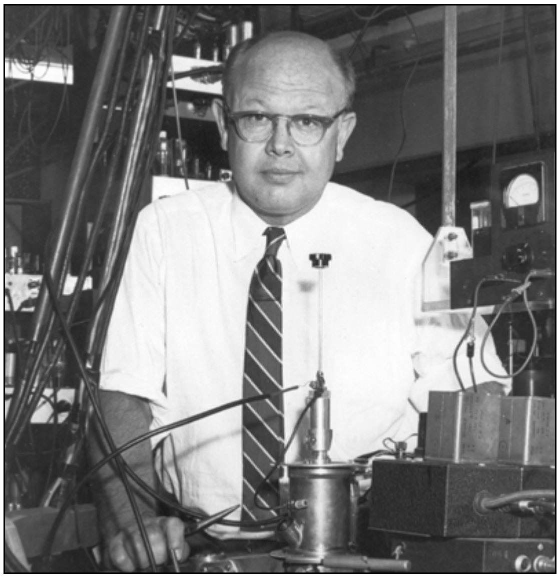

WILLIAM A. FOWLER
(1911-1995)

INTERVIEWED BY
JOHN GREENBERG &
CAROL BUGÉ

May 3, 1983 – May 31, 1984,
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Abstract

Interview conducted in eight sessions between May 1983 and May 1984 with Willy Fowler, Nobel laureate and Institute Professor of Physics, emeritus. In a career in nuclear physics and nuclear astrophysics that spanned more that sixty years, Fowler was primarily concerned with nucleosynthesis—that is, the creation of the heavy elements by the fusion of the nuclei of lighter elements. In 1957, with Fred Hoyle and Geoffrey and Margaret Burbidge, Fowler coauthored the seminal paper "Synthesis of the Elements in the Stars," now known as B^2FH. In it, they showed that all the elements from carbon to uranium could be produced by nuclear processes in stars starting only with the light elements produced in the Big Bang. In the interview, Fowler discusses his early education as a physicist at Ohio State; his work with Charles C. and Tommy Lauritsen at Caltech's Kellogg Radiation Laboratory; the history of nuclear physics and nuclear astrophysics at Caltech; and the evolution of nucleosynthesis. There are recollections of many of his mentors and colleagues, including Robert A. Millikan, Hans Bethe, J. Robert Oppenheimer, the Lauritsens, Fred Hoyle, the Burbidges, Jesse Greenstein, A. G. W. Cameron, Richard P. Feynman, and H. P. Robertson. A 1986 Supplement contains

an interview on Fowler's work for the Naval Bureau of Ordnance and the Manhattan Project during the Second World War.

Administrative information

Access

The interview is unrestricted.

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(Above) Willy Fowler celebrates the news of his Nobel Prize (physics, 1983) with well-wishers at Caltech. Photo by Robert Paz. (Below) Willy Fowler lands a prize-winning seat at the Nobel banquet next to Sweden's Queen Sylvia (December 1983). Svenskt Press Photo.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH WILLIAM A. FOWLER

BY JOHN GREENBERG

SUPPLEMENT BY CAROL BUGÉ

Caltech Archives, 1986, 1987

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with William A. Fowler **by John Greenberg**

Pasadena, California

Session 1	May 3, 1983
Session 2	May 17, 1983
Session 3	May 31, 1983
Session 4	June 21, 1983
Session 5	August 30, 1983
Session 6	September 23, 1983
Session 7	May 30, 1984
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Supplement	October 3, 1986

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GREENBERG: How and when did you first find out about Caltech?

FOWLER: My first recollection of finding out about Caltech is when I took a physics course from R. W. Edmiston, the head of the science department at Central High School in Lima, Ohio. Everyone called him “Pop”—Pop Edmiston. He had an enormous influence on me, probably the first person in my life, other than my parents and my grade school teachers, who had direct influence in guiding my choice of a future career. Caltech had become well known by the time I was in high school—between 1925 and 1929, and Pop knew about Caltech, and he knew that [Robert A.] Millikan had gone there. He was a great enthusiast for Millikan, who was, to a certain extent, considered an Ohio person. He had been at Oberlin, and his family had some connection at Wooster College in Ohio, as I remember.

Anyhow, I first heard about Caltech through Pop Edmiston, who also had an enormous influence on my decision to become a scientist—not necessarily a physicist. The following year

I took a course in chemistry, and there I became interested in an essay contest held by the American Chemical Society for high school students. I entered the Ohio contest and won third prize—400 bucks, a lot of money in those days—with an essay on the production of portland cement. That got me interested in ceramics, which is a big business—certainly was in those days—in the state of Ohio, with the manufacture of bricks; all our roads and streets were paved with bricks in those days, if they weren't paved with wood blocks or just gravel.

I wanted very much to go to Caltech as an undergraduate when I graduated from Lima Central in 1929, but I found out that Caltech charged tuition. I was also very much interested in going to Oberlin, which also charged tuition. There were practically no scholarships available; after all, that was the year of the stock market crash. So I went to Ohio State, where, because I was a citizen of Ohio, I was able to go without paying any tuition. My parents weren't able to help me all that much, so I had to wait table and stoke furnaces in fraternities and sororities all through my four years in college. I also worked on Saturdays.

GREENBERG: What was the text that you used in high school physics?

FOWLER: I recalled that it was something called Millikan & Mills, but when I went over to the library and looked, I couldn't find it. But I found Millikan & Gale [Robert Andrews Millikan & Henry Gordon Gale, *A Laboratory Course in Physics for Secondary Schools*, (Boston: Ginn, c. 1906)] and once I looked at it I realized just from the illustrations that that was the book we had used in high school.

GREENBERG: Was that a standard text for the time?

FOWLER: Yes, I think that Millikan and Gale wrote it for high schools.

GREENBERG: Was it particularly good?

FOWLER: Yes. It was considered *the* book. So there again I found out all about Millikan, although he wrote Millikan & Gale while he was at [the University of] Chicago. In fact, the book must have been at least ten years old by the time I used it in 1927.

So I knew about Millikan and I knew about Caltech, all through taking Pop Edmiston's course. I wrote to Caltech asking for application papers when I graduated from high school. I think the tuition was $300 a year. [Laughter]. But that was just more than anyone could afford in those days; it was the equivalent of $3,000. So I went to Ohio State, where, again, there was a great deal of enthusiasm about Millikan. It was clear that everyone in physics considered him the Great American Physicist; after all, he was the second American—first native-born American—to get the Nobel Prize. [Albert] Michelson, the first American—born in Poland [in Strelno, in what was then Prussia—ed.]—to win the Nobel Prize was of course revered, too, but by that time Michelson was not nearly as active. Millikan was doing all these wonderful things with cosmic rays, and we all measured the charge on the electron in the laboratory, so his name was known to every interested student.

Although I went to Ohio State to enroll in ceramic engineering, at the end of the year, after having taken physics courses and physics labs and hearing a freshman assembly lecture by Alpheus Smith, the head of the physics department, in which he said there was a new option in engineering called engineering physics, I opted for that.

Then at the end of my sophomore year I was still enamored of Caltech. So I wrote again to see if there was any way in which I could transfer, because I'd made all A's at Ohio State. Maybe with my grade record they would give me some kind of a scholarship. Well, 1931 was no better than 1929. I got a letter back saying I could transfer, but I'd have to pay the tuition, which by that time, I think, was $400 a year.

So I stayed on at Ohio State until I graduated in 1933, when I wrote again, this time for admission to the graduate school and for a graduate assistantship. And—very happy ending—I got a telegram, which I still have someplace, signed "R. A. Millikan," saying I was admitted to the graduate school and had been awarded a graduate assistantship which consisted of room, board, and tuition.

There was no cash, no money for travel, but I was fortunate. My father, who was much interested in athletics in the recreational and park system in Lima, had used his influence to get me a job as director of one of the playgrounds. So all during the four years I was at Ohio State, I made $400 a summer, part of which my father saved for me. I had to give it all to him, and he parceled out about $200 a year. By the time I graduated, I had a little nest egg with which I could pay my fare, but even so my father still doled it out. But I was able to come, so that's how

I wound up at Caltech, and as I jokingly say, "Now I'm the oldest graduate student at Caltech."

GREENBERG: Let's talk a little about Ohio State. You did begin to get into physics while you were there.

FOWLER: Yes. As I said, I went there thinking I wanted to be a ceramic engineer, influenced largely by the fact that I had won a prize on the production of portland cement. It's incredible that I got thrilled about physics, because the text that the engineers used was a book by Duncan & Starling [*A Textbook of Physics*] which approached physics in a very elementary, practical, but boring fashion, with enormous detail about pulleys and levers. But nonetheless I did, and I had considerable contact with the Ohio State physics instructors. I also found that I liked the physics laboratory, which an assistant professor named R. V. Zumstein ran. I was just fascinated by the experiments that we did in the physics lab. And then I soon got permission to work in the advanced laboratories. There was another Smith at Ohio State—Professor Alva Smith—who was a marvelous, very sweet man, much in contrast to Alpheus Smith, the head of the department, who was called "Bulldog" Smith because he had such a formidable appearance, although it turned out when I got to know him later on that underneath he also was a rather nice old boy. Anyhow, the physics department had a few well-known physicists: Alfred Landé of Landé g-factor fame, and L. H. Thomas, who discovered the correct relativistic relation for the spin-orbit energy of a moving electron.

GREENBERG: These men were all theoreticians?

FOWLER: They were theoreticians. But there was a very good experimentalist named M. L. Pool, who died just recently. He became a nuclear physicist and built an accelerator at Ohio State. He was very good. I took his course called X-ray Physics when I was a junior. It was the first course where I learned any atomic physics at all. Then I started going to the physics seminars, the colloquia, held by the department. Toward the end of the time that I was at Ohio State—I was a senior there in 1932-33—Caltech was in the middle of all the news, primarily because [Carl] Anderson discovered the positron. This went along with the fact that [James] Chadwick discovered the neutron, and [Harold] Urey discovered the deuteron, and then [J. D.] Cockcroft and [E. T. S.] Walton discovered that nuclei could be disintegrated with artificially accelerated particles.

Fig. 1. Niels Bohr and William A. Fowler, Ohio State University, 1933. Caltech Archives.

GREENBERG: Could you understand the significance of these things?

FOWLER: Oh, yes. The Ohio State seminars were devoted almost entirely to these developments in what we now call nuclear physics. They made an attempt to make the subject clear to the undergraduates. And then I did my undergraduate research under Willard Bennett. He was and is still a maverick, but he had a great influence on me in showing me a lot of experimental techniques. I did some glass blowing, and he had me build all kinds of heaters, and I did an experiment with a tube that he helped me build—the focusing of electron beams just by the gas through which they passed, with a magnetic field. And he took the time to explain a lot of the new things that were happening. He had been a National Research Council Fellow at Caltech before he came to Ohio State, so he was very enthusiastic about my going to Caltech when I finished. He wrote a recommendation for me that helped in my getting the graduate assistantship.

GREENBERG: It seems to me after 1932 Landé may have done some nuclear theory; is that right?

FOWLER: Yes, but Landé got into some kind of a controversy that I can't recall the nature of. He was not very sociable; for that matter, neither was Thomas. Thomas was by far the more capable physicist. He was really very good and his lectures were excellent, although they were difficult for an undergraduate to understand. Although I never took his courses officially, I sat in on one graduate course—and much of it was over my head. It was clear that Thomas was right in the middle of everything in those days, which gave a considerable degree of respectability to the Ohio State faculty. The main thing was that I became aware that in what was going on in physics, Caltech played a very, very important role. And that just confirmed my feeling that that's where I wanted to go to graduate school, and fortunately I did.

GREENBERG: Your undergraduate degree was a bachelor of engineering.

FOWLER: Physics. Bachelor of engineering physics. The option had been started at most one or two years before I took it, so there hadn't been very many graduates; there may have been five or six the year I started. Two people remained lifetime friends—Leonard Schiff and Howard Gundlach. Schiff went on to Stanford, and Howard Gundlach spent his professional life at Oak Ridge. My degree had a lot of advantages, because in addition to taking physics, I took electrical engineering courses. Thermodynamics was taught in engineering. I took mechanics under James Boyd in the mechanical engineering department.

Then I made friends with a young assistant professor in electrical engineering, Johnny

Byrne, who wasn't much older than I was, and he arranged for me to be able to use the engineering laboratories on Sundays. He gave me a key so I could get in, and I remember I spent, oh, almost a year determining the characteristics of a pentode. [Laughter] It was one of the fanciest tubes available in those days; with five electrodes you had all kinds of parameters you could vary, and I had a lot of fun and learned an enormous amount. Then I took a lab in electrical machinery, motors, and generators. So in addition to physics and chemistry, I had all this practical training which stood me in good stead when I came to Caltech and wanted to become an experimental physicist.

GREENBERG: You had a lot more practical experience than most physics undergraduates.

FOWLER: Yes. If I had taken physics in the liberal arts college at Ohio State, my time probably would have been spent in courses in history and Greek and economics. That might have been a good thing, but by taking engineering physics I got a lot in addition to physics that has been useful to me in my career.

GREENBERG: It appears that a number of other successful Caltech physicists were retreaded engineers: [William H.] Pickering, [H. Victor] Neher, [Charles C.] Lauritsen, [Jesse] DuMond, all began as engineers. Is this the direction you came from?

FOWLER: Well, in the case of Lauritsen and DuMond, they were actual practicing engineers. I never practiced engineering; and, of course, Pickering and Si [Simon] Ramo and Johnny Pierce were graduate students in electrical engineering at Caltech. But you must realize that the electrical engineering option for graduate students at Caltech in those days was very little different from the physics option. So although they can say they got their graduate degrees in engineering, their work to get that degree was little different from mine, although their theses were in engineering subjects rather than in nuclear physics, like mine. Another student in physics was Dean Wooldridge, who along with Ramo formed Ramo-Wooldridge, which later on became Thompson-Ramo-Wooldridge. Wooldridge did his graduate work under [William Ralph] Smythe in the separation of isotopes. In fact, Wooldridge prepared an isotopically enriched target of carbon-13, not completely pure, but carbon-13 enhanced beyond the normal one percent. Later on, Charles Townes, who was a student of Smythe, made an even better nitrogen-13 target for me. Now, it's true that Ramo and Pierce then went into engineering. I don't know what you'd say about Pickering, whether you'd call being director of JPL [Jet Propulsion Laboratory] an engineering job; it's really space science. But Lauritsen had a great deal of engineering experience, particularly in radio engineering, although his training was as an architect. He had an enormous amount of practical experience that could be used in the physics laboratory from his work in radio engineering. I think the same is true, to a certain extent, of Jesse DuMond. Both DuMond and Lauritsen were exceptionally skillful experimentalists in quite different ways, quite different ways. So their engineering background, I would say, is different from that of Ramo and Pierce and Pickering.

GREENBERG: All that practical experience that you got in pursuing the bachelor of engineering physics degree didn't do you any harm.

FOWLER: No. It certainly helped me, but I didn't get as much from taking courses and working in laboratories as DuMond and Lauritsen got from actually working in engineering as adults. Another interesting aspect of this was that Caltech was one of the first technical schools—after all, it is an institute of technology—to turn engineering into what we now know it to be: namely, in a sense, applied science.

Royal Sorensen, who was head of electrical engineering, was so adamant about the connection between electrical engineering and physics that he insisted, when Millikan came here and set up the divisional structure at Caltech, that electrical engineering be in the Division of Physics, Mathematics, Astronomy, and Electrical Engineering, not in the Division of Engineering. It was only after the war that electrical engineering was transferred to the Division of Engineering. So one of the reasons why so many people at Caltech seem to have come from engineering was that there was this very close association between engineering and physics. Millikan had no compunction at all in hiring an engineer as a professor of physics. The main requirement for Millikan was either that you be a top-flight experimentalist, which an engineer could be, or that you be a top-flight theorist. He had good theorists in engineering. Theodore von Kármán was the best known.

GREENBERG: You had developed an interest in physical chemistry at Ohio State, I think.

FOWLER: Yes.

GREENBERG: And you began to get interested in physics—and on another occasion you mentioned having read [Lee] DuBridge on the photoelectric effect.

FOWLER: Yes. [Arthur L.] Hughes & DuBridge may have been published about the time of my senior year at Ohio State. I can't remember, but I do know that the first book of physics I ever purchased, other than one I had to buy as a textbook, was their *Photoelectric Phenomena*. I think DuBridge wrote part of that while he was a research fellow at Caltech. So again, the Caltech connection was made. I didn't know that DuBridge had left Caltech by that time. I have a vague recollection that I wanted to come to Caltech for two reasons: one, because it was doing all of this interesting research in cosmic rays and in this new field of nuclear physics, but also because I was interested in the photo effect just from reading this book. Of course, the photo effect was something quite interesting to physicists, because it was really the effect that, to most of us, showed the duality of waves and particles. Einstein had predicted the effect, and, again, Millikan had made measurements on the photo effect. The thing that attracted me most was that DuBridge had all Millikan's work laid out in great detail in that book. It wasn't the fact that I thought DuBridge was still here; I thought maybe Millikan was still doing work on the photo effect.

GREENBERG: At any point did you think you might want to work with Millikan?

FOWLER: Well, I guess I did. It's hard to recollect. I may have had the feeling in the back of my mind that I could go out to Caltech and work with Millikan. There certainly was the connection with the possibility of working on the photo effect, which he wasn't doing any more, as a matter of fact. That was the first thing I found out when I got here. Between the time that I was accepted by Caltech and given the assistantship—that must have been in, say, February or March of 1933—and my graduation from Ohio State, the papers by [H. Richard] Crane and Lauritsen were appearing in the *Physical Review,* and nuclear physics was just burgeoning. Berkeley was beginning to publish, so by the time I finally got here, as I recollect, I was pretty much convinced that what I really wanted to do my research in was nuclear physics.

I hadn't been here more than a month and gotten started in my course work in graduate school than I went to Earnest Watson, who was the actual head of the physics division, although I think Millikan always retained the title. No, I think Millikan's title was director of the Norman Bridge Laboratory as well as chairman of the Executive Council. Anyhow, all the graduate students went to Watson to discuss what they wanted to do in their graduate research. I went to Watson, and the upshot was that he assigned me to the Kellogg Radiation Laboratory and told me that the director, Professor Lauritsen, was away in Denmark for a few months but that I should report to one of the graduate students who was running things while Lauritsen was away. So I came over here and went to work for Dick Crane. Then Charlie eventually came back from Denmark. So that's how I got started.

The important thing to me was that even in my high school days Millikan was recognized as the premier physicist of the United States. Caltech, although it was essentially new as Caltech, had become very well known. It all came to a head for me in 1932 when all these developments came along—I call 1932 the golden year of classical nuclear physics. And there again, Caltech played an extremely important role in, first of all, Anderson's discovery of the positron and then Crane and Lauritsen following up on what Cockcroft and Walton had done. So it was just inevitable for a young fellow who was susceptible to the glamor that goes with these things, that I would make Caltech my choice.

GREENBERG: Did you get to know Millikan?

FOWLER: Yes, in thinking back on it I realize now that I got to know him rather better than I may have sometimes indicated in thinking about what we might discuss. I looked at Millikan's—I think it's the 1947—edition of *The Electron.* [Title of the 1947 edition is *Electrons (+ and -), Protons, Photons, Neutrons, Mesotrons, and Cosmic Rays,* by Robert Andrews Millikan (Chicago, University of Chicago Press, 1947)—ed.] I hadn't looked at it in years, and I opened it and it says, "To William A. Fowler with keen appreciation, Robert A. Millikan." And then on reading the preface to this edition, I find that he thanks me along with Bob Christy and Paul Epstein and Charlie Lauritsen, Bill Pickering, and Vic Neher, Carl

Anderson, and Ike [Ira Sprague] Bowen. In looking at it, I realized that I had supplied him with quite a few cloud chamber photographs he used—he used at least one—and several diagrams of the electrons and positrons produced by the gamma rays, which we were able to produce by bombarding lithium and fluorine with protons. I now recollect that he came into the lab and discussed the material with Charlie—he'd come into Charlie's office, which was right next door to mine even then, and Charlie would always call me in. So a part of what you find in *The Electron,* Charlie and I actually gave him. We also discussed at great length with him many of the developments, because he had problems keeping up, as anyone would have who was as active as he was.

Then he would come every once in a while to Kellogg seminars. If we had some subject like Tommy Bonner talking about the detection of neutrons by proton recoils, Millikan would come, even though it was just a very small seminar on Friday nights. He didn't come to very many, but there were those contacts that I had forgotten. Then he and Mrs. Millikan had Sunday teas. I can remember now I would go maybe once every two months, and I got to know him well enough that, for example, he came to my wedding. When Ardie [Ardiane Foy Olmsted] and I were married, Millikan came, and Mrs. Millikan.

Another way I got to know him was when I was a graduate student, because he insisted on signing all purchase orders over $100. Charlie Lauritsen didn't like going over and arguing with him about purchasing something new for the lab, so he'd send me with it. I recall one thing that showed that his relationship with me was a little closer than I had recollected. When I finally finished up, the faculty in those days awarded the honors for the degrees—either a *summa cum laude* or *magna cum laude* or *cum laude* or nothing at all. And, sure enough, the faculty called all of the graduate students in one day and announced what the awards were going to be, and Dean Wooldridge and I both got *summas.* And I remember that when it got around to me, Millikan made some crack. He said, "Well, Willy,"—no, he never called me Willy. He said, "Well, Fowler, we couldn't give Dean Wooldridge a *summa* without giving you one." [Laughter] The implication was that I was just tagging along, which, in a sense, was very true.

Millikan was, to me, such an amazing man. I never saw some of the attributes which have been attributed to him, although if you read *The Electron,* it doesn't take you very long to find that he ascribes everything that was done after 1900 to Caltech [laughter]—to someone at Caltech, if not to Millikan. His whole life after 1920 was Caltech. He really worked hard to turn it into the institution it is now. Without him it would just never have occurred. He was parsimonious at times, but it was only because he had to be. Even he, with his ability to get funds from wealthy donors, was limited in what he could do.

Begin Tape 1, Side 2

FOWLER: There was another person who had a great influence not only on me but on Caltech. That was Ned Barrett—E. C. Barrett—who was controller of the institute. I got to know Ned and his family, particularly his wife Mary, very well. They had three boys, or was it four? Anyhow, one of their sons was about my age, Newell Barrett; I became very close friends with him. And Ned was just wonderful. I remember there were times when I didn't have a dime and needed some money, and I'd go to Ned and he'd give me ten bucks so I could go to a party or buy something to eat. [Laughter] He did all of the administrative business for Caltech. There was a treasurer, Herb Nash, and there was a registrar. And there were a few secretaries. Millikan had a secretary, Inga Howard, who was very powerful and did a lot of things. That was the whole administrative structure. It worked just fine, but of course that's impossible now and certainly has been impossible ever since the war. But Ned Barrett, more than I think he's ever been given credit for, made an enormous contribution to Caltech. He worked very well with Millikan, although they were entirely different. Ned liked his martinis before dinner and [laughter] Millikan never touched the stuff.

GREENBERG: Let's talk a little bit about your graduate education. From whom did you take courses?

FOWLER: I took courses from Bowen—that was optics. I took atomic spectroscopy—oh, I also took spectroscopy from Bowen at some time or other. I took atomic physics from Millikan, who taught the introductory course. It was mainly an early edition of his *Electron* that he used. I took mathematics from [Eric Temple] Bell, but I also sat in on courses by [Morgan] Ward and by Aristotle Michal. I got to know Bell very well, but he was not a very good teacher. He was a poor teacher, as a matter of fact, but the year I wanted to take what was called math analysis, which was essentially [E. T.] Whittaker and [G. N.] Watson [*A Course of Modern Analysis*], he taught it. But I got very little out of it from Bell, although I became very good friends with him and with his wife, Toby, and with their dogs. So the next year I sat in on the course again, because Morgan Ward taught it. Morgan was a superb teacher. Then Aristotle Michal taught a course on quantum mechanics from a mathematician's point of view. I think he used [Hermann] Weyl's book [*The Theory of Groups and Quantum Mechanics*].

