone used his work. And later Fay Ajzenberg [professor of physics, University of Pennsylvania] collaborated with him, and then she continued it after he died [1973]. Initially, as he started, it was energy levels of light nuclei. That was the name of the volume, and it was published periodically. As sufficient new data became available, he would publish a revised edition—not annually, but every two or three years, something of that sort. And it was largely in the form of large graphical displays of the energy levels of nuclei, with energy plotted upward. Greatly enlarged versions of these level diagrams were always posted along the wall in the hall on the second floor of Kellogg. He'd start off with a brand new set, and then penciled lines would be added in, when somebody added a new measurement. And occasionally something would be erased or crossed out, as other people had tried to repeat an experiment and couldn't. [Tape ends]

**Begin Tape 2, Side 1**

WHALING: The wall diagrams were kept up-to-date and eventually, as they got more and more pencil marks, why then a new edition would come out and start off fresh again. That was a very valuable and useful work that he initiated and carried on throughout his lifetime.

ERWIN: And what happened to it after he passed away?

WHALING: Well, he was joined maybe in the last two or three editions by Fay Ajzenberg-Selove, a nuclear physicist who's at Pennsylvania. She visited Pasadena many times to work with him. Whether they were very long visits—I mean, two, three weeks, as they were working on a new edition—I don't remember. Yearlong visits I don't recall. She and Marge Lauritsen Leighton are still very good friends. Marge, I think, is off on a trip with the Seloves right now in Europe; Fay and her husband [Dr. Walter Selove] contributed to Caltech an undergraduate physics prize called the Margie Lauritsen Leighton Prize.

ERWIN: And did you say that afterward this work was taken over by the government?

WHALING: Tommy started working with the light elements, from atomic numbers 1 to 10—up to neon—because those were the only nuclei that we could study with the low energies that we had available here. With a low energy beam of particles, you can't get them in close to the nucleus if the nucleus carries a high positive charge, so we couldn't study heavy nuclei with large atomic number. But in more recent years, with higher-energy accelerators, one can make studies of much heavier nuclei, much farther along in the periodic table than neon. Collecting and analyzing the many measurements on nuclei throughout the entire periodic table has outgrown the capabilities of one or two people. It's carried on by the government and mostly centered at Oak Ridge. And there are teams of people doing it.

As I started to say, at that time MIT was also working on magnetic analysis of nuclear reactions and we were in a friendly competition. And as my two-year fellowship was going to expire, I visited Bill Buechner at MIT with the idea of perhaps joining that group. That would be an obvious place for me to go to work. But along about 1951 came the Korean War. And at that time, a great number of the Kellogg physics faculty left to work on a project [known as] Vista.

Charlie, all along, had very close connections with the Defense Department—with the ONR [Office of Naval Research] particularly.

ERWIN: When you say all along, do you mean since the beginning of World War II?

WHALING: Yes or at least “all along” means ever since I arrived on the scene and maybe before.

ERWIN: Well, I was thinking of the [Caltech] rocket project.

WHALING: Yes, the rocket project was during the war. When I arrived at Kellogg, the ground floor was still full of lathes that had been used to machine rocket casings during World War II. Tommy and Charlie worked on rockets, and Willy, too, but this was before I came to Caltech. Charlie’s connections with the navy went back a long way, I think. He consulted with them, I believe, on various defense problems. So he traveled a lot to Washington, and he traveled around to various defense establishments, and knew what they were thinking about, what was of interest, what their concerns were. And I think it was probably through him—not single-handedly, by any means, but he would certainly have been involved in the establishment of the Vista Project. And he left to work there full time, and did not come to Kellogg. Likewise Willy Fowler and eventually Tommy—and many others in the physics division, although some of them may have come back during the school year.

ERWIN: So that was an intensive thing.

WHALING: Yes. It was a full-time job for some of them.

ERWIN: Where did they go? I know Project Vista was housed locally. It was in one of the old hotels.

WHALING: Yes. The Vista del Arroyo, the building you see if you're driving toward Pasadena, just as you go across the Colorado Street Bridge; it's a circuit court now.

ERWIN: I see. So they were actually physically there.

WHALING: And they didn't come to the campus to teach courses. But the postdocs were not involved; it was all very hush-hush. We didn't even know what they were doing exactly.

ERWIN: Yes, well, it was classified.

WHALING: Yes. So they just sort of disappeared.

ERWIN: You were going to say you were left to pick up the pieces?

WHALING: Yes. I don't remember whether they left during the middle of the school year, but I remember I got drafted to teach Willy Fowler's graduate course in nuclear physics. Charlie never taught anything, at least since I was at Caltech; it's a funny sort of professorship. He must have taught before I came.

ERWIN: So you presumably had a formal appointment at that time?

WHALING: Yes, or I got one shortly thereafter. [Whaling was appointed assistant professor in 1952—ed.] Vista del Arroyo started in '51.

ERWIN: Did it get the name Vista from the Vista del Arroyo?

WHALING: Well, that's what I always assumed. As I say, it was all secret. Nobody told us anything, just "He's working on Project Vista."

ERWIN: But you knew at some point what it was about?

WHALING: I still don't know exactly what it was about.

ERWIN: Well, I think it was about long-range missiles; the word "strategic" comes into the picture, but that's all I know.

WHALING: The trouble is, I tend to get that military problem sort of mixed up in my mind with the ones that were going on in Kellogg during the war, when they were building rockets in Kellogg. I never got a clear picture of Vista, and the other one I only got a picture of after the fact, and in my head they tend to get mixed up. For example, Bob [Robert B.] King [professor of physics 1948-1968] I know worked on the World War II part—he did rocket work—but not the Vista Project.

ERWIN: Right, he was the fuse man.

WHALING: Exactly. So I sometimes get them mixed up. The one I want to talk about at the moment is the one that was going on in '51 and took Willy away from his teaching. I was assigned the responsibility of picking up his course, and I think I did it for two years—maybe picked up a fraction of a year and then did it a full year while he was away.

ERWIN: Now, was that your first full-time formal teaching assignment? It sounds like a pretty tall order.

WHALING: Yes, but it was a small group, and I was teaching the subject that I had worked on. Nuclear physics I felt was the only thing I could have taught. I had not taught anything as a research fellow, but then Willy disappeared and I began to teach. And then they decided, “Well, let’s make him an assistant professor—he’s teaching a course.” [Laughter]

ERWIN: At this time, [Robert] Millikan was still alive but in his decline. [Lee] DuBridgE came in [as Caltech's president] in 1946. I’d like to talk a little about this at some point. He recruited [Robert] Bacher [chairman of the Division of Physics, Mathematics, and Astronomy 1948-1962] to the campus.

WHALING: Bacher came in '48.

ERWIN: So it was Bacher who was in charge of the physics program?



WHALING: Yes. He was division chairman and when I would get a promotion or anything, he would call me in to see him—the announcement came from him. We became good friends over the years, and I still see him from time to time.

The reason I did not go to MIT was that it looked like I had a sort of permanent job here—or at least as an assistant professor. I had no tenure, but at least it was a little bit more permanent and there was a possibility that it might continue. When you look back on things, well, a lot of this was luck. I mean, if the Korean War had not come along, I probably would not have stayed here. And had the Second World War not come along, I might well not have been in nuclear physics; I might have gone into philosophy or mathematics or something else. This doesn't seem like a very self-directed and carefully planned career. It was sort of going along and reacting to things around you, to a great extent.

ERWIN: Well, I think one comes to understand that luck—if you want to call it luck—does often play a role.

WHALING: Things that go on outside may have a large effect on directing which way you go.  
[Tape ends]

**WARD WHALING****SESSION 2****April 27, 1999****Begin Tape 3, Side 1**

ERWIN: When we stopped in our last session, you noted that you would like to begin the next session by talking about some of the postdocs and grads that you met here in the postwar years in the Kellogg laboratory when you first came.

WHALING: I would be happy to do that. I mentioned that my first research activity was with a graduate student who was here at the time, C. W. Li. When that work was completed, I did a problem with Bill [William A.] Wenzel, who was also a graduate student. Bill had been building a very small generator—a new electrostatic generator, a new Van de Graaff machine—to study nuclear reactions at very low energies. The idea was to have low energy but high currents, so that we would be able to see very weak reactions. I think already Willy [Fowler] was beginning to take quite an interest in the processes that go on in stars. And of course their reactions take place at very low energy, like 30 kilovolts, where we can't see the reaction going on in the laboratory: it's too weak; they're too rare; the reaction probability is too small. But his idea was to push the energies down as low as we could go. So a new Van de Graaff was built.

ERWIN: So this was Willy Fowler's idea, to take this line of investigation?

WHALING: Yes, it was—and I'm sure Charlie [Lauritsen], too, was [interested]. But that was the main motive, and Bill Wenzel was given the job of putting this machine together. The mechanical design was done by our engineer, Victor Ehrgott. And I was then asked to join Bill Wenzel and help him get it to run. And I did. Associated with this 0.6 MeV accelerator was a small magnetic spectrometer, similar to the large 16-inch spectrometer that I had worked on in 1950, and I was assigned responsibility for that instrument. But when the new accelerator was actually operating, the first thing we worked on was not one of Willy's interests but one that had come up in connection with hydrogen weapons. In 1950-1951 there was a great interest in the reaction cross section of deuterium and tritium and how these hydrogen isotopes might be used

in a hydrogen weapon or perhaps in a reactor for producing thermonuclear energy in a laboratory. Discussions with the people in the AEC all took place at a higher level, but we were told to see how low in energy one could measure the reaction of deuterium colliding with deuterium. As we thought about how we could do that, it seemed that the most feasible method was to use a solid target of deuterium ice and bombard it with a deuterium beam. But to get quantitative results for the cross section of the reaction as a function of energy, one needed to know how the deuterons slowed down as they went through the ice.

So the first problem we worked on was to measure the so-called stopping cross section of deuterons in solid  $D_2O$ —solid heavy water. Bill and I made such measurements and published a paper on it in 1951 or '52, in that period [W. A. Wenzel and Ward Whaling, "The Stopping Cross Section of  $D_2O$  Ice." *Phys. Rev.* 87:3, pp. 499-503 (1952)]. And then, with that information in hand, we proceeded to work on the cross section for the reaction of two deuterons to produce helium-3 and a neutron, or tritium and a proton.

We were able to push this measurement down to about 50,000 electron volts, and our result stirred some interest. [Edward] Teller came to see us, I remember, to see how we were getting along and see what our results were.

ERWIN: What was Teller's position at that time? Was he on the Atomic Energy Commission?

WHALING: He was one of the senior scientists in the whole weapons project, and I don't know exactly what his title was. I don't think Livermore was in existence at that time; I think he was still at Los Alamos then. But he was one of those very much involved in the idea of a super nuclear bomb.

ERWIN: Would this be what was popularly called the hydrogen bomb?

WHALING: Yes.

ERWIN: So this was the follow-on to the Manhattan Project?

WHALING: Yes, that continued on, and initially it was at Los Alamos and later moved to Livermore, to a special lab. And I guess Teller is still pursuing that, though in different

approaches now. But I remember his visit, and I thought that was a very impressive interview for a very young, junior scientist.

ERWIN: And how did you find him?

WHALING: Well, I was surprised to see that he was quite lame. He walked with a cane. He had a very thick accent. And he clearly knew what we were doing and asked all the sensible questions. He didn't say very much about what he was doing, but I wasn't surprised at that.

ERWIN: Did he have contacts with Lauritsen and Fowler? How were their relations?

WHALING: Well, I think they were OK. It was later, when the [J. Robert] Oppenheimer business came up, that relations went sour.

ERWIN: But that would have been fairly soon after.

WHALING: Very soon after. I remember that in '55, when I was to be married, Teller was invited to our wedding. So were Charlie and Willy. And I told the photographer, Tom Harvey, who was a Caltech employee, that if you see two guys taking their coats off and getting ready to have a fight, well, be sure and get a picture, because you can sell it. [Laughter]

So I worked with Bill Wenzel, who then got his degree and went off to Berkeley to work in high energy physics. Of the postdocs who were here, one that I particularly enjoyed getting to know—we shared an office for a while—was Tony [A. P.] French. He was from England—Cambridge University. When he left Caltech he went to MIT and has been on the faculty there ever since. He was a little older than I was and far more sophisticated, and I learned a lot from him and enjoyed getting to know him very much.

ERWIN: Scientifically sophisticated or socially sophisticated?

WHALING: Well, perhaps both. But I was thinking in terms of his genteel British manner. I remember one thing he told me that impressed me. I don't know how the subject came up, but one day we were talking in the office and he discovered somehow that I was named for my

grandfather, who was a bishop in the Methodist Church; his name was Seth Ward. And Tony said, “Well, you know there’s a Bishop Seth Ward buried in Salisbury Cathedral. I wonder if they’re related.” I was impressed. It turns out that later on I had an opportunity to visit the cathedral, and I asked the—I can’t remember the title they have for sort of the custodian in a cathedral. Anyway, I said, “I understand there’s a Bishop Seth Ward buried here. Can you point out where his grave is?” And he said, “Oh, yes. He was a very prominent bishop here.” And he took me around and showed me a great high structure. And on top of it there were some geometrical figures—there was a globe and maybe a straightedge and a compass, and various mathematical tools. The bishop was apparently a scientist as well, and perhaps that’s how Tony happened to be aware of him. But the bishop went off his rocker before he died, and they had to sort of ease him out of his chair. I guess he was better known in British scientific history. So Tony recognized the name.

Tony has not visited us in a long long time, but I continue to follow his work, and he’s been very active in teaching. He has written some very popular textbooks that I’ve used.

Some of the people who were postdocs at the time have later gone on to become members of the faculty—Charlie [Charles A.] Barnes [professor of physics, emeritus], for example. And, of course, there’s been a great long string of graduate students too numerous to remember, actually, over the long period of time.

ERWIN: I have here the *Festschrift* written for Willy Fowler’s seventieth birthday. And you were one of the authors of one of the contributions there. I wondered if all of those people had worked in Kellogg around the same time that you did? Or over various time periods?

WHALING: Oh, well, over quite a period of time. Many of the authors in there would have been later than that.

ERWIN: Yes, but you probably knew them all in some way.

WHALING: I probably did know nearly all of them sooner or later. Yes, Kellogg has had a great stream of visitors over the years. In its early days, there were many from Copenhagen, from [Niels] Bohr’s laboratory. And they were particularly delightful. Torben Huus was one that I

remember. He was here for several years and went back to Copenhagen. I think he never married, but he was a very active bachelor while he was here, as I recall—a very handsome and charming fellow.

ERWIN: Now this Copenhagen connection came about through Charlie Lauritsen, who was Danish.

WHALING: That's right. And Tommy had spent time in Copenhagen on several occasions. He spent a year there and spent some time working in that laboratory. So has Charlie Barnes spent time there, and others have gone there as visitors for longer or shorter periods. So we've had quite a stream of visitors. Of course, Niels Bohr visited Caltech on many occasions. And his son, Aage Bohr, has spent time here. So there've been close connections over the years.

ERWIN: It's interesting to speculate on how those connections were shaped and how they worked out. When the Lauritsens passed on, did it change? Or did anyone else put his particular stamp on it? Did Willy Fowler attract a certain number of people? How did that work?

WHALING: Willy attracted many visitors. But in the last twenty or thirty years, they were primarily theoretical students, whereas the ones from Copenhagen were primarily experimental students and worked in the laboratory, where I would work closely with them.

ERWIN: That was your area?

WHALING: Yes, my work was all experimental work. Whereas Willy began to develop his theories of the cosmogenesis of nuclear materials, how the elements were built up. He had many visitors who would come and work with him on theoretical calculations.

One of the early visitors, of course, was Fred Hoyle. He did not do experimental work, but he did give a series of lectures while he was here on his visit. And they were given in Robinson [Henry M. Robinson Laboratory of Astrophysics]. I think Hoyle would consider himself primarily an astronomer, though he's written papers on all manner of things. But my recollection is that he gave one lecture a week. He was going to explain how the elements were formed, but he had a different beginning. At that time, he was a great advocate of the continuous

creation theory. That is, a proton or an atom of hydrogen was created every cubic meter in space every  $n$  years—I don't remember the rate. There was no Big Bang at all; there was no beginning and things just went on forever.

That idea initially had many followers. It would explain how the universe that we see is continually expanding. There's something appealing about something that's continuous rather than a Big Bang, which immediately raises the question of what happened before. With continuous creation one avoids that question. But Hoyle needed to build up the elements: starting with hydrogen, how do you build up the heavier elements? He did not have much background in nuclear physics, so he would speculate about how things might be built up and what sort of cosmic or astronomical environment it would take to allow such processes to go on. And it would seem to me that he'd go along for an hour, and then somebody—maybe Jesse Greenstein [DuBridge Professor of Astrophysics, emeritus], who was sort of skeptical—would say something like, “Well, that process you just described doesn't happen in stars,” or “That can't happen.” And Hoyle would stop and say, “Well, let's stop here for today and we'll continue next time.” And then by next time, a week later, he would come back and it seemed to me that he had backed up a step and had gone around the obstacle and would proceed along for the next hour, until—and certainly it must have happened much more than once, because I got the impression it always happened—that is, Greenstein would stop him at some point and then Hoyle would say, “Well, let's continue next time.” He gave the impression of making it up as he went along, and perhaps he was, I don't know, because this was all quite new. And he was working in a laboratory where he could go and ask people like Tommy or Willy or any of us, “What happens? How much energy does it take to combine a proton and lithium-7?” or “Under what conditions will this happen? Is it a process of a large cross section or small?” He had his office in Kellogg, where he could ask people, or go look at the charts that Tom Lauritsen had on the wall, showing all the reaction processes. I think he was sort of making this stuff up as he went—I mean he was developing his theory just from week to week, and then he would talk about it. And the astronomers would criticize it from their point of view. It was quite exciting to see.

ERWIN: How did he come here in the first place? Who invited him?

WHALING: Well, you know, Willy would spend time at Cambridge. I remember Ardy Fowler [Ardiane Fowler, Willy Fowler's wife] once told me that they thought of moving there. He would spend summers at Cambridge, and I'm sure it was on one of those occasions that he invited Hoyle to come.

ERWIN: So then the main connection was with Willy Fowler?

WHALING: Yes.

ERWIN: I think that emerges from Hoyle's contribution to the Fowler *Festschrift*. I'm not trying to undercut anything you would say here, but he says that he came to the United States in the fall of '44 and he had some job connected with the war at that time—radar. And that's when he first came to Caltech and met Willy, that early.

WHALING: It could well be. I wasn't here at the time and did not know of this. But I would imagine Willy renewed their—and they worked together, I'm sure, when Willy was in Cambridge.

ERWIN: Yes, they did.

WHALING: And he would go there frequently. I frankly don't know what Hoyle was working on in 1944. Well, you mentioned he was doing some kind of war work, but what else he was doing I do not know.

ERWIN: So, really, they met each other because of the war, probably. And then continued somehow.

WHALING: Very likely. It seemed to me that Hoyle developed a lot of his ideas of what he would call nucleogenesis—how the elements were built up—sort of from week to week, as he was staying here and living in the midst of a bunch of nuclear physicists who were combining these elements in the laboratory.



One thing that I was involved in: Hoyle, starting with hydrogen and then combining things, could build up the elements as far as beryllium-8. He could make lithium and beryllium and understand how these might combine. But when he tried to do it quantitatively, he got into trouble. Once he had formed deuterium he could build up helium, and then when two heliums combine, they would make beryllium-8. But beryllium-8 is unstable and would decay right back to two helium atoms. Helium is very stable, and it was easy to build up a high concentration of helium. And then he got the idea that maybe three heliums could combine to make carbon-12, which is stable. But in order for this three-helium reaction to have any probability of forming a stable carbon-12, it is necessary that carbon have an excited level at about 7.65 MeV above the ground state. If you can combine three alpha particles, they make carbon-12, but they have more mass than carbon-12 does, and they would form carbon-12 in an excited state. But if there is such a state, the probability of such a combination would be sufficient to account for the production of carbon. And from there on, you can add protons and make nitrogen and oxygen and so on. You can go a long way up the periodic table if only you can get past the barrier at beryllium-8.

ERWIN: So this creation of carbon was a sort of key to the whole problem?

WHALING: Yes, it has been very important. Charlie Barnes has worked on carbon-12 and on captured alpha particles practically his whole career at Caltech.

So Hoyle said, “Well, maybe we could not have two particles combine”—which is the usual process. But a three-body collision is very rare. I mean, the two things aren’t going to stay together very long, and in the very short fraction of a microsecond that they’re together another one has to come along, just at the right time. So it’s very unlikely. But it can be made more likely if there is an excited level of carbon-12 at just the proper energy.

So we looked at Tommy’s level diagrams, and you could see that at one point somebody had penciled in a level there, but then other people had tried to see it, and then Tommy had erased it; it seemed not to exist. And its energy wasn’t exactly where it needed to be, anyway, for Hoyle’s purposes. It was close by—like 7.4, or something like that, instead of 7.6. But the idea immediately occurred: “Well, let’s look and see if we can see such a state in carbon-12.” I don’t recall now whether other people had tried other methods, but we decided to look at it by

bombarding nitrogen-14 with deuterons and looking at the alpha particle. The reaction goes to carbon-12 plus an alpha particle. And by looking at the energy of the alpha particles, we should find high-energy alpha particles that leave carbon-12 in its ground state. And groups of alphas of lower energy, because some of the energy was left in the carbon-12 residual nucleus. So we decided we would try that.

ERWIN: Now, who is “we” at this point?

WHALING: Bill Wenzel—I think he had graduated, but he was still in Kellogg as a postdoc; and a visitor from Australia named Noel Dunbar was a postdoc here; and one of my graduate students, named Ralph Pixley, and I. We decided we would try to do this. An interesting experimental detail that I recall was that the big magnet that I mentioned C. W. Li having built was in one room in the Kellogg Laboratory, and the small accelerator that Bill Wenzel had built, which had a high current which we thought we would need to do this experiment, was in another. The magnet weighed several tons, and there are little narrow halls. Kellogg was not built, you know, for moving big machinery. So we had to move this great thing down the hall. And our engineer, Vic Ehr Gott, said, “Well, let’s get a strong steel plate and put the plate on hundreds of tennis balls on the floor. We’ll support the plate on tennis balls, so when you push the plate, the tennis balls will roll, and we’ll put the magnet on top.” And there were enough balls so that each one wasn’t compressed very much. And sure enough, we moved that darn thing down the hall, rolling it.

ERWIN: You said several tons?

WHALING: Yes, yes. And the graduate students in the back would throw the balls up to the front, and we’d put them up under the front. And so we rolled it about 25 yards—100 feet, something like that.

Anyway, we got the magnet moved and set up. And sure enough, when we looked, we found a level where Hoyle said he needed one. And this was sort of interesting. It got some attention at the time, because this was the first time that anybody had ever predicted in advance where a nuclear level should be. Nuclear theory had not then, and has not now, progressed so far

that one can predict the position of levels. And this is not on the basis of understanding how the nucleus is held together; Hoyle had said that in order for carbon-12 to be formed in the universe, you had to have a level there. So it was a sort of indirect prediction, but it was a prediction.

ERWIN: Did Hoyle's stock go up, then, with the likes of Jesse Greenstein and others?

WHALING: I think it did. And certainly that has continued, though the continuous creation theory does not have many adherents today. But the idea is that after the Big Bang you started off with all of these elementary particles—which gradually formed hydrogen—so you still had to build up the elements in the same way Hoyle did; Hoyle just didn't require a Bang to start it off. But it is still presumed that the carbon in the universe was formed by the combination of three alpha particles in the way Hoyle proposed. Hoyle took quite an interest in the work, but by the time we got ready and felt sufficiently confident to publish it, he'd gone back to Cambridge. We wrote to him and said, "We're going to publish a paper and we'd like to put your name on it, too." Give ourselves some association, rub elbows with some famous people. And he acceded, since the experiment—he didn't get in and turn and twist the knobs and read the counters, but it was his idea, his concept, that led us to do it in the first place. [F. Hoyle, D. N. F. Dunbar, W. A. Wenzel, and W. Whaling, "A State in Carbon-12 Predicted from Astrophysical Evidence," *Phys. Rev.* 92:4, p. 1095a (1953).] Well, that was one of the fun, early exercises that we worked on.

Backing up just a moment, I might mention that after we did this work on deuterium ice, we measured the stopping cross section. What that means is, when a charged particle goes into any material.... Normally they're moving in a vacuum and do not lose any energy, and that's why we had all these vacuum pipes around. But when they go through any matter, they lose energy and slow down and stop. So after we had just devised a way of measuring the energy loss—using an incident beam of very accurately known energy and measuring the energy of the scattered particles from the target very accurately by magnetic analysis, bending them in a magnetic field—we decided that this was a sufficiently novel method and that it yielded important information of value to other experimenters. So we did quite a series of measurements on the energy loss of charged particles in matter. We measured energy loss in gases, and I had a couple of graduate students working in that area, and then we did others in solids. We published several papers. And about this time, the *Handbuch der Physik*—it's a big German reference

volume, sort of like our critical tables; it has been for many, many years the standard reference on all aspects of physics—they were going to revise the volume on nuclear physics. And I was invited to do the paper on the energy loss of charged particles. So that was probably my most distinguished publication, in the sense that it was published in an international handbook that you'll find in every library. [W. Whaling, "The Energy Loss of Charged Particles in Matter." *Handbuch der Physik XXXIV* (Berlin: Springer Verlag, 1958), pp.193-217.]

ERWIN: While you were doing this work in Kellogg, give me a sense of how much else was going on. In other words, would one major project kind of take everybody's interest? Or were there a lot of different things going on?

WHALING: Generally I would only be involved in one active project at a time, though we might begin to think about the next one and maybe do a little preparatory work. But there were a number of different people involved—I mean, Tommy Lauritsen and his students; Willy would have students working. Typically, Willy might have some kids measuring cross sections or reaction cross sections that were of interest to him. Tommy would be doing the same thing, though he tended to specialize in beta spectroscopy and in the fast, short-lived beta activities that are produced in nuclear reactions. He built a big spectrometer for that purpose, and he would have students working on that. Later on, [professor of physics Ralph] Kavanagh would have students, though he was working primarily with neutron physics. Charlie Lauritsen did not have students. So there would be different projects going on under the supervision of the four or five active faculty members, and some maybe with two going at once. But generally we had to schedule time on the accelerators, and four or five projects going on at one time is enough.

ERWIN: Did it seem that you were working together? Did you feel as if you were a team, in a sense?

WHALING: Well, that's a good question. We were working together in the sense that we were all working, generally speaking, toward the same end—of measuring what we can do in the laboratory that is similar to what goes on in stars. For example, some of the work I did on measuring stopping cross sections was what Willy needed in order to interpret his results. That

is, he needed to know how the protons stop, slow down in a boron target, so that he could take his yield measurements and get a cross section out of it. So I was, perhaps, supplying data that he would use. Though I was not working on his experiment, we were working together in that sense.

ERWIN: Well, if it was stars, then had there been a perceptible transition from nuclear physics to nuclear astrophysics? And if so, how did that come about?

WHALING: It came gradually. But let me say, the work was all related. I mentioned that others would use the data I'd measured on how particles move through matter. But we also measured nuclear masses, for example, and others would use our nuclear masses in their calculations. I mean, we were all working together in that sense. But there was not a competition. I've thought about this sometimes. You hear about young people in the laboratory who are competing, with the idea that, well, whoever wins the competition will stay and the others will leave. That did not happen. Charlie Barnes was working—and we both came at almost the same time and we were both assistant professors, hoping to have tenure—he worked on, I don't quite remember, probably carbon-12 alpha gamma. That's an important reaction, and it's such a hard one that they're still struggling with it. And I was working on the energy loss of charged particles in matter. We were not competing to both try to measure the same thing. But the work that I did, he could use. And I was tickled when he or others found my results useful, of course.

So there was cooperation in one sense: that the general overall purpose of the lab was to see what we could learn about what goes on in stars. I think that was always Willy's primary interest, and he was trying to correlate all the results that came together to see how you can understand what goes on in a supernova and things of that sort. But as I am measuring a mass, or measuring the range of alpha particles in aluminum, I'm not thinking about stars. I don't know whether that makes any sense or not. But it all goes together, it all fit together, in the end. The project I mentioned on the excited level of carbon-12 was immediately related to a stellar process, but I think that was probably the only one that directly and immediately—well, that's not true, there were later ones, but at least early on that was the only one that had an immediate application to stellar situations.

ERWIN: I think people outside of science often wonder if those on the inside, those working on particular projects, have a certain goal or a certain outcome in mind. Did you at that time?

WHALING: No, I didn't. I must say that I get buried in the immediate problem—on “How do we clean this surface so there's no carbon on it?” I mean, the immediate details, you get so buried in them that that's what you're thinking about. Now, the choice of the problem that you're working on originally may have been connected, but the thing you do day by day is “This damn vacuum system has a leak in it.” You know, you don't see the relation to the end product until, once in a while, somebody asks you to give a talk about it or you hear a visitor come and talk about it.

ERWIN: But meanwhile, big stuff is coming out of it.

WHALING: Yes, that's right.

### **Begin Tape 3, Side 2**

ERWIN: Tell me a little bit more about your family. You got married in 1955.

WHALING: Yes. I came here as a bachelor. There were a lot of attractive young women in the library. One of them became Mrs. Fred Anson [Gilloon Professor of Chemistry], I remember. And there were others. Some are still around. Mrs. Ed [Edward E.] Zukoski [professor of jet propulsion and mechanical engineering 1966-1996] was working as a secretary in electrical engineering.

ERWIN: Was there competition among the guys? [Laughter] Or did it go kind of smoothly?

WHALING: [Laughter] I don't recall that there was great competition. But there was a great esprit, camaraderie. I can remember going out with Ed Zukoski. He was dating the daughter of the president of Scripps College. And a very good friend of Mary Lou Slichter, my wife, who when I met her was working for James Bonner [professor of biology 1938-1981] in the basement of Kerckhoff [William G. Kerckhoff Laboratories of the Biological Sciences]. We were

introduced and became better acquainted. She had graduated from Radcliffe a year or two before, and she taught school for a while but decided to come out to the West Coast. Her parents were living across town, where her father was on the faculty at UCLA, but she lived here in Pasadena.

ERWIN: They were originally East Coasters?

WHALING: Yes. He had been on the faculty of MIT until the war. Then he left for various war projects and actually was here in Pasadena for a year or two. He and his family lived in the [Richard C.] Tolman house, here on campus, and Mary Lou went to Poly. He knew the Tolmans and many others on the Caltech faculty well. But that was before I appeared on the scene. After the war, in 1954, Mary Lou came to work here for James Bonner, and I met her at what was the Greasy Spoon in those days. It was the old dorm.

ERWIN: That was the student cafeteria?

WHALING: It was, right, but there were rooms upstairs. It was the only thing we had in those days, except the Athenaeum; it was where you went for coffee. Or some of us went for breakfast at ten o'clock in the morning, since we tended to work very late at night.

After a reasonable, proper period of time, we were married at her home on the other side of town. Actually, it was Louis Slichter, her father, who was a friend of Teller's, and that's how Teller happened to be invited to our wedding.

ERWIN: Now, Slichter is a German name.

WHALING: Yes, but Louis Slichter's father was a mathematician at the University of Wisconsin, and Louis was actually born in Madison. I think her great grandfather may have come from Switzerland, but there was no recent connection with the Old World.

We were married in 1955. At that time, I was an assistant professor and I did not have tenure. But I thought, Well, times are pretty good, we can afford to get married. So we did. When I told Charlie Lauritsen that I was getting married he took his feet off his desk, sat up straight, and said, "Oh, you poor fellow!!" But he was only joking. He knew Louis Slichter

very well from work they had done together during World War II. We have two daughters, who came along in '57 and '59. Anne was born in '57 and Carol in '59. They grew up here in Pasadena and went to the public schools, and now they're both married; one lives in Berkeley and one lives in Nashville.

ERWIN: Did they have a scientific bent, either of them?

WHALING: Anne graduated from Berkeley and majored in history. Carol worked in biology and earned a PhD at UC Davis, studying the endocrine system of birds, but now, with a two-year-old daughter, she is not pursuing her biological activity. She may take it up again when her children are older, but it's not easy to do with a young child. But they, of course, have been a very important part of our life. We now have three grandchildren to play with.

ERWIN: And you get to see them enough.

WHALING: Yes. My wife is going to Nashville tomorrow for a short visit, and we had the two grandsons from Berkeley down just a week ago. So we see them frequently, and they are a great joy in your old age!

Getting back to our academic work, where were we?