I took a quantum physics course from [Paul S.] Epstein which was very interesting. He started with the iconal, which was essentially a way of going from classical physics into quantum physics and rather old fashioned, even then. Then I think I took a course in thermodynamics from Epstein. He was an amazing man. He came in, walked to the board, started writing on the board, never brought any notes with him, but just at the board he gave these very polished lectures with everything written out, in quite legible handwriting, in a beautiful way. But he would tolerate *no* interruptions. We were not to ask any questions or interrupt him in any way. In fact, one day he was interrupted, and he asked—who was it? Carl Fine, I think his name was—Mr. Fine to leave the class. [Laughter] I won't ever forget that as long as I live. [More laughter]

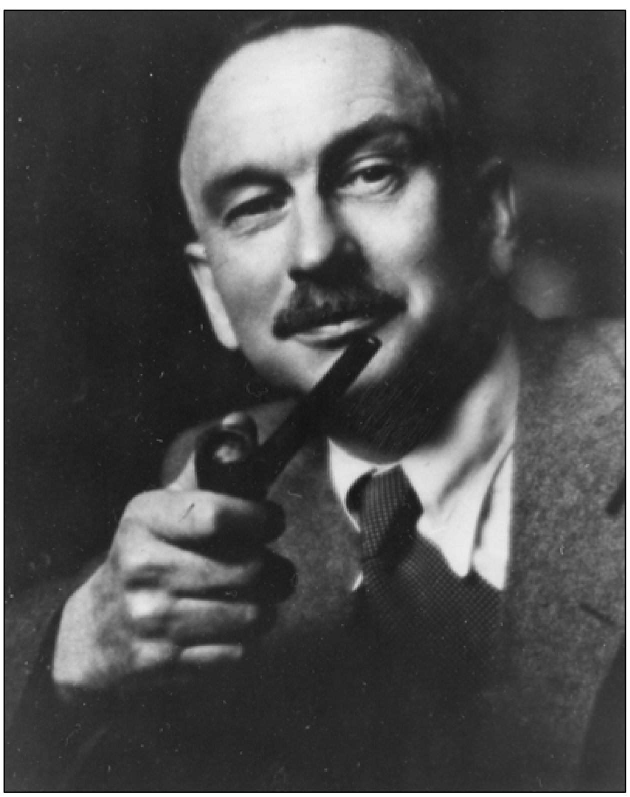

Fig. 2. Richard Chace Tolman, *ca* 1935. Caltech Archives.

Then, of course, the most enjoyable courses of all I took were from Richard Tolman. Richard—I'm afraid I later on adopted some of his habits—Richard wrote his lecture out on the board over in the big chemistry lecture hall, so when we came into the class he had five or six blackboards covered with what he was going to talk about. Then he sat there and smoked his pipe and went through what he'd written on the board. That was an incredible experience, because out of those beautiful notes that he lectured to us came his book. He hadn't written the book yet—*Relativity, Thermodynamics, and Cosmology* [Oxford University Press, 1934].

I also took electricity and magnetism with Smythe. Smythe hadn't written his book yet either [*Static and Dynamic Electricity* (McGraw-Hill, 1950)], although he had a lot of notes that were later incorporated into it. But he insisted that we purchase *Electricity and Magnetism* by [James] Jeans. I'll never forget that, because it was full of the Cambridge tripos questions, and from time to time Smythe would assign us one of these tripos problems, which were always extremely difficult. But the graduate students had gotten onto this, and so there was a crib book with the solutions [laughter] of all the problems in Jeans. It was passed around, so it wasn't as painful as it appeared to be.

GREENBERG: Angus Taylor remembers you and Wooldridge and Conyers Herring as the stars of the course.

FOWLER: Well, I tried to do many of the problems without using the crib, but after I'd worked on them for a little while, because I was so actively involved in my thesis research, I'd just use the crib and to hell with it. Then, of course, I took [Fritz] Zwicky's course, which was the most miserable—and I use the word advisedly—experience of my life at the time.

GREENBERG: This was analytic mechanics?

FOWLER: Yes, yes. Zwicky believed in the Socratic method of teaching. He never lectured. We had to buy a book called Webster's *Dynamics*. [This was probably *The Dynamics of Particles and of Rigid, Elastic, and Fluid Bodies: Being Lectures on Mathematical Physics*, by Arthur Gordon Webster (New York: G. E. Stechert)—ed.] He would come into the class with our little blue cards that we all had to fill out and went through them day after day and usually during class he would have us go to the board. There would be about three students up there during the duration of the class, and when it was our turn and we got to the board he would give us a problem. We were supposed to look through the textbook to find out some way to solve this problem that he'd given us. Well, it was just so embarrassing at times, because if we hesitated at all, he would start shouting. [Laughter] But in later years, of course, I came to realize that it was really excellent training, excellent training. You very soon caught on to the idea that you had to read that whole book, or at least study to a certain extent all the parts of it so you could cope with

Zwicky's questions, because you could get help from the last chapter just as much as from the earlier chapters. So the fact that we had to really try to learn some physics on our own and answer questions about physics on our feet was really in some ways excellent training. But Zwicky made it hard on us, because we didn't do all that well, so he gave practically no A's. He was very proud of the fact that the students did much more poorly in his class than they did in any of the other classes, which were run in an entirely different way: We were given lectures and had problems to do, and the problems were discussed, and we took exams. Zwicky also gave exams, but he put a lot on our performance at the board, and none of us did very well, actually, you see. He rarely gave anything better than a C.

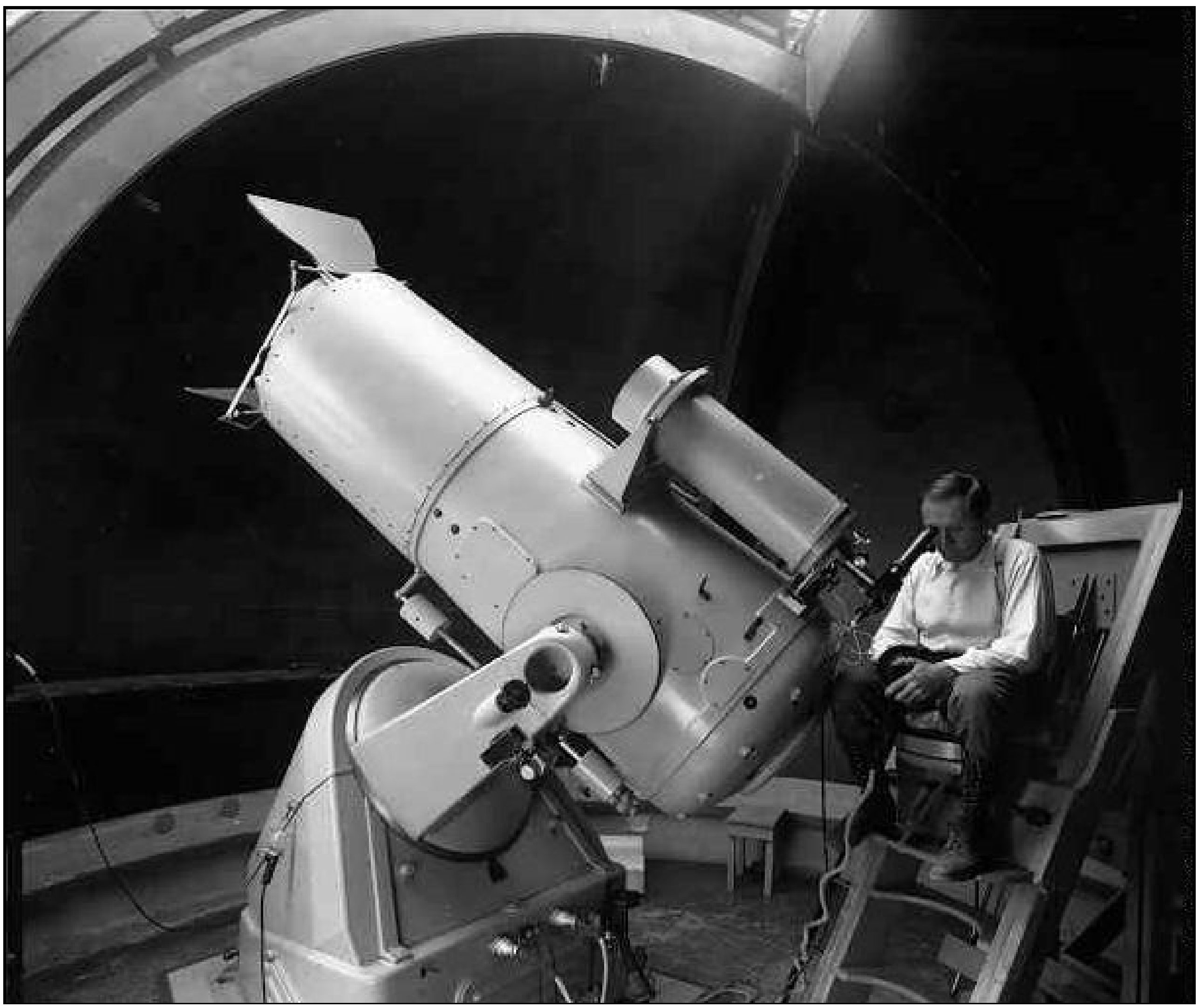

Fig. 3. Fritz Zwicky at the 18-inch Schmidt telescope at the Palomar Observatory, *ca* 1936. Caltech Archives.

That was the origin of the rest of the faculty getting upset—well, they didn't get upset, but they just decided to get back at Zwicky—and they invented a student named Helmar Scieite. At exam time, the Caltech tradition was that the faculty member could not remain in the classroom while you took your exam. They came in, gave you the exam, and the honor system was in, so it was possible for a couple of the students to take the problems out and give one to [William V.] Houston and one to [H. P.] Robertson and one to Bowen. [Laughter] So Helmar Scieite always did very well on the examinations, and Zwicky finally had to give A's to a student. I don't know whether Fritz ever found out that Helmar Scieite was a fake or not. But it doesn't matter.

So those were the courses. But in spite of all the courses to take—and I did pretty well in them—I still had plenty of time to work in the laboratory. And I know that I started working in the laboratory during the first quarter that I was here. It may have been toward the end of that first quarter, but I got started very early, because my name is on papers that were published in 1934, and I came in the fall of 1933. I can't remember whether I had the cloud chamber that I used for my thesis research built during this time; it must have been completed during 1934, but if not during 1934, certainly very early in 1935.

GREENBERG: At this point was there a course in nuclear physics to take?

FOWLER: I certainly took a course under Houston. I can't remember exactly what it—mathematical physics, because that's what Houston was interested in.

GREENBERG: On the whole, was the mathematics taught by the mathematicians useful?

FOWLER: Oh, yes. The course I took from Bell, and then again from Morgan Ward—essentially the advanced theory of functions—I've used all of my career. That's where I learned about Bessel functions, and everything beyond the sine and the cosine trigonometric functions, you see. No, in those days the mathematics department taught graduate and undergraduate courses in mathematics, and the main course was the course in math analysis. Morgan Ward, in particular, was such a superb teacher that I learned a great deal from him, and I suppose I learned something from Bell. I also sat in on the course that [Harry] Bateman gave, but it was pretty much over my head, as I remember. I didn't get as much from Bateman. He talked mostly about very specialized matters, again, on higher functions. He had such a dilettante's approach to the whole business—quite different from Ward's. Morgan really knew what we were going to need to be physicists if we were going to understand what we were going to do. And Michal, too, to a certain extent. He tried very hard in teaching a course in quantum mechanics.

I think from Houston I must have taken something that he called "wave mechanics," although my recollection is rather hazy there. Of course, Houston was another person who was extremely kind to me. When I became a postdoc and started helping him teach his course, I became very close to Will, and I was greatly disappointed when he left Caltech to go to Rice, but [Lee A.] DuBridge had been made president of Caltech, and Rice needed a president, and Houston was the obvious candidate, so he left. And his wife, Mildred, was very kind to me and to my wife. So we remained very close friends with the Houstons all through the years.

GREENBERG: Was he a native son of Texas? Is that where he came from originally, Rice?

FOWLER: No, he was an Ohio State graduate, but where he was born, I don't know. There, again, was an Ohio State/Caltech connection; Houston had come to Caltech, whether directly from Ohio State I just don't know.

The courses that I took were very useful to me. In Bowen's course I really learned about atomic spectroscopy and about the shell model. And of course that became extremely useful to me once I got into nuclear physics and nuclear spectroscopy and the nuclear shell model.

So the courses were, in general, extremely well taught and proved to be quite useful, and I suppose this is still true, although I understand that the mathematicians do very little of the teaching of the mathematics as used in physics now. The physics courses now tend to be very highly theoretical, which is just a commentary on how sophisticated physics has become. The students just have to learn all these high-powered things.

You must remember that when I came, even at Caltech, there was not really a decent course in quantum theory. There wasn't really a course that introduced us to operators and that sort of thing. It was all largely Schrödinger's equation, wave mechanics, although Michal tried using Weyl's book. But although I sat through it, I really didn't understand. Perhaps if I had been able to understand a little more of what Michal was talking about, I would have learned more quantum mechanics, but I—and I have suffered from that throughout my whole career—

have never really had a decent training in high-powered quantum theory.

GREENBERG: But this must have been a problem for most people in your generation.

FOWLER: Yes. It's been a problem for a great many of us; people like Schiff surmounted it, of course. And, of course, all of [J. Robert] Oppenheimer's students or postdoctoral fellows went on to make great contributions—Willis Lamb, Robert Serber, Leonard Schiff, and George Volkoff. Then I shouldn't overlook the fact that I took courses from Oppenheimer. But he wasn't interested in teaching abstruse quantum theory. He taught a theoretical nuclear physics course, and it didn't last very long, because he was only here for one quarter, the spring quarter, and only about two-thirds of that, because he came down from Berkeley in the spring after Berkeley finished its second semester. Once he got down here, he started giving his course in theoretical nuclear physics and that was really one of the highlights, because Robert was an excellent teacher, and he knew what was going on in nuclear physics.

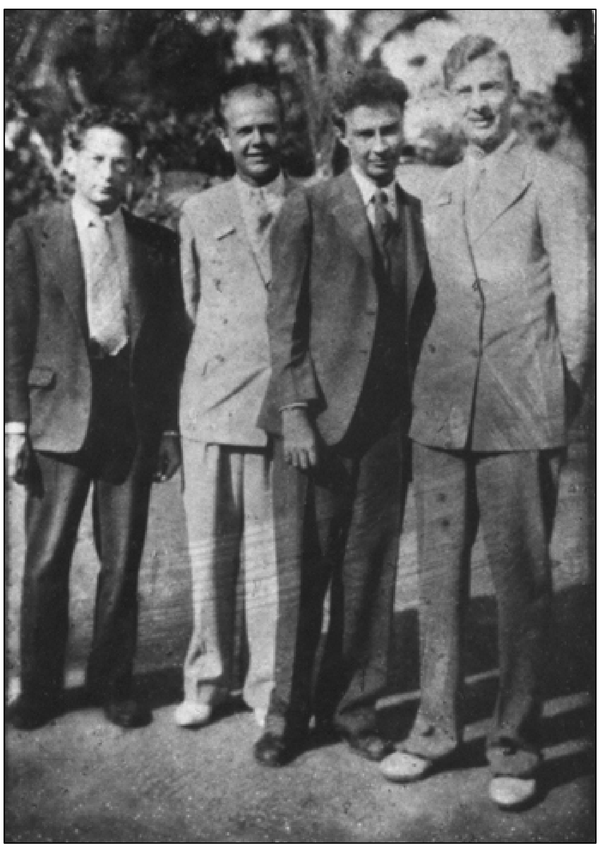

Fig. 4. Four future Presidents of the American Physical Society: Robert Serber (1971), William A. Fowler (1976), J. Robert Oppenheimer (1948), and Luis Alvarez (1969), San Diego Zoo, June 1938. Caltech Archives.

One of the great things for me was that I didn't have to read the literature all that carefully, because Robert Oppenheimer knew everything that was going on, and it was much easier to get it from Robert either by going to his course or by discussing things with him, which Charlie [Lauritsen] and I did. I still have the notes that he—it wasn't a mimeograph machine; it came out kind of bluish-purple—distributed to us. Then he'd lecture, and it was then that I really learned the basic theory I needed to understand what we were doing in the lab.

GREENBERG: I guess the thing that fascinates me is that he started off six or seven years earlier teaching badly, and then developed into such a model; at least, people claim that he was terrible.

FOWLER: I suppose my judgment is not one that is held unanimously. There were certainly some graduate students who didn't like Oppie's mannerisms and, I think, didn't get very much out of his teaching. And it's true, he tended to a certain extent to talk over our heads. But we had the notes, and of course I was interested in what he was talking about. I was doing nuclear physics in the lab, so I had a special reason for responding to his teaching. A fellow working in some other field—say, with Houston on classical mechanics or with Smythe on mass spectroscopy—just didn't have the motivation for learning from Oppie that I had. So my judgment of Oppie as a teacher is colored by the fact that I really wanted to learn what he was talking about. And then I did have the advantage that he came into the lab to talk to Charlie and me all the time when he was down here, because he was very much interested in what we were doing and in what was being done at Berkeley. So we learned from him what was being done at Berkeley, and they learned from him what we were doing.

It was all just an incredibly exciting time. The results were, to a certain extent, easy to come by once Charlie had built an accelerator and we had detectors—either the Lauritsen electroscopes or my cloud chamber. All we had to do was bombard a new target with protons or with alpha particles or with deuterons, and make some observations and write a paper and send it in. Then there was the exciting competition with Berkeley and with DTM [the Carnegie Institution's Department of Terrestrial Magnetism] in Washington, and eventually with Wisconsin and Minnesota and Yale. It was a terribly exciting time.

As far as I'm concerned, Robert played just an enormous role, because he understood so much more completely than either Charlie or I, or even Tolman, the meaning of what we were doing. He made mistakes; he often had built-in prejudices, particularly about the resonance phenomena. He just couldn't believe that the resonances could be as sharp as Lauritsen and Crane originally found even with their AC [alternating current] tubes.

But in the main, Robert had a very sound knowledge of all aspects of atomic and nuclear physics, which made the whole difference between grubbing away in the lab not knowing what it was all about, and being able to understand what we were doing and share in the great enterprise that nuclear physics was in those days.

GREENBERG: There are a number of things that you brought up that we want to come back to and probe, one by one, later. You were a graduate assistant. How were you compensated?

FOWLER: Well, as I said earlier, I was given room, board, and tuition. There was no cash as a graduate student. I received my first salary when I became a High Potential Research Fellow—I love that. It's because the laboratory was the High Potential Laboratory of the Southern California Edison Company. My first salary was $1,500. But I had no compensation from Caltech as a graduate student except what I could borrow from Ned Barrett and Charlie Lauritsen, which I usually paid back. [Laughter] My room was originally in the old dorm. The tuition, of course, I didn't know anything about. It was just paid from one pocket in the institute to another. Meals were at the Athenaeum. If we wanted to have our meals paid for, we had to eat at the Athenaeum. A great number of the graduate students did that, and I became a member of a group of eight of us, or nine sometimes, who ate at the round table, under the picture of Millikan, Noyes, and Hale—the trinity, the holy trinity in the Athenaeum. We had quite a gang.

Fig. 5. The Athenaeum "round table gang," June 14, 1934. Clockwise from the near left: John Read, Norton Moore, Wolfgang Finkelnburg, Henry DeVore, Richard Crane, William A. Fowler, Lucas Alden, Walter Jordan. The insulator for their ceremonial candle was "borrowed" from Royal Sorensen's High Voltage Laboratory. Caltech Archives.

At the end of my second quarter, kind of as an award because I'd been doing fairly well, Charlie Lauritsen arranged for me to move into the Athenaeum. A lot of the graduate students lived in the loggia in the Athenaeum, but Charlie got me a room, and somehow or other he got Seeley Mudd, who was running the medical part of the cancer research here in Kellogg, to pay for my room. Charlie never divulged the slightest details of how any of the finances that went on came about, except I knew about expenditures to buy equipment and tools and that sort of thing. But I was completely unaware of where the money came from. In a large measure, Millikan did it all. He was very tight-lipped about what he was doing, where he got the money from, until he named some building for a donor, or some scholarship, or something like that. So, I went through my graduate and postdoc life without knowing anything at all about where the money we spent in the laboratory came from, nor where my assistantship and then my stipend as a postdoc came from.

I did have financial worries as a graduate student, because I did need cash, but, there again, Charlie [Lauritsen] helped out, because he was the one who arranged it. One of the doctors had purchased some radium needles—one of the doctors in the cancer unit here, Dr. Clyde Emery. He also had private practice over in Los Angeles. He had purchased some radium needles to use in his private practice, and he didn't have enough money to purchase the standard equipment for handling radium. So Charlie arranged for Louie [Louis N.] Ridenour and me—Louie was another graduate student—to make the equipment that was needed for shielding the radium for hauling it around, mainly lead-filled brass containers. So we made him all kinds of little lead shields. We scrounged all over and used up all the lead on the Caltech campus, and we used all kinds of brass tubing that we could find in the shop—one thing and another. Louie and I made a couple of hundred bucks that way, so I was able to get by.

That kind of continued throughout my graduate school. There were always jobs of one kind or another that I could do for the doctors. That got me enough cash to get my laundry done and to go to a show or take a girl out, or one thing or another. The main thing that I did was play poker on Saturday nights with the other graduate students. That was a great drain on my resources, because I wasn't a very good poker player. There was one mathematician, Max Wyman, who cleaned all of us all the time. But we didn't play for very high stakes—a couple of bucks on a Saturday night was cheaper than doing something like going on a date, actually.

Father Bolger supplied us with all the wine we needed. He was a graduate student who later on founded the physics department at Notre Dame—a very great man, Father Henry Bolger. He was sent here by the Catholic Church, and for his duties he celebrated mass at St. Andrew's in downtown Pasadena. Of course, when I first came here, Prohibition was in force. Roosevelt was elected in 1933 and Prohibition was turned off, I guess, by the start of 1934. But anyhow, Father Bolger's parishioners brought him wine, more than he could use himself, although he was able to drink his share, so he shared the wine he got from his parishioners. So we had wine, and then we were able to buy beer. And Charlie Lauritsen supplied gin—which he pronounced "yin"—at the Friday night seminars, so we made do, we made do. It was really a halcyon period.

Of course, that's the other thing we must talk about: Another incredible help for me was the Friday night seminars that Charlie had for his group of six graduate students, where we really learned a great deal by each of us giving a seminar, week after week, and having to dig in to some problem. The first year we went through Rutherford, Chadwick, & Ellis, [*Radiations from Radioactive Substances* (Cambridge Univ. Press, 1930)] which had just been published. Boy, we really learned the background of nuclear physics—that is, radioactivity. Then after the seminars on Friday evenings, we went over to Charlie's place and drank and sang. Sigrid Lauritsen, Charlie's wife and an MD, would give us a midnight supper, and it was really something.

WILLIAM A. FOWLER
SESSION 2
May 17, 1983

Begin Tape 2, Side 1

GREENBERG: When you arrived, you began to work in the High Potential Lab, and your boss, Charlie Lauritsen, shared this lab with a Caltech electrical engineer named Royal Sorensen. Was this kind of lab sharing unusual?