ERWIN: Well, for the sake of color and interest, we could talk a little bit about the Kellogg social scene, because we hear from time to time different accounts, and so let's get yours.

WHALING: Well, it was quite interesting and I think rather unusual. At least I don't believe a similar situation existed on other parts of the campus even then, and certainly not even at Kellogg these days. I suppose it started with Charlie Lauritsen—the senior Lauritsen—but was later enthusiastically supported by Tommy [Lauritsen]. The first thing we'd do, we had a Friday evening seminar. It was a weekly get-together after dinner—seven o'clock, maybe. Someone in the group would speak about the work they were doing, or a visitor would speak. But it was just to the Kellogg students and faculty, fifteen or twenty in all. And occasionally Felix Boehm [Valentine Professor of Physics, emeritus] would come, if it was something of interest to him, or something of that sort. We would have a program in the basement of Kellogg Lab, where there



were the blackboards and a slide projector and Tommy's level diagrams covering the walls, and talk on work in progress or whatever. Then after that, we would go to someone's house. And it varied. I can't now recall just how the schedule was established. You'd think it would take some planning, but as often as not it would be at Tommy's house or Willy's house, or occasionally at our house, or Charlie Barnes. And we would all bring beer or soft drinks. I don't think there was ever much liquor beyond beer.

ERWIN: Did the wives come?

WHALING: Yes, they were all invited. Some of the younger ones I think objected to spending their Friday evenings in that way. Very few of them had children at that time. But if you did, you could park them in a back room somewhere; they generally were asleep—they were often very young. It was not for children, but if you had an infant, you had to bring it along. Tommy Lauritsen and Willy Fowler were essentially party animals; they just loved to raise hell. Another participant, very active, was Alvin Tollestrup, who was a graduate student here and stayed on afterward; he was helping with the synchrotron here. Joe [Robert V.] Langmuir [professor of electrical engineering 1952-1980] would come. He was on the synchrotron staff.

ERWIN: Is that Robert Langmuir?

WHALING: Robert, yes, but he was known as Joe. They were pretty lively and fairly noisy. I can remember occasions when the police would come and say, "The neighbors think you're making too much noise; please tone it down a little bit."

ERWIN: Was there music?

WHALING: Yes, there was, sort of. Tommy played the piano, and later Charlie Barnes played the piano, and they would sing Danish songs sometimes. [Laughter] I'm sure Charlie Lauritsen knew what the words meant, but most of the rest of us didn't. They were sort of dumb songs anyway—they were loud and noisy, and required stamping on the floor to keep time. Occasionally there were other musicians. We were always encouraged to bring an instrument if

we could play, or even if we thought we could play. Tommy was pretty good. And we would sometimes sing songs.

ERWIN: It's hard to imagine that this would happen today.

WHALING: It is unusual. Though the opposition came from wives, who felt that this was not the way they wanted to spend their Friday nights. Some of them enjoyed it, but many of them said, "Gee, my husband works during the week all night and stays at the lab late." And, of course, there was an awful lot of shop talk that went on, too.

ERWIN: Well, for the men, you were building your community, I think, in some way. It seems to me that this was a part of forging those relationships.

WHALING: It was.

ERWIN: And making you feel like a real group—giving you an identity.

WHALING: Yes.

ERWIN: Maybe your wives felt you had enough identity. You didn't need any more. [Laughter]

WHALING: Gradually, because of their objections, the things were moved to Friday afternoon. And we still have our weekly laboratory meeting on Friday afternoon. But parties are very rare now, unless there's maybe a visitor here that they want to entertain, or something of that sort.

ERWIN: Do you remember Sigrid Lauritsen [Mrs. C. C. Lauritsen]?

WHALING: Oh, yes.

ERWIN: Would she involve herself in these affairs?

WHALING: She was certainly there, yes. Charlie himself was not nearly as outgoing and sociable as Tommy and Willy. They were the main people stirring things up. Charlie would take part in some of this stuff, but he was older and had a little more natural dignity—certainly than Willy.

But those parties were quite unusual, and it was a little bit surprising—or shocking, to some visitors, perhaps—to see them carrying on.

ERWIN: Do you recall that Lee DuBridge ever attended these affairs?

WHALING: No, I don't think he ever did. Of the outsiders, or the senior folk, Jesse [Greenstein] would sometimes come.

ERWIN: Bacher?

WHALING: No, no. And Mrs. DuBridge would not have approved. It was pretty much just the...

ERWIN: So it sounds like it was a fairly circumscribed group of people.

WHALING: Yes, right.

ERWIN: Did Robert Christy [Institute Professor of Theoretical Physics, emeritus] participate in any of this?

WHALING: Yes, he did. And that's a little bit of a surprise, isn't it? You think of Bob Christy as having a certain amount of reserve, but he was there.

ERWIN: How much was he a part of that Kellogg group?

WHALING: Oh, very much. And I'm afraid I have overlooked it, but that was a mistake and I'm glad that you mentioned him. I was thinking primarily of the experimental people who were in the lab. But we would all go and talk to Christy when we needed advice on some theoretical

aspect of what we were doing. He was very, very helpful. He was the kind of theorist every laboratory needs. I mean, theorists like to work on fundamental things that are new and nobody understands very well. He would be willing to stop and explain stuff to us that we didn't understand. And if he needed to, he'd go look up stuff for us. He was a very, very helpful house theorist. I mean, he was almost like a tame theorist; he worked for us and made a very great contribution. And along about the middle of the afternoon, he would come around and look into the lab and walk around you with his hands behind his back, looking over what we were doing and ask questions and nod. He took quite an interest in what was going on, but he didn't tell us how to do things unless we went and asked him a question; then he would talk to us. He was a very valuable member of that operation, and I'm sorry that I have not mentioned his name more frequently, because he was a very important part of it.

ERWIN: Well, on that same line of thinking, how about some of the other theoretical physicists who were at Caltech at that time? Richard Feynman [Tolman Professor of Theoretical Physics; d. 1988] came in 1950, and Murray Gell-Mann [Millikan Professor of Physics, emeritus] around that time [1955]. They were two who later on became very highly regarded. Now, did they interact with you and your group at all?

WHALING: No, not really. Both of them at one time—first Feynman, and later Murray—were involved with theories of beta decay. Some of Charlie Barnes's experiments were immediately related to what they were doing, or could confirm some of their work. But I don't think I ever saw Dick in the lab, and certainly not Murray. And it wouldn't occur to me to go ask either of them about the sort of things we were doing. Christy was very approachable and right there down the hall and was very easy to deal with, whereas those guys—and especially Murray—were a little harder to deal with, to some extent.

ERWIN: Is there a great divide between the theory and the practice here, or is it just really a personality thing, individual preference?

WHALING: Well, I'm sure that, for example, in some of the low-temperature work—say, the experimental work of [James E.] Mercereau [professor of physics 1969-1987], his work with liquid helium was certainly closely identified with the work that Feynman did on liquid helium

and I'm sure they worked closely together. But to my knowledge, Feynman—and certainly not Murray—was never really involved with what we were doing in the lab about nuclear processes.

ERWIN: Of course, there were other types of physics going on.

WHALING: Sure. And they would be involved with the stuff in the synchrotron, for example, and in Feynman's case with the liquid helium. But those two theorists were not involved at all with what we were doing.

ERWIN: Do you have any Feynman anecdotes that you want to relate here?

WHALING: No, I don't think I know any new ones. I knew Murray slightly outside of the lab, in that his first wife had connections with some of the people that Louis Slichter knew. I have been at the Slichters' home in Pacific Palisades when they were entertaining Murray and Margaret. And I remember driving with Murray—and maybe Margaret—bird watching. That was one of his interests. And Mary Lou is a bird watcher. I remember meeting them once in Santa Barbara, and we drove up into the hills, logging birds. But I really didn't see Dick Feynman outside of the campus.

ERWIN: Well, maybe we could talk just a little bit about the physics division in general. Bacher had two titles, and I wanted to ask you what this signified. He was chairman of the Division of Physics, Mathematics, and Astronomy, but he was also director of the Norman Bridge Laboratory.

WHALING: Laboratory director really didn't mean a great deal.

ERWIN: Because Millikan had been that, of course.

WHALING: I don't know what that directorship would mean, frankly. That's a good question. Bacher was chairman of the division, with all of the duties that that entails, which were perhaps not as time-consuming then as they are now. Money was easier to come by then. And I think renewal of grants was easier and more certain then than it is now. I don't think he spent as much time worrying about finances as [Thomas A.] Tombrello [division chairman 1998- ] or [Charles

W.] Peck [division chairman 1993-1998], or some of the other recent division chairmen have. But he was also very much interested in the synchrotron laboratory. That was his primary interest. He may not have been actually the director of the synchrotron lab. But if he was a laboratory director, it was over there where his interests were. And, of course, he was most interested in attracting a first-class faculty.

ERWIN: Yes. [Bridge] was the original physics lab, of course; that was the name of Millikan's lab.

WHALING: Well, Millikan had a lab there, of course, and there were others in the building. A guy named Barnett on the third floor who did magnets, and Carl Anderson's cosmic ray stuff in the basement, and [Jesse] DuMond's [professor of physics 1938-1963] work, and [professor of physics, d. 1995] Bob [Robert B.] King's spectroscopy. But I don't think it was unified in any way. They were all financed separately—different contracts supported each group and so on. So that title—director of the Norman Bridge Laboratory—doesn't really mean anything to me.

ERWIN: But do you feel that it was a unified group? I mean, the division also included astronomy and math—this was a pretty far-flung empire here.

WHALING: Yes. Neither of those were located in the Norman Bridge Laboratory. When I first arrived on the scene, the division was physics, astronomy, math, and electrical engineering.

ERWIN: That's correct. Electrical engineering was a part of physics.

WHALING: And that really didn't make any sense. They were located up on the third floor of West Bridge, or some of them were. The guy who was doing analog computing—Gilbert McCann [professor of electrical engineering 1947-1966; professor of applied science 1966-1980]—was up there, on the third floor of Bridge. But the reorganization took place very shortly after I arrived, and I wasn't really involved or even aware of what was going on.

ERWIN: Do you think those were Bacher's decisions—how to reorganize the division at that time?

WHALING: I wasn't a part of it, so I don't know. I would imagine people like Carl Anderson had a lot of influence. During Bacher's tenure as division chairman [1948-1962], it seemed to me that many of the decisions were made by Bacher and Carl Anderson—and maybe Charlie Lauritsen—sort of getting together and deciding how it was going to be. I don't remember meetings of the division to vote on things. Now it's very democratic and all decisions are made in meetings with the whole division, but in those days those guys just sort of did it on their own, perhaps consulting or keeping DuBridge informed, but it was that triumvirate that sort of ran things, it seemed to me.

I knew Bacher pretty well and got to appreciate his contributions. He was often away for one thing or another. And he said to me, "Five percent of your salary is going to be charged to my office, and that means that on the average you're supposed to spend five percent of your time helping the division secretary when I'm away and she gets stuck with a problem of some sort."

ERWIN: Did you get a title to go with this, like assistant chairman?

WHALING: No, and I didn't get a raise.

ERWIN: They took a bite out of you.

WHALING: Yes, and he attached a number to it. So I did that. Mostly there'd be letters coming in, some question about how the division was run, or something like that. Or, what courses do you offer in this? Or how many faculty members do you have teaching? People write a lot of questions to division chairmen. Sometimes the secretary could answer them and sometimes she'd ask me to help her. Occasionally you'd get an interesting phone call. I remember once she called me in a dither because she had a call from somebody in the National Academy of Sciences saying that they had notified Feynman that he'd been elected and he'd turned it down. She said, "What do I do with this?" I said, "Well, we better tell Bacher real fast, so he can fix it." So we had to chase him down wherever he was. I told him "That's the sort of problem I don't deal with." [Laughter] But things of that sort come up. I spent most of my time for Bacher trying to help figure out how much, or what fraction, of certain common costs should be assigned to overhead.

So I got to know Bacher pretty well. And we've tried to keep in touch with him since he moved up to Santa Barbara. We see him once or twice a year, maybe. And I must say, he's looking quite well.

ERWIN: Did he leave a legacy, do you think, for physics? What kind of mark did he put on the division as an administrator? And, of course, after that, he became Caltech's first provost [1962-1970]. That bespeaks a special relationship with DuBridge, which I think he had all along, and maybe the creation of the provostship was a formalization of that relationship.

WHALING: I think that as the institute grew, DuBridge got involved to some extent with things going on in Washington, and he may have been away from the campus more than had been the case in the past. He wanted somebody on hand here to deal with the faculty day to day. I'm not really in a position to say much about his legacy. It just seemed to me that Bacher's way of running things was the way departments ran. I've never seen others run differently, so I'm not able to make any comparison. I think everyone felt he was fair, and he consulted widely with the faculty before he took any action. I don't think there was much of a feeling of arbitrary administrative fiat. Has he done an oral history?

ERWIN: He has done an oral history, yes.

WHALING: I don't remember any great serious problems he had. Things seemed to go very smoothly then. [Laughter] I've always attributed that partly to the times; physics was easier to fund in those days than it is now.

ERWIN: Well, maybe this is leading a little bit to the idea that physics had been preeminent at Caltech for so many years under Millikan and it continued to be that way for quite a while. But in some sense, by the 1970s, maybe, physics didn't seem to hold the place it had held formerly. [Marvin L.] Goldberger said when he became president [1978-1987] that he was going to rebuild physics at Caltech, which would indicate that he thought there had been a decline. Whether he did that or not I think is still open to debate. But anyway, please comment.



WHALING: Well, our eminence in theoretical physics, of course, has declined with the departure of Murray [Murray Gell-Mann became emeritus in 1993 and moved to the Santa Fe Institute—ed.] and the death of Feynman [1988]. And they are not going to be easily replaced. It's not only at Caltech where this is true. I think it's true that particle physics is not the exciting field—since the SSC [Superconducting Super Collider] was not built—that it was in the days when great discoveries in particle physics were very frequent. And I think it is true everywhere that biology is the thing you read about, more and more, in the newspapers. Biology is certainly assuming an importance on this campus, and on many others, that maybe physics occupied at one time but no longer does. It's going to be the study of living systems, I think, which will occupy the forefront in the foreseeable future, whereas once it was the study of matter. Well, the study of the universe is still a pretty big subject, so in that sense physics has moved from some areas to another. But nuclear physics, such as the sort that I worked on when I came here, is no longer as active a field as it was then.

ERWIN: These are natural progressions?

WHALING: Yes, I think so. I don't think they're a reflection on any one individual here, but this is just going on in the society around us and Caltech is a part of that society. LIGO [Laser Interferometer Gravitational-wave Observatory] may rejuvenate interest in one area of physics. But that's really going to be related to cosmology and astrophysics, probably.

ERWIN: There's definitely physics that goes into LIGO.

WHALING: Oh, indeed. We'll see how it works out.

**WARD WHALING****SESSION 3****May 4, 1999****Begin Tape 4, Side 1**

WHALING: One of the activities that I enjoyed very much and let me make the acquaintance of many people outside of physics were the musical shows that [J.] Kent Clark [professor of English 1947-1986] and Elliot Davis put together. Davis was an off-campus neighbor of Clark's; he was a self-taught pianist and he wrote music to Kent's lyrics and accompanied all the shows. And starting I think about 1955 was the AAAS [American Association for the Advancement of Science] show, which was actually staged at PCC [Pasadena City College], because we didn't have a large enough auditorium. I think I've been in all of them. There were two shows for DuBridge, and then Bacher, and then [Earnest] Watson [chairman of PMA Division 1946-1949; dean of the faculty 1945-1960]. And there was one for [George W.] Beadle [chairman of the Biology Division 1946-1961] and there was one for Arnold Beckman [Caltech Board of Trustees 1953-present].

I'm trying to remember how it got started. I'm not exactly sure, but in the AAAS show, Kent wanted a young physicist to do a take-off on Feynman. And my song was [singing], "It's been established that no physicist of forty, can get ideas, can get ideas. Oh, how they long to be back riding on their scooters, and now all they do is put numbers in computers." [Laughter] It was something like that. He wanted a young physicist, and perhaps I was the youngest one he could find in 1955.

ERWIN: And you could sing!

WHALING: Well, let's say, perhaps it's more accurate that I would come to rehearsals on time and I would memorize my lines, whereas some of the rest of the cast took this much more casually. So he kept asking me to come back.

ERWIN: Did your wife do this, too? I know some of the wives participated.