FOWLER: Well, whether you would call it "sharing" in the strict sense might be questioned. Actually, Sorensen had essentially built the laboratory for the Southern California Edison Company in order to study high-voltage transmission of electrical power in anticipation of the construction of Hoover Dam. So Sorensen was in charge of the laboratory. It was called the High Potential Research Laboratory. But, as far as I know—although everything had been done before I arrived—he had generously allowed Charlie Lauritsen, probably with Millikan's backing, to build vacuum tubes for the production of high-voltage X rays in the laboratory, along with all the equipment that Sorensen and his collaborators were testing.

So when I came, Charlie was well established. He and Ralph Bennett had built a cold emission tube, and Charlie and Dick Crane had constructed a million-volt X-ray tube. And in fact they had converted this X-ray tube, which accelerated electrons on one-half cycle of the AC voltage supply, into a positive ion accelerator, merely by replacing the filament source of electrons with a positive ion source, which then operated on the other half of the AC cycle. And that had, of course, been stimulated by the discovery, by Cockcroft and Walton of Cambridge, that nuclei could be disintegrated by protons, well below the classical Coulomb barrier. I understand that when Charlie heard about Cockcroft's and Walton's discovery, he immediately changed his X-ray tube over into a positive ion accelerator and began doing what we now call nuclear physics.

So Sorensen, I think it's fair to say, had been quite generous in letting Charlie do this, although there may have been arrangements made by Millikan with the Southern California Edison Company, in addition to the testing of the high-voltage equipment, so that Lauritsen was to be able to use the tranformers for the work that Millikan was interested in at the time—mainly, the X-ray cancer therapy.

Fig. 6. Charles C. Lauritsen and Robert A. Millikan stand atop the one-million-volt X-ray tube, developed and built at Caltech by Lauritsen and colleagues in 1928. In 1931, Millikan persuaded the Detroit cornflake magnate W. K. Kellogg to finance a new laboratory to house research. Photo by Wide World Photos, Caltech Archives.

GREENBERG: Did anything come out of this cohabitation, I mean anything specific? Did you fraternize with any of Sorensen's people or talk shop with them at all?

FOWLER: No, but I depended upon them for a great deal of assistance. The man who was most helpful was Francis Maxstadt, who I think at that time was an assistant professor in electrical engineering and used the laboratory a great deal. He seemed to be involved in the testing of insulators and towers—more than Sorensen himself, although I can remember Sorensen frequently coming into the laboratory, putting on an apron, and actually going to work himself. Then there was another faculty member, Dr. Stuart MacKeown. He was there a great deal of the time and was extremely helpful.

I think it's fair to say that the two lines of work were entirely independent; they went along in parallel. There was the necessity of scheduling the time of using of the big transformer set, but that seemed to go very well, and we were always willing to take the late afternoon or night shift. In fact, I did a great deal of my thesis research at night, just because the engineers used the set in the daytime.

Another thing was very helpful to me. Once Lewis Delsasso and I had our cloud chamber built and running, we took an enormous number of pictures at night. We used a little French step movie camera that we could step along with a solenoid arrangement to take one picture after another. Of course, those had to be developed, and Sorensen very kindly gave us one room at one end of the ground floor of the laboratory to use as our darkroom. It was a fairly happy arrangement; at least, I never saw any problems, and Francis Maxstadt was very helpful any time anything at all went wrong. All I had to do was go to him and say, "Look, there's something here that I don't understand," and he would take time off to look into it and tell me what to do.

GREENBERG: Sorensen was a power engineer.

FOWLER: Yes.

GREENBERG: And he didn't do much of anything in the way of electronics himself.

FOWLER: No, not himself.

GREENBERG: Pickering and DuBridge and [Frederick C.] Lindvall all seem to agree that there was very little in the way of electronics, at least in the curriculum, at Caltech in the twenties and thirties. Lauritsen did design and build high-voltage X-ray tubes, and I think one should put Lauritsen in the same league as people like W. D. Coolidge and Irving Langmuir at General Electric.

FOWLER: Well, I would put what Lauritsen did in the development of high-voltage vacuum tubes in the same class and the same category as the parallel developments that were going on under Coolidge. On the other hand, I wouldn't call that "electronics" in the sense that we use it nowadays. Electronics has more to do with the circuitry that is used, although of course it went through a stage where vacuum tubes at much lower voltages and on much smaller scales and in much greater complexity were being developed. No, Lauritsen's work was very similar to that that Coolidge was doing, and in fact those developments are described in some detail in the paper that H. R. Crane presented at the fiftieth anniversary celebration of Kellogg, and I think there are copies in the Archives.

To boil it down, Coolidge developed the idea of the use of metal cylinders inside the glass vacuum tube to protect the glass walls from bombardment by electrons. And then what Charlie and Ralph Bennett essentially did was to introduce a reentrant electrode that ran down through all these cylinders, so that the gap between the electron emitter at one end and the target at the other was considerably reduced. So Lauritsen made a very significant contribution, and I think it matches in some measure what Coolidge did, although I think it's true that in the minds of most people it was Coolidge who made the first—and I think that's right—significant step in the construction of high-voltage X-ray tubes.

Lauritsen was rather single-minded, in that he wanted to build a million-volt X-ray tube no matter how big and how clumsy it was, to see whether million-volt X rays, which would be more penetrating than lower voltage ones, were effective in cancer therapy. The effort came out with some triumphs and some disappointments. The upshot of it was that going beyond about 400 kilovolts in energy really wasn't worthwhile, and so the standard commercial X-ray tubes, which were then built by GE [General Electric] and were DC rather than AC tubes because they had rectifiers in the circuits, went up to about 400 kilovolts.

It wasn't until much later that higher-energy therapy was tried, but even then the higher-energy therapy used gamma ray lines from radioactive nuclei, like cobalt. Later developments introduced the use of pi mesons for therapy, as well as neutrons and protons. But by that time Charlie had lost interest. The whole X-ray therapy program had been stopped around 1940 in Kellogg, and Lauritsen and all his associates and students were primarily interested in positive ion acceleration to do nuclear physics.

Lauritsen found that there were two theories for the cross section for the Compton effect. The Klein-Nishina theory of the scattering and absorption of X rays and gamma rays by an electron included the spin of the electron, whereas an earlier theory due to [P. A. M.] Dirac and [Paul] Gordan neglected the spin of the electron, in spite of the fact that Dirac eventually gave the theoretical foundation for the spin of the electron through his relativistic wave equation. Charlie realized that these two theories predicted different results for high-energy X rays. He and John Read, a Commonwealth Fellow from England, showed that the Klein-Nishina formula was the correct one. As far as I know, they were the first ones to do this in the critical energy range from 0.2 to 0.7 MeV [million electron volts]. I'm supported in that, because if you look in [Walter] Heitler's book on *The Quantum Theory of Radiation,* a key reference is to Read and Lauritsen on this particular subject. Earlier references on experiments up to 0.1 MeV were in somewhat better agreement with the Klein-Nishina formula than with that of Dirac and Gordan.

Once that had been done and the therapy program ended, then there was no way in which further X-ray physics could be continued. You must remember that the X-ray physics was bootstrapped on the back of the X-ray therapy program. Once the X-ray therapy program was stopped, there were no longer funds to keep the X-ray tubes going. Lauritsen, with the meager funds that were available for research in Kellogg, preferred to concentrate on the positive ion and nuclear physics and not do any more X-ray physics. But the paper by Read and Lauritsen [*Phys. Rev.* 45: 433, (1934)] is a classic, not only in the result it obtained but in the fact that they were using an AC tube. Of course, even with a DC tube, when the electrons are decelerating, they give off a continuum of X rays and radiation, but with an AC tube it's even worse, and so what Lauritsen and Read had to do was send the X rays through a crystal and, by Bragg reflection, separate out the X-ray wavelength that they wanted. So it was quite a technical *tour de force* that they were able to get a discrete line with enough intensity from their AC tube. But they did it, and they got very beautiful results. It was quite an achievement. Read was an exceptionally capable person. He went back to England and remained in X-ray physics and X-ray therapy in one of the medical hospitals in London.

GREENBERG: I went back and looked at Chadwick's paper in *Nature* in 1932, and I guess the Klein-Nishina formula enters into his demonstration that the penetrating beryllium radiation is not quantized electromagnetic radiation but particles. So is the Klein-Nishina formula part of nuclear physics, too?

FOWLER: Oh, yes. Much of early nuclear physics involved bombarding a target with protons or alpha particles, especially with protons, which resulted in the production of high-energy radiation. We call this gamma radiation, but it's no different than X radiation, except if you want to set some arbitrary line of energy below which you call it X rays and above which you call it gamma rays. Or you can divide it in terms of how it's produced. X rays are produced by stopping electrons; gamma rays are produced by either radioactive nuclei decaying or by bombarding nuclei with protons or other particles. So, because gamma radiation was produced so frequently in bombarding nuclei, one had to know about the penetrating properties of this gamma radiation. These penetrating properties are given by the same formula as for X radiation with the difference being only the energy range involved.

The Klein-Nishina formula was used by Chadwick in showing that the radiation he was observing in his pioneering experiments was much more penetrating than he would have expected if it was gamma radiation. All of those things were tied together, and I believe that Charlie realized that what he was doing with his X-ray tube would have enormous applications in nuclear physics.

As a matter of fact, continuing for some years longer than one might have expected, we used the penetrability of gamma rays to measure roughly—it had to be roughly—their energy. The difficulty was that in addition to the Compton effect, which the Klein-Nishina formula describes quantitatively, once the radiation is above a million electron volts in energy, electron-positron pairs can be produced, and that becomes another means of absorption. The upshot is that the absorption coefficient of gamma radiation goes through a minimum somewhere above a million volts or so, and then rises again. Or the penetrating power goes through a maximum.

This, however, depends on the material, because the pair-formation effect is much greater in, say, lead than it is in aluminum. So by using lead absorbers and aluminum absorbers, we were able, in the early days, to get some measure of the energy of gamma radiation from nuclear reactions, even though the absorption is double-valued, since it first drops and then rises again.

Of course, once we had a cloud chamber, we were able to make measurements of much higher resolution in energy, because we could look at the secondary electrons and the secondary electron-positron pairs produced by the gamma radiation in thin foils in the cloud chamber. The cloud chamber was surrounded by Helmholtz coils, which produced a magnetic field that curved the electrons in one direction and the positrons in another direction. From the curvature in the known field, we were able to determine the energies quite precisely.

But cloud chamber observations were long and tedious—in Kellogg, at least. For many years, absorption measurements were used in parallel with cloud chambers, until the time that scintillation counters were discovered by Robert Hofstadter and were introduced into Kellogg by a graduate student, Alvin Tollestrup. Later on, when the germanium and silicon counters came into use, absorption measurements and cloud chamber measurements went by the board.

GREENBERG: Yes. You said elsewhere that the perfection of detectors in your time revolutionized the way physics is done.

FOWLER: Yes. Yes. It's just incredible. The increase in resolution plus increase in sensitivity that all of the new developments have brought has made physics—particularly spectroscopy, which always leads to fundamental knowledge—so much easier, so much quicker, that the time in how long it takes you to do some things that have been achieved in my lifetime in nuclear physics is shorter by many orders of magnitude.

An example is the thesis that my graduate student Robert Hall did. He was studying the production of nitrogen-13 by the bombardment of carbon-12 with protons. It's the first reaction in the CN [carbon-nitrogen] cycle, and one can detect the nitrogen-13 because it decays with the emission of positrons. Hall was measuring the rate of this production of nitrogen-13 at very low energies, trying to get close to the even lower energies that occur in stars, and the measurement part of his thesis research took him something like three years. I once calculated that he, in that whole time, detected something like 1,000 positrons from the nitrogen-13—1,000 in three years!

Now you can do 1,000 in a few microseconds. So it just shows the incredible advances that have been made in detection sensitivity, and in addition the resolution in energy of those positrons is incredibly better than it was for Bob Hall and me when we were doing the first work on the first reaction in [Hans] Bethe's CN cycle.

I don't want to imply at all that I played a role in these advances. In fact, I have to say that in the early days no one at Kellogg played a very great role. These developments were largely done elsewhere, but as they came along we took advantage of them. I think that's a rather interesting aspect of the history of the technical parts of experimental physics in Kellogg, and I don't know whether we want to talk about that now or a bit later.

GREENBERG: Well, we want to at some point.

FOWLER: The main point is that in the *very* early days Kellogg did play an important role. Charlie Lauritsen invented the Lauritsen electroscope as an all-purpose detector for X radiation, not only for his measurements of how many X rays the million-volt tube that he built was producing but also as a safety monitor so that people could measure how much dosage they got. So for that purpose he built a pocket-pen variety that had a clip on it so you could clip it to your shirt. It had a little chamber with the almost microscopic quartz fibers that formed the operating part of the electroscope. It also had a hole so you could hold it up to the light, and it had an eyepiece so you could read a scale against which the fiber moved.

The development of the Lauritsen electroscope was extremely important in the early days of both the X rays and the nuclear physics in Kellogg, because it was the way in which radiation was detected. When neutrons were discovered, all Lauritsen had to do was line the electroscope chamber with paraffin so that neutrons would eject protons out of the paraffin—which is part carbon, part hydrogen—and those protons then would produce ionization which the electroscope could detect, whereas the neutrons produced no ionization directly. It was an extremely useful detector in the early days, the early thirties. And although it never gained widespread use in other laboratories, it was used at Berkeley, and it was used by Merle Tuve and [Lawrence R.] Hafstad at the Department of Terrestrial Magnetism, in Washington. It played a role.

The next thing was the construction of cloud chambers, but that was just a follow-on to what Carl Anderson and Seth Neddermeyer were doing. I played a role in that, because Delsasso and I built the first cloud chamber with magnetic fields and all the necessary modifications for actually introducing a beam into the cloud chamber, striking a target, letting the radiation come out through a thin window, and then having a magnetic field to bend the electrons or positrons, so that quantitative measurements could be made.

At the same time, Tom Bonner came to Caltech from Rice Institute—now Rice University—and built a high-pressure hydrogen-filled chamber, so that when he produced neutrons, the neutrons could strike the protons in the hydrogen in the chamber. Then by looking at the length of the proton track and at the angle it made with the neutron beam, he could determine the protons' energy and ultimately the neutrons' energy. So we had a cloud chamber that could do gamma-ray spectroscopy by observing secondary electrons and positrons, and we had a cloud chamber that could do neutron spectroscopy by observing the secondary protons.

GREENBERG: Tom Lauritsen said you were a world expert on cloud chambers.

FOWLER: Well, I never was able to build a cloud chamber as good as Carl Anderson's. Carl had a magic touch, and although I talked to him and Neddermeyer many times, I don't know whether they held back some of their secrets from me—I doubt it—but anyhow, what we did was workable, and we had considerable success, and we used cloud chambers far longer than other laboratories did, just because we had ones that worked and that we were familiar with.

So although we played a role in the very early days through Charlie's invention of the Lauritsen electroscope and through our development of cloud chambers in a magnetic field and high pressure cloud chambers, once the use of scintillation counters and solid state counters came in, we played very little role in those developments and we just took what other people were doing. I think it's interesting to point out that it was a graduate student, Alvin Tollestrup, who introduced us to scintillation counters. Of course, I must say that when Charlie Barnes came to Kellogg, he had considerable knowledge of electronics and all the new counters, so he has always been a kind of leader in Kellogg in the use of modern detectors. But it was Tollestrup who first introduced them. Tollestrup has gone on to be probably the major person in the development of the big accelerators at the Fermilab. After he got his thesis in Kellogg, he went into the synchrotron lab and became a professor, but he was never greatly interested in teaching. As a user he went more and more to the Fermilab, and finally he decided to leave Caltech and is now one of the leaders in all their new developments of bigger and bigger accelerators.

GREENBERG: During the early thirties, at least, you built your own apparatus.

FOWLER: Yes.

GREENBERG: Was that unique to Kellogg? Was that standard practice all over the campus? Or was that just typical of the times, would you say?

FOWLER: I would say it was largely typical of the times for a number of reasons. One was that there was not much money available to purchase equipment, except for the mechanical vacuum pumps that were used—no one tried to construct the mechanical roughing pumps to get a rough vacuum. But, for example, the glass-walled mercury vapor pumps for getting high vacuum could either be purchased or could be made in the lab. And most schools had glass blowers who could blow the necessary pieces of glass to make mercury vapor vacuum pumps. So I think in general, most people made their own.

Then came the oil diffusion pumps, which used metal walls and could be made out of brass tubing and assembled in a shop. The glass blower wasn't needed any more. And, of course, it produced a much better vacuum. The oil diffusion pumps, which Charlie turned to just as soon as anything was known about them, were built either by Charlie himself or by Tommy or by one of the shop men. Charlie and Tommy made significant contributions to the development of the oil diffusion pump, but everybody was doing much the same thing in the other laboratories, too. Of course, nowadays you wouldn't think of building such pumps. They're produced, and we can get enough money in our grants to buy such things. There were two things: one, we didn't have very much money to buy equipment, and two, the oil diffusion pump and a whole series of such things came about largely because of needs in the laboratory. I don't know who developed the diffusion pump, but I know that once it was heard about, Berkeley began to make them, we began to make them, and the DTM began to make them, and I suspect they were being produced in Cavendish. But there was no place to buy them, although within a few years there were versions that you could purchase.

Begin Tape 2, Side 2

GREENBERG**:** How did Lauritsen go about building equipment for the lab?

FOWLER: Charlie didn't feel that a finished drawing was a necessary part of getting a piece of equipment made in the shop, even if one of the shop men was going to make it. He would make a sketch and then discuss it with the shop man, and usually between them the equipment would come out the way Charlie wanted. He always gave the shop man a great deal of latitude. In fact, that's why he was rather opposed to finished drawings; because it put a lot of constraints on the shop man that might not have been necessary. But Charlie also loved to go to the lathe himself. In fact, there was one small lathe in the lab that was "Charlie's lathe." He did a lot of small work there, and the larger work he usually got Tommy or me to do, or Delsasso or Crane. But whenever there were some really fine, small pieces to be put together, Charlie was the one who would do it. He got a lot of enjoyment out of that.

He not only worked at the lathe, he designed equipment. And I have to say that in the period when we *were* making a contribution to the technical end—not in detectors but in the construction of electrostatic analyzers and magnetic spectrometers, which was the area in which Kellogg made a real contribution in laboratory equipment—it was Lauritsen who had the design ideas and who made the initial sketches. Tommy Lauritsen, his son, who worked with him, took the initial sketches that Charlie made and transformed them into what you could call a finished drawing, because by that time these pieces of equipment were sophisticated enough that in order to get them made in the shop, more or less finished drawings were needed.

Charlie was really the one who had the initial ideas. For example, he had the idea of how a double-focusing magnetic spectrometer should be built. I'll have to say that I was never very adept at that. Tommy Lauritsen was very good, too, and he made enormous contributions in this regard, but there is no question in my mind—and if Tommy were still here I'm sure he'd say the same—that it was Charlie Lauritsen who, after the war, equipped the accelerators with the necessary equipment to make them really useful in nuclear experiments. He would sometimes ask us for our ideas on things, but he would very soon be able to talk us out of them, because he had the unique ability to see a complicated piece of equipment in three dimensions and be able to lay it out in such a way that we could make it, or the shop men could make it.

But, again, the situation was such that we were way ahead of any manufacturers. I think it's fair to say that there must have been a Charlie Lauritsen in all the laboratories, or a combination of people who added up to a Charlie Lauritsen. Because almost all of the nuclear physics laboratories first built analyzers, so that the projectile beams they produced could be analyzed in such a way as to have high resolution in energy, and then spectrometers, which took the products of nuclear reactions and made it possible to determine their spectra. These developments went along in parallel in all the laboratories. Eventually manufacturers found out about them, and in the long run, after three or four years, you could purchase equipment.

GREENBERG: And be satisfied with the manufacturer's product that it would do what you wanted?

FOWLER: Yes. But we continued to build our own, because our needs were always very specific and had to be adapted to just exactly what we wanted to do. The manufacturer always wanted to make something that would be a compromise between the demands of a lot of different people.

I can't emphasize too strongly that Charlie Lauritsen played a major and a unique part in that regard, and without him I don't think the role that we played in nuclear physics would have occurred. In fact, in later years, although Charlie had been thoroughly involved in the initial experiments that were done in Kellogg—especially those that were done with Dick Crane and then those that were done by Crane, Delsasso, Fowler, and Lauritsen—Charlie became more and more interested in the construction of new equipment in the laboratory, much more than in the experiments themselves. And of course he began to spend more and more time on the national scene, so he had less time for research in Kellogg. What time he did have, he spent in making sure that we got new devices with which we could do more things. So he really played an enormous role in Kellogg, right up until his death in 1968.

GREENBERG: I thought we might talk briefly about some of the earliest things done here in nuclear physics. I am curious about the artificial production of neutrons by Crane, Lauritsen, and [Andrzej] Soltan, and the publication of that accomplishment in the *Comptes Rendus* of the Paris Academy [197: 913 (1933)]. This is a minor point, but you've said elsewhere that publishing in France was one way to get in print quick. I noticed that two of the *Physical Review* papers on the artificial production of neutrons were dated earlier—a week or so—than the *Comptes Rendus* paper.

FOWLER: I was not actually here at the time when that happened, although I did overlap briefly with Soltan. It may have been, since Soltan came from Europe, that he wanted to publish these results in Europe, and in those days the place to publish nuclear physics was the *Comptes Rendus*, because that's were Curie and Joliot were publishing. So it may have been that that was the motivation. I recall jokes to the effect that one reason for sending the papers to the *Comptes Rendus* at the same time as sending them to *Physical Review* was to get a republication, and it may have been that it just didn't work out quite the way Crane and Lauritsen and Soltan thought it would. But the story I'd always heard was that they sent it to the *Comptes Rendus* in the hope that it would get published.

GREENBERG: In another instance it did turn out that way—the artificial radioactivity.

FOWLER: You see, Curie and Joliot found the neutrons, the radiation, even before Chadwick. But they interpreted it as electromagnetic radiation and had published this in the *Comptes Rendus*. Then Chadwick came along and showed that you just couldn't account for the properties of the radiation as electromagnetic radiation. He just flatly said this has got to be neutrons with roughly the same mass as the hydrogen nucleus, the proton, but with no charge. So it may have been that Lauritsen, Crane, and Soltan wanted to publish their results in France so that Curie and Joliot would know what they were doing. One can surmise all sorts of things as to what it was all about, but the story I've always heard was that they thought maybe they would beat Ernest Lawrence of Berkeley by coming out in the *Comptes Rendus* first. But actually they beat him by their publication in the *Physical Review*.

I think that the very next article after the Kellogg paper is a Berkeley paper. In subsequent developments, the Kellogg papers sometimes followed the Berkeley papers and vice versa. But in any case, Lauritsen, Crane, and Soltan first published a paper on the production of neutrons by using artificially accelerated particles. Actually, they just repeated Chadwick's experiment, but instead of using radioactive alpha particles, they accelerated the helium nuclei in their positive ion tube and bombarded beryllium and showed that they produced neutrons.

GREENBERG: I wanted to ask you about that, because elsewhere you had been talking about the use of the alpha particles as the first projectiles. You said that you weren't too sure about why they did it.

FOWLER: Well, of course, in hindsight it's easy to say why they had to do it. Because in all of the production of neutrons by protons, all of those reactions have thresholds. Only if the protons exceed usually a million volts or so are neutrons produced. With alpha particles, the reactions producing neutrons on some nuclei, like beryllium-9, have positive Q values, so there is no threshold. And the same is true of deuterons. So in order to make neutrons in the first instance, they had to use helium, because in the very early stages they didn't have any deuterium. So they did it with helium, and as soon as they got some deuterium from Gilbert Lewis, G. N. Lewis, then they were able to produce the neutrons much more copiously with deuterium. You don't produce neutrons with protons until you get well above at least a million volts or so, and that's because you always produce a radioactive nucleus, along with the neutron, that is heavier than the isotopic target nucleus you start with. So there's a threshold in the reaction.