WHALING: Yes, it was not only faculty, by any means. There were wives, and there were secretaries. Virginia Kotkin [head secretary, Humanities and Social Sciences Division] played a prominent role in these. And Tom Harvey, who was the stockroom man in Bridge. And Mu [Muriel] Harvey, who had a nice British accent, was often called in when you needed a British accent for something or other. I can remember Muriel Beadle [Mrs. George W. Beadle] played a large role in one of the shows. She was very good. And Rudd Brown [Mrs. Harrison Brown; Harrison Brown was professor of geochemistry 1951-1977 and professor of science and government 1967-1977—ed.]. Oh, gosh, if you go back long enough, there were an awful lot of them. And when the cast works together, you know, there are a half a dozen rehearsals and they get to know each other. So I suppose there were a hundred people or so on the campus outside of physics that I got to know in that way, and I enjoyed that very much. That was one of the very positive aspects of Caltech. And I feel indebted to Kent and to Elliot Davis for the work they did. Sometime you should talk to Kent about all his shows.

ERWIN: Well, actually, we did do an oral history with him and he did give us the texts and tapes. So we have preserved that part of Caltech history.

WHALING: Those are fun.

ERWIN: Do you remember any of the other songs or texts?

WHALING: Well, we did a show for the Caltech Y that was repeated a number of times under various circumstances.

ERWIN: I'm trying to think of the title of that. Was it *A Broader Point of View*, or something like that?

WHALING: *A Broader View*. The idea was to take the Caltech students—who were very narrow and concentrated—take them to The Burbank, which was a striptease joint downtown, and educate them. And the kids were very innocent, you know. It was just typical foolishness. In that one, Muriel Harvey played the striptease artiste. We did that one on several occasions. But

most of the shows were very topical. I enjoyed those very much. There haven't been any recently. And Elliot Davis is no longer living. He did the music; Kent did the lyrics.

ERWIN: Was Kent also the director?

WHALING: Yes. Elliot was always at the piano. He was just a wonderful pianist. He couldn't read music very well, but he could improvise. And in some fun he could drag in a few bars of "Rock of Ages" when it was needed in some geology song. He could pull in little bits and snatches of other tunes. I was just amazed; I could sit and listen to him play for a long time. He was just great! But he wasn't sure about music, and he'd say, "Let's see, what key are we going to do this in?" And he could do it in any key.

ERWIN: He was just a natural born talent.

WHALING: Yes. I don't think he had ever studied music, but he was a natural.

The other thing that I wanted to mention is a dining club that started in 1964. And it came about because at that time the Athenaeum had a manager named Camille Jaguet. He was French, with quite an accent. He was very difficult to deal with, but he liked to do a little fancier cooking than the \$1.75 menu for Athenaeum dinners provided. But he was hampered because the Athenaeum had no liquor license then. So fancy dinners, like for the [Caltech] Associates, were not held there; they were always done downtown, at the California Club. Incidentally, that's another way I got to meet a variety of people outside physics. Somehow, Edythe Baker [President DuBridge's senior secretary] discovered that I owned a tuxedo. And when she had to supervise a banquet—place the seating cards and inform guests of their table number, and so on—she would say to me, "If you'll come down and help me to pass out the tickets, you'll get a free dinner out of it." So I did a lot of that at the California Club in the early days, before I was married.

The Athenaeum had no liquor license. So Harold Wayland [professor of engineering science, emeritus] and Fred Lindvall [professor of electrical and mechanical engineering 1931-1970] organized a dining club to which we could bring our own wine.

ERWIN: Was Wayland a biologist?

WHALING: No, Harold was a very old-time member of the Caltech community. In recent years, he has been working on blood flow, and that's why you were thinking that, but he got his PhD here under Millikan in physics. And then he taught AM [Applied Math] 95, that famous basic course in engineering mathematics. Before he got a job on the faculty, he worked for [Harry] Bateman [professor of mathematics, physics, and aeronautics 1917-1946] in mathematics here, and then he got an appointment on the faculty and has been here ever since. But he and Fred Lindvall organized what they called a little dining club. The idea was that three or four times a year we would get together in one of the private dining rooms in the Athenaeum. And for private functions, you could bring in—or we did; I'm not sure how legal it was—we would bring in wine. The Athenaeum didn't have it to sell, but Fred Lindvall stored the stuff in his cellar. Harold would buy wine through his connections in Northern California. And sometimes on his European trips, he would ship back stuff from France and Germany. And Fred maintained the wine cellar. Beneath the regular cellar in his house on Arden Road—the Christys live there now—there was a little refrigerated subcellar, and he kept the wine there. It was brought over to the Ath for these dinners, which were in the private dining rooms and later on mostly in the library.

The original group included Bob and Jean Bacher, Bob [professor of economics 1959-1988] and Doreen Oliver, George [professor of chemistry 1958-1971] and Virginia Hammond, Virginia Kotkin, who was a secretary in the Humanities division, Norman [professor of biology, emeritus] and Pearl Horowitz, and Ed [Edward] Hutchings [editor of *Engineering & Science* 1948-1979] and his wife Liz—perhaps altogether eight or ten couples... Oh, Jack [professor of organic chemistry] and Min Richards and Don [professor of mechanical engineering and applied mechanics, emeritus] and Phyllis Hudson. Dick and Janet Webb—he was director of the Health Center. They were not all always present for every dinner, but there were generally sixteen, and maybe as many as twenty-four. We would have nice dinners that Camille Jaguet prepared, and he really loved to do it up with flair. The little pats of butter were always sculpted. Very fancy touches. And we would have two or three wines that Harold Wayland had chosen.

Those were fun, and I got to meet a lot of people. Ray [professor of biology, emeritus] and June Owen, Murray and Margaret Gell-Mann, David [professor of history, emeritus] and

Nancy Elliot, Noel [professor of applied physics] and Cynthia Corngold, Gary [professor of mathematics] and Louise Lorden joined the group later. I met many members of the faculty through that group, which was one of the few social activities that Mary Lou and I took part in. The Apicians—Harold Wayland gave it that name—have held more than a hundred dinners.

ERWIN: Do you think this was the earliest attempt at fine dining on the campus?

WHALING: To the best of my knowledge, yes.

ERWIN: That might be a landmark.

WHALING: Yes, I think it is. There were some dining clubs here in town that I heard Harold Wayland speak of, and I think Fred Lindvall may have also taken part in those, but they were not on the campus and did not make use of the Athenaeum. So I think you're right; I think it is a first for the Athenaeum. And that continued up until after Harold Wayland retired [1979] and moved to the retirement community in Claremont. Bob Oliver took over and directed it until his death [1998]. And we've had only one dinner since then, and we're sort of trying to figure out what to do next. The need for it is no longer nearly what it once was; the Athenaeum now has a fine wine cellar of its own. The real motivation or initiative no longer exists. But that was a fun activity.

Another—and this is now getting completely off the track, but I should mention it—in 1967, my wife and I bought a vacant lot in the hills between Carpinteria and Santa Barbara. Mary Lou wanted to have some farm activity. You can't raise animals from a distance, but you can do an orchard from a distance, so she went down to the agricultural school in San Luis Obispo—Cal Poly—and studied citrus culture, but she decided it was too complicated. Then she found that avocados were idiotproof; they require nothing except a little water. So she was looking around for an area where we could plant an avocado orchard, and she discovered one that had just been planted by a young French instructor at UCSB [University of California at Santa Barbara]. He spent so much time on his orchard that his contract was not renewed at UCSB and he was leaving, and he wanted to sell it. So we bought this in 1967, and we still maintain it. Our original idea was that we would build a house there, I guess, eventually.

But then we had a very bad experience, in that in 1969, very shortly after we arrived on the scene, a disastrous forest fire went through the area. And after it was over, the hillsides looked like a moon landscape—just black. It was quite a serious fire—four firefighters were killed in it. It was one of those things that was pushed by very strong winds over the mountain passes. Our orchard had been watered just the weekend before the fire commenced. The trees around the edge were either destroyed or they lost their leaves and had to be regrafted. But the center of the orchard was OK. It's not big—only 225 trees. We sell the fruit through Calavo.

But this certainly dampened our enthusiasm for investing in a house in that neighborhood. So for years we've gotten sort of used to—well, we'll go up and if we spend the night, we'll spend it in a tent and water overnight. Or we'll drive up for the day. Or we'll rent a summer house on the beach for a month in the neighborhood and go tend to farm chores during the day when necessary. So we sort of got used to living without a house up there. And at this point, I'm struck by a quotation that I heard from Garrison Keillor this past week—something like "Property is the enemy of leisure." He talked about people who have lots of chores at home, and so they buy a country place to get away from the chores, and now they have more chores. So we have not built a house there and we still go up—we were there last weekend—and do the watering when necessary. In the winter we don't have to be there, but in the summer we pretty well have to be around, or arrange for somebody else to be there, to turn on the irrigation system. And that's been our main hobby. I don't know whether you asked me if I had any hobbies or interests. If you did, that's one of them; that was our real main hobby. Only one year, I think, have we made money off of it. It's been a source of tax deduction the rest of the time. [Laughter] But the crop occasionally has been as large as 10,000 pounds. Generally it's smaller.

ERWIN: Do you do the harvesting?

WHALING: Yes, we do it all. But it's just a hobby. And fun. And we go to meetings of the Avocado Society and rub elbows with the big growers. But it's just a hobby. You go around and look at the little buds and see if there are any fruit set this year. And if the wind blows all the crop off, as it sometimes has done, it's not a disaster, because I have a day job, too.

ERWIN: You're not subject to the vagaries of nature.

WHALING: That's right. I don't have all the problems that a real farmer has, but we enjoy some of their pleasures.

OK. I thought I would like to mention those diversions. Now, to get back to the main topics.

ERWIN: Well, maybe we could do just a little with the astrophysics—how it came about and some of the development of that. And also, of course, your own involvement. Because I know from some of the little reading I've done, that you played a role.

WHALING: The Kellogg Lab, as I think we brought out, had two functions. One, we were interested in nuclear structure and how the nucleus was put together, and [we were] trying to find patterns in the level structure in nuclei that would give us a clue as to what the forces were, or what the laws were, in the same way as studying the regular pattern of levels in the hydrogen atom led to the discovery of quantum mechanics. That was one aspect of the work. But alongside that was the idea of seeing to what extent one could understand the processes that go on in stars, in terms of what we can learn about the nuclear processes that we can observe in the laboratory. And gradually the second goal became the dominant one, partly because there were many other laboratories working on the nuclear structure puzzle, and as time went on, other laboratories—particularly the government laboratories—developed equipment and accelerators that we did not have, and they could explore with higher energies and do things that we could not do. So we began to concentrate more on the processes that we thought might be related to what goes on in a stellar interior.

ERWIN: Did this become, then, by definition, more theoretical?

WHALING: Yes, in part. But as the theories developed, they could point out the crucial importance of certain measurements that we had not really understood the importance of before. I think I mentioned before the work that Charlie Barnes has done on the reaction of carbon-12 and alpha particles, to see how you build up carbon-12 from alpha particles—that becomes a crucial experiment. And the knowledge of what the probability is that that reaction will take place as a function of temperature is a very important parameter in models of how stars evolve



and develop—how they build heavy elements and how stars end up in the later period of their evolution, what the time scales are going to be and how fast things will proceed.

So the experiments went on side-by-side with theory. But the experiments tended to focus on particular reactions. The work on solar neutrinos, which went on in the sixties, pointed out the great importance of the reaction of beryllium-7 with protons. Ralph Kavanagh has worked on that subject for almost as long as Charlie has worked on carbon-12. They've concentrated on particular reactions of critical importance rather than simply exploring as many different nuclei as they could.

And another thing that guided our work, as I mentioned, was the fact that we were limited in the energy available with our accelerators. And soon we'd pretty well exhausted everything you can do with accelerators that only go up, initially, to about 3 million volts, and then, after the tandem came, up to maybe 10 million volts. A number of accelerators were built in the lab. I think I mentioned earlier that one was built just before the war and then not used until after. And then shortly after the war, two others were built, one at low energies and high currents—the one Bill Wenzel and I used for some of our work—and then the 3-million-volt accelerator, following the design of the old 2-million-volt one. So we had three different accelerators in the time I was there. And then right toward the end came the big tandem accelerator.

ERWIN: Is that the one that was called the Yellow Submarine?

WHALING: No, that accelerator was even later; it was installed in 1982, long after I had ceased using accelerators in my research. The tandem accelerator was the last accelerator I used for nuclear research. It was located underground between Sloan and Bridge. It was installed in 1960, at the time the old High Voltage Research Lab was converted into the [Alfred P.] Sloan Laboratory of Mathematics and Physics.

As the things we could study with our limited energies became a more important factor, there was some thought about, say, closing down the original machine—the old 2-million-volt accelerator, which was getting well along in years by 1970, since it started its serious work in 1948-50, somewhere in that period. So we were always keeping our eye out for things that we could do with our limited energy.

And about this time—this would be '68, I guess, because it took a while to get it developed, and the first papers were done in 1970 or '71—Willy had been developing the ideas of nucleosynthesis, the building up of nuclei in stellar interiors and in the processes that went on in a supernova event. Another way of putting it is that all of his theories were designed to explain the elemental abundances that you see in the stars—the fact that there's very, very little lithium, but there's a lot of carbon and a lot of oxygen. And all the elements that we know of on Earth are present in the stars—we've seen them—but their abundances are not at all what they are on Earth. Actually, you know, the discovery that the stars were made up of the same elements that we have here on Earth is a very, very basic and important discovery. It makes man feel a lot more at home in the universe than he would otherwise. I mean, if there were weird stuff up there that we didn't know what it was, it would be quite a different story.

ERWIN: When was that known?

WHALING: I think it was about 1865 that Fraunhofer discovered that the absorption lines he saw in the sun's spectrum were the same lines he saw when he put sodium in a flame. From that initial discovery, many elements were rapidly identified in the sun and later in other stars.

But what Willy was aiming for was a quantitative theory of how *much* of something was present in the star—not how much in terms of “How many tons?” but how much relative to other elements. Astronomers always use hydrogen, the most abundant element, as the reference element, so all stellar abundances are stated as fractions very much smaller than 1.

Astronomers have been working on stellar spectra for many years. Astronomers can tell, just from the wavelengths they see, what elements are present in the stars, but some lines in the spectrum are strong and some are weak, even when both lines are from the same element. To make a quantitative analysis of how *much* of the element is present, one needs to know a property of the radiating atom called the transition probability—the probability that the atom will radiate a particular transition. Astronomers need transition probabilities in order to interpret their spectroscopic measurements quantitatively. They can be calculated theoretically for some lines but, for technical reasons I can't go into, they must be measured experimentally for many of the elements of greatest interest to the astronomers. One of the first supporting facilities built at the Mount Wilson Observatory was a laboratory where Arthur S. King—Bob King's father—

measured transition probabilities in the iron, nickel, and cobalt atoms. That facility was later moved to the Santa Barbara Street laboratories of the observatory. When Bob King joined the Caltech faculty, he continued the measurement of transition probabilities here in a lab in the northeast corner of the Bridge basement; it was a spectroscopy lab originally set up for Millikan that included provision for reflecting sunlight down from a mirror, a heliostat, on the roof of Bridge to a 21-foot spectrometer in the basement lab later occupied by Bob King—and when King retired, by me.

Both Bob and Arthur King tried to reproduce conditions on the surface of the sun where metals like iron, and everything else, are vaporized. They would heat a piece of iron in an evacuated furnace that was supposed to contain only the vapor of the metal being heated, and they would measure the amount of light emitted in a particular line emitted by the hot vapor, in order to measure the transition probability of that line. But as generations of graduate students who used that furnace well know, working with metal vapors at thousands of degrees is a hard, and hot, way to do spectroscopy. After years of working with this method, Bob King was well aware of its weaknesses and he always had his eye out for a better way, so he was understandably excited when a fellow at the University of Arizona—his name was Stanley Bashkin—invented a scheme of measuring atomic transition probabilities by using beams of particles accelerated to high velocities in a DC generator, such as we had in our laboratory. So when Bob heard about this, he approached Willy and said, “Look, this looks like maybe what we’ve been looking for all along—a way to measure transition probabilities.” The method, known as beam-foil spectroscopy, is a little hard to describe without a blackboard. [See W. Whaling, R. B. King, and M. Martinez-Garcia, “Lifetimes of Some Fe I States,” *Astrophys. Jour.* 158, p. 389 (1969)]. In this method, an atom of iron, for example, is accelerated to a high velocity in a vacuum and then passed through a thin foil—we used carbon foils. As it moves through the foil, the atom is excited. The excited atom will decay after it emerges from the foil and is again moving in a vacuum. The lifetime of the excited electronic state is such that it typically moves a few millimeters or a few centimeters away from the foil before it decays.