Then, of course, the next thing was the production of artificial radioactivity, both beta minus and beta plus. There again, they published in *Science* [79: 234 (1934)] rather than in *Physical Review* in order to get ahead of Berkeley and the Department of Terrestrial Magnetism. So whether my anecdotes in this regard are exactly true, nonetheless there was no question, when I first came to Kellogg in 1933, there was still this feeling on the part of Lauritsen and Crane that they had to do their experiments and do them now and get them published in order to get priority over the two other laboratories in the country that were in the business.

GREENBERG: Do you know Ernie Lawrence's reaction to the first artificial production of neutrons here? Do you know what he said?

FOWLER: No.

GREENBERG: "The usual Caltech ballyhoo."

FOWLER: Yes, yes. That might very well have been the case, although Ernie was, in my book, the closest parallel to Millikan that one could imagine. He used and improved on all the tricks that Millikan had developed, obtaining funds and getting ballyhoo in the newspapers, and so forth and so on. And of course once the applicability of his cyclotron to nuclear research became clear to everybody, Ernie was largely in the driver's seat. He was not only doing nuclear physics but he produced an accelerator that other people could use, whereas the work that Charlie Lauritsen did here on the million-volt AC-powered positive ion tubes was just a dead end. And it was even a dead end here, because as soon as Ray [Raymond G.] Herb of Wisconsin invented the pressure-insulated electrostatic accelerator, that's the way we went.

That's one thing about Charlie, he had no reluctance at all, once someone had devised something better than he had, to change over immediately and fall into their footsteps. Charlie never got excited about cyclotrons, because our interests very soon turned, with the discovery of resonances, to doing experiments in which you needed ion beams with very high energy resolution so that you could study narrow resonances without introducing extraneous effects. The cyclotrons produced higher energies and higher currents than the electrostatic accelerators—often called Van de Graaffs, after their inventor, Robert Van de Graaff, although the types that are used are due to Ray Herb of Wisconsin. The cyclotrons produced much higher voltages and of course have gone on through all the developments to extremely high voltages and much higher currents, but they did not have the resolution that the electrostatic accelerator had. So when Lauritsen—and by that time I was involved and Tommy was involved—and I, when we had to make a decision as to which way to go, we opted for the electrostatic accelerator. I suppose really there was probably some feeling that, Aw, we weren't going to do things the way Ernie Lawrence did it, and that Charlie just didn't want to get himself indebted in any way to Ernie Lawrence.

There were also very good technical reasons. Our whole program was based, by that time, on measuring excitation curves, as we called them, cross section versus energy, and we needed high resolution. In addition to high resolution in the machine, we had to improve on that by building analyzers to give further resolution in the beams we were using as the projectiles. So Charlie went into the business of building electrostatic and magnetic analyzers. Then we needed high resolution in detecting the energy of the outgoing particles, and Charlie built high-precision magnetic double-focusing spectrometers for that purpose.

He had the ideas, in large part, but one of the big aspects of building that equipment in the laboratory was that the graduate students could take part in the construction of the apparatus that they were going to use. Conway Snyder, Sylvan Rubin, all of that early generation learned a great deal about practical physics by helping to construct, in large or small roles, the equipment that they were going to use in their experiments. That's a thing I haven't emphasized. That was another reason why we, and in particular Charlie Lauritsen, wanted to build equipment in our own shop—because the graduate students could then play a role. They may not have made the heavy parts, but there was always some part of what was being built that they could actually go to the lathe and build themselves. They could also follow what the shop men were doing, you see. So it was excellent practical training for the graduate students to build their own equipment in the laboratory.

GREENBERG: Do you lose a great deal if you go out and buy manufactured things?

FOWLER: You'll find considerable debate about that nowadays. There wasn't much argument to the contrary in the early days, and in fact up to the sixties. Now the experimental equipment has become so sophisticated and complicated that there is some question as to whether a graduate student should take the time necessary to construct such equipment. The general tendency nowadays, especially in other laboratories, is to purchase practically everything and to have the designs done by a technical staff. Berkeley Radiation Lab has an engineering staff. Fermilab, SLAC [Stanford Linear Accelerator Center], all have very large engineering staffs, and so that part of it has disappeared from the training of the student. Since he will probably be employed in a laboratory that has an engineering staff, there is really not all that much need for it.

You must remember that in the forties and fifties, and even into the sixties, the university laboratories just did not have big engineering staffs. Berkeley, of course, was a contrary example; Berkeley Radiation Laboratory used an engineering staff just because they were by that time operating on machines so big that they just had to. But it didn't hurt, even for a graduate student who was going to get a job at Berkeley, to know something about machinery. It has changed—it has changed. It's just inevitable that it should change, especially in elementary-particle high-energy physics. The users of the big laboratories nowadays—the users at SLAC, the users at Fermilab—usually construct some part of the equipment themselves that they are going to use. There's the accelerator already there that brings a beam to them; analyzers are already there. But they usually have something that they do themselves, especially in building detectors, and graduate students play a role in that.

It's the same in space physics. The people who are going to put stuff on a satellite—Ed [Edward C.] Stone's group here—build equipment of their own. So I shouldn't say that it's gone entirely. But there's not nearly the emphasis on it that there was in those early days. It meant that you got training in Kellogg and in other places in those times whereby you could go to another place and take on a job where you were essentially hired not only to teach but to build a new laboratory in nuclear physics. So having practical knowledge was extremely important.

GREENBERG: One more question about Lawrence. I gather he didn't really do much nuclear physics in the thirties.

FOWLER: Well, I wasn't there. My impression is that Ernie did not take a very active part in the actual research experimentation, although he stimulated much research and had unique ideas on what research to do. He had to spend an awful lot of time just getting the necessary funds. And you must remember that Berkeley Radiation Lab was the first big, big laboratory and he was the director. So he had a million and one things going on.

The main thing about Ernie Lawrence was that he inspired very, very good people and commanded their loyalties, and they worked hard. And he, of course, by doing the money raising, made it possible for them to do what they wanted to do. But he certainly picked good people—Ed [Edwin M.] McMillan, Luis Alvarez, Don Cooksey, who had so much to do with the actual administration of the laboratory.

I think it's fair to say that Ernie didn't spend as much time in the laboratory directly on experiments as, for example, Charlie Lauritsen did. It's very true that toward the end, as Charlie got more and more involved on the national scene, he confined his part to doing things on the construction and design of equipment, but in the thirties—I think that's what we were talking about—Charlie was in the lab every day with Dick Crane when I got here and Charlie had finally returned from Denmark. He and Dick worked day and night, and I'm pretty sure that Ernie was never involved in any one experiment to that degree—at least that's what I've heard. I visited Berkeley quite frequently and the men you saw actually working with the equipment were Ed

McMillan or Luis Alvarez or Franz Kurie or Robert Wilson. Ernie would run through and ask how things were going and say cheerful things and everyone would feel better. But that's not to denigrate Ernie Lawrence. He had a different job. He built the prototype of what the modern physics lab is, so you've got to give him full credit. The only thing that he got really involved in scientifically was a thing on which he came a cropper—namely, the production of neutrons by deuterons. He had some pretty wild ideas about that, but that's another story.

GREENBERG: What were the relations among the early labs—yours and Lawrence's and Tuve's? Maybe there were some others.

FOWLER: The very early ones were those three. Very soon Johnny Williams at Minnesota came along, and Sam Allison at Chicago. There were other places—oh, and of course Ray Herb at Wisconsin. So the monopoly of the Pasadena/Washington /Berkeley axis really lasted till just before the war. These other places started to begin to play a role. Oh, and then there was a group at Yale, too.

GREENBERG: And the relations were amicable as a whole?

FOWLER: Well, that was my feeling. There was always a lot of argument. In fact, one of my very first recollections is going to Berkeley to a meeting of the American Physical Society and hearing Merle Tuve's voice come out of the distance, shouting at Charlie that Charlie was all wrong about the resonance, about the production of nitrogen-13 by a bombardment of carbon-12 with protons. And Merle just said, "Charlie, you're crazy. You've got deuterium contamination in your beam." And Charlie knew that sure, he had contamination in the beam, but the excitation curve as you increased the energy in the protons was entirely different from that for the excitation curve for deuterons. So Charlie knew he was right, and of course he was wise enough to know that his AC tubes smeared things out, and that if you took that into account, what he was seeing was a sharp rise and fall in the cross section, similar to what was being found by [Enrico] Fermi with neutrons.

GREENBERG: I want to talk a little bit about the long sequence of papers on gamma rays, with which you were very much involved. Your thesis was part of that sequence, wasn't it?

FOWLER: No. My thesis was on the production of radioactivity ["Radioactive elements of a low atomic number" (1936)], and we didn't care about the gamma rays that were produced at the same time. In fact, we mainly used deuterium as the projectile and produced the radioactivity along with either a proton going off or a neutron going off. If a neutron went off, we produced a positron emitter; if a proton went off, we produced a negatron emitter. So that's what my thesis was about—the production of radioactive elements of low atomic number, carbon-11, nitrogen-13, oxygen-15, and fluorine-17. And out of that, due to Robert Oppenheimer and Robert Serber's help, we were the first to come to the conclusion that the nuclear forces were charge symmetrical on the basis of experiment. That was very nice and very fundamental.

Then there was a long series of papers. Well, I shouldn't say "then," because it was almost simultaneous. In fact, those papers probably preceded my thesis. The series of papers was by Crane, Delsasso, Fowler, and Lauritsen [six papers in *Phys. Rev.*, 1935, vols. 47 & 48.]. You'll notice the names are alphabetical. Crane and Lauritsen had found that when they bombarded various targets with protons they produced gamma radiation. They used absorption measurements to get rough ideas as to what the energy of this gamma radiation was. But that's a very crude way to do things, and that's why Delsasso and I were assigned the problem of building a cloud chamber, so that we could study the gamma radiation by the secondaries that it produced in a thin sheet of metal in the cloud chamber. It was surrounded by a magnetic field so that the electron and positron secondaries would be bent in the field and their energy could be measured.

Many of the results of that long series of papers were just wrong. The reason for that was that we looked first of all at the secondary Compton electrons produced by the gamma rays. In fact, in the very earliest instance we just let the gamma rays produce the secondaries in the glass wall of the cloud chamber. Now, the glass wall was about a quarter of an inch thick, so a secondary Compton electron produced on the outer edge of that wall lost a lot of energy in getting through the quarter-inch into the cloud chamber. So we had very poor resolution. In fact, the secondaries produced by a number of gamma rays—and there usually were a number of gamma rays produced in any reaction—were almost a continuum, and our early results were merely statistical fluctuations in that continuum. We identified these ups and downs, the ups as gamma ray lines. And there is no question that we published purported spectra that were just wrong.

It took us some time to realize that we were going in the wrong direction, and the first thing we did was put a thin foil in the cloud chamber so we could see that the secondaries were produced in that foil and the foil was thin enough that they didn't lose much energy. We could observe the angle that the secondary electron made with the line back to the target. One only wants to count a Compton electron that goes in the forward direction, because otherwise it doesn't have the full energy of the gamma ray, and it fools you. So we made that improvement. Even so, when a photon produces a Compton electron, the photon becomes a degraded energy photon, which you don't see. So you don't get all the energy of the gamma ray into the secondary Compton electron. So that didn't work out very well.

Finally we began to notice that the gamma rays were producing electron-positron pairs in the cloud chamber. Those electron-positron pairs, if you count their rest-mass energy, get the full energy of the gamma radiation, and so you have a high-resolution technique, looking at the energy of the electron-positron pair for the gamma radiation spectra.

WILLIAM A. FOWLER

SESSION 3

May 31, 1983

Begin Tape 3, Side 1

GREENBERG: Last time we were in the midst of the series of papers on the gamma rays, and you were talking about some errors that were in the work. Could you recapitulate basically what the achievement amounted to when you got the whole thing sorted out?

FOWLER: Those papers were almost all, but not entirely, devoted to the study of gamma rays from the bombardment of the light nuclei with protons. From the study of such gamma rays, one learns about the excited states of the light nuclei. If you do a lot of detailed work, which we did not do, you can get a great deal of information about the properties of those excited states. This was all motivated by our belief at the time that if you could learn everything there was to be learned about the excited states of the light nuclei, you would solve the basic problem of what was the nature of the nuclear force—the force between protons and neutrons.

We now know, of course, that the problem is much more complicated than that—that the light nuclei, even though they may only contain ten to twenty nucleons, are still a many-body problem and have a very complicated spectroscopy, which goes along with a complicated set of levels or excited states. We know that it's a much more complicated problem and that in large measure it's not at all as fundamental as the excited states that were found in the hydrogen atom, which were due to the electromagnetic interaction of electrons and protons.

I think it's interesting that largely from the great success that atomic spectroscopy had had in elucidating the nature of atoms—the electrons surrounding nuclei—we thought that the same results would come from looking at the nuclear spectroscopy of the light nuclei. And, of course, you do learn a lot, but you don't learn nearly as much, in a fundamental sense, as you do in atomic spectroscopy. So those papers were an attempt, using the crude—by modern standards—techniques that we had available then, to elucidate the spectroscopy of the light nuclei.

My main interest, at least—and I think that of the other people, Crane, Delsasso, and Lauritsen—was in establishing a technique whereby we could get high enough resolution to determine the nuclear gamma rays' lines with some certainty. At first we let the gamma rays produced by bombarding a target eject Compton electrons from a thin aluminum or lead foil placed in our cloud chamber, which had a magnetic field around it in order to bend the electrons and thus make it possible to determine their momentum and energy. We concentrated on looking at the single Compton electrons, but, as is well known, when a gamma ray produces a Compton electron, it doesn't give all the energy to the Compton electron. There is a scattered gamma ray which takes away some energy. So that and other reasons meant that we got essentially a continuum of electrons from every gamma ray, and a great deal of what we did was look at this continuous spectrum, which, because of the bad statistics [laughter], had rises and falls in it. And we misinterpreted, right in the beginning, some of the peaks as gamma ray lines. The way we solved this was eventually to look at the electron-positron pairs produced in these secondary producing foils in the cloud chamber. There is the advantage that the electron and positron in a pair get the full energy of the gamma ray minus two electron rest masses in energy equivalents—roughly 1 million volts. We found that going to the measurement of the electron-positron pairs gave us much simpler spectra and, looking back on it, gave us essentially, within the accuracy of our methods at that time, the right answer. But I think the interesting thing is that we thought we were going to solve all the problems of the nucleus! We were just convinced! So that's why we made such a lengthy and systematic study of these gamma rays.

GREENBERG: With that in mind, let's turn to another discovery here—l934, the discovery of proton capture and the associated resonance effects.

FOWLER: I was not very much involved. That was primarily the work of Dick Crane, who was getting his PhD under Charlie Lauritsen. What they found was that when they bombarded carbon with deuterium and looked at the neutrons that were produced, or the gamma rays that were produced, and then looked at the excitation curve—that is, the yield of any particular reaction versus energy—there was a smoothly rising curve that was essentially a measure of the fact that as they increased the energy, the deuterium projectile was able to penetrate the electromagnetic barrier, the Coulomb barrier of the carbon-12, that much better.

Now, you must remember that they were using a tube powered by an AC transformer, so that projectiles were accelerated during the full part of the AC cycle, and thus they had a continuum of bombarding energies. On the other hand, when they looked at the results of the bombardment of carbon-12 plus protons, it didn't look at all like the curve they got when they bombarded carbon-12 with deuterium. The curve actually did rise, but it had kind of a bump in it, and the only possible interpretation that they could make was that if they had had high enough resolution, they would have seen a sharp rise *and fall,* which came to be called a resonance, in the excitation curve for carbon-12 plus protons. And of course that subsequently turned out to be the case.

Now, there was a great deal of skepticism about this, because both Lawrence and his group at Berkeley and Tuve and Hafstad at the Department of Terrestrial Magnetism thought that what Charlie and Dick Crane were actually detecting when they bombarded carbon with protons were reactions produced by the small amount of deuterium that occurred in the ordinary water from which they actually produced their hydrogen. But Charlie and Dick were convinced that they had seen something from carbon-12 plus protons, and they said that there was an essential difference in the excitation curves.

They found the same results when they bombarded the heavy isotope of lithium—lithium-7—with protons, where the alpha particles produced in that reaction showed a nice, smooth monotonically rising curve, whereas the gamma rays from that reaction showed a bump again. And further work soon showed that there was a resonance in the lithium-7-plus-protons-producing-gamma-rays reaction.

The detailed data that showed the resonances in all their beauty, the rise and then a drop in the yield, were primarily the work of Tuve and Hafstad at DTM. Once Tuve had been convinced that Charlie was right—that there was something coming from carbon-12 plus protons and that it had a strange excitation curve—he and Hafstad repeated the work with their open-air Van de Graaff machine with much higher resolution than Crane and Lauritsen had. So they were able to really get resonance curves. But I think it's fair to say that the original idea was due to Crane and Lauritsen.

How much they knew about the resonances that Fermi had found in the bombardment of nuclei by neutrons, I don't know. They probably did know about that, and certainly the general belief in nuclear physics is that Fermi and company in Rome discovered resonances in nuclei using neutrons and detecting the gamma rays produced by the neutron capture. But it's very clear that Crane and Lauritsen were the first ones to do it with charged particles, although one has to be a little careful about that, because I think there is a reference in Rutherford, Chadwick,

& Ellis to some strange excitation curves when boron-10 was bombarded by natural alpha particles. I don't think they explicitly mentioned the term "resonance." It would be interesting to look that up, because I have a recollection that the work—it was not the work done in the Cavendish; it was work before Cockcroft and Walton—was done somewhere in Europe. So the whole business of getting excitation curves that are not smooth and merely a reflection of increased penetration through the Coulomb barrier I would say is even older than the work that Fermi and his colleagues did in Rome.

The main thing was that the work by Fermi was so beautiful and so explicit. You see, neutrons have no Coulomb barrier, so at lower energies where narrow resonances occur, they just got beautiful results. They also showed that with neutrons, the tail of those resonances going down to low energies and into the thermal region gives enormous cross section for thermal capture. That's the $1 / v$ law superimposed on the resonance expression so that at thermal energies where the relative velocity, v, is very small, the cross sections were even larger than they were at resonance.

GREENBERG: These kinds of measurements did go on and become a big part of the enterprise here, right? And the story began here, with these particular papers?

FOWLER: Yes. Lauritsen saw that Tuve and Hafstad, and probably Minnesota by that time, and perhaps even Wisconsin, were able to get much better evidence for these resonances, which, again, you must remember are excited states in the compound nuclei and thus another way of learning about the spectroscopy of nuclei.

GREENBERG: Are the gamma ray papers all part of the same game plan, to determine the nuclear forces from excited states of nuclei?

FOWLER: Crane and Lauritsen found these resonances in their very first investigations of bombarding nuclei with protons, deuterons, and alpha particles. Then it became clear that this was a fruitful field to look for not only the resonances in the compound nucleus; they also looked for the transitions as the compound nucleus cascaded from its excited state that was produced in the lab—cascaded through lower excited states down to the ground state. That's probably one of the reasons why Charlie had Delsasso and me build the cloud chamber—so we could do that part of it.

He then became interested in getting better accelerating equipment than he had using the AC transformer. That's why, in about 1938, he asked Tommy Lauritsen and me to build an electrostatic accelerator. I suspect the reason it took so long—if you count four years as long—was the fact that the Van de Graaffs, the open-air accelerators, just didn't work. Tuve and Hafstad really performed a miracle in getting the results they did using an open-air accelerator, which was always breaking down and couldn't be used on a wet, damp day.

The big contribution was that of Ray Herb at Wisconsin, who put an electrostatic accelerator into a pressure vessel, where he could control the conditions in the gaseous medium around the high-potential electrodes, and he essentially solved the problem. Once Herb had shown that the idea worked—you know, he first tried to evacuate the vessel and to operate the electrostatic accelerator in a vacuum. Well, that just turned out to make things worse. If you could get a perfect vacuum, fine—but of course you can't. And then he pressurized his vessels and everything just fell in place. I think Herb must have been doing that in 1936 or 1937, but by 1938 certainly his success was well known.

I made a visit to Herb's laboratory around 1938. Tommy Lauritsen also visited Herb's laboratory. Anyhow, we borrowed Herb's idea. We made one change. His accelerator had been horizontal, and there were all kinds of problems in building a horizontal tube where you can only support it from one end. So Lauritsen, who had had a great deal of experience with vertical tubes, decided to build a vertical Herb-type accelerator. I would guess that we were the first ones, certainly among the very first ones, to build vertical Herb-type electrostatic accelerators. And once that was done, then we went back to spending a lot of time doing excitation curves. But as I remember, we continued to produce radioactivity and to study the radioactivity of light nuclei in addition.

GREENBERG: Before you built the first Herb-type Van de Graaff, you still tried doing these measurements of excitation curves?

FOWLER: Oh, sure. There wasn't anything else to do. That was the accelerator we had. I am pretty sure that Charlie Lauritsen realized that the way he was accelerating particles was never going to be the answer to what was really needed. He exploited what he had available—a million-volt transformer set which could produce high-energy particles at the peak of the AC cycle. He exploited it to the hilt, primarily so that Kellogg and Caltech could stay in the business.

I think it's fair to say that a great deal of Ernie Lawrence's motivation in building the cyclotron is because he was convinced that that was the way to go to get to higher and higher energies, and that it was something that Berkeley could export. As a consequence, because he was right—although again I think it's fair to say Berkeley didn't do all that much more physics than we did, or than Tuve and Hafstad did, or than Ray Herb eventually did, or Johnny Williams at Minnesota eventually did. Nonetheless, and rightly so, Berkeley became known as the nuclear physics capital of the world. Well, you can see it in the talk that Ed McMillan gave at the symposium on nuclear physics at Minnesota a few years ago [May 1977]. Ed admits that the great thing Berkeley did was construct a tool that other people could copy and get into the business. So in Europe, in particular, Berkeley was well known. Kellogg and the rest of the places were known to people who were interested in the specific things we were doing, but the work that Berkeley had done—the technological development of a marvelous tool—had, quite rightly, much more interest.

GREENBERG: Did Tuve and Hafstad and Herb concentrate on very similar sorts of things to what you were doing? Did they have low-energy light-element labs doing quantitative studies?

FOWLER: To a certain extent. They also did something that we didn't do. At both places—Herb was the most successful—they came to believe that the way to find out about the nature of the nuclear force was not to spend all this time looking at the excited states of the light nuclei but to actually study the one thing they could study: the interaction between two protons—that is, bombard a hydrogen target with protons and look at what one calls the proton-proton scattering. Now, at low energy that scattering is just a reflection of the fact that the two protons are charged, and you get Rutherford scattering, which falls off very rapidly with energy. But eventually, if you go to high enough energy, and a million electron volts is plenty, you get deviations from the Rutherford scattering which tell you that the two protons are beginning to feel the nuclear force between them, which is attractive at first, rather than repulsive as the Coulomb energy is. So

Tuve and Hafstad began bombarding protons with protons and, with other participants, they found the first evidence for the so-called anomalous scattering of protons by protons. But that's neither here nor there, because the really fine work was done by Herb, and in that he was guided by Gregory Breit. Breit was the theoretician who was, I think, the most influential in getting Herb to look at proton-proton scattering.

It gave us a great deal of information, and there was high hope that the whole nuclear problem would be solved. In my thesis, we showed that the neutron-neutron force had to be equivalent to the proton-proton force, and by that time it was realized that the force between a proton and a neutron could be different when the proton and neutron had their spins parallel. If one has two protons or two neutrons in the simplest *S*-wave or zero orbital angular momentum interaction, their spins have to be antiparallel by the Pauli principle. In the ground state of the deuteron, the proton spin and the neutron spin are parallel; there's an excited state that's unstable—that's virtual, if you wish—which is the counterpart of the also virtual states of two protons and two neutrons.

Unfortunately, in a sense, not even that solved the whole problem. Because even though you know the two-body forces between two nucleons, if you put them into a nucleus where they are practically touching, you have three-body forces, at least. The whole nuclear problem is a many-body problem; it's the worst kind of a many-body problem. Many-body problems where you have an enormous number of bodies—like in solid state physics, in metals—aren't too hard to handle.

So the dream of solving all the problems of nuclear physics either by looking at excited states or by looking at proton-proton scattering never came to fulfillment. Nonetheless, Herb at Wisconsin and others—I know Johnny Williams at Minnesota did a great deal—did some beautiful work on the interaction of two protons. And of course that became a very fashionable trend in nuclear physics. Bethe worked on the problems. Someone invented the concept of effective scattering lengths, and the literature was full of nice theoretical treatments of the scattering of nucleons by another nucleon due to the nuclear forces, and certainly in a phenomenological way we understand those two-body nuclear forces with great precision.

It led to [Hideki] Yukawa's theory of short-range forces, because these nucleon-nucleon forces were short range, quite different than the long-range electromagnetic forces or gravitational forces that physicists had always been used to. So it was a very exciting period.

Yukawa attributed the short range to the exchange of a quantum that had a mass—in contrast to, in the Coulomb force, the two interacting particles' exchange of photons, which have zero mass. That leads to the long range $1 / r^2$ force, whereas Yukawa showed that if the quantum being exchanged had a mass, then you got a range which was essentially the Compton wavelength of that mass. So the people who went off into basic nucleon scattering opened an enormous field. They didn't solve the problem they thought they were going to solve, but they started a great deal of exciting physics.

GREENBERG: And at higher energies, where Kellogg did not follow.

FOWLER: We played no role in that, because we decided not to go to high energies, and that was that.

GREENBERG: The kind of nuclear physics you've been talking about up till now has been described by some of your colleagues as fundamental nuclear physics, just trying to dope out the nuclear forces from various kinds of experiments—the kind that you were doing, and also the proton-proton scattering experiments. I gather that things like the compound model of the nucleus represent a different approach, where there is less fundamental input. At least these colleagues who talk about the fundamental nuclear physics make a distinction between that and [Niels] Bohr's introduction of the compound nucleus in 1936 or whenever it was.

FOWLER: Well, you see, it was Bohr who essentially introduced and used the idea that the nucleus consisted of many bodies. And when you bombarded a nucleus with another nucleon, the nucleon entered into the target nucleus and formed what Bohr was the first one to call a compound nucleus, shared its energy with the other nucleons, and thus left the compound nucleus that had been formed in an excited state. Of course, the nucleon that was put in had only a very small chance of coming back out with the energy it went in with; thus, the compound nucleus lived a long time, and by the uncertainty principle, relating the uncertainty in energy and the uncertainty in time of a quantum system, you can show that if it lived a long time, then the uncertainty in energy was small and the resonances were sharp.

GREENBERG: It explained the resonances? .

FOWLER: Well, Bohr was the one who understood why the resonances could be sharp and at the same time you could have a background or a continuum. You must remember, these resonances are always superimposed on a continuum, which was much easier to understand in terms of previous concepts. Even Oppenheimer had serious reservations about the work that Crane and Lauritsen found, although it was Tuve and Hafstad who showed how sharp these resonances were. But Crane and Lauritsen's work implied that the resonances were sharp. That tells you that the systems they were producing lived a long time. That was completely foreign to the ideas essentially based on atomic spectroscopy and the application of quantum mechanics to atomic spectroscopy.

Bohr then showed that if you put a compound nucleus together and put a particle in, it has to share energy with dozens of other particles by bumping into them in there and knocking them on with energy. The chance that it's going to get that energy back and come out is small, so it's going to take a long time; that means that the energies are going to be sharp. We didn't see resonances in the interaction of two nucleons at the energies we had in those days, but if you go to high enough energies, then you actually begin to see the production of excited states of the nucleon. And there's a whole elementary-particle, high-energy physics spectroscopy. But the interaction, as far as we could study it between two protons, was a continuum, but not the continuum that Rutherford expected on the basis of Rutherford scatter. It was nonetheless nice and smooth, but the minute you put more than two of these nucleons together and formed a many-body nucleus, then you could get sharp resonances in the interaction. So the only sense in which the Bohr theory isn't fundamental is that it's a complicated interaction between many particles.

GREENBERG: Yes. Well, I think the idea was to contrast it with the Yukawa theory, which fit in more along the lines of what you were doing—a different sort of an approach.

FOWLER: The Yukawa theory of the basic interaction between two nucleons is, in a sense, much more fundamental than Bohr's theory of the formation of the compound nucleus and the long time that it takes it to decay, and the fact then that the excitation curves in the reactions that you study can show sharp resonances. But I would say that's in part a matter of taste. [Laughter]

GREENBERG: When did you stop thinking that you could do what you thought it was you could do in the early thirties?

FOWLER: You know, I'm not sure that I, at least, was convinced that it was hopeless in the thirties. My recollection is that even after the war, when we came back to go into nuclear physics, I had a feeling that there was still some residual hope that by doing a better job on the energy levels of the light nuclei that some very fundamental knowledge of the interaction of nucleons could thus be found.

Nowadays, starting with the interactions between two nucleons, and including three nucleon forces, you can construct a potential—with as much detail as you want, although the theory gets pretty hairy and takes a lot of computer time to work out—you can construct, from the basic nucleon-nucleon interaction, what a nucleon experiences inside a nucleus where it interacts with many nucleons. But that's kind of backwards, you see. What we thought was, we could go from the excited states to these basic forces. Now, knowing as much as we do about the basic forces, people can predict where the excited states ought to be and what the properties ought to be. Now, quantitatively, in the sense of getting the exact energies, they don't do very well. What they usually do nowadays is use a theory that has free parameters in it. And they adjust those free parameters to fit the known energies of the excited states and then they use that same theory, which has been calibrated to fit the known energies, and at least get something right to predict many other properties of the nucleus. So in a sense the idea and the kind of motivation that we had wasn't all that wrong. But it's worked out kind of in the opposite direction.

Begin Tape 3, Side 2

FOWLER**:** If you want the way in which nuclear forces most obviously differ from all the other forces that we have known about in nature—the gravitational force, the electric force, and the magnetic force go essentially as $1 / r^2$, where r is the distance between the two interacting bodies, and here along comes a force which we knew had to be short-range. Yukawa gave an explanation that it would have the hr^2 behavior, multiplied by a decreasing exponential, which if you got to large r, essentially meant the force was zero. So Yukawa's discovery was a very exciting one.

The discovery of the muon by Anderson and Neddermeyer was terribly exciting in cosmic rays. It explained a great deal of the properties of the cosmic rays, because these darn things were penetrating. And of course that was the big puzzle. How could these intermediate mass particles be the Yukawa particles which produced the strong nuclear forces if they were so penetrating? The answer was that the muon wasn't the Yukawa particle. We still don't know what the muon is. The best you can say is that it's a heavy electron.

The meson that [Cecil] Powell discovered [1947] turned out to be Yukawa's quantum, and then, as you know, there is not only one type of meson exchanged but there are even heavier mesons that have subsequently been discovered. And one can, with all of the various mesons that have been discovered, give a quite accurate description of the interaction between two nucleons.

It did not change, as far as I remember, what we were doing. We thought, "Ah, this is very exciting, and it's telling us a great deal about the nuclear forces, and it's going to make our job of really getting all of the facts much easier." So I don't think any of those things changed our feeling that we were on the way to essentially elucidating the nature of the nuclear interaction by looking at the excited states of the light nuclei. In a large measure, we were just wrong. But the work has had other implications, and it still contributes a great deal to our knowledge of nuclei—useful knowledge in particular when we begin applying nuclear physics to a field like energy generation in stars, in astronomy, or nucleosynthesis, the building up of the elements in stars.

GREENBERG: Were you interested in energies higher than what you were able to do in the laboratory at the time?

FOWLER: We realized that there was a field of endeavor in which higher energy was needed and that Ernie Lawrence had shown how to go to higher energies. I am pretty sure that Ernie, talking to both Millikan and Lauritsen, said, "Oh, get out of this low-energy field now that the war is over. The thing to do is to go into high-energy physics." Lauritsen responded to that in a way—although he and I and Tommy Lauritsen decided that we would stay in low-energy physics in Kellogg. Charlie Lauritsen responded, and he got me to help, by telling Millikan and DuBridge that Caltech should go into high-energy physics. And, of course, the final upshot was the building of the Caltech synchrotron. Charlie Lauritsen brought Robert Langmuir here from Schenectady, because Langmuir had built a 60- or 70-million volt synchrotron at GE. Then [Robert F.] Bacher came, and Robert Walker came and Tollestrup, one of our graduate students, went to work for them. I don't want to take any of the credit for the Caltech effort in that field away from Bacher and Walker and Langmuir and Tollestrup and others. But I think it's fair to say that it was Charlie who started the basic thing. It was because he realized that Kellogg wasn't going to do it and that if Caltech was to really participate to the full in the prospects of nuclear physics, then it had to have a high-energy machine.

You see, by that time Charlie was a little older. He was so thoroughly involved in the politics of science in Washington that he wasn't prepared—although he loved doing such things—to design, build, and construct a great big new machine. I had no interest in doing it; in fact, I would not have been able to do it, because I just didn't have the capability that Charlie had in that regard. And by the time the war was over, I was so thoroughly convinced that the way to go was in low-energy physics, and its application to what Bethe had shown us was such an interesting thing—namely, the operation of nuclear reactions in the sun and other stars.

GREENBERG: I noticed, in looking over some of the correspondence you gave me, some between C. C. Lauritsen and Millikan in 1938, in which they talk about possible 5-MeV installations. I checked it out, and at that time Lawrence's 37-inch cyclotron produced 4.8-MeV deuterons. I was wondering whether you were at that point toying with the idea of going on into higher energies. This was around 1938.

FOWLER: Charlie Lauritsen may have toyed with the idea, but I can't believe that he ever took it too seriously, because Lawrence's progress was so fast that it was clear that Lawrence—well, maybe he was stuck for a little while at 4.8 MeV—was a dedicated man who was going to go on to higher energies. If Charlie had built a tube that would withstand 5 million volts, he knew that was getting pretty close to the end. And in fact he was right. Now there are electrostatic accelerators that go, I guess, to about *25* million volts in energy, but they're *enormous* structures, and they haven't been all that successful, actually, because people have learned to get high-resolution beams out of cyclotrons with various modifications. So what used to be the great advantage of the electrostatic accelerator—that when it does produce a beam it produces it with fairly high resolution in energy—has been overcome in the circular machines.

I'm sure Millikan would have been overjoyed if Lauritsen could have developed a tube of his type that would have been competitive to Lawrence's cyclotron. I can say that to my recollection, Charlie never discussed it seriously with me. But as I've said to you before, he never discussed with any of us a great number of things that he perforce had to discuss with Millikan.

GREENBERG: Were you aware of Millikan's interest in nuclear physics or in cyclotrons? Where did he figure in the nuclear physics in the thirties, anyway?

FOWLER: Well now, when was it? When did he bring [Arno] Brasch here, and Lewis Strauss? Was that before the war? Must have been. Well, my recollection is a little faulty. Millikan was very much impressed by Lawrence's being able to go to higher energies. And the one thing I do recall is that he brought Brasch here from Switzerland. Brasch and [Fritz] Lange in Switzerland had strung insulated cables between peaks in the Alps and were using lightning to develop very high energies. Brasch had been trying to develop a tube that would withstand the many millions of volts that you can pick up from a lightning discharge.

Millikan got Lewis Strauss, who later on became chairman of the Atomic Energy Commission, interested. So it had to be before the war. And Brasch was brought here, and Strauss was going to provide the money to set Brasch up in operation. I remember Millikan had to go to the clinic in Rochester, and Charlie was away in Europe, so I had to try to take care of Brasch. We actually started building a tube for him. It became very clear to me that he was not a very good experimentalist, and he didn't have any really very good ideas. I remember when Charlie came back, I said, "Look, we've got more to do than work with this fellow, no matter how much money might be coming out of it."

GREENBERG: I think Brasch may be tied in with that correspondence I mentioned before, about the big voltage installations.

FOWLER: Yes, yes. Well, Millikan certainly had dreams that Charlie could build a tube that would withstand—a single-ended tube, or double-ended—voltages that were comparable to what

Ernie Lawrence had in mind. But I'm pretty certain that Charlie never took it all that seriously.

GREENBERG: In 1938 you were in big demand. You had a lot of offers.

FOWLER: Oh, yes.

GREENBERG: For example, you could have gone to the University of Illinois, where they were building a cyclotron. But you stayed here.

FOWLER: Well, I liked it here. And my relation, frankly, with Lauritsen was such that I would have been very reluctant to give up my collaboration. And, John, this was in large measure because I realized that although I could do some things well, I did not have the special ability to design and build any new equipment. And all of the offers that I had, as I remember, pretty much assumed that I would come and start a nuclear laboratory, or at least participate in the construction of a nuclear laboratory in the various places. And I realized that that wasn't what I did well. That wasn't what I had been contributing primarily in the work here with Charlie Lauritsen and Tommy Lauritsen, although I did what I could at the drafting board and the lathe. So I was reluctant, in a sense, to take a chance. With Charlie and Tommy Lauritsen as essentially the leaders in building new facilities in the laboratory, I could help. And I did help. But it took them to take the initiative in practically everything that we did. Once equipment was built and running, I think it's fair to say that I took the major initiative in what we were going to do with our accelerators and with our detectors, and so forth and so on.

GREENBERG: You've said that Charlie Lauritsen was happiest when in the middle of an experiment something broke or you needed a new little gadget.

FOWLER: [Laughter] Yes. Yes.

GREENBERG: Was he the experimentalist's experimentalist?

FOWLER: Well, I've always thought so. There's no doubt that Charlie got very little pleasure out of performing experiments—especially the dull parts of writing down in a notebook all of the numbers. See, in those days we didn't have computers that printed out everything you did. You had to stop, read instruments, record it all, and Charlie did very little of that. I do have a few notebooks in which I can tell his handwriting, where he made columns and tables and took down the numbers. But in the main, I did most of that. What Charlie enjoyed was keeping the experiment going—or improving. Charlie would see me laboring away, and he'd find some way that we could do things a little easier. But I don't want to give the impression that that's all there was to Charlie Lauritsen. He was, it's true, an experimentalist's experimentalist. He was just superb at that. But you must remember that he had a superb knowledge of classical physics and the applications of classical physics. He knew mechanics; he knew electricity and magnetism; he knew how to use them.

What I'm trying to say is that Charlie wasn't just a technician, wasn't just a plumber, wasn't just an electronics wizard. He knew, he really knew, classical physics, and he did as much as any of us did, given the time we had and the fact that our formal education was over, when we tried to learn the basic things about relativity and quantum mechanics. But his greatest pleasure came not from taking the measurements after an experiment was performed but in making it possible by building the necessary equipment for the experiment to be done. He liked to design, too. He got his biggest kick, I think, out of actually doing something with his own hands. There was always a small lathe in the corner of the lab right here next door which was mostly Charlie's. The rest of us used the lathes down in the basement. If anything was needed, he'd go to his lathe and pick up a piece of scrap brass and turn out what we needed. He was very, very clever at making little gadgets. He was really just an exquisite machinist for small things. He could do things with a lathe that I've never seen any other machinist do.

GREENBERG: Let's talk a little bit about Oppenheimer, who also influenced you greatly.

FOWLER: Well, we have talked about Oppie. He made an enormous contribution to what we were doing, because he understood all the quantum mechanics and special and general relativity in a very deep way. He was able to translate what we were finding in the laboratory into useful contributions to physics. And so that was the role that he played. If it hadn't been for Oppenheimer, I think we would have missed [laughter] practically all of the significance of what we were doing. We were extraordinarily fortunate that we had him to tell us what was the significance of what we were doing. When the war was over and Oppie eventually went to the Institute for Advanced Study, Lauritsen characteristically realized that we had to have someone like Oppie in the lab, and that's when we brought Robert Christy here.

GREENBERG: I guess Oppenheimer figures in your thesis.

Fig. 7. William A. Fowler in the W. K. Kellogg Laboratory, Caltech, in 1939. Caltech Archives.

FOWLER: He's the one who told me that the regular progression of energies in the beta decays of the mirror nuclei meant that the forces between two neutrons were the same as the forces between two protons except for the Coulomb interaction, and that the energy differences between mirror nuclei that we were measuring were nothing more than the electrostatic interaction. When you took that out, then the nuclear forces between two protons and two neutrons had to be identical to within a few percent, and we know it's even better than that now.

GREENBERG: And another instance is the pair formation that helped you straighten out the gamma-ray production.

FOWLER: That's right. When we convinced ourselves that the oxygen-16—produced in the bombardment of fluorine with protons and the emission of alpha particles—was producing pairs directly from the O^{16} rather than emitting radiation, which then produced pairs outside of the O^{16}—when we convinced ourselves of that, we didn't know what it meant. But Oppenheimer and [Julian] Schwinger, who was in on it, showed us that the pairs came from the first excited state of oxygen, which had the same spin and parity as the ground state—namely, zero plus—and that an electromagnetic transition between those two states giving off one photon is completely forbidden. You can have two-photon emission, but because this state had an excitation energy of about 6 million electron volts, it had more energy than necessary to produce an electron-positron pair. It decayed by emission of the electron-positron pair, which was created then right in the oxygen-16 nucleus when it made a transition from its excited state down to the ground state.

GREENBERG: I went back and checked out some of the literature from the time, 1936 and 1937—Bethe's *Reviews of Modern Physics* articles and [Franco] Rasetti's book on nuclear physics [*Elements of Nuclear Physics* (Prentice-Hall, 1936)] that was published then—and I noticed that in both the mirror nuclei (your work) and the discovery of resonance in proton capture, Caltech's role has somehow gotten lost.

FOWLER: I think that's true.

GREENBERG: At both nuclear physics conferences, Robert Serber tried to explain this, and it went over their heads.

FOWLER: Well, I think the rivalry was mainly among the theorists as to who thought of it first. [Laughter] That had a great deal to do with it.

You know, an example of this was in one part of those articles that Hans Bethe wrote in 1936 and 1937. [See Session 4, p. 70.] Melba Phillips and Oppie had shown that one of the reasons why a deuteron gives such enormous cross sections is that as the deuteron comes in, the proton in it is repelled, but the neutron isn't, by the charge on the target nucleus, and they had developed a theory of that. Bethe didn't like the way they did it, and in this series of famous articles he wrote, he was so positive that he was right that he entitled the section "Disproof of the

Oppenheimer-Phillips Mechanism." Now, who was right about that it's hard to say, because in my book it's really a matter of semantics.

It just shows you that there was a great deal of competition. Just as there was competition between us and Berkeley and DTM to get experimental results, there was terrific competition between the theorists as to who had the ideas first. So it was hard to admit that Kellogg had shown the equality or the charge symmetry of the nuclear forces without admitting that Oppenheimer had had a great deal to do with the original idea.

Actually, the first people to suggest this equality *theoretically* were Lloyd A. Young [*Phys. Rev.* 48: 913 (1935)] and K. F. von Weizsäcker [*Zeit. fur Phys.* 96: 431 (1935)]. Although you say Serber's claims were somewhat overlooked, there is a book called *Isospin in Nuclear Physics* [North-Holland, 1969] with a very authoritative article in it by Joachim Jänecke. It's a book which is a series of contributions—Denys Wilkinson was the editor. And Jänecke goes into the history of the charge symmetry of nuclear forces in some detail. He makes it very clear that as far as he could find by digging through the history, the first *experimental* statement came from our work on the mirror nuclei. And I, in turn, have to say that we got that statement from Oppenheimer, who appreciated what we were doing. Now whether Oppie had heard the idea from Young or from von Weizsäcker is very hard to know. Because he was on top of everything that was going on. But nonetheless, when he saw what we were doing he immediately realized what it meant, and that was the important thing.

GREENBERG: All right. Maybe we'll close out with some questions about other areas of physics at Caltech in the thirties, the ones that you were aware of, maybe you weren't aware of. Were you following the work of people like DuMond, Bowen, Houston, and Tolman during the thirties?

FOWLER: Frankly, not in any detail at all. The only work that I really followed closely was the work that Seth Neddermeyer was doing with Carl Anderson on the muon and the work that Anderson and Neddermeyer were doing in observing high-energy cosmic rays. I was very much interested in what Ralph Smythe was doing in Bridge, because he was in mass spectroscopy and he was able to produce enriched targets of carbon-13, which we needed for our work. He had Dean Wooldridge as a student at the same time I was a student of Charlie's. Wooldridge actually was working for Smythe, and Wooldridge produced a slightly enriched carbon-13 target for me. I've got some correspondence and some notebooks somewhere on that. Then Charlie Townes came along, and I think I got a target later on, an enriched target, from Charlie Townes. So there was great interest in what Smythe was doing and, of course, in all of the mass spectroscopy, which Smythe didn't do quite so much.

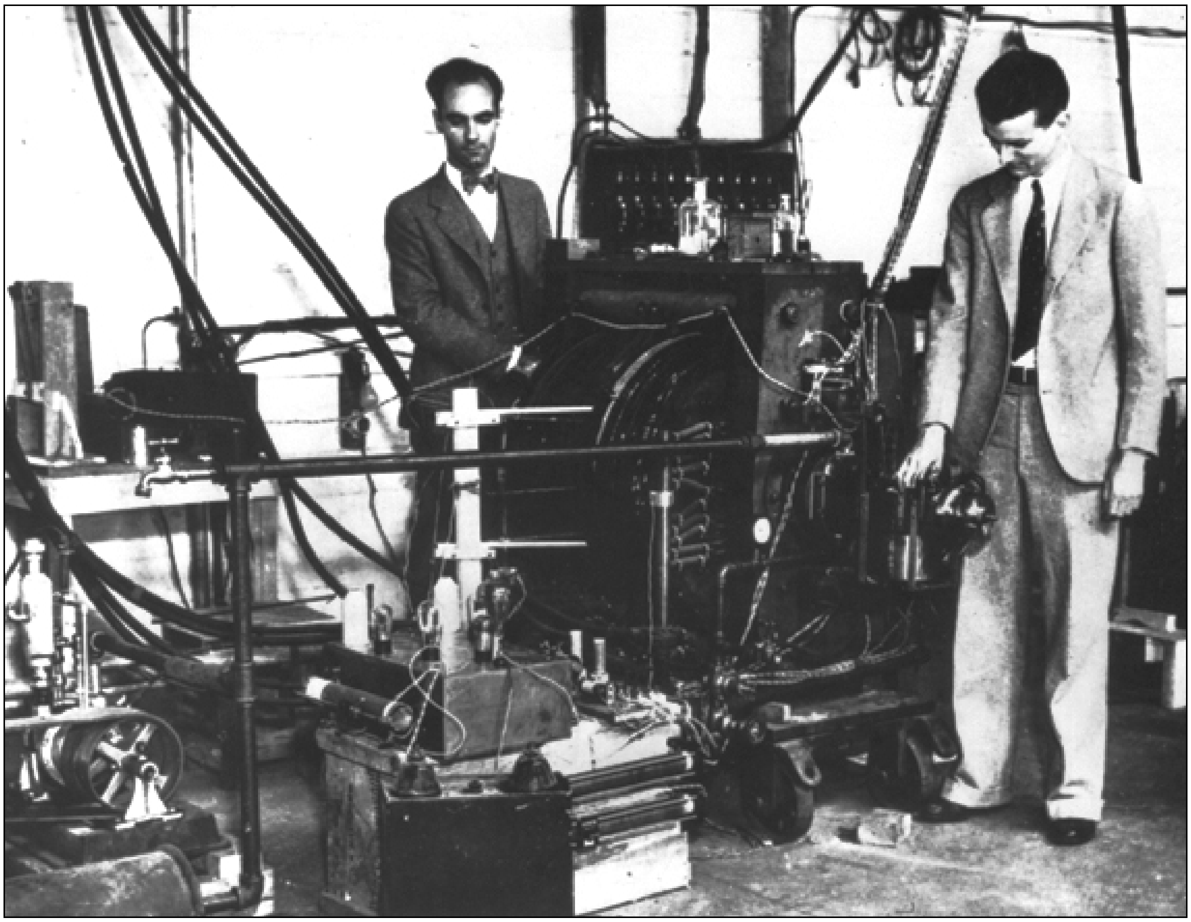

Fig. 8. Carl Anderson and Seth Neddermeyer with the magnet cloud chamber used by Anderson to discover the positron—the first empirical evidence for the existence of antimatter—in 1932. Four years later, they announced the discovery of mesons (muons). Housed in the Guggenheim Aeronautical Laboratory, the apparatus was in service for seventeen years. Photo courtesy *Engineering & Science*.