Many different states are excited as the atom passes through the foil, so many different wavelengths are radiated, and we use a spectrometer to pick out radiation from a particular excited state. If instead of a single atom we send a continuous stream of atoms through the foil and observe the radiating beam downstream from the foil through a spectrometer, we see the

beam glowing with a brightness that fades away exponentially as the distance from the foil increases. By measuring how that radiation decays as a function of the distance from the foil, we can find the distance for the radiation to decay by a factor  $1/e$ , which is readily converted to the time for the radiation to fall by a factor of  $1/e$ , because we know the velocity of the beam. But that time is just the lifetime of the excited state; it is the inverse of the transition probability, the atomic parameter that the astronomers need in order to interpret their observations quantitatively.

Bob King saw this method of measuring lifetimes and transition probabilities as a great way of avoiding the problems that had long plagued the traditional methods used by the astronomers at Mount Wilson and other observatories. Bob talked to Willy and convinced him that our knowledge of the elemental abundances in stars that Willy was trying to explain in terms of nuclear processes would be greatly improved if the astronomers had the more accurate atomic transition probabilities that this method should be able to produce. King proposed that Kellogg Lab convert one of our old accelerators to produce a high-velocity beam of atoms. The deal was that if Kellogg produced the atomic beam, Bob would furnish the spectrometer and the spectroscopic expertise; he would be a full partner in this work and devote his full time to making it a success. At that time, I was the only user of Kellogg's original 2-MeV accelerator, which was the natural choice for this experiment. It had plenty of room in the high-voltage terminal to accommodate whatever would be needed to produce ions of iron and other metals of astrophysical interest. And it was no longer useful for the most interesting nuclear physics research. Willy asked me if I would like to join Bob King in this experiment, and I jumped at the chance to work in atomic spectroscopy, an entirely new field for me.

It was my assignment to build the metal-ion source and install it in the terminal of the 2-MeV accelerator and deliver a beam of iron atoms of high, accurately known velocity. It took about a year to finish my part of the assignment. Meanwhile Bob brought from Mount Wilson a two-prism spectrometer that would cover the visible and near-ultraviolet spectrum—the wavelength region that ground-based astronomers can see. Most spectrometers used in astronomy are big and would not fit in the room where the 2-MeV accelerator delivered its beam. Bob found a small spectrometer at Mount Wilson built to fly in an aircraft as far above the UV-absorbing atmosphere as possible. He also designed lithium-fluoride transfer lenses to transfer an image of the glowing beam to the entrance slits of the spectrometer and had the lenses ground

in the Mount Wilson shops. Bob King's contribution to this experiment was essential. I learned an awful lot from him.

When we finally got to our first exposures—or rather the first exposures on which we saw anything—we were in for some surprises. I should add that our spectrometer, like most spectrometers that astronomers were using in those days, made use of photographic plate detectors. Ours were little glass plates about two inches long by a quarter-inch wide—special plates that Bob ordered from Kodak. What we were supposed to see was many images of the radiating beam, spread out along the length of the plate according to their wavelength, red images at one end, blue images at the other. Each image was bright, close to the foil, and gradually faded away as the excited atoms in the beam decayed. The lifetime was to be measured from the density of darkness of the negative image on the plate.

Once we saw some of these images, the first step was to measure the wavelength to identify the particular transition. The iron spectrum is very well known, better known than that of any other element, and we expected to identify our lines by simply looking up the wavelength in the *Handbook of Physics and Chemistry*. To our surprise, the strongest lines we saw were not in the *Handbook* at all. We saw a few lines that we could identify with the iron spectrum, but they were quite weak compared with the lines of unknown origin.

### **Begin Tape 4, Side 2**

WHALING: These strong unidentified lines puzzled us for several months. We consulted with all the spectroscopic experts we knew, in this country and in Europe, without success. Finally a clue from the British atomic energy lab at Aldermaston led us to the answer. The Aldermaston people had seen the same lines when high-power laser beams struck a steel beam stop and ejected iron atoms into the vacuum. The common feature of their experiment and ours was the vacuum. What we were seeing was radiation from very highly excited states in the neutral iron atom. Atoms in these states are very large—many times as large as atoms in the common radiating states—and hence are nearly always de-excited in a nonradiative collision with another atom or molecule in ordinary spectral sources. In a vacuum, where there are almost no other particles to collide with, the highly excited states can decay only by radiation and they produce lines not seen in the ordinary laboratory spectral sources, or in stars.

The first of my PhD students who worked in beam-foil spectroscopy was from Mexico—Mario Martinez-Garcia. He was able to identify some well-known iron lines in our spectra that were suitable for measuring the solar iron abundance. From his lifetime measurement, we derived a value for the solar iron abundance that was about ten times lower than the accepted value at that time—quite a surprise! Later on, when we measured other elements, we found even larger discrepancies, but the iron result created the greatest commotion, because of the importance of iron in stellar structure.

ERWIN: Was that surprising to you?

WHALING: Well, I didn't have enough experience in the area of stellar abundances to be surprised. It certainly was surprising to Bob King, who had worked on it all his life. To find that his results were off by a factor of ten was quite a shock. But it was interesting because it had implications for stellar structure.

Another interest in Kellogg—though I had not been involved directly, but certainly it entered into Willy's theoretical work—was the production of neutrinos in the sun. Willy took a great interest in the experiments that were detecting fewer neutrinos than the theories predicted. The detection scheme used chlorine-37 to capture a neutrino and the emission of an electron to make radioactive argon-37. Ralph Kavanagh worked a good deal on measuring the cross sections for that reaction. But it turns out that if you change the abundance of solar iron, you change the temperature of the center of the sun, and that changes the rate of neutrino production. So the revised iron abundance had a number of consequences which were of interest to everyone. I guess if I had to say what's the most interesting or significant experiment that I have been involved with, I would say the iron abundance work was probably it.

We went on to measure other ions. Another graduate student, Bill Lennard, worked on nickel. Peter Smith worked on iron ion  $\text{Fe}^+$ . When you look at the spectrum of the sun, more than half the lines you'll see are iron lines, but of the iron lines you see, nine out of ten—ninety percent—come not from the neutral iron atom but from iron missing one electron, the iron ion. So Smith worked on transition probabilities in the iron ion. And that turned out to be important work, because the iron abundance based on the abundant  $\text{Fe}^+$  ion confirmed our results from neutral iron.

One aspect of that work was disappointing, in the following sense. All that spectroscopy can see are the iron lines, whereas often what Willy wanted to know was, “Is it iron-56 or -57 or -59?” His theory treats each isotope as made in a separate way. Now, for some of them—and I think iron is one such case—practically all of it in the sun is -56. So if we measure iron in the sun we measure all the isotopes together. So that one we’re OK on. But sometimes he wanted to know the abundance of some rarer isotope, and all astronomers could see was the sum of all of the isotopes lying on top of each other. So it was a disappointment in the sense that what he really wanted to know was the isotopic abundances of the elements. That’s what his theory deals with; it produces various isotopes, whereas the astronomers cannot see that, in general.

ERWIN: Was that part of the problem ever solved satisfactorily for Willy?

WHALING: No. The way we get isotopic abundances now are from—well, from the sun. The spacecraft that go up and analyze the solar wind can pick out isotopes....They are using essentially a mass spectrograph instead of an optical spectrograph, to pick out the various components of the solar wind by mass, and they can tell what the isotopic abundances are.

Then it is also true that in some of the molecular spectra which are in the far infrared—and I have not worked in that area myself—the astronomers can see the mass difference between at least some of the light isotopes. But in general, for iron and nickel, no. And for silicon, and for many elements, we’re stuck. When we’re looking at a distant star, we can just tell how much iron, but not how much iron-56. And Willy’s theories of production of the elements always dealt with the particular isotopes. So that was a disappointment, but in other respects that work went quite well.

ERWIN: I have a question about the synthesis of the elements. That’s a different problem, isn’t it?

WHALING: Not really. Because you say, “Well, if we make it this way, there ought to be this much of it present; if we make it another way, there will only be this much.” So the abundance tells you about the synthesis. The synthesis predicts what abundance you will observe.

ERWIN: Backing up a little more: Where did the idea first come from that the [heavy] elements were being synthesized in the stars, as opposed to having been created at the Big Bang?

WHALING: Well, I'm sure that must go back in astronomy a long way, from people wondering where the energy came to power these bright sources. Gravity is one source of energy that can do it, and of course, that's the initial one: take a gas, and as it contracts and its gravitational energy is reduced, it goes into kinetic energy. The particles are attracted together—or toward the center, if you like—and they begin to fall, and they acquire velocity. And fast-moving atoms are what temperature is. So gravity starts the warming. But there comes a time when the gas stops contracting. It reaches a stable state and then it just sits there and radiates. And the question is, How is the radiated energy produced? And when the nuclear energy was discovered—I suppose this happened early in the 1930s, when they first saw two alpha particles come out when they bombarded lithium-7 with protons. These alphas came out with tremendous amounts of energy. And then they found that although the energy production is tremendous, the conversion of mass is very small.  $e = mc^2$ , and  $c^2$  is a big number. So you didn't need very much mass consumption to account for the production of energy that a star radiates. As light nuclei are converted into heavier, more tightly bound nuclei, energy is released until one reaches iron, the most stable nucleus. Certainly, early on, [Hans] Bethe was studying this process, but I think that some of the English guys—[Ernest] Rutherford—who were first using the accelerators, probably originated this idea.

ERWIN: Did Hoyle then take his cue from them? For example, Hoyle talks about it in his article in the Fowler *Festschrift*—about always having been interested in these problems. And he came to be involved in your work at Kellogg.

WHALING: I think Hoyle was interested in how to account for the abundances. He would also be producing energy as the hydrogen that he assumed was being created was gradually heated by gravity and then combined to make deuterium and then helium and then carbon and so on. I think all of these processes—initially they are converting hydrogen into helium to make the energy; later on, the helium can be converted into carbon-12, oxygen-16, neon-20. But to make elements heavier than iron requires some catastrophic event—a nova event or something, with a



tremendous amount of energy released in a short time and a tremendous amount of neutrons released, which are captured. It's not a steady-state thing, by any means. I think Willy's initial work—and likewise Bethe's initial work—was to try to understand how in a steady state you could convert hydrogen into carbon and oxygen. And while a star is evolving along what's called the main sequence, the star is changing, but slowly and steadily, before something catastrophic happens to it. Willy got interested, later, in all of the elements that were created as the star slowly converts its hydrogen to helium. Eventually, you were going to get up to where you produce iron and nickel, which are the most stable elements. They are the ultimate nuclear ash, if you like.

ERWIN: I brought up Hoyle because I thought perhaps you would have something to say about him and his participation in your work. Because he became a rather well-known figure and somewhat controversial, perhaps.

WHALING: Yes, he was. And he continued to create controversy. It was my impression initially that the honest-to-goodness astronomers in Robinson were skeptical of this guy who, as near as I could tell, had not had much background in astronomy and very little in nuclear physics. He had a lot of mathematical background and a lot of imagination.

ERWIN: Is that right, that he was a mathematician by training?

WHALING: Yes, and he really was strong in that area. Well, I guess he had an astronomy course somewhere along the way, but he seemed not familiar at all with what our astronomers knew.

ERWIN: Yet he and Willy Fowler did a lot of collaborative work.

WHALING: Yes. Well, Hoyle was very imaginative. And more recently, he has worked on how bacteria populated the Earth. That's how life on Earth originated, in his view. Some people would say that this is sort of wild. But he can sit down and calculate—or estimate, that's probably a better way to say it; I don't think he works with anything except exponents—that the age of the universe is not long enough for evolution to account for everything that's evolved on Earth.

Now, to make such a calculation, you have to make a hell of a lot of assumptions. How fast do things evolve? We don't really know very much about that. And he assumes a smooth situation without extinctions such as happened when the meteors came in. You know, there are an awful lot of uncertainties. Of course, he doesn't try to estimate more precisely than plus or minus a factor of ten. But he has all these ideas, and I think he would be a stimulating person to work with. He would think of new and unusual approaches.

ERWIN: You didn't work with him yourself?

WHALING: No, no. I think I mentioned earlier that when he was trying to build up the elements starting from hydrogen, he found that he could not, even with all of his mathematical skills, account for the production of anything much heavier than helium. Two heliums can collide to form beryllium-8, which is unstable and flies apart. So he said, "Well, maybe when they collide, another helium comes along and a three-particle collision occurs." Well, a two-particle collision is unlikely, and adding a third particle squares the unlikelihood. But he says, "Well, if we assume that there are excited states in carbon which favor such a three-particle combination, maybe that would do it. So there must be such a state in carbon-12." And we talked about that and we looked for one, and, sure enough, we found one where he said it ought to be.

So that was really my only immediate contact with Hoyle. We all followed what he was doing while he was here, and we went to hear his talks. But it's not easy to argue with him about his ideas. Of course, we've seen him on all his visits, and we've entertained him in our home. But he hasn't been here for some time.

This is completely speculative—I don't have any facts on this subject, and I don't know who would—but I have a feeling that Hoyle thought that the Nobel Prize that Willy got [1983] might well have been shared between them. Some people say that. Or it might have been shared with the Burbidges [Geoffrey and Margaret Burbidge]. There was certainly collaboration and discussion, though Willy had been perhaps longer at it than anybody else. The fact that Hoyle did not come to the symposium we held in Willy's honor, in spite of early invitations.... Well, I don't know. But frankly, I have not seen Hoyle since Willy won his prize. I'm just guessing.

ERWIN: That's a long time now—sixteen years ago.

WHALING: Yes.

ERWIN: Would you like to give your opinion on the prize? Was it fair?

WHALING: I think it was. If you go to any conference on nucleogenesis, nucleosynthesis, all the speakers, almost without exception, will be people who earlier in their careers spent some time with Willy—some as graduate students, others as postdocs. He ran a school here, essentially, for astrophysics. And all the people who are active in that field spent time here at one time or another working with him, consulting with him. He was a great teacher, something like [Arnold] Sommerfeld in Germany or [J. Robert] Oppenheimer in this country. He traveled a lot to talk about his work. He was a popular speaker. So he spread the word—he evangelized, if you like.

ERWIN: But that isn't really what you win the prize for, is it? Feynman, for example, never did anything like that, really.

WHALING: No, that's true; you're right. The prize committee may have recognized one person for work that was done by many people in a field of physics that he initiated and fostered. But I'm suggesting that he was an appropriate person to recognize for this very significant area of nuclear physics. You can go to any meeting of the American Physical Society now and you will find a session on nuclear processes in stars, or nuclei in the cosmos. It's become a very large field, and Willy Fowler did more to stimulate the development of that field than anyone else.

ERWIN: What's interesting is your perception of this whole evolution of a field and how that in itself is clearly very important—that it's not just one scientific discovery or experiment that works.

WHALING: That's right. It wasn't [Rudolf] Mössbauer observing an effect, or Carl Anderson seeing an electron curve the wrong way. It wasn't one thing like that; it was the development of a whole school or branch of science, if you like. Bethe himself was very active and productive in this field, and he got a Nobel Prize for it [1967]. Willy just continued on with it a little bit further and kept at it and stuck close to that problem, whereas Bethe worked on many areas. If

you wanted to pick out some individual to recognize for this field, I think Willy would be an appropriate one. There were many, many contributors but he influenced most of them.

ERWIN: Well, it is important to take the longer view sometimes, and to see what kind of a legacy is left by an individual.

WHALING: And to see what Oppenheimer's students have done, and what Sommerfeld did for atomic spectroscopy in Germany, and I guess Rutherford in England, and Niels Bohr. They had people working with them and students—or disciples, I don't know what the word is—who have gone on to push the field forward. So in that sense, I think Willy's prize is certainly very appropriate.

I remember the morning that he got it very well. It's my custom to wake up fairly early in the morning and listen to the news before I get up. They started about five o'clock, and I heard the announcement that William A. Fowler had won the Nobel Prize in physics. I poked my wife and woke her up, and then I called the secretary—Evelyn Gibbs, in those days—and told her what had happened, and that the phones were going to be ringing in the lab, and there were going to be reporters knocking on the door before she normally gets there. So she got up and went over.