Others were measuring the masses of nuclei with mass spectroscopy with higher and higher precision. We were determining the masses by the energy differences in nuclear reactions, so we were very much interested in that. There was the work that Houston was doing that I was mainly interested in; I was rather close and friendly to Houston, because I taught a section of his course in mathematical physics. The work of Neher was not nearly of as much interest as that that Neddermeyer was doing, although we were very much aware that Neher, with Millikan and

Pickering, was doing very exciting work.

The main thing was that those of us in Kellogg were just so immersed in what we were doing—we worked night and day in the lab—so the interaction directly with the others, except when we had some very special thing that we wanted to do, like get a target from Smythe, wasn't all that great, although the work that DuMond was doing was in many ways related to what we were doing. He was studying X rays, which are just a lower-energy form of gamma rays. But he was set on making high-precision measurements, and we didn't have much interest in that, so that there wasn't the exchange of information with DuMond's group that there might have been.

You see, things were different, I think, then. Kellogg had its own Friday night seminar. The only people who came were the Kellogg people. DuMond, even those days, may have had a small seminar. There wasn't quite the exchange then, as I remember, that there is now. Now we even have this little physics Xerox bulletin that supplements the Weekly Calendar that tells us what the various seminars are. Ed Stone and his group have a luncheon, and every once in a while we see something that interests us, and so we go. We didn't have that in those days. So there wasn't all that much interaction.

GREENBERG: You mentioned that Bowen's spectroscopy became important to the shell model, but I suppose that wasn't until much, much later. That wasn't in the thirties.

FOWLER: No, it certainly wasn't in the thirties.

GREENBERG: After 1950, or afterwards .

FOWLER: Well, no. The shell model is a very interesting story. The man who first applied the shell model to nuclei was Walter Elsasser. And Walter Elsasser was here about the time that he was doing that work. Elsasser may have been here before the war. The trouble was that his shell model didn't work. Now, it worked fine for us, and still does: The magic numbers are 2, 8, and 20, just fine for low-energy nuclear physics, because they worked. The next magic number that Elsasser got was 40. But in experiment, the next magic properties occur at 50, and it wasn't until the spin-orbit force was discovered and applied by [Hans] Jensen and [Maria] Goeppert-Mayer that the shell model suddenly fitted the data. In fact, one of the things that we thought we could do with excited states in nuclei was get detailed information on the shell model.

I remember at one time taking all the many levels that we had "discovered" in beryllium-8 using statistical fluctuations [laughter] in the spectrum we got from lithium-7 plus protons before we went to pairs. I tried to fit all those gamma rays into some kind of an energy-level scheme, based on a shell model—at least, based on a potential shell—and based on the idea that the nucleons moved inside, even though they had all these complicated interactions with their buddies in there [and] there was an average potential that one could use. But no, no.

The shell model came to have a great influence on us—although, there again, Oppenheimer was very, very skeptical. Because as he pointed out, what Elsasser had calculated just didn't work once you got beyond twenty nucleons. So that's a part of it that I'd forgotten. I'm pretty sure Elsasser did this in France before he came to the United States. He came as a research fellow here at Caltech. He wasn't in Kellogg; I have a suspicion he was a research fellow with Houston, although I'm not too sure. I got to know Walter quite well. He subsequently became a very famous man for his idea of the dynamo theory of the earth's magnetic field. I'd forgotten all about the influence of Elsasser's shell model. It worked perfectly for the light nuclei. It gave the magic numbers 2, 8, and 20, you see. That is all we were interested in.

GREENBERG: You also mentioned on another occasion that Houston was building nuclear models.

FOWLER: Yes. Houston tried to hold the nucleons in nuclei together by putting springs between them. So he had a force which would bind the nucleons together, but it was a force that increased with distance, just like Hooke's law. What he should have done was apply it to the quarks in the nucleons [laughter] and he would have really been ahead of everybody. But, again Oppenheimer—and in this case I think rightly so—just pooh-poohed what Houston was doing. Houston was, again, essentially a classical physicist, and so he tried to use essentially an idea of holding the nucleons in the nucleus together with springs. And of course the nuclear force is of an entirely different nature. It's the quantum mechanical force that was essentially explained by Yukawa.

GREENBERG: But you also mentioned to me that you and Lauritsen did like classical analogs to things.

FOWLER: That's true. I surmise that when I first heard of what Houston was doing, I thought it was great. And then I'm sure Oppie came down and I mentioned this to him. I can't be sure of this, but it could very well be what happened. And Oppie said, "Oh, that's all wrong; don't pay any attention to it." He could be very critical of the efforts that he thought were just wrong, and he told me that Houston's ideas were wrong. Whether or not I had previously thought that they were very interesting, that I'm kind of extrapolating a bit. The thing you have to realize is that Caltech was a terribly exciting place, largely because of what Carl Anderson was doing. He had discovered positrons, and muons followed, in collaboration with Neddermeyer.

WILLIAM A. FOWLER
SESSION 4
June 21, 1983

Begin Tape 4, Side 1

FOWLER**:** I think there is good reason to try to get the record straight on the role that Kellogg played in the early days of nuclear physics. Our conversations have led me to think that the contribution that Lauritsen and Crane made—with the eventual help of Delsasso and myself—on the discovery of resonance phenomena in nuclear physics has really never been recognized. The priority has never been recognized, because of the almost simultaneous discovery by Fermi and his colleagues in Rome on resonances produced by slow neutrons, which had, in many ways, many more implications.

But I've been looking into the history of the publications and things, and it's very clear—to me, at least, and of course I'm biased—that the first discovery of resonances with artificially accelerated particles was made here in Kellogg by Lauritsen and Crane about six months before Fermi ever published what he and [Emilio] Segrè and the others had been doing on neutron resonances and on slow neutrons. With neutrons, a resonance has a tail at low energies which rises rapidly to give the cross section proportional to one over the velocity of interaction ($1 / v$). You don't get that with charged particles, which is what Lauritsen and Crane were working on. So the big thing about what Fermi and company did was in part the fact that they had discovered resonances. However, the effect of the resonances on thermal neutron cross sections was the overwhelming thing that caught everybody's eye. It led eventually to all work on fission and bombs and so forth. But I think we ought to try to get straight the history of just what happened back in 1933, 1934, and 1935.

GREENBERG: Well, it would seem to me it's important, because, after all, from your viewpoint you thought the resonances were the key to the nuclear forces, or would be. Isn't that it?

FOWLER: We realized that those resonances were the effect of excited states in the compound nucleus. Now, how much was realized about the implications in those early days is much more difficult to say. The important thing to me is that the resonance effect—the large rise and fall in the yield of a nuclear reaction due to a resonance at low energies—can be such that it completely wipes out the small factors due to the penetration factor. So that superimposed on the steady rise of a nuclear cross section with energy—because the higher the energy the particles have, the more readily they can penetrate the Coulomb barrier, as [George] Gamow and [Edward U.] Condon and [Ronald W.] Gurney theoretically predicted—superimposed on that can be what's frequently called tunneling. This is where, because there is a state with high probability in the nucleus that is being formed, the probability that a projectile will penetrate the Coulomb barrier and interact with the nucleus is enhanced by many, many orders of magnitude.

The famous case is the state in carbon-12 that Fred Hoyle predicted on the basis of astrophysical argument. I've been looking into what difference our contribution really made, and it turns out that if you compare our present rates for the formation of carbon-12 from three alpha particles—which are based on what Hoyle said, and only minor modifications due to all the experimental work that's been done—relative to the rate that [Edwin E.] Salpeter had calculated without putting this resonance in, the factor is 10 million. And that's, of course, what got that all started.

The interesting thing was that as far as I can tell, Crane and Lauritsen found the resonance effect in the bombardment of carbon and lithium with protons the order of six months or so before Fermi published. Now, that's not quite fair, because the Italians had also clearly been working on this before Fermi and Segrè and the others published their paper on October 22, 1934. They claim the discovery date of the effects of slow neutrons was October 22, 1934; it's recorded either in McMillan's discussion or Segrè's discussion at that meeting that was held in Minneapolis a few years ago. [Symposium on the History of Nuclear Physics, University of Minnesota, May 1977, *Proceedings: Nuclear Physics in Retrospect,* ed. Roger H. Steuwer (University of Minnesota Press: 1979)]

It's very clear that the idea of resonance was certainly discovered here in Kellogg quite independently and under terrific handicaps, because what Lauritsen and Crane had was an alternating current accelerator. In an accelerator supplied by alternating current transformers, the effects of resonances get wiped out to a large extent, or smeared out, because you're using a beam of particles which have all energies; whereas what you needed to do, and what we do nowadays, is use electrostatic analyzers to produce beams with very homogeneous well-resolved energies, and then we can study these resonances in detail.

Fig. 9. Charles C. Lauritsen and H. Richard Crane in the W.K. Kellogg Laboratory, Caltech, in 1934. Caltech Archives.

Well, the history of Kellogg's role is a very interesting one. The main catch is that Lauritsen and Crane did not publish their results when they did all this work in 1933. When I came in September of that year, I became aware that they had this very strange behavior of nuclear cross sections that they could even see, in spite of using a beam of particles which had energies all the way from the maximum down to zero. They were finding these strange results which they clearly could see in their experimental work.

The first announcement to the outside world was at the Berkeley meeting. It must have been early in 1934. And that's the thing that I think we ought to try to get completely straight. I remember going to that meeting. I remember Lauritsen having a violent argument with Tuve, because Tuve did not believe the experimental results. But Lauritsen presented the work that he and Crane had done at the Berkeley meeting, and the good thing about all of this is that Tuve eventually capitulated, made the measurements with an electroscope that he had borrowed from Charlie Lauritsen, and found that essentially Charlie was right and—of course, because he had an electrostatic accelerator—was able to do a much more definitive job.

You called my attention to papers by Breit and Yost. In the second of those papers, Gregory Breit and F. L. Yost—this is what we have to tie down really definitively, I think—refer to the Pasadena measurements which show resonance in the radiative capture of protons by carbon-12, and point that out in their first paper on the radiative capture, which is interesting in itself [*Phys. Rev.* 46: 1110 (1934)]. That's what a lot of the argument was about: Could carbon-12 plus a proton produce nitrogen-13 by radiative capture with the emission of a gamma ray, or was what

Charlie and Dick Crane were seeing merely contamination of deuterium in the beam which produces the nitrogen-13 plus a neutron in copious quantities? Well, Tuve claimed that Charlie and Dick had to be looking at this contamination. Charlie and Dick knew it couldn't be a contamination. First of all, they did other things with their hydrogen beam, and if it had some deuterium in one part in 6,000, which is the normal concentration of deuterium in ordinary hydrogen, they didn't get effects, other effects. But in addition they found that the yield curve was entirely different than when they bombarded carbon-12 with deuterons to make nitrogen-13 and a neutron. So they had good reason to think that they had come upon something quite different.

The first clear-cut recognition came in the second paper by Breit and Yost [*Phys. Rev*. 48: 203 (1935)], who confessed that in their first paper, where they had a model of what this reaction process should be, they had missed the resonance effect. They had made numerical calculations, and actually they say they made some mistakes. And sure enough, when Charlie gave Breit the unpublished version of what he and Crane had done, Breit and Yost went back and looked. They make it very clear that if they had made their model calculations correctly and in enough detail, they would have seen what Crane and Lauritsen were claiming. So there's no doubt in my mind, at least, that the resonance phenomenon with artificially accelerated particles—charged or uncharged, protons or neutrons—was discovered here in Kellogg.

The only thing one can say about previous priorities was that a man named Pose had discovered, with even less convincing evidence than Crane and Lauritsen, resonances and the bombardment of various nuclei with alpha particles. So the resonance idea was known before the work that Crane and Lauritsen did, but as far as I know it was not generally accepted, except that Gurney, of Condon and Gurney fame, discovered quite independently and theoretically the idea of resonances. And then he published that in 1929. Pose's work was even earlier. So the idea of resonance in nuclear reactions has many implications, in that a resonance enhances the cross section by enormous factors over what it would be otherwise. The idea was known, because of Pose's work—I think in Vienna, although we've got to check that [see p. 73]. There had been this long controversy between the Cavendish and people in Austria. I must say the Austrians got all the natural radioactivity mainly wrong, whereas the Cavendish under Rutherford did a very good job. So there had been this long controversy, and my feeling is that Rutherford said, "Well, now they've made another mistake, you see." So clearly it was not a very well-accepted idea.

The real tragedy, if you want to call it that, was that Crane and Lauritsen did not immediately publish their excitation curves, because once Tuve borrowed Charlie Lauritsen's electroscope so that he had a good detector and did it back at the Department of Terrestrial Magnetism in Washington, he got so much better results that I suppose Crane and Lauritsen felt, "What the hell! Why should we publish these curves that we have to differentiate in order to get the resonance effects."

We finally did publish the curve that they had gotten of the resonance in lithium-7 plus protons, producing gamma rays [*Phys. Rev*. 48: 125 (1935)]. But it was just stuck in; the main thrust of that paper was to show the energies of the gamma rays produced when lithium is bombarded with protons. Of course, it was all wrong, because what we were looking at were these statistical fluctuations [laughter] in the Compton electron background, and it took us another year to realize we had to use pairs, and then we found there were just three gamma rays, 17 million volts, 14, and 3, something like that.

GREENBERG: I think that at the 1977 conference in Minneapolis it was said that Rutherford had mentioned discovery of the 17-million-volt gamma ray without attribution.

FOWLER: I can't go back and look at just what happened, but what I have been impressed by was that if you look at Hafstad and Tuve's paper, they first have a paper where they talk about the Pasadena results and the Cambridge results [*Phys. Rev*. 45: 902 (1934)]. Cockcroft and his gang had also found radiation in carbon-12 plus protons [*Nature* 133: 328 (1934)]. Tuve and Hafstad in their first paper said [in effect], "It's all nonsense. All they're measuring is the background due to the deuterium contamination in their hydrogen beam." Then I remember this: Merle Tuve came out to the Berkeley meeting in January of 1934, I guess it must have been, and I remember Charlie telling Tuve, "It can't be a contamination; it's got to be real." And Merle, who was a very excitable, wonderful character, said, "Well, Charlie, you're just wrong this time." "Well," Charlie said, "your trouble is you don't have a decent detector. How about borrowing one of my electroscopes?" I was there when this conversation went on. So Tuve took it and went back, and then in their second paper [*Phys. Rev*. 47: 506 (1935)] they confess everything—that Lauritsen was right, and in order to make the observations they had to use his electroscope.

Breit, who was very closely connected with Tuve, then realized what he and Yost had done on their radiative capture, and published a second paper, and it starts out by saying that on the basis of unpublished information supplied to them by Charlie Lauritsen, they think that the idea of resonance in nuclear interaction introduced by artificially accelerated particles is real. And that all transpired before the Italian group under Fermi came out, later on that year, with the resonance effects that make slow neutrons so effective in disintegrating the heavy nuclei. I thought it would be worthwhile to go through and record all of the papers that bear on this problem so that, at least to a certain extent, the literature that applies to the situation is all kind of laid out. So I collected a bunch of volumes of *Physical Review,* and I thought we might spend some time today just going through in getting this story straightened out insofar as the history of the papers that were written on it. The key one, I think, is when Breit and Yost definitely say they were motivated in what they did in their second paper by the unpublished results that Charlie Lauritsen had given them, which he announced at the birthday meeting of the American Physical Society. Unfortunately, we can't find any copy of that [Lauritsen] paper. We've looked and looked, but we can't find it. Judy Goodstein asked me in particular to look, a few years ago, and I looked and looked, and somehow or another it's lost. He not only gave the paper at the western meeting but later in the year he gave it at a meeting in Minneapolis—same paper, I'm sure.

GREENBERG: The society doesn't have a copy of it?

FOWLER: No. In those days you were invited to give a talk; you gave a title. Nowadays when you are invited to give a talk, Bill Havens writes to you and asks you if you won't supply an abstract. But you don't have to supply a paper. The presumption is that you've done something important enough that you will eventually publish it in *Physical Review* or wherever you want to. Unfortunately, in a way, Charlie and Dick never published what they did on the carbon, although they did slip in an excitation curve on the lithium-7 plus protons in the paper that they wrote with Delsasso and myself, which was mainly devoted to what the gamma ray energies were. We did find that the gamma ray energies went up very high, but we thought there were lots of lines instead of just the three that you see nowadays. One was a direct 17 million volts, another a cascade through the excited stage of beryllium-8, I guess it has to be. I thought that since we had never, in a way, got this all finished, rather than starting on a new thing, we should get this straight. And maybe what we could do is now look at some of this stuff and then we can come back.

GREENBERG: Let me just ask one more question in this regard that I want to get straight. OK, Oppie was critical of radiative capture. Now, was he criticizing Breit and Yost here, or Lauritsen?

FOWLER: Well, it was very clear that Oppie was pretty much convinced by the claim that Tuve and Hafstad had made that what Charlie and Dick were seeing was contamination due to deuterium in their hydrogen beam. So at least that part was fairly reasonable. You see, there are two things involved. One is, first of all, does radiative capture occur? Can a proton hit a nucleus, be absorbed by it? No particle comes out, but a gamma ray comes out. That's one thing. The second thing is, can you have resonance in that process? It's very clear. Two things are involved; one is the existence of radiative capture, two is the resonance in radiative capture. In other processes where you put a particle in and get a particle out, we now know that you can get resonance in that, too. But in those days if there were any resonances with a particle coming out, the resonances were so wide that you just looked at it as a continuum. So Oppie had good reason to think that what Charlie and Dick were doing was just wrong—that they were seeing a contamination in the beam.

Of course, the whole atmosphere at that time was fairly difficult, because Charlie was the one who pointed out that what G. N. Lewis and Lawrence thought was the breakup of the deuteron in some of their reaction processes was not that at all. Oppie was on both sides of the fence, being most of the time at Berkeley and a small part of his time, our third quarter, down here in Pasadena. But the main thing, as I remember, is that the cross sections were so large due to the resonance effect that Oppie just didn't believe that. He just could not believe that the yield for this radiative capture could be as large as it was—because he completely missed the idea of resonance. So he just told us we were wrong, and by this time I was here, you see; I came in the fall of 1933, and this was all going on at the time.

It was mainly Oppie's influence that in the one paper they published on the subject they actually suggested that instead of C^{12} *p*-gamma making N^{13}, they said—I know this was due to Oppie's insistence—that an alternative mechanism for the production of the N^{13} was to hit the C^{13}, the rare isotope of carbon, with a proton, make N^{13}, put out a neutron. We know now, and Oppie should—well, it's all of us; it's a long story—we know now that the neutron is heavier than the proton. So that reaction, C^{13} plus a proton—and the N^{13} is heavier than C^{13}—going into N^{13} plus a neutron has to be endoergic. It's got a threshold above where Crane and Lauritsen were working. It wasn't known then, because the mass of the neutron wasn't known. There were years when everyone thought the neutron was lighter than the proton; it had to be, because the proton had an electrical charge on it which had to add to its mass, in Thompson's rule e^2 over the radius [e^2 / r], you see. Well, it turned out that the neutron people didn't even know that the neutron decayed. They didn't know it was all just terribly confused.

Actually, Crane and Lauritsen had made a measurement of the neutron mass which was still lighter than the proton but damn close to it. But anyhow, in their paper on the detection of the production of N^{13}—this was the radioactivity—by hitting C^{12} with protons, they made the suggestion that an alternative was that they were really hitting the C^{13} and there was a *p-n* reaction that made the N^{13}. That was, I swear to God, largely Oppie's complete reluctance to believe that that process could have as large a cross section as it had. And of course, Crane and Lauritsen, with their alternating current voltage, had protons that just hit the resonance, so they were getting these enormous yields. It was one of the few times, I would say, when Oppie was just completely wrong.

The follow-up is that Gregory Breit apparently talked to Eugene Wigner about this effect, and out of that came, in 1936, the Breit-Wigner formula. All of their ideas were very parallel to, and much more specific than, Bohr's idea of the compound nucleus. If you look at Bohr's paper, in which he discusses the formation of the compound nucleus and why the resonances that Fermi was finding with neutrons could be so sharp, Bohr interpreted that in terms of the uncertainty principle as due to the long lifetime of the intermediate state, which is what you see when you go over it in your excitation curve and get what we call a resonance.

I've noticed that Bohr did refer in *Nature* [137: 344 (1936)] at the very end to the fact that these effects that Fermi had so beautifully found with neutrons should be expected with charged particles and in fact had been seen. But he didn't give any references to the work either Charlie and Dick had done or that Tuve and Hafstad had done or that Cockcroft and Walton had done. I don't know; it's rather amazing to me that in this basic paper that Bohr wrote he did not mention the fact that resonances—well, I shouldn't say that. He did mention the fact that resonances had been found with alpha particles and with artificially accelerated particles, but he gives no references, you see. He doesn't refer to Pose; doesn't refer to Gurney; doesn't refer to all the stuff that had been done in this country and in England. In fact, that paper is mainly a tribute to Fermi. That's well enough, because what Fermi did was excruciatingly beautiful; I'm not going to argue [with] that. But the history has been confused, because Oppie refused to believe in resonance. Breit and Wigner worked quite independently from what Bohr was doing. And although Breit and Wigner did refer to the charged particle work, their work was also mainly connected with Fermi.

The beautiful thing they found is that if you have a low-energy resonance and use the Breit-Wigner formula, when you extrapolate it down toward thermal energies, you get a large rise due to the $1 / v$ law, which comes naturally out of their formula. They specifically say you will get two maximums with neutrons—you won't get it with charged particles—one at the resonance and one as you approach thermal energy, zero energy. In fact at zero energy, if you neglect small effects, the cross section becomes infinite. Because it's one over the square root of the energy, one over the velocity ($1 / v$), and then the energy goes to zero and the velocity goes to zero and the cross section becomes infinite. Well, it doesn't actually do that, as you might surmise, but it gets very large.

In Breit and Wigner's paper [*Phys. Rev*. 49: 519 (1936)], they do eventually at the end refer to the charged particle work. But by 1936 the only reasonable reference and the only reference they had was to Tuve and Hafstad. The actual history is that Tuve and Hafstad first of all didn't even get the radiative capture and wrote a paper in which they said that Pasadena and the Cavendish work is just wrong. It's very specific: "Those guys are wrong, period." If you go back and look at it, it's one of the bluntest papers I've ever seen in the literature. Charlie Lauritsen convinced Merle that he should try it again, and then you go read their second paper. Merle Tuve made a very gentlemanly admission that they had goofed and that Lauritsen was right, and that not only was he right that radiative capture occurred but there was resonance in the effect. Then they showed the beautiful resonance curves that they were able to get with their machine and that had fairly high resolution in energy.

I looked at the paper and I was amused by their energy scale, compared to what we now know to be the case. It turned out there were two resonances, one in carbon-12 *p*-gamma and one in carbon-13 *p*-gamma. Now we know the carbon-12 one is at 460 kilovolts; well, they got it at 400. They were more than ten percent off, in spite of the fact they were claiming very high precision. The carbon-13 resonance is at 550, and they got it at 500.

One of the first things that Tommy Lauritsen and I did when we built our Van de Graaff was to repeat the work of Tuve and Hafstad, and we made fairly precise measurements. If I remember, we got 455 kilovolts instead of 460. Well, hell, that was quite good for the techniques we had available to us.

By the time Breit and Wigner came out with their detailed numerical analytical expression, in 1936, of the resonance effect with neutrons and the tailing into the thermal region—the famous Breit-Wigner formula—when they referred to charged particle work, all they referred to was Tuve and Hafstad. That's fair enough, because Tuve and Hafstad had published.

You have to go back and read the literature and see what Tuve and Hafstad and Breit and Yost did. Tuve and Hafstad, when they finally confirmed what Charlie had done, gave full credit to Lauritsen and Crane. Breit and Yost, when they finally corrected their first theoretical paper—and this is now about the resonance effects—point out that they hadn't done their numerical calculations accurately enough, and sure enough, there was a resonance effect and this confirmed the unpublished stuff that Charlie Lauritsen had given them.

So I think the main thing that should be done—it may be that someday a paper should be written on this which doesn't make any claims one way or the other but just points out the various papers and quotes from them. Because, quite apart from the priorities question, there is no question in my mind that Charlie Lauritsen and Dick Crane, when I came here, were involved in really the most exciting aspects of nuclear physics. Given that Cockcroft and Walton discovered that you could do the whole thing, and that Berkeley devised machines that would do it even better, nonetheless the Kellogg experimental work, handicapped as it was by having to use an alternating voltage accelerator, found radiative capture and resonance in radiative capture. And in spite of opposition, they were finally proven to be quite right.

GREENBERG: Are these kinds of oversights not typical?

FOWLER: Well, you yourself have pointed out that there is no general recognition of this.

GREENBERG: That's right.

FOWLER: [Laughter] John, that's one of my objections to oral history, not only oral history between you and me, where I try to do the best I can to recall, but these meetings where people get together and talk about what the history was. They have largely obscured the fact that Charlie Lauritsen and Dick Crane made the basic discoveries in these two areas of radiative capture and resonance in radiative capture. The only proviso that you have—and you have to have honesty—is that the old boys, using alpha particles, had already discovered the resonance effects, but with such marginal evidence that nobody believed them. It's also true that what Crane and Lauritsen did wasn't believed at first, but then when Tuve and Hafstad repeated it and got the beautiful evidence they had, then it was clear what Charlie and Dick had done.

[Note: A fragment of the interview is missing on the tape at this point. Greenberg has asked a question leading to discussion of a paper by Hans Bethe and M. Stanley Livingston, "Nuclear Dynamics, Experimental," published in *Reviews of Modern Physics* 9: 245 (1937). This paper, which cited the published results of work done on accelerated particles and radiative capture by C. C. Lauritsen's group at Kellogg Laboratory, appeared as Part C of "Nuclear Physics," a three-part report Bethe had undertaken to provide a detailed record of accomplishments in the field. Livingston, then a colleague of Bethe's at Cornell, had previously worked with Ernest Lawrence at the Berkeley Radiation Laboratory.]

Livingston wrote this review paper, which was just a marvelous thing in its time. As you pointed out, the Kellogg work is on p. 312. See, that has to do with this whole business; they're talking about the *p*-gamma reactions, and later on they give plenty of credit to Kellogg. There's Lauritsen's private communication; Crane, Delsasso, Fowler, and Lauritsen. Then the original thing on the carbon-12, all they mention is that—

Begin Tape 4, Side 2

FOWLER: —Cockcroft reference to that international conference, which Dick and Charlie attended and where Charlie also talked about it, but this doesn't refer to what they did. So your question about here and here [points to locations in the Bethe-Livingston article], this is just not fair, because, at least in this country, the radiative capture was definitely found by Lauritsen and

Crane under great difficulties. So in this discussion of the type reaction *p*-gamma, Livingston just forgot that it was Charlie and Crane who insisted that it happened and that it showed resonance. [Laughter]

Later on, I have always been pleased to see my name mentioned every page. “Jesus,” I just thought, “Boy, I’m really in there boxing.” Because Livingston and Bethe clearly read the literature, and they have all our papers—Crane, Delsasso, Fowler, and Lauritsen, all spelled out there. But on the controversy concerning radiative capture and resonance, this is not historically correct.

GREENBERG: I haven’t been treating the discovery of the resonances as the discovery of another fact but as a motivation for a research program that gave rise to some of what you tried to go on and do after that.

FOWLER: There were two things. Resonance in nuclear reaction enhances the yield of the reaction enormously. It wipes out the low factors due to the need to penetrate the Coulomb barrier. That’s often expressed by saying that the particle “tunnels through.” But it can only do that at just the energy where it can be received in the compound nucleus or a state of the compound nucleus. So by studying resonances, you can study the excited states of nuclei and study all their properties. That has been one of the most fruitful fields of gathering information about nuclear structure. It doesn’t solve all the problems.

Our original hope was that by studying all the states of nuclei, you could figure out everything about nuclear structure—just like in the early days, when my namesake but no relation, Alfred Fowler, found series in the emission from the hydrogen atom. And of course other people got into it, too. That led eventually to an understanding in terms of an atom being made up of a nucleus surrounded by a cloud of electrons. We thought it would be the same with nuclear structure. It didn’t turn out that way, but you still learned a hell of a lot about the collective motions and the single particle behavior in nuclei by looking at the resonances which correspond to excited states in the compound nucleus. It’s been terribly important. It isn’t just, as you say, a fact. It was a fact that had enormous implications in learning about the behavior of nuclei. Whether you learn anything about the fundamental physics that determines this striking behavior is beyond the point.

The other thing is, of course, that this enhancement of cross sections and yields due to resonance is of enormous implication; first of all, with neutrons and the whole business of making bombs and making reactors, and, second, in charged particles in what happens in stars. So the resonance effects, John, had such important and significant implications, that, boy, it was, to say the least, very exciting, and it's the thing that primarily convinced Charlie Lauritsen that he had to give up powering his accelerators with AC transformers and build an electrostatic accelerator in Kellogg.

Even before the war, in 1938, Charlie could see the handwriting on the wall. He wasn't going to be able to compete with Hafstad and Tuve and with Herb at Wisconsin and with Johnny Williams at Minnesota. He wasn't going to be able to compete unless he had a tube, an accelerating device, which was powered by a voltage supply that had high resolution in energy. That's why, in 1938, he told Tommy Lauritsen and me to start building a Van de Graaff accelerator, although as I've said several times, I preferred to call them the Herb accelerators because Herb really made them work. And of course we continue to this day to study resonances. Then there was the marvelous outcome that Fred Hoyle said there had to be a state in carbon-12 that served as a resonance in the burning of helium to synthesize carbon. So it's been a very interesting history.

GREENBERG: Right. And I think it's important to get Kellogg's role in the discovery of these things originally right.

FOWLER: I think so; even if we only write it down for our own amusement at the moment. I think to get this very interesting history of how a laboratory that essentially had the poorest tool—see, even Cockcroft and Walton powered their sets with transformers, but they were rectified, so they had DC [direct current]. And of course Ernie Lawrence's cyclotron, you don't use the term, but it essentially produced only a fairly homogeneous beam of particles in energy, and it never was able to compete with the Van de Graaffs or the Herb-type machines at low energy. What the cyclotron did was go on to higher and higher energies where they have learned to get fairly highly resolved beams by a whole bunch of marvelous tricks so that they don't even call the machines cyclotrons anymore. It's just fantastic that this laboratory [Kellogg], with the poorest possible tool, was able to participate very actively in the forefront of research in nuclear physics at the time. And they got some things right, and the things they got right were very important. They got some things wrong, but just like everybody else, eventually, by improving our equipment—what Crane and Lauritsen did wrong and what Delsasso and I did wrong originally—we eventually got it all straightened out.

We have only a few minutes now. I've got this list here: I don't know when I made this. Here's the artificial production of neutrons in all the papers, artificial production of radioactivities in all the papers, artificial production of gamma rays in all the papers, and then the resonance thing.

GREENBERG: Could you make a copy of that for me?

FOWLER: One thing I don't understand is what—oh, I erased Walton and Gilbert, because in that paper that Cockcroft gave at the international conference, that's the only place Cockcroft ever talked about it. He and these guys got the resonance in carbon-12, but they didn't publish, because they didn't have good enough resolution. Then, as I said, when Livingston referred to the fact that this finally all got cleared up, he only mentions Hafstad and Tuve and Cockcroft, and the reference to Cockcroft is just this international conference, but I want to get the date written down there, because that's why I'm sure—I thought it had been in one of Cockcroft's papers, and it may be Tuve. I can see that I erased Walton and Gilbert and just wrote Cockcroft, and that's 1934. Then here is what Robley Evans says about resonance. Another couple of fellows who never got—you see, Fermi and his gang never really appreciated that; they weren't able to—the difference between resonance and the thermal effects with slow neutrons. The guys who really found the resonance effects were [P. B.] Moon and [J. R.] Tillman. Here's Pose's paper in German [*Physiks Zeits.* 30: 780, 1929]. Wasn't he in Vienna? Let's see where he was. Then here's Gurney's paper here [*Nature*, 123: 565 (1929)]. There's Moon and Tillman; there's Bohr's paper; there's Gurney's paper. That's '29, man! What year was Pose?

GREENBERG: 1930, maybe?

FOWLER: No, it was also '29 Actually, this got mentioned in one version of Rutherford, Chadwick, & Ellis. But where the hell was Pose? Oh, he's at Halle; he wasn't in Vienna.

That's Germany, isn't it?

GREENBERG: Yes.

FOWLER: Aha! So it had nothing to do with the controversy with the Viennese. But look, man, he got it; he got it—with alpha particles, yet! Jesus Christ, amazing!

WILLIAM A. FOWLER

SESSION 5

August 30, 1983

Begin Tape 5, Side 1

FOWLER: First of all, Crane and Lauritsen were the first to show that carbon-12 could capture protons and radiate. But then they noticed that the excitation curve, even with an AC tube, looked different than the excitation curve for carbon-12 *dn*, which makes the same thing, nitrogen-13, which is the radioactivity of that curve. And Tuve claimed that they just had impurities. Then it all got sorted out.

By that time, the Fermi, Moon, and Tillman stuff was so well established and well recognized that the Crane and Lauritsen discovery was essentially obscured by the controversy. So, what difference does it make? But then, of course, when Bohr came out with his paper on the compound nucleus formation, by that time Fermi was into the resonance part of it, as I remember, and everybody and his brother. People were, I think, beginning to build machines that would produce slow neutrons with high resolution—low-energy epithermal neutrons—and they were seeing sharp resonances. Then Bohr came out with his idea of formation of the compound nucleus that lived a long time and thus showed sharp resonances.

GREENBERG: Did Bohr account for the sharpness of the resonances?

FOWLER: He based what he said almost entirely on what people had been doing with neutrons, and maybe we ought to look at that. He may have referred to Cockcroft's work with protons. Did we look at that?

GREENBERG: No.

FOWLER: We ought to look at Bohr's paper and see—I think he referred to Cockcroft, but he certainly didn't refer to Lauritsen and Crane. So the mystique all was built up around Bohr's theory and the fact that it was shown so beautifully by the neutron phenomenon.

GREENBERG: I want to get the thirties cleaned up so we can get into some nuclear astrophysics. A few last questions. But first, let me make some remarks. We know that there's almost no C. C. Lauritsen correspondence that remains. But there is one that's interesting between Lauritsen and one of his very first students—Benedict Cassen, who, after he graduated, around 1930, went on to Princeton. He was at Princeton at the same time that [Robert J.] Van de Graaff was. There's a very extensive correspondence between them to around 1930, 1931, in which Cassen is trying to persuade Lauritsen to hook up one of his X-ray tubes to a Van de Graaff generator.

FOWLER: I've never seen it. That's in the correspondence you have in the library?

GREENBERG: Right. In the C. C. Lauritsen correspondence. There are also some references to electrical engineers at Princeton who were making some outlandish claim that a DC source would produce a monochromatic beam [laughter] around 1931. I just thought that was interesting.

FOWLER: This was all in 1931? Oh, when Van de Graaff was plodding along trying to make the thing work. Did Cassen work with him? I think Cassen went into other fields by that time. Though he eventually [1947] came back out here, to UCLA. I think he became a professor at UCLA—may still be over there [Dr. Cassen died in 1972—ed.]. He was a very interesting fellow. He started to work in theoretical physics with Oppenheimer, and his story was that he was smarter than Oppenheimer so he saw no reason why he should be his student. He had a pretty high opinion of himself, Benedict did. But he was very good, except he tried to build a higher voltage accelerator and he needed some great big insulators and he tried to core them out of paraffin and he got paraffin all over [laughter] the floor of the High Voltage Laboratory. God, for six months we could hardly walk down there without slipping, no matter how hard we tried to clean the stuff up. Cassen was a pretty smart guy, but there's no indication in the correspondence that Lauritsen took him seriously, is there?

GREENBERG: No. Because we don't have copies of the replies.

FOWLER: Ah. All you have are Cassen's letters to Charlie. Copies weren't kept in those days.

It's a funny business. I can't believe that Charlie took it seriously, although eventually, just before the war, Charlie and Bill [William B.] McLean built an open-air Van de Graaff—what I would call a Van de Graaff—which was mainly to test some of Charlie's ideas on what the tube should be like and what the belt should be like. We soon realized that even in this dry climate the breakdown through the air was just always a nuisance. Charlie became pretty discouraged with the Van de Graaff idea as a practical device, because although in principle it had the ability to get the very high resolution in energy that we wanted, nonetheless the breakdown in the corona negated this possibility.

So it wasn't until Ray Herb, after trying to operate a Van de Graaff in a vacuum, came up with the idea of operating it in high pressure, that it was a great success. In the paper that the two Lauritsens and I wrote in 1940, published in 1941, "Application of a Pressure Electrostatic Generator to the Transmutation of Light Elements by Protons" [*Phys. Rev*. 59: 241 (1941)], we give full credit: "During the past several years we have constructed and operated a pressure electrostatic generator similar in principle to the Wisconsin generator. Ours differs only in being vertical, rather than horizontal." Herb is the one who turned the whole business around. And this ties in with what I've told you before. This paper is mainly about building and operating this device, the first pressure Van de Graaff—or pressure electrostatic accelerator, it should be called, to get away from whether it's Herb or whether it's Van de Graaff. This is mainly about how we built it and designed it, but we did have something of how we used it, mainly to disintegrate fluorine with protons. We do have the two rare isotopes of carbon and nitrogen interacting with protons, N^{15} plus protons—there's a curve [points to paper]. And then we have carbon-13 plus protons. Although we had this high-resolution machine, we did thick target work.

This was November 1940, and we had to rush, because Charlie had already been in Washington since June or July, and Tommy and I knew we were leaving on the first of the year. You notice he says, "This has been going on for several years." I just read that; that puts it before 1938, you see. It is very interesting. I've looked hard. Nowhere in this paper, which is all we ever got around to publishing about the CN cycle before the war—well, there was one very preliminary paper—nowhere is there a mention of Bethe. [Knocks on wood three times.] We were doing this before Bethe came out with the CN cycle. By the time we got around to publishing—which was after he had published—we just treated it as nuclear physics. We didn't—I can't find anything that says, "Oh, boy, we're glad we were doing these things, because Bethe tells us they're important." We just treated them as nuclear physics, and that's because we got started that way.

It wasn't until after the war, and we had had some time to really think about it for five years, that the implications became important to us. But even then—if you look at this history that I made up in November of 1981 [*W. K. Kellogg Rad. Lab. Pub.*]—right after the war, we got into some interesting problems in nuclear physics which were quite independent of the CN cycle. As a matter of fact, we didn't get really serious about the carbon-nitrogen cycle until 1948 and 1949. The first paper where we really made precision measurements at low energy on carbon-12 plus protons didn't come out until [R. N.] Hall and Fowler in 1950 [*Phys. Rev.* 77: 197 (1950)]. I'm saying all this to indicate that it took some time after the war to get started again. The lab had to be rebuilt, because it was just gutted. We had taken everything out, you see, or moved the accelerators over into a corner.

So right after the war we did things that kind of came naturally or were exciting in nuclear physics—like the experiment in 1947 to show that neutrinos did carry away momentum in beta decay, as Bohr and Fermi said they did, and that they traveled at the velocity of light. That was a really hot problem, so we spent the first year and a half after the war working on that. Then we did the beryllium-8 in 1949, and that, of course, didn't pay off until Ed Salpeter got interested in it in 1952 and 1953.

GREENBERG: We didn't talk at all about Bonner and Brubaker. Is there anything to talk about there? Or were they just working with you?

FOWLER: Bonner and Brubaker were quite independent of my effort. Bonner was actually either senior to me or, well, practically the same generation. But he came as a National Research Council Fellow, as I remember, to Kellogg, as what we would now call a postdoc. And Brubaker was a graduate student. So Brubaker started to work with Bonner, and whereas Tommy Lauritsen and I were working mainly on reactions that produce gamma rays, Bonner decided he wanted to work on reactions that produced neutrons. So he built a cloud chamber that was specifically designed to detect the proton recoils when neutrons struck the high-pressure hydrogen in his cloud chamber, just like we let our gamma rays produce electron or positron secondaries. He measured his neutrons by the knock-on energy that they gave protons. You see, if a neutron hits a proton dead on, since they are roughly the same mass, just like a billiard ball collision dead on, the proton goes off with all the neutron's energy. So by measuring the range of the proton and using range energy tables, he could get the energy. So he was measuring neutron energies quite independently, and Brubaker worked with him. I was primarily working on gamma rays. Tommy took off in 1939, you know, and went to Denmark, and I had John Streib as a graduate student, and several other people—Robert Becker. So there were two teams with Charlie as the head man, of course, although Charlie worked, I think it's fair to say, more closely with me than he did with Tom Bonner.

There is the one thing that I should mention in that regard. The interest in neutrons in Kellogg was started much earlier by Hans Staub. Well, now I have to be a little careful about that. Was Staub before...? He must have been before Bonner—no, he was simultaneous. But of course, the important thing that Staub and [William E.] Stephens did was to show that there was no stable mass 5. Of course, the real discovery in that regard was by Johnny Williams and his colleagues at Minnesota, but it was done almost simultaneously by Staub and Stephens. They showed that the continuum state in helium-5, which you make by shining neutrons on helium 4, is the ground state and has just the spin and parity—namely, three halves minus—that you expect from the shell model. That was really the first astrophysical thing done in Kellogg—or cosmological, you might say, because that was then the real impediment in Gamow's theory of the origin of all the elements in the Big Bang.

GREENBERG: And it was done at the time with the cosmology in mind?

FOWLER: No.

GREENBERG: Was it done with pure nuclear physics in mind?

FOWLER: Yes, it was done with pure…. I'm pretty sure, if you read Staub and Stephens, you'll see no reference to Gamow's theory, which didn't come until much later. Staub and Stephens wrote in 1939 [*Phys. Rev.* 55: 131 (1939)], and Gamow's theory didn't come until around 1945 or 1946. George just waved his arms and said, "Well, those experimentalists have made a mistake. They haven't found the ground state of helium-5; and they'll find it now that I've shown how important it is." A lot of work was done, and everyone found that Staub and Stephens and Johnny Williams and company were right. Then everybody had to do a hell of a lot of work on lithium-5. Well, we didn't have to do it, because we knew that lithium-5—being more highly charged than helium-5, ratio three to two, and being the mirror nucleus of helium-5 due to the Coulomb effect—would have a ground state even more unstable. And that of course has all been verified. And it's very beautiful. And there's no way around it.

GREENBERG: One thing I did want to talk about concerns your getting the gamma-ray spectrum finally straightened out. We talked about Oppenheimer and Schwinger's account of pair formation in the bombardment of fluorine with protons; the paper was published in 1939 [*Phys. Rev.* 56**:** 1066 (1939)]. You had isolated the 17-million-volt gamma ray back in 1936, 1937.

FOWLER: But that was for lithium.

GREENBERG: For lithium, I understand. But you were talking about pair formation back then, weren't you? Had you actually begun to understand pairs?

FOWLER: Well, but those pairs were pairs produced as secondaries by gamma rays from the primary reaction striking, usually, a thin lamina of lead. Those were secondary pairs.

GREENBERG: We're not talking about the same things then, at all.

FOWLER: I'll explain. We started to bombard fluorine with protons. We were using electroscopes to detect the radiation and the way we tried to measure the energy was by putting absorbers between the target, where the gamma rays were produced, and the electroscope, which detected them. By knowing the absorption coefficient essentially as the function of energy, although it's very complicated, we were able to get a rough measure of the energy. This was all done before [Robert] Hofstadter invented the sodium iodide crystal, which just put electroscopes and absorption techniques out of business. To get to the point as quickly as possible, when we were bombarding fluorine with protons, we found lots of resonances where 6-million-volt and 7-million-volt gamma rays were produced in the target. When did we build the cloud chamber?

Well, I had the cloud chamber for my thesis. We used the electroscope when we wanted to run an excitation curve.

Once we got an excitation curve and could see the resonances—there were dozens of them—we would sit on one of these resonances, bombard the target at one of those resonance energies, and look at what was produced there in the cloud chamber. So we'd see the gamma rays producing secondary pairs. We measured the curvature of the electron and the positron of the pair. That made it possible for us to measure the energy of the electron and the positron. We added those together, added 1 million electron volts for the rest-mass energies to the electron and positron; that gave us the gamma-ray energy.

Well, just by accident, somehow or other, when we were running excitation curves with the electroscope, we found that when we left the absorber out entirely, we got readings in our electroscope at energies that were quite different from the energies when we had an absorber in—a whole new set of resonances! Now, we thought that because this radiation was stopped by an absorber, that it must be very soft radiation. We fiddled around to substantiate such a thing, with aluminum absorbers and copper absorbers and gold foil and lead, and usually you could, by hook or crook, figure out what the energy of this soft radiation was.

We couldn't make head nor tail out of what we were finding at these new resonances until finally we looked with the cloud chamber at one of these new resonances and found that there was a lot of radiation coming from the walls. Now, the wall was a glass cylinder a quarter-inch thick. But in the section toward the target, I had ground out a window and put just a thin piece of aluminum over that. My heavens, when we put the cloud chamber up against the target and looked at these resonances for soft radiation, we saw the electrons and positrons in great profusion, coming right through the aluminum wall. Then, put a piece of lead in, I mean a half-inch of lead, between the target and the cloud chamber—*poof!* it stopped. You wouldn't want to use a half-inch, say, if something like a millimeter will do it. Whereas when you did that with the old resonances, then the gamma rays just made that many more secondaries.

So Charlie and I, as I remember, didn't really catch on to what was going on. Although I have to be a little careful. I think, frankly, that I finally realized that there were electrons and positrons coming from the target but I didn't realize what caused them. I remember once I came in that door. I was in this office, and Charlie was in the next one, and I was coming in the door, and I said, "Charlie, I think I know what it is. Those electrons and positrons are pairs, coming directly from the target." Charlie says, "That's right. What in heaven's name can they be?" And it wasn't until we talked to Oppenheimer and convinced him, that he then pointed out that if the first excited state of oxygen-16 had the spin and parity zero plus, that it could not give a single photon by going to the ground state, which has the same spin and parity. That's a forbidden transition; the photon intrinsically has angular momentum, so it's got to carry away angular momentum. The transition goes from zero to zero angular momentum—you don't have any to give away. So you could have two photons emitted, but because the state was 6 million volts, it preferred to make an electron-positron pair. And then Oppie got Julian Schwinger interested in it, and when we published our paper on the experimental aspects, then Oppenheimer and Schwinger published this theoretical explanation.