ERWIN: So you were kind of the first warning system. You knew before anybody else?

WHALING: I think Willy was out of town. So I came over and unlocked the laboratory, and the secretaries were there, and then the reporters showed up. I can't remember where Willy was, but I think he was not in town until a day or two later.

ERWIN: That must have been a fun day.

WHALING: That was a busy day. There were a lot of reporters and a lot of uproar.

ERWIN: Did you really feel as if you had shared in it, even then—that the whole lab shared in it? Willy was gracious enough to make a comment like that.

WHALING: Oh, I'm sure he would put it that way. I don't remember thinking, Well, we all shared in this. But I think we were very proud to be associated with the work.

ERWIN: Well, you knew the value of what you were doing, whether the prize came along or not.

WHALING: Yes. I do recall that after Carl Anderson got the prize [1936]—I don't know what the cycle is, but periodically the Swedish Academy sends an invitation to people in the same institution to nominate candidates—and Kellogg put together a nomination package for Willy that Tommy Lauritsen signed.

ERWIN: Well, that must have been some years before—ten years before he got the prize. Tommy Lauritsen died in 1973.

WHALING: Yes, and after Tommy died, when the next invitation to make a nomination came, I remember we updated the nomination package but we sent it to Hans Bethe to submit. So he signed it. I do not now recall whether Bethe initiated that or whether we just decided to send it to Bethe because his sponsorship would carry clout. The nomination package is a lot of paper; I mean, it's a big fat thing. Does the Archives have one of the original ones for Willy?

ERWIN: No, we have a nomination that Feynman made of someone else, who didn't subsequently get the prize, or hasn't yet—he's still living. We may have some in Willy's papers.

WHALING: It may be in there. Or it may be that we pulled it all out and sent it to Bethe. If the Xerox machine had been invented by then, we would have made copies to keep, but I don't really know whether those records exist or not. It was submitted more than once, but the one that was effective I know Bethe submitted. [Tape ends]

**WARD WHALING**

**SESSION 4**

**May 14, 1999**

**Begin Tape 5, Side 1**

ERWIN: Well, you've been secretary of the faculty board for fifteen years. What was that like?

WHALING: Well, it's been quite interesting. Over that period of time, I've seen President Goldberger, President [Thomas E.] Everhart, and for the last year President [David] Baltimore. They have different ways of operating, which have been quite interesting.

ERWIN: Does the president always attend meetings.

WHALING: When he is in town he does. In his first year, Baltimore was often away, but he's been coming more often this year. And Goldberger and Everhart were here at least seventy-five percent of the time, I think—perhaps more.

ERWIN: You know, I don't think we have an account anywhere of how the faculty operates as a group in this way. That is to say, there are officers of the faculty, and there are these faculty groups, like the full faculty or the faculty board. I don't think we have any accounts of that structure or how it came into being. Can you provide any of that?

WHALING: The structure preceded me, so I can't say anything about its origin. But it's been well established and operates to my mind effectively.

ERWIN: This has probably been since Millikan's time, wouldn't you think?

WHALING: I just don't know. The bylaws and the machinery were in place in 1949. I wasn't really familiar with them until I became a member of the faculty and began to receive all the mailings from them.

ERWIN: But one could conclude, perhaps, that from that date this was a postwar, DuBridge-era creation.

WHALING: Yes, it may well have been a postwar development. There are faculty bylaws, which are published in the faculty handbook, and they are revised from time to time. We made revisions in the past year. These last ones, for example, were recommended to us by the institute's general counsel, to respond to changes in federal legislation. So sometimes they're motivated that way, sometimes they're motivated by gradual changes in the way the faculty tends to operate. But we try to comply with the bylaws. For example, they specify who are members of the faculty. We have a lot of people around the campus in different capacities. Technicians at a very high level—you know, with PhDs—and scholars, such as are in the Archives and in the library.

But there is a clear definition of what the faculty is and who is allowed to vote for officers and who can serve as officers of the faculty. There's a chairman and a vice chairman. The vice chairman serves in the chairman's absence; otherwise he has no particular responsibility.

ERWIN: I see. And doesn't automatically succeed to the chairmanship?

WHALING: Frequently that happens, but not always. During my recent experience, there have been at least three vice chairmen who have not succeeded to chairman: Mary Lidstrom [professor of applied microbiology 1991-1995] left Caltech; John Ledyard [chairman of the Division of Humanities and Social Sciences 1992-present] became a division chairman and was ineligible because division chairs are considered members of the administration. But generally speaking, that's the way you learn the ropes.

The chairman's job is a pretty busy one—and often, I believe, the chairman is relieved from any teaching obligations during the two-year term of office. He meets with the Institute Administrative Council. The faculty board is something corresponding to a legislative body; it has eighteen faculty members elected for three-year terms—six are elected every year. And in the nomination process, we try to have all the divisions represented. Nominations are made by the nominating committee, but any five members of the faculty can nominate a candidate for the

faculty board—that has happened once in my term, but it’s rare. This group meets once a month and considers matters of interest and concern to the faculty. For example, at the last meeting of the faculty board, we approved a request from Oberlin College to join our three-two program. We heard a presentation of revisions in our policies and procedures on harassment of various kinds. Now, harassment policy is really an administrative responsibility that will be adopted by the administration as Caltech’s official policy. It used to be sexual harassment, but now it’s called unlawful harassment, because it includes other kinds of harassment as well. A committee was appointed to look over our existing policy and recommend changes; they brought them to the faculty board; the faculty board discussed them for an hour and suggested a number of changes of their own and sent the committee off to make some revisions. But in that case, it’s really only advisory to the administration. I think the administration makes valuable use of the faculty board as a sounding board: “Here’s something we were thinking about doing. What do you think of it?”

ERWIN: So what does the faculty have final authority on?

WHALING: On all matters having to do with education. We have to approve all course changes, and anything that goes into the catalog is decided by the faculty and must be approved by the faculty board. Really, everything involved with academics and student life on the campus. For example, it was the faculty board, in that case representing faculty opinion, that raised such a hubbub when there was some thought of turning over our bookstore to Barnes and Noble a couple of years ago. There was a great uproar. The faculty board has a voice in what goes on on the campus, and the administration is very careful to inform us before they do something. For example, before they tore up the lawn in front of your building [the Beckman Institute], they were careful to tell us that this is coming up and this is why it’s going to be done, and we will put it back at the end of the construction period better than it was before. The administration uses this as a means of communicating with the faculty, through the faculty board minutes, which go to the entire faculty.

So those are the only officers. And there’s a secretary who keeps the minutes.

ERWIN: And that is you.



WHALING: Yes. It's also a two-year term, and I will be finishing my eighth term in 2001. And I think I must be replaced before I become so doddering that I make even more mistakes than I do now.

David Elliot preceded me. And he had great skill in wording things with a good deal of wit and humor—a talent that I do not have and I think everybody misses. He was very good at that sort of thing. And Don [Donald E.] Hudson, who recently died, held that job before David. And Hardy Martel [professor of electrical engineering, emeritus], I think, was secretary before Don. Those are the only ones I can recall who have done the job.

ERWIN: That's a small, select group.

WHALING: [Laughter] A small group, I'll agree.

ERWIN: So your principal duty is to provide the minutes?

WHALING: Yes. The so-called officers of the faculty—that's the chair, the vice chair, and secretary—have help, in the sense of a half-time secretary. She makes the minutes look pretty and also handles mailings and communication with the faculty. She's now struggling with getting a ballot ready to go to the voting faculty next week, listing the candidates for various offices and committees.

Operations of the faculty, many of them, are farmed out to various committees, who submit recommendations to the board. When they're approved they become official faculty decisions. There are many, many faculty committees, and they are elected. There is a nominating committee appointed by the faculty chair that finds people to serve on the committees. There are committees that look over any change in the undergraduate curriculum, and they must approve any course before it's considered by the board. Likewise for the graduate program. There's a committee on upper-class admissions, on many things, about thirty committees in all. They're listed in the back of the catalog.

ERWIN: I see. Well, if one reads the catalog, one has the impression that there are standing committees, but also that there are ad-hoc committees.

WHALING: There are occasional ad-hoc committees. For example, when the bookstore problem arose, there was an ad-hoc committee appointed by the faculty chair, to look into this and make recommendations to the board.

ERWIN: Does the chairman of the faculty decide if that committee is necessary, first?

WHALING: The chair can appoint an ad-hoc committee or the faculty board can vote that a committee be appointed to do something or other. At the last meeting, it was decided that an ad-hoc committee should be appointed to study the status of women faculty. This followed the MIT report on the status of women faculty there, and then a report last month by Alice Huang [the wife of President Baltimore] on her observations of conditions here. And some member of the board got up and said, "I make a motion that we have such a committee here." And it was seconded and approved. The faculty chair selects the committee members and its chairman. And now, the last time I heard, the faculty chair was still struggling with finding whom to appoint—how big a committee it should be and what points of view should be represented. Should it be all faculty women? Or should there also be administrators? You know, things of that sort.

ERWIN: So that is actually up to the chairman of the faculty—that person has a fair amount of responsibility?

WHALING: Responsibility and power. And he needs a lot of help. Let me back up for just a moment. I said that there are eighteen faculty members on the faculty board. There are also a bunch of administrators—the division chairs, the graduate dean, the undergraduate dean. The president and the provost. They're on the faculty board; they're all ex-officio; they all vote.

ERWIN: And the faculty officers are on the board, too.

WHALING: Yes, but the faculty officers don't vote. The faculty chair votes only in case of a tie.

ERWIN: Now you mentioned Mary Lidstrom, who left Caltech, which made me think—has a woman ever succeeded to the chairmanship of the faculty?

WHALING: I doubt it, because I think Eleanor Searle [Wasserman Professor of History, emeritus, d. April 6, 1999], who was a vice chair, did not succeed. And Mary Lidstrom, as I said, did not become chairman. And I think those are the only two—they're certainly the only two I have known. And we haven't had women around that long, so I don't think there were any before Eleanor. Incidentally, I think many people felt it was a pity that Eleanor did not succeed, because they thought she would be a very good chairperson.

ERWIN: She was in the humanities. Now, does the chairmanship rotate through the divisions?

WHALING: No. Dan [Daniel J.] Kevles [Koepfli Professor of the Humanities and chairman of the faculty 1995-1997] was the chair before Dave [David J.] Stevenson [Osdol Professor of Planetary Science]. Kevles was in the humanities, and of the ones I have worked with, he was the only one who was from the humanities division.

The chair meets with the steering committee between meetings of the full faculty board and they plan the agenda for the next meeting: What questions will we deal with at the next meeting? They'll distribute an agenda to the entire faculty before the meeting.

ERWIN: I see. So there's a steering committee? That's another level down.

WHALING: And they're not elected. They're just appointed by the chair, from the membership of the faculty board, and their essential function is to decide what's going to be taken up at the next meeting. Because people will come to the chair and say, "I want the faculty board to do something about automobile thefts on the campus." And then the steering committee will decide whether this is of sufficient interest, or are there other things that are more important right now that we need to deal with. So the steering committee plans the agenda. And that's their essential point, though they often talk about other things. But they don't vote on anything; it's just talk. And the chairman decides, OK, these are the things we're going to put on the agenda. It's just to help him with the problems that come along.

ERWIN: The faculty as a full body meets how often?

WHALING: Once a term—it's required in our bylaws.

ERWIN: Do you do the minutes for that?

WHALING: Yes.

ERWIN: And you do the minutes for the board, as well?

WHALING: Yes. And the steering committee. The steering committee minutes are fairly informal; they're just a page, normally. And it winds up at the bottom of the page with the agenda for the next meeting, which the secretary will make look pretty and mail to everybody.

I should add that on the steering committee is the provost—or the vice provost, and sometimes both of them come. The provost is often away, or he's tied up and can't get away. So the administration is represented on the steering committee by the provost, and then there are four faculty members and the chair, vice chair, and secretary. The secretary comes to take notes. The administration brings up topics for faculty board information: "We want to tell you about our plans for the campus. We've just had revisions in our master plan reviewed by the city and we want to tell you what's coming up. And this is so it won't be a surprise when they tear up your lawn out there." And things of that sort. Because the faculty is interested in what's going on, and at the faculty board meeting, they'll make suggestions: "Well, how about a crosswalk to get to all these parking structures on the other side of Wilson?" Or "What's the chance of getting a traffic light on Wilson?" And we learned that Wilson is a main north-south thoroughfare for fire trucks, so we can't have a crosswalk.

ERWIN: Now, I'm a little surprised that the faculty really takes a lot of interest in crosswalks, or any interest in crosswalks.

WHALING: Do you think they walk without paying any attention?

ERWIN: You mean, they're people like everybody else? [Laughter]

WHALING: No, they do. That actually was discussed. And I guess it was [assistant vice president] Hall Daily, who is our contact with the city of Pasadena, who told us Caltech had raised that issue but Wilson is considered a main north-south thoroughfare and we're not allowed

to tinker with it. And likewise, Del Mar is an east-west thoroughfare, and we cannot put any more traffic lights on Del Mar.

ERWIN: Well, after your long service as an officer of the faculty, you must have some particular insight into where the power lies. Who runs Caltech? Does the faculty?

WHALING: I think it certainly has a lot of influence. Well, generally speaking, the administration does pretty well. It initiates things, but if the faculty objects, they will certainly let it be known, and occasionally have been able to overrule the administration. I'm thinking, for example, of efforts during Goldberger's time having to do with JPL [the Jet Propulsion Laboratory] when the work there seemed to be falling off. In the late eighties, there was not as much work from NASA as had been anticipated, and JPL was letting some people go. And the administration decided to take on some classified work for the military.

ERWIN: Now, is this the so-called Arroyo Center that you're referring to? It came to be called the Army Analysis Center?

WHALING: There were centers like that. I don't recall the name; you may well be right that they called it the Arroyo Center. But it was a part of the plan for JPL that an increasing percentage—and I've forgotten the numbers now—maybe as much as twenty percent of their activity would be in classified areas. There's always been a little bit of classified work at JPL, but the idea was to increase it as a way of keeping the lab up to its current level of activity. And there were serious objections, led by [Feynman Professor of Theoretical Physics] Kip Thorne and John Benton [professor of history 1965-1988]. And I can remember a faculty meeting with a motion from the floor—and it's very rare to have a vote taken at a faculty meeting. Actually, I think simply a vote was proposed from the floor to be carried out by a letter ballot—this was twenty years ago. And the administration backed down and decided not to increase the classified activities.

ERWIN: So that was an initiative that came from the administration but was overturned in the faculty.

WHALING: Yes. And I think to some extent, the bookstore thing was an initiative that came from the administration. Gary Lorden [vice president for student affairs 1989-1998] was representing the administration in recommending an outside operator, and the faculty rebelled. I think the administration is careful to keep the faculty informed of what they're thinking about or what plans they have. And the faculty complains when they don't like it.

More recently, Everhart [president 1987-1997] was very eager to increase the diversity of our faculty and student body. And some of the steps he proposed met with resistance. But he was able to convince a majority of the faculty of the wisdom of what he was trying to do. Sometimes the administration has to do a lot of education, or pushing, or prodding to achieve its ends. It just can't say, "OK, we're going to do it this way," as sometimes happens at other universities.

I can't at the moment think of an initiative supported by a majority of the faculty that the administration opposed. With 274 intelligent, energetic, creative members, the faculty proposes lots of changes that the administration does not favor. On the other hand, the faculty proposes new research initiatives and the administration works hard to support them with money and space. In the area of education all decisions are made by the faculty. There is no administrative officer with responsibility for any aspect of undergraduate education.

The faculty government seems to operate pretty well. It surprises me sometimes, because the main goal on this campus is research and teaching—perhaps the research comes ahead of the teaching for many people. And the idea of taking time away from research to work on problems of diversity and freshman admissions and things like that—it's amazing that you can get people to do it, but they do.

ERWIN: They do. Do tenure questions come up before the faculty?

WHALING: No.

ERWIN: They're done in committee, I guess?

WHALING: The divisions, in a vote amongst the professors, will decide to recommend somebody for tenure, and then it goes to the administration, with a lot of supporting documents. And the

Institute Administrative Council, made up of the division chairs and the president and the provost, considers the recommendation and reaches a decision. The faculty is only involved if a person wants to protest a denial of tenure. Then there is an Academic Freedom and Tenure Committee, which is elected by the faculty. The AFTC is unique, in that it is the only committee of the faculty that is both nominated and elected by the faculty. Well, all committees are elected but the slate of candidates comes from the nominating committee. In March, the faculty secretary invites members of the faculty to nominate candidates for the Academic Freedom and Tenure Committee. The six people who receive the most nominations will become candidates, and the faculty will then vote to choose three of those candidates, for a two-year term. [Tape ends]

### **Begin Tape 5, Side 2**

WHALING: The AFTC will act in cases where a member of the faculty objects to a tenure decision. All actions by the AFTC are confidential and I am not aware of them unless the plaintiff makes it public. One very well-known case is [professor of literature] Jenijoy La Belle. Her division [Humanities and Social Sciences] did not recommend her name to the administration for tenure, and she appealed to the AFTC. And it was decided that, yes, she did merit a tenure appointment. And the administration approved it. But the original recommendation for tenure normally comes from the division—from your fellow physicists, in the case of a physicist—and it's forwarded to the administration. The administration can reject the recommendation, but I'm not aware of any case where the administration has turned down such a recommendation. In Jenijoy's case, it was a failure of the division to forward her name to the administration, and she appealed to the AFTC.

ERWIN: Can the committee choose not to hear the appeal, or not to consider it?

WHALING: Well, they may pretty quickly decide that it has no merit.

ERWIN: So it's on their authority that the appeal is denied?

WHALING: Yes. An AFTC decision can be appealed to the president, but I am unaware of any such case. It's my belief that no case has come to the AFTC in a couple of years; they're all treated confidentially. Most of us don't know anything about those at all.

ERWIN: So that never comes to the full faculty?

WHALING: No. There's no provision in the bylaws for that sort of situation. Personnel matters are generally not treated that way at all; they're treated in confidence. So it's only in rare instances, where the grievant may go to the newspaper and tell the story. And they have that right—they can do whatever they want to. But they often have an interest in keeping it quiet, too. I have personal knowledge of only a couple of cases since I've been here. I'm sure there must have been others, but I haven't been aware of them.

ERWIN: Well, there was an Academic Freedom case connected with LIGO. You, of course, know about it.

WHALING: That is true. That was a little different.

ERWIN: Was that the first academic freedom—as opposed to tenure—case that you encountered?

WHALING: That's the only one, I believe. Normally it's tenure decisions that lead to action by the AFTC.

After that [LIGO case] was over, we modified our procedures... Because it was a unique case, it made it clear that our machinery for handling such a case was not ideal. The scientists and engineers on the AFTC have no legal training, and AFTC cases are so rare that the committee members are usually confronting a tenure case for the first time. So we have recently established some guidelines and suggested procedures. When a case comes up, the committee can call on the institute's general counsel for advice on procedures and legal matters. Sandy Cooper, who was the assistant general counsel—or deputy, I think they call it—involved with the LIGO case, suggested procedures or mechanisms that might help avoid such confusion in the future. And those were voted on by the faculty board and adopted.



ERWIN: Have you served on that committee?

WHALING: I've never been a member of the AFTC.

ERWIN: How do you think Caltech makes out, in general, in those sorts of situations, as opposed to comparable schools? Do you have any thoughts about that?

WHALING: Well, we've managed to keep [such situations] much quieter as a rule. We don't read about them in the papers nearly as much as you do about Harvard, and maybe Columbia. And I think that's very good, to keep these matters out of the public eye.

ERWIN: And perhaps it argues for the fact that choices are well made to begin with. It's fairly rare that people are not given tenure—is that the implication here?

WHALING: No, I don't think that's true. For example, the Jenijoy situation came at a time [the mid-1970s] when women's status was changing throughout our society, and maybe some people were a little behind the curve. And she may have been a little ahead of the curve. There have been differences of that sort, because of the changes in the world we live in. But generally speaking, before a member of the faculty is denied tenure, he or she can be reappointed as an assistant professor without tenure for a period of six years. But well before that, maybe after four years, the division chairman will call him or her in and say, "Your teaching is fine, but your publication record....You start a number of papers, but you don't finish them. You have a lot of things in progress. Why don't you finish up some of these so we have something to judge you on." You know, they'll talk to you about how you're doing in that respect. So you're advised as you're going along; it's not just at year six. There's advice well before the tenure decision has to be made, and most people seem to take it, or at least understand it, and understand why the decision was made the way it was.

ERWIN: It's been said that the pressure on younger faculty today is tremendous, and that they are under much more pressure now than would have been true when you, for example, were starting out.

WHALING: I think that's true. There were just a lot more opportunities in those days in the academic world. And a lot of young people, if they did not get a job here, the experience they'd had here served them very well in finding a position in a school perhaps not quite of the stature of Caltech. They could go to USC [University of Southern California] and get a very good job at a very respectable school. But it's not a research university of quite the caliber that the institute is. Or there were other places they could find a job. I think that's harder now. There have not been that many opportunities in academia—though in the last year or two there have been jobs in industry. But the faculties have not been growing. Caltech's faculty grows by maybe one per year out of 274. Our faculty is not expanding. There will be some new faculty members for the biology initiative, but physics and mathematics and engineering do not grow.

In connection with the AFTC, Caltech I think does a good job of keeping out of the newspapers. And in another area this confidentiality stands out when we are searching for a president. You keep hearing about people who turned down the job at Pennsylvania or Yale, or some other university. It gets in the paper. That doesn't happen here. Of the people who have been invited to become president of Caltech, if they don't take it, we never hear about it, and more important, the newspapers never hear about it—that somebody has turned down the job, or that we had to settle for our second choice, or things of that sort. So I'm glad that our presidential selection is kept very quiet. And the other area that we keep private is that of tenure decisions. They will only be known if the aggrieved person who's making the complaint goes to the papers. The institute will never publicize it.

On the matter of government, I think we've talked about it pretty thoroughly. The faculty board is the one that meets monthly and makes decisions. And the faculty chairman presides over those as best he can; sometimes the faculty is hard to steer. And the administration uses that machinery, and is an active part of it; as I mentioned, they are voting members of the faculty board. I guess practically all of our administrators have faculty positions—well, maybe not on the business side. But some of our vice presidents—like Christopher Brennen [vice president for student affairs and professor of mechanical engineering] and Ed [Edward C.] Stone [JPL director and Morrisroe Professor of Physics]—have faculty credentials. So it's not such a surprise to see them sitting on the faculty board. I guess Everhart was a professor of EE [electrical engineering]. And I think Baltimore is on the biology faculty, but I'm not sure about that. I think the machinery works pretty well.

ERWIN: Well, it's been able to accommodate changes in the administrative structure at the upper levels, I suppose, in that after DuBridge left there were considerable changes, with the addition of the vice presidents and so on—a new layer, you might say, between the president and the faculty.

WHALING: That's true. I guess I don't understand exactly how some of these things.... I mean, why Ed Stone is a vice president as well as director of the Jet Propulsion Laboratory, unless that is something that makes NASA happy—to see their laboratory director in a higher level of the administration here. I really don't know how those things work. I'm sure the president needs a vice president for student affairs and for business affairs, and for development. Those are big activities that he needs somebody in charge of; to call them vice presidents is fine. And I don't know what DuBridge did about those things. I hadn't really thought about the fact that he did not have people like that to help.

ERWIN: Yes, all he had was a provost—he had Bacher, who was the first provost. But one reads that DuBridge had everybody at the institute reporting to him. [Laughter] And whether that was by design or that was how it had been, of course, under Millikan. I mean, Millikan had a kind of idiosyncratic way of running the place.

WHALING: He was very authoritarian.

ERWIN: He was very authoritarian, right. And DuBridge wasn't exactly of that mold, but he had his own...

WHALING: Harold Brown brought Dave Morrisroe here, and I guess he was our first vice president for business. We had people who were in that position, but they were called... I can't remember what they called George Green, who was business manager before Morrisroe.

ERWIN: I was thinking that the vice president for student affairs probably was a dean of students, or something like that, before it became a vice presidency. Certainly there were people who had oversight for student affairs, but maybe not with that elegant title.

WHALING: I don't recall who did that. We certainly had Earnest Watson and [William N.] Lacey [professor of chemical engineering; dean of graduate studies 1946-1956; dean of the faculty 1961-62] before we had a provost.

Well, I guess I can't say much more about government. But in general, when I hear about what goes on in other schools, I think, Boy, we do it better than they do!

ERWIN: Maybe we could talk a little bit about the physics teaching at the institute.

WHALING: I mentioned that I came to the institute initially on a two-year appointment, but it was renewed because I found myself teaching a graduate course in nuclear physics when Fowler and many others went off to work on military problems during the Korean War. That was in 1951. After I taught his course for a half year, when Fowler first departed, and then a full year while he was gone, I next taught a junior course—Physics 106, called Principles of Mathematical Physics. It was a course in differential equations and their applications in mechanics and electromagnetics. I took that course from Bill Houston when I was at Rice, and when I got here they were using his book as the textbook in the course. After taking the course from Houston, I had an assistantship and graded homework papers for that course the following year. So I found it a very pleasant, easy course to teach—a natural course to teach. I enjoyed teaching it, and I think I did it well. That may have contributed to my staying here. Somehow or other I got the impression, working with the students at that time, that I would be a better teacher if I had a little more contact with student life.

ERWIN: In what way?

WHALING: At that time I felt I did not know the students very well or how they worked or how they lived. They seemed very different from the pre-war college students I had known as an undergraduate. I wanted to try to understand them a little better. And an opportunity came up to be the RA [resident associate] in Fleming House, so in 1954 I applied for the job. I think the job then—and still today—has almost always been filled by a graduate student, occasionally a postdoc, and sometimes a postdoc married couple. But I was single at the time—an assistant professor—and I applied for the job and got it. I would be curious to see what I wrote in my

letter: why I thought I could do that sort of thing, or why I wanted to do that sort of thing, now that I know more about it. But anyway, I did.

ERWIN: It sounds very idealistic.

WHALING: Maybe so. Maybe I was more innocent in those days and idealistic. But I did it and it was interesting. I had to look after some fairly difficult cases. Some of the students were—one was very badly crippled and could not walk very well without crutches, so he was an unusual student in that respect. Another was very, very young, and very, very innocent. The house system has changed a good deal over the years and is still changing, and the way students are selected for the houses is changing. But I was surprised at the way the guys lived in the houses. They're not very civilized, and at mealtime is where you particularly notice it. And this young student would come to me and say, "Why do students do these things?"

ERWIN: But that's rather sad, actually.

WHALING: It was.

ERWIN: Such as, Why do they write graffiti? Things like that?

WHALING: Yes. And they spit on the sidewalk, and they do all sorts of things, and why do they do that? Certainly there were some fine people in the house, and some that I had in my class. But in my class they were much better behaved, I must say, than they were in the houses.

I don't think of that [experience] as a great success. That is to say, I don't think I did very much for the house. And thinking back, I'm not quite sure what one could do for them. And I still feel that way today, when you visit the houses. Many faculty members are unhappy with our student culture but don't know what to do to change it.

ERWIN: Have the girls made a difference?

WHALING: I'm sure they must have, but I haven't really had a great deal of contact with the houses since the women undergraduates came. One hears rumors of wild behavior, but I suspect they are exaggerated.

ERWIN: What you're saying—it seems to me—is that at Caltech, as at other places, there was a kind of *Animal House* syndrome, if you will.

WHALING: There was.

ERWIN: But these are associated with all-male institutions.

WHALING: I guess so. I guess I was a little bit shocked—maybe that's too strong a word, but I was disappointed and felt that I was not able to, either by example or in any other way, do much for them. And I kept the job for only a year. Fortunately, there were no untoward incidents during that time. The master of student houses then was George Mayhew [professor of English 1968-1988]. He said, "Be sure that before the police arrive, I've been notified. You let me know." [Laughter] He said that to the RAs. There were only four houses in those days [Blacker, Dabney, Fleming, and Ricketts].

ERWIN: What do you think he meant by that? So he could run away?

WHALING: [Laughter] No, no. He didn't want to hear about it first from the police. He wanted to know what was going on. And, of course, the idea was that you will do the best you can to control it so that the police don't arrive. I certainly don't remember—and I'm sure I would—that the police ever came to see us in Fleming for anything the kids had done. There were no serious accidents. And I guess that's all you can hope for: that the police haven't been there and the students didn't burn the place down; that's about all you can do. I hope Avery House is running at a much higher level. And that was the idea; that's one of the reasons that Avery House was conceived. I think many administrators coming here for the first time are a little bit shocked. And the trustees—I don't know how much they see of the houses. But I know Everhart was somewhat horrified when he first visited them. They may be better now, as you say, due to the civilizing effect of the women's touch.

ERWIN: It seems to be somewhat of a cliché—that women are going to have this effect. But then, on the other hand, it's also a bit of a cliché to think that the men without the women are wild and unruly. But that seems to have been the case. Well, did this happen to shape your teaching in any way afterward? Or your approach to dealing with the students on an academic level?

WHALING: No. I must say, when you get them in your office or you meet them socially, outside of the houses, or in class, they're much better. They're under a lot of pressure. And when Friday night comes, or Saturday, the pressure is off and they sort of blow up, to some extent.

I thought I'd mention that. It was an interesting little episode in my experience at Caltech, but I don't think I was a great success at it.

ERWIN: Did you ever talk to others who had been in that position? Or people who were closely involved with the residential life? And did you think there was anyone who had a special touch?  
[Tape ends]

### **Begin Tape 6, Side 1**

WHALING: Bob [Robert A.] Huttenback [lecturer and professor of history 1958-1978; master of student houses 1960-1969], who succeeded Mayhew, was a big, outgoing athlete of some sort—soccer or rugby or something, I'm not sure what he did. He was somewhere from the British Empire initially, I believe.

ERWIN: Yes, he's done an interview with us.

WHALING: He was more outgoing than Mayhew. He would drink wine out of a wineskin, great long streams of wine cascading down his front. He was a little more at home, I think, with them. Whether he did anything for them, I can't say. I think he was a more popular housemaster.

ERWIN: So this is the idea of coming down to their level.

WHALING: I'm afraid so. But to get back to your original question, I am unaware of people who have been an immense success with them. The rowdy behavior appears to be a self-perpetuating tradition in the houses.

ERWIN: So, in fact, the situation just goes on, basically.

WHALING: I think Avery House is an attempt to move away from that. And I'm sure there was the hope—whether it's been realized, I really can't say—that women would be a significant influence. They've got to be; I can't imagine that they aren't a civilizing influence. I think that's a cliché that probably has a lot of truth in it.

Well, getting back to teaching. I've taught every term—maybe with one exception—since I came. And very early on, I became involved with an advanced laboratory in physics. First, it was a lab that Vic [Henry Victor] Neher [professor of physics, emeritus] had conducted for many years, and it was taken by physics students in their senior year. The senior lab tries to introduce them to some of the experimental techniques of modern physics. Back in Neher's day, you couldn't buy amplifiers; you couldn't buy counters; you couldn't buy many types of scientific equipment that many companies are making now. You had to make them yourself. We would show them how to make simple equipment and how to work in the shop, and we would give them some experience in soldering and welding and glass blowing and vacuum techniques and so on. It was a practical introduction to the sort of things that you have to learn how to do when you do experimental work in physics. I think it was a valuable course, and I continued it after Vic Neher left. And soon I was joined by Ralph Kavanagh, and he and I operated that lab together for ten or fifteen years. The course changed over that period, from making Geiger counters in 1950 to using high-speed oscillographs to locate troubles in commercial amplifiers in 1980.

ERWIN: What was the name of it?

WHALING: Methods of Experimental Physics. Physics 77. It was a required course, taken by all physics seniors. As a consequence, I found myself writing an awful lot of letters of recommendation to graduate schools, since I got to see the students in their last year and got to



know them well. They spent three hours in the lab, once a week, and it often took them more than that. I'd look at their notebooks and give them an oral quiz on each experiment to grade them. I was there to help them when they got stuck, or when they had a question, so I got to know them quite well in the lab. And I wrote letters, I think, for practically every physics major who was going to any sort of graduate school at that time.

I felt we did a good job, because a number of kids, dozens, have written to me after they left and asked for copies of the notes that we gave the students. "Because I'm teaching such a course at"—wherever—"please send me a copy of your notes; we'd like to try some of those things here." That's happened quite a lot. Whereas I felt that the RA experience was not a success, I think the laboratory teaching part was quite successful. It has now become less important. You still have to fix things when they don't work, but you don't build equipment anymore.

ERWIN: Not at all?

WHALING: It's very rare. The equipment that you have to build is generally so big that you can't do it. I mean, LIGO has to build its own equipment, but they hire Caterpillar Tractor, or somebody like that, to build the huge thing they're doing. And you can buy from a catalog many of the things we had to make in the old days. So the course is not as vital, and it has become a lab more about how to use some of the very complex and valuable equipment, like modern oscilloscopes, which will do a lot of things for you. It's like learning how to use a computer with a new program: you don't just walk in and use it the first day; you've got to learn. So we give them practice in using oscilloscopes to measure the velocity of light, to measure things of that sort. It's useful in that sense, but students rarely go into the shop and cut metal and use lathes and so on. We don't do that anymore.

That part of the teaching, I think, was quite good. I've also taught a good many lecture courses—just the regular classroom courses, but I think it was the laboratory course that I did best, at least I'm happiest about. I did that for a period of fifteen or twenty years and thought it was quite good. Kavanagh is still doing that, and Ken Libbrecht [professor of physics] is doing some of that sort of thing now with lasers. But it has changed from what it was in Vic Neher's time.

ERWIN: Have the lecture courses changed?

WHALING: Well, there has always been—and I guess always will be—a tendency for things that were taught to the seniors last year to be taught to the juniors next year. I mean, things tend to move down. The kids coming in are more advanced, so we don't have to give them so much of the stuff that we used to include in the freshman course; we can include some of the stuff from the sophomore course. And so there is a general shift to teaching more advanced material all the time. And that goes on, I'm sure, today, though I haven't been involved in teaching now for eight years.

ERWIN: Does that involve redesigning curriculum?

WHALING: Yes, it does. And even at the time of the adoption of the core curriculum about three years ago, there was a major shift in physics, in that.... Well, I guess I should be more careful. We changed the subject matter of some of the courses, but in many ways it's the same thing. They've got to learn sometime what classical mechanics is and classical electromagnetic theory is. But there are more specialties that one can go off into. So there's [a greater variety] of courses for kids as they go along: they can move into solid state physics, or they can move into high-energy particle physics. There begins to be an option, depending on which way you want to go. We did not have that, early on; that specialization came in graduate school.

ERWIN: So that comes sooner now.

WHALING: That comes sooner. But there are certain fundamentals that have not changed and still are there. And they are always shifting down, toward the earlier years.

ERWIN: When Feynman gave his famous series of lectures in the early sixties, it was said that faculty came to attend those lectures. And as time went by, toward the end of the quarter, the room would be less and less populated by students and more and more populated by colleagues. I just wondered if you could verify that with your impression.

WHALING: Well, it's certainly true that as the year goes on, and the weather gets nice in the spring, the attendance at lectures tends to dwindle. That happens no matter who's teaching. And the faculty, who is on the payroll, probably doesn't dwindle as much.

ERWIN: I was just wondering about Feynman's teaching, if you would comment on that. Because so much is said about it.

WHALING: Oh, well, it is certainly true that his classes were attended by an awful lot of people who were not freshmen or sophomores. And graduate students, particularly, there were a lot of. Of the faculty, the ones that you could be sure had to be there were those who were responsible for the lecture notes. All of the physics faculty shared in editing his lectures. Each professor was assigned in advance two or three lectures per year. We were given a typed transcript of what he said and photographs of the blackboard and told to put it in readable form. And you would want to be sure that you were there to hear the lecture for which you were responsible. If our schedule permitted it, I think an awful lot of us went to hear all of them.

ERWIN: Was he a really great teacher?

WHALING: Oh, yes. And also an entertainer, to some extent. Of course, it varied: sometimes he was better than other times. But he certainly had an irreverent way of putting things, which appealed to the young—and appealed to everybody. He was about as far from stuffy as you can get.

ERWIN: Did he really leave a mark on physics teaching in your division, or was he just an idiosyncratic phenomenon?

WHALING: Oh, I think he made a mark in the sense that before Feynman, [there was a] fairly dignified demeanor. I'm thinking of people like Paul Epstein [professor of physics 1921-1953], and more formal types. Willy [Fowler] wasn't really all that formal, but the older ones were. But Dick was in his shirt sleeves and very casual. I don't think people wear ties anymore to give lectures. [Laughter]

ERWIN: So that's his main mark. [Laughter]

WHALING: In dress, for sure. But I think it's part of his youth. He was a very young fellow when he was doing all this stuff. And other young people—[professor of theoretical physics] David Politzer, when he was giving the freshman lectures, was similarly youthful.

ERWIN: Well, I think the question here is, How does one go about teaching science? I mean, there's not really one way. But it seems that Feynman found a way to do it that was refreshing and brought some new life to the classroom experience. Would you say that's a fair assessment?

WHALING: The presentation was not formal and not always as well organized as one might hope. I know sometimes when we were going over the lectures later, we would fill in things that were omitted that he ought to have said. But I really don't feel I can say much about Feynman's teaching that hasn't been said before. It's his personality, to a great extent. I don't think other people can say, "Well, I'm going to do it that way." If you don't have that personality, it's not something, like a method, that you can learn. It was a personal performance to a great extent.

I think in my case the teaching was most successful when it was in the lab, one on one, with the students, rather than in the lectures. And I think they learned a lot from the oral exams at the end of each exercise. We would just talk. "Well, how does this work? What would happen if we tried this?" It was very informal, one on one, the student at the blackboard and me generally sitting down. That's the way I think teaching went best, at least in my experience; I thought that was my best approach to teaching. Lecturing before a large audience like the Physics 1 and 2 classes is not something that I feel much at home with or have done very much of. They've all been much less formal, and that's the way I prefer to work and the way I work the most effectively.

ERWIN: Would you like to say a little more about your scientific work?

WHALING: Yes, let's go back a little bit on that, because we haven't really talked about that. And that's harder to do orally, without a piece of paper and a blackboard, but I'll see what I can do.

I think I mentioned earlier that one of my PhD students, Bill Lennard, who had been measuring the atomic transitions in nickel wanted to use these to measure the nickel abundance in the sun. And I frankly don't know how we discovered that it was available, but we had heard that at the new observatory at Kitt Peak one of the pieces of equipment was the McMath solar telescope, and it was equipped with a type of spectrometer that does not exist anywhere else in the world. So Bill Lennard went to Kitt Peak to measure Ni lines in the sun. I think he wrote to somebody there and they said, "Come on over and we'll measure the lines in the sun that you're interested in." So he did. And while he was there—I didn't go with him—I became familiar with what their machinery could do at that time and asked them to make other measurements for us. It's a national laboratory, and that's one of their functions—to serve scientists throughout the country who can use their work. And they did make other measurements for us, and for other of my students.

The first time I went there, I was impressed with what I saw. The instrument they were using on the solar spectrum was what's called a scanning interferometer, or Fourier transform spectrometer. It's a device like a Michelson interferometer, but instead of working at one frequency, the mirrors move and scan many frequencies with the high precision that an interferometer can achieve. The instrument at Kitt Peak is a huge thing. It's in a vacuum chamber, but the mirrors move over a distance of something on the order of a meter—and so they call it a 1-meter Michelson interferometer.

I went there and was able to use it myself for measurements in the spectrum of cobalt. These are all elements—iron, cobalt, and nickel—that are abundant in the sun, and accurate values of their atomic transition probabilities would be of considerable interest to astronomers. I became acquainted with the scientists who had built the instrument, and we talked about various things we could do with it. We developed a completely new use of the instrument to measure atomic branching ratios. Let me back up a little to explain what branching ratios are and why they are of interest to astronomers.

I told you earlier that we had used the accelerators in the Kellogg Laboratory to accelerate beams of atoms to a high velocity, and we measured the lifetime of atomic states in that way. In the 1980s, '85 or thereabouts, people began to measure atomic lifetimes by a more accurate method that makes use of a laser tuned to a particular transition in the atom—say, from the ground state to a particular excited state. They would pulse the laser—turn it on for just an

instant, then turn it off very quickly. And what one would see is that any atoms that had been excited by the laser would decay in time, so the lifetime could be measured directly. It involved a new technology of lasers, of very fast timing—measuring time in nanosecond intervals,  $10^{-9}$  seconds. With a pulsed laser it is possible to measure directly the lifetime of atomic states with better precision than we could measure them here with our accelerated beams.

So I began to use the published lifetime values that had been measured in other institutions—there were many places doing this, both in Europe and in this country—and convert them into atomic transition probabilities. When the astronomers look at a star, they can tell what elements are present by the spectrum, but to tell how much of the elements are present, they need to know the atomic transition probabilities. Now, if a state decays by only one transition, by a single transition to the ground state, then the lifetime is simply the inverse of the transition probability. But if an atomic state decays in many different ways, it is the *total* transition probability that equals the inverse lifetime, and in order to know the atomic transition probability for each decay mode, you have to know the relative intensity of the various decay channels.

When a level that can decay by several decay channels is constantly repopulated as in a spectral source, the source will emit radiation of the energy—or wavelength—characteristic of each decay channel. And the strength of each of these lines in the spectrum of the source will be proportional to its transition probability. If there are only two decay channels, of equal transition probability, then the spectrum will show two lines of equal strength. So all one has to do is measure the strength of all the decay channels from a level of known lifetime to find what fraction of the total transition probability to assign to each line or decay channel. But this is more difficult than it sounds. A level may have a dozen or more decay channels, covering a spectral range all the way from the infrared to the ultraviolet or beyond. We learned from our beam-foil experience that photographic measurements of light intensity are useless when you're comparing light of different wavelengths, and we quickly abandoned photographic detectors for photomultipliers and the counting technology of nuclear physics. The disadvantage of this method is that you can measure only one spectral line, or decay channel, at a time, and you might need several hours to measure a dozen decay channels from a single level. During that time, it's essential to monitor the level excitation in the source, to see that it stays constant.

For this purpose, I had converted a 5-meter Paschen-Runge spectrograph in the extreme south end of the West Bridge subbasement into a modern spectrometer. Bob King had started to

build this instrument before he became interested in level lifetimes, and he made excavations so that critical parts of the spectrometer—the pedestal supporting the grating and the track on which the photographic plates were mounted—were anchored in the earth with no connections to the building that might transmit vibrations. As far as I know, he never used this instrument. Bob retired in 1968 and moved to Mendocino before I undertook the conversion to a two-channel spectrometer by replacing the photographic plates with two independent photomultiplier detectors and replacing the original grating with a modern Bausch & Lomb grating. I used this spectrometer from 1975 to 1980, to measure the relative strength of the decay channels from levels of known lifetime—usually called the branching ratio for the decay of the level.

When I visited Kitt Peak in 1980, I saw that their big scanning interferometer, which was designed to observe the sun, could make branching-ratio measurements with greater precision and over a wider wavelength range—it was evacuated—than our Paschen-Runge spectrometer. All you had to do was set up a spectral source in front of the instrument and be prepared to run at night or on cloudy days when solar observations were impossible. Other advantages of the instrument are discussed in a paper I wrote with Jim Brault, a Kitt Peak staff member who built it [W. Whaling and J. W. Brault, “Comprehensive Transition Probabilities in Molybdenum I, *Physica Scripta* 38, p. 707 (1988), in which the transition probabilities for 2,835 lines in the molybdenum atom are reported].

After 1980, I carried out branching-ratio measurements using spectra I recorded at Kitt Peak, using atomic level lifetime values measured by pulsed laser fluorescence in various laboratories and reported in the literature. The spectra were analyzed here at Caltech, and the spectral light sources were built and tested here before being taken to Kitt Peak. The lab that housed the Paschen-Runge spectrometer was taken over by the LIGO group along with the rest of the West Bridge subbasement.

So instead of measuring just one or two transitions we could measure hundreds, and in some cases more than a thousand transition probabilities from an atom. I abandoned the beam-foil work in Kellogg Lab and began using the Kitt Peak instrument in collaboration with laser spectroscopists in laboratories around the world. The Kitt Peak instrument is, of course, used mostly by astronomers, but it sits there in the daytime not being used, so there's a lot of free time on it. You apply to the observatory for time, and you're typically given a period of three or four days in a row. In that amount of time, enough data is acquired to keep me busy back here at

home analyzing it for several months. I hired Caltech undergraduates to help in the analysis; some of them would do a problem as a senior thesis, some would just do it for money in the summertime.

So maybe two or three times a year I would make a visit to Kitt Peak and record spectra of elements that are of astrophysical interest. I worked on quite a number over the years—in collaboration with laser physicists who measure the lifetimes of the atomic states. I worked with Jim Lawler at the University of Wisconsin and with Peter Hannaford and R. M. Lowe at CSIRO [Commonwealth Scientific & Industrial Research Organisation] in Australia, and with others in England who had the pulse lasers that measure the lifetime of levels. I did quite a lot of this over the last ten years. I found that in order to do this work with the precision of which we are capable, I had to know more about the energy levels of the atoms. People have been studying atomic energy levels for years and years and years... Astronomers did most of this, actually, just so they could identify what they were looking at in stars. But all they needed to know were the spectra they saw in the visible light from a star, and often an atomic level will also decay in ways the astronomer cannot see, by channels in the infrared or ultraviolet, and those decays contribute to the lifetime. So in order to interpret the lifetime, we had to measure *all* of the decay branches, and we found that the levels we were studying were decaying to levels that had never been seen before. This got us into the business of studying the energy levels of the atoms with a greater precision and more completeness than had been available before. We've had to work on the energy levels of iron, for example. And we've worked on energy levels in many elements, just to get more nearly complete spectral data.

The work has been productive, and I think has made a useful contribution to the elemental abundances information and to the knowledge of the energy levels. I guess I get more requests nowadays for reprints of the energy-level papers than any other; they have applications in many areas. Often the data were so extensive that we would only publish a part of it; page charges begin to mount up if you have great long lists of stuff. So, often we would show samples of the results from an atom and then say that the complete tables of all the results were available on request. When we got requests for the full tables, we would send them by computer; they are too extensive—I mean, forty pages, or something—and you hate to pay \$100 a page for forty pages to publish it in *Physical Review*. Many users prefer to have extensive tables presented in machine-readable form.



I continued to use the Kitt Peak scanning interferometer until I retired in 1993. By then I had pretty well exploited the spectral range of that instrument. One needs to go farther into the vacuum ultraviolet than that instrument can. And other people are building instruments now—at Lund, and at Imperial College, London, and also at NIST [National Institute of Standards and Technology]—which will allow them to continue their research at the short wavelengths. But it's been, I think, reasonably satisfactory work, and I'm quite pleased to have been involved in it.

We still have spectra here that have not been thoroughly and completely analyzed. The modern instrumentation can collect data so rapidly and store it—you know, you put it on magnetic tapes and you bring back several big reels of magnetic tape with spectra on them. It takes quite a while to go through and identify all the things and measure their strengths. I have not in the last few years continued that; I think we have milked the cream out of that supply. I still have the data, and sometimes I think about further work on it. But as time goes on, I doubt that I will get to that.

ERWIN: Did Bill Lennard stay at Caltech?

WHALING: No. He was Canadian, and when he left he went to the atomic energy commission in Chalk River, Canada.

ERWIN: Did anybody else pick up this work?

WHALING: Some of my collaborators from, for example, Wisconsin, who are still measuring lifetimes of single levels of an atom with pulse laser excitation, have sent their people to Kitt Peak. And so the instrument is being used by them now for that sort of work. Jim Brault has left and gone to NIST, in Boulder, Colorado. But the instrument is still maintained and is still operating and is used a good deal by a group at JPL, who are using it to study molecular spectra. It has visitors coming from all over the world, because it's a unique instrument, and for many years it was the only one of its kind. The idea of having the federal government build and operate equipment like that is, I think, a very valuable one. Many people from overseas come over to use it and they're very generous in letting foreign visitors come.

ERWIN: Have you spent time at facilities in other countries?

WHALING: I haven't. I've visited many of them, but I've not spent extended periods to do work. It's actually only with the advent of e-mail that it has been feasible to collaborate with people in Australia, for example. I've published several papers in conjunction with Australian and Belgian coworkers, and those would not have been possible without e-mail.

As I say, now they are building other scanning interferometers, and some that have extended the wavelength range, both on the infrared and then on the ultraviolet end. But I don't think any are of the size that can scan over such a long distance and get such precision. Spectroscopy, I should say, is a science of the early 1900s. It's not what I would call an active field today. The only reason we did it is that we found we needed more data than was currently available, so we made the spectroscopic measurements just largely for our own use. The graduates who worked on that have gone on into other areas. But for our purposes it was really a very remarkable instrument, and I'm glad I had an opportunity to use it.