We called them nuclear pairs for a while, and that's what they are. They're pairs that are produced and emitted directly by the nucleus, just like when a nucleus de-excites, it produces a gamma ray. Well, when this one de-excites, it produces an electron-positron pair, and that's quite different from the ones that the gamma rays then produce as secondaries, you see. What you remind me of in bringing this up is that it was—the question in your mind—was what made it so hard for us [laughter] to get the whole thing straightened out. Because this kind of thing, these nuclear pairs, were just unheard of.

GREENBERG: Even after the Bohr and Breit-Wigner theories of 1936, you continued on undaunted with your earlier research?

FOWLER: We began to realize that nuclear states were much more complicated than we had thought, and were really the result of the collective motions of the nucleons, essentially the Bohr picture. It's perfectly true that this realization took away the motivation to study these states in detail so that we could learn the fundamental properties of the nuclear forces. On the other hand, it stimulated the desire to understand these complexities, because even though the Bohr compound nucleus model is in many ways thought to be directly antithetical to the shell model, nonetheless there are very intimate connections between the two. And, oh dear, the shell model began to be called the independent-particle model, and now there's a term, the unified model, which is used to describe the actual model, which is intermediate between the Bohr collective model and the independent-particle model in one sense and unifies them in another.

It was Bohr's son, Aage Bohr, and his colleague Ben [Benjamin R.] Mottelson, who was an American but has become a Dane and is now a staff member of the Niels Bohr Institute, who were the ones who in large measure showed the relation between these two extreme points of view. So there was a period there when getting the spectroscopy of the individual nuclei was quite exciting. Because even though the problem was complicated, people did make predictions, never trying to pinpoint the energy of the states they predicted but just ordering them. They would say, "We've calculated on this intermediate or unified interaction model, and the ground state of this nucleus is one-half minus, the next state's going to be three halves minus, the next state's going to be five halves minus, and then there's going to be a one-half plus." There were predictions of the ordering of the states with their properties, mainly spin and parity. It was lots of fun and very informative. And then, of course, in addition, we got the numerical position of the states and we determined their width and their partial widths.

It became quite an industry to look at the resonances, determine their exact position, and determine how wide they were. This width is due to the various ways they break up. What was the proton width? What was the gamma ray width? What was the alpha particle width? What was the neutron width?

We had to give up the grand picture that we were going to solve all that one needed to know about the nuclear forces and thus about the physical universe. However, we were going to learn a great number of details about nuclei, and in our case we needed these details, because they were essential to the applications in astrophysics. You had to have the details.

GREENBERG: Let me recall a proposal for an Elizabeth Clay Howard Scholarship you made back in 1938. You explain why you're studying excited states of the light nuclei and the gamma-ray spectra. You said that you hoped that such experimental data "will form the basis of a theory of nuclear structure in much the same way that the study of atomic radiation led to the Bohr theory of the atom." I wonder whether you hoped that you'd get to the nucleus in the same way that people had been able to get to the structure outside the nucleus—with that much precision, that much detail.

FOWLER: We realized from the beginning, even before Bohr, that the nucleus differed from the atom in that the nucleus did not have a massive center of force. The atom has a massive center of force—namely, the nucleus, which is positively charged—and all the light electrons circulate around this massive center, which is essentially the center of mass of the system. And of course that means that the electron potential can be specified as the function of radius. If you have a composite atom with many electrons, you do have to worry about the shielding of the electrons by each other, but that all can be done as kind of a perturbation on the central potential.

We realized right from the beginning that there was no central potential, but we were hoping there was an average potential that each nucleon felt and that the states that we got in such an average potential would turn out to be the actual states of the nucleus. That did not turn out to be the case. That model, which is essentially the independent-particle model and leads to the shell model in many ways, just has to be modified. Because those details were fascinating to us, once we began to understand what we were doing, we didn't lose completely our desire to go ahead. What I said in 1938 was still a hangover from before Bohr of the thought that some average potential and single-particle states, like you have in the atom, would do the trick.

GREENBERG: In his interview with C. C. Lauritsen [American Institute of Physics, June 1966], [Charles] Weiner referred to a paper that Lauritsen and Crane had published in the proceedings of the International Physics Conference in London in 1934 [*Papers and Discussions of the Int. Conf. Phys.* 1: 130, London, Physical Society (1934)]. Weiner asked if they hadn't foreshadowed the idea of a compound nucleus, and Lauritsen said that he was thinking along those lines at the time. I wonder if that means anything to you or not.

FOWLER: It's something that I really can't comment on. Let me say why. It's because Charlie was working with Dick Crane then. I was a new boy. They had given me a job to do to build a cloud chamber. And I really just didn't know enough about the whole business, John, for Charlie to be able to discuss such a thing intelligently with me. I was trying to learn what was in Rutherford, Chadwick, & Ellis. On the other hand, I don't think that Charlie would claim any real priority.

Begin Tape 5, Side 2

GREENBERG: You say you don't think he would claim any real priority in this regard.

FOWLER: No. You see, when I came here in 1933, during that fall term, the first quarter I was at Caltech, Charlie was in Denmark. He and Bohr were fairly close friends. I don't know whether Charlie had known Bohr before he escaped from Denmark or not. Anyhow, Bohr may not have been talking about the compound nucleus in 1933. But Charlie was around when many of these things were developing. The Fermi stuff didn't come until 1934, but Charlie certainly did nothing about it, as he would be the first to point out, whereas Bohr did.

My recollection was that Charlie was just as surprised by Bohr's paper as all the rest of us. You just can't imagine what that did. Here, our own great man Robert Opperheimer had just been telling us, "Well, look, all of this is impossible!" Robert just completely missed the boat. He was so stuck with the single-particle model, and that the states should be wide and short-lived, that he just…. And he caused a lot of trouble for Crane and Lauritsen, because he was very skeptical. He had every right to be, because the results were done with an AC tube and the resolution was so poor. But Charlie stuck to his guns on that.

You know, what he told Weiner might have an element of truth. Charlie stuck to his guns in spite of the fact that he normally was so greatly influenced by Oppenheimer, which may well have meant that he had an inkling that there was another way to do it. But I'm glad you told me about that. It's an interesting point; it could well be that Charlie, in his intuitive physical way of thinking about things, may have come across the idea. It could very well be.

GREENBERG: Do you remember H. P. Robertson's sabbatical year at Caltech in the thirties?

FOWLER: Oh, *do* I. [Laughter] Boy! Yes. That's when we became great drinking companions.

GREENBERG: Can you talk about that?

FOWLER: Well, yes. We were neighbors. Bob and Angela moved into the Athenaeum. I had a room in the Athenaeum, and I was always trying to get into bed with Angela. It was kind of a joke; I was *much* younger. But Angela and I were always trying to pull tricks on Bob. I was poor as a church mouse. I did earn a little bit of money doing various things around the lab for the doctors, and I spent what I could on booze. But Bob had plenty of dough, so he and I had cocktails down in his room before dinner and then we'd have nightcaps together, and so forth, so

I remember that year extremely well.

GREENBERG: Did he ever talk about his earlier experience here in the late twenties?

FOWLER: Never in any detail, no. It was through him that I met Eric Bell. They were great pals. Bell lived right on Michigan Avenue, and he'd invite me over to have a drink with him and Toby, his wife, before I went back to the Athenaeum. Bell gave me the impression that Robertson got somewhat of a dirty deal at Caltech. But I just can't remember the details. But Bell was much more bitter about it than Bob. Bob never…. Oh, he may have mentioned it. Maybe Angela mentioned it. But Bell was quite bitter. Was Robertson Bell's student?

GREENBERG: Yes. They both had been at the University of Washington. Robertson came here first, for his degree [PhD 1925], and I think he was in part responsible for Millikan's having brought Bell here.

FOWLER: Oh. Well, I can't help you with any of the details, except there was this undercurrent. But by the time Robertson came back—what year was it, '50?

GREENBERG: 1947.

FOWLER: 1947. DuBridge came in 1946, didn't he?

GREENBERG: Yes.

FOWLER: Oh. But essentially Earnest Watson was president until 1946.

GREENBERG: That's right.

FOWLER: Earnest knew Robertson was really a great man, and Tolman knew Robertson was a great man. And Charlie knew that Robertson was a great man. And so I think, when Robertson came here, he and Angela had a very happy time. And I think it led eventually to his coming back here. But I don't know any of the details of what happened in the twenties.

GREENBERG: Physicist C. N. Yang tells a nice story about the uselessness of mathematicians to physics. Was this your experience with the mathematicians here?

FOWLER: No, no. Of course, the situation was entirely different then than it is now. Now I would say, regrettably, not only at Caltech but at other places there is very little interplay between pure mathematicians and physicists. There is considerable interplay between applied mathematicians and phyicists. But when I was a graduate student, one of the glories of Caltech was the mathematics department in those days. Millikan had really assembled a very fine group of people. Bell, although he was very eccentric and a very poor teacher, was nonetheless a recognized leader in number theory.

And the really great man was Morgan Ward. Morgan was an extraordinarily good teacher. He didn't teach mathematics currently in vogue then; he taught us the mathematics of advanced functions, Whittaker & Watson [*A Course of Modern Analysis*.] Now, to just illustrate what I felt about Ward, the year I took Whittaker & Watson, it was called Math Analysis. The year I took it for credit, Bell taught it. And I realized that if I was going to be a physicist, I had to know something about advanced functions. I had to know something more than sines and cosines and logarithms and exponentials. I had to know something about Bessel functions and even Mathieu functions and all this, that, and the other. The next year, when Ward gave it, I audited it. I sat in. I asked Morgan, "May I just come and sit in the class? Because I didn't learn anything under E. T. Bell." Ward said, "Of course. Come in." Someplace I've got some beautiful notes. Bell had used that *stupid book* called McRoberts. I've got it someplace, maybe at home, on these functions, which was, well, just impossible to use. Ward went back to using good old Whittaker & Watson. So I learned something about advanced mathematical functions which I've used all of my career.

One other thing: no one in the physics department was prepared to teach advanced quantum mechanics. Houston was busy writing his book on classical mechanics. Oh, yes; Epstein gave a course—a lecture course. And, boy, it was a lecture course! You never asked Epstein any questions. He called it The Quantum Theory, and he tried to base it on a modification of classical theory using the iconal. So I got absolutely nothing out of Eppie's lectures except that he was incredibly good, that he had no notes, came in at the start of the class, went down to one end of the blackboard, started writing and lecturing and would go through, make all kinds of mistakes in his derivation, but always come out with the right answer. [Laughter] And the other thing about Eppie was, every date was wrong by 100 years. He'd come to some big thing in quantum mechanics in 1928, and it'd be 1828. None of us would say a word. We just loved it. We just loved it. So there we were.

I had had a discussion of Bohr orbit theory at Ohio State in a course on X rays by Pool. I learned about the K shell and all those things, using Bohr orbit theory. The man who took the job of teaching advanced quantum mechanics was Aristotle Michal in the math department. He used *Wave Mechanics* by [Richard] Courant. It was in German, which was tough, but he gave beautiful lectures, so we learned something about the basis of quantum theory.

Oppie was only here for one term, so he felt that he was so much more interested in talking about nuclear physics that he never gave any fundamental treatment of quantum mechanics. He just assumed you were supposed to know that stuff. Of course, all that he wanted us to know was Schrödinger's equation, really, and the Dirac equation. The basic principles, the commutation relations and things, we had to get from the math department.

As far as I'm concerned, when I was a graduate student between 1933 and 1936, I got some of the things that have been most useful to me in my career from Morgan Ward and Aristotle Michal in mathematics, because they were both very much interested in the applications of these things to physics. Aristotle Michal realized that quantum mechanics and group theory were going to be big things. He approached the whole thing from the mathematical point of view but then told us a lot about what was going to be in physics. He didn't know all the details, but he knew what things we should know that we weren't going to get from anyone in the physics department.

But that all changed. That all changed. There was a short period after the war when Houston became chairman of the physics division, and he asked Morgan Ward and myself to—mind you, Morgan Ward and myself—to draw up the courses which were to be in the physics option and in the math option. Morgan was to help me with the physics option. I think he had someone else who worked with him on the math option. Then [H. Frederic] Bohnenblust came, and [Robert P.] Dilworth, and the chap they brought—Marshall Hall. And before we knew it, in the fifties and sixties these two parts of the division were just going apart like that [gestures].

Actually, it's my feeling, John, that it was inevitable. [Richard P.] Feynman and [Murray] Gell-Mann came, and Feynman felt very strongly that physicists should do their own mathematics, and furthermore that they could do the mathematics that was useful to physicists. Feynman invented a whole new way of doing quantum mechanics, and his diagrams didn't spring from any contact with the mathematicians; in fact, Dick has generated things. And he sometimes made remarks that mathematicians don't really help very much. He got very angry about the way mathematics was taught in the California schools. He was on a committee or something, for the governor or somebody. So you see, the whole thing turned around. We had two superb theorists who liked to talk about the fundamentals and talk about them in an entirely unique way—especially Feynman. And the mathematicians went off doing other things.

One result at Caltech was that we gradually built up, in kind of a sub rosa fashion, it seems to me, an applied mathematics, partly in engineering and partly in the Division of Physics, Math, and Astronomy. And what's the story behind why it's divided is something you'll have to find out from others. But it's a clear indication of a schism between the pure mathematicians and those who are interested in applications.

Whenever I want to know anything now, the man who must be given a great deal of credit is Hans Liepmann. In the years since what I'm doing no longer interested either Feynman or Gell-Mann, and a lot of the things that I do have roots in classical things anyhow, I go to Hans Liepmann. He can tell me what to go read, you see, to find out. Now, I guess Hans doesn't consider himself an applied mathematician. The other man is the Englishman, what is his name, [Gerald B.] Whitham. You can ask questions and he'll give you a straight answer and put you straight.

I became very friendly with Bohnenblust, because he was a good friend of Louie Ridenour, who was a graduate student at the same time I was. I got a lot of help from Bohney. Some strange function would appear in a paper and I never heard of the damn thing, and I could ask Hans Liepmann and he'd tell me, "Oh, you want to read about that? There's a book on it." Or I'd go to Bohney.

GREENBERG: Bateman was never much use to you? He died in 1946.

FOWLER: No, but [Arthur] Erdélyi was very useful. Yes, I'd forgotten Erdélyi. You see, he was here for years editing the Bateman manuscripts. And Erdélyi was very helpful. He was a very quiet man, and you could never get him to say very much, but if you wanted to know about some function that Bateman had ever done any work on, Erdélyi was the man to talk to. I used to wander into his office and I'd say, "What about this thing?" And Erdélyi would say, "Oh, Bateman wrote three papers on that, and I'll give you the references." Yes, Erdélyi was very useful. And when he left, I distinctly remember feeling, "Well, there goes the last of the old guard, a mathematician interested in functions that are useful to physicists."

GREENBERG: All right. Last thing before we finally get into nuclear astrophysics. What was the morale like here in the late thirties? I came across a reference, a statement DuBridge made in which he said he thought—this was before he came—that it was rather low.

FOWLER: Well, if it was, I never noticed it, because certainly the morale in Kellogg was incredible. I'm talking about the period between 1933 when I became a graduate student and January first, 1941. We drank too much and caroused too much, but, boy, look at the list of papers! Now, I suppose there were problems in other places. I don't know whether the Linus Pauhing problem had—no, the Pauling problem hadn't arisen. That came afterward, when DuBridge was here. I don't know what Lee could have been talking about. Was he talking about the years that he was here, in the late twenties?

GREENBERG: No, no.

FOWLER: How did he know about this? .

GREENBERG: I don't know how this all came out.

FOWLER: I'm very surprised, because to me geology was jumping; of course, there wasn't any astronomy. All the astronomy was at Mount Wilson, but, boy, that was jumping! Charlie's closest friend was Sinclair Smith, who essentially designed the 200-inch telescope mount, so we had very close relations with people at Mount Wilson. And of course there was another very active lab—Houston was productive, and Ike Bowen was the acknowledged leader in atomic spectroscopy in those days. He published these one-page papers in the *Physical Review* every month, giving all the lines that he'd found in some zirconium tube, or something like that. Smythe was very active. I'm surprised. There were good graduate students all over the place. My heavens! Look at my class—Ramo, Wooldridge, Pickering, Pierce, and myself. And Charlie Townes came along. Good heavens!

GREENBERG: There was a mathematician in your class named Angus Taylor.

FOWLER: Angus Taylor? Terrific, yes, yes. The only sign I ever had of things that somehow didn't work out right—Conyers Herring, who is a very famous physicist, came as a graduate student the same time I did, but he only stayed one quarter, so maybe he saw something I didn't see.

GREENBERG: All right. Let's get to the—

FOWLER: Well, it's four o'clock now. I'm a just a little weary. So have we finished all of the preliminaries? Now what are we going to do the next time?

GREENBERG: We are going to take as long as it takes to—

FOWLER: What is the first question you were going to ask me?

GREENBERG: How did you first come in contact with Bethe's work in 1938 and 1939? How did it come to your attention?

FOWLER: Bethe had written the 1936 and 1937 articles in the *Reviews of Modern Physics*. That's what established Hans Bethe in all of our minds as really the best. That's where, other than what I'd learned from Robert [Oppenheimer], that's where I really learned nuclear physics.

GREENBERG: OK, so you read everything that he wrote after that.

FOWLER: And from then on we paid attention to what Bethe did. Now, it could very well be—and, in fact, I think it was—that Robert first learned about Bethe's carbon-nitrogen cycle work at some theoretical conference that was held in the East.

GREENBERG: The Washington conference of 1938.

FOWLER: Was that it? I have a feeling that Robert was the first one to tell Charlie and Tommy and me that this work we were doing, using the AC accelerators, which took so darn long to get published, although there was a publication—Robert almost certainly was the first one to tell us that Bethe had pointed out the importance of these reactions in the sun and other stars. But the main thing was, Bethe first had a brief letter to *Physical Review* in which he says [in effect], "People have been talking about this all summer long, and I want to get my point of view straight and I'm going to publish a full paper." I think this letter came out in January of 1939, although, as I say, we had heard about it in 1938, just after we'd finished building the pressure Van de Graaff and were doing things with it—on the carbon-nitrogen isotopes. Then, in *Physical Review* in March of 1939 [55: 436 (1939)] out came Bethe's paper ["Energy Production in Stars"], and I can tell you, it was reading that paper word for word, two or three times, that was the thing that convinced me, Boy, this is the way to go!

GREENBERG: And that's essentially it.

FOWLER: That was it.

GREENBERG: After that, you went back and you did a little research on the history of the stellar energy generation problem?

FOWLER: Well, Bethe gave references, and mainly to [R. d'E.] Atkinson and [F. G.] Houtermans. Then I uncovered Eddington's Cardiff address. Then I read what Perrin had said about nuclear energy in stars, and then I read what Kelvin and Helmholtz had said about gravitational energy, very quickly. But I have to be a little careful because after all this long time, John…. I also learned an enormous amount in the seminars that Ike Bowen held just after the war.

GREENBERG: Oppenheimer was working on neutron stars in the late thirties. Astrophysics. Did this influence you?

FOWLER: Well, only that it was terribly exciting. He and George Volkoff, his graduate student, who would come down here in the spring term, were able to solve this problem when Zwicky had made such a mess of it. Tolman, of course, worked on it, using polytropes. I personally like very much to do things with polytropes, as Tolman himself did. His solutions were almost as good as Oppenheimer and Volkoff's. The neutron star is a polytrope of Index 3, whether you like it or not.

The point was that Oppenheimer and Volkoff's was an exact, elegant solution. It was beautiful. But it didn't influence me, because there was this kind of dichotomy in the lab. I was working on gamma rays, and there were other people working on neutrons. You couldn't do everything. It was pretty clear that the properties of neutron stars didn't depend on anything you were going to do on nuclear reactions. You were talking about neutron matter, and now, of course, we know there are very important reactions that occur.

GREENBERG: Well, the neutron hypothesis came up later in your work with the Burbidges. That has no connection?

FOWLER: No connection with what Oppie did on neutron stars.

GREENBERG: One of our problems is to try to come up with some sort of an assessment of astrophysics at Caltech in the thirties. In other words, astrophysics before nuclear astrophysics. Was there astrophysics at Caltech in the thirties? Now, obviously, there was some.

FOWLER: Yes, but the main thing was Ike Bowen's work. Ike Bowen was interested in the application of atomic physics to astronomy. And all of the astronomers at Mount Wilson weren't interested at all in nuclear physics. They were interested in atomic physics. Every time Ike would get a new line—get the wavelength of a new line of some element, that's something they could use. But then Ike also was the one who explained the nebular lines. That was—Oh, my goodness, that was very exciting! And that was astrophysics, but it wasn't nuclear astrophysics, it was atomic astrophysics. But, boy, that was hot stuff! Houston dabbled a bit in that sort of thing, but Ike was the one who had the vision. After the war, he realized that things were going to change, that astrophysics was going to widen its horizon and go from atomic physics into nuclear physics. And that's why he encouraged Charlie and me and Tommy to stay in low-element nuclear physics and arranged these seminars at his home so we'd get hooked, thoroughly, by listening to Olin Wilson and Walter Baade and Rudolph Minkowski. And Ike gave one, too. Oh, god, it was just marvelous, listening to their lectures on what the stars were all about! And then I talked a little bit about what we had already done on the CN cycle.

WILLIAM A. FOWLER

SESSION 6

September 23, 1983

Begin Tape 6, Side 1

FOWLER: [Explaining advantage of cloud chamber for Kellogg experiments before the war]. And the major reason was that to do the pair formation, the absorption coefficient of gamma rays in material, due to the Compton effect, drops very rapidly—something like one over the energy, as I remember. Then when pair formation starts with a million volts, the absorption begins to rise. So you get a double-valued function. Measuring the absorption coefficient, you can either be on one side of the absorption versus energy curve or over on the other. By that method, 17-million-volt gamma rays were absorbed in lead just as much as 2-million-volt gamma rays. That was one of the reasons why we went to the cloud chambers. But of course the cloud chamber gave much more precise values for the energy of the gamma rays, in any case. When you had a complex spectrum, there was just nothing that the absorption method could do. It couldn't tell you that there were several gamma ray lines, whereas the cloud chamber could.

GREENBERG: Now, what was the significance of Lauritsen's production of neutrons by bombarding beryllium? You didn't go on and use neutrons as projectiles.

FOWLER: No. It was mainly to show that you could duplicate much more readily in the accelerator laboratory the reactions which were extremely difficult to produce using natural radioactivity. And this is a point that we should make sure about. You see, the neutrons were discovered by Chadwick, and he used alpha particles. I think his target was beryllium. And the beryllium-9 alpha-*n* is a very copious source of neutrons. Chadwick was able to use the very strong—at that time—sources available in the Cavendish to bombard beryllium and see the neutrons. What Lauritsen and Crane and Soltan showed was that Chadwick's experiment could be duplicated. In fact, I think they actually bombarded beryllium-9 with alpha particles and made neutrons in exactly the same way that Chadwick did. But, you see, an accelerator can produce a beam of alpha particles that is incredibly stronger than even Curie sources of natural radioactivity. And then, of course, there was a great deal of interest in the properties of the neutrons.

What Charlie Lauritsen did was just line his electroscope chambers with paraffin, which contains hydrogen, and take advantage of the fact that when a neutron collides with a proton in the hydrogen in the paraffin, one has essentially a billiard ball collision between particles of very closely the same mass. Thus, if there's a dead-center collision, the proton is knocked on with the full energy of the neutron, and the neutron just stops. So that was all.

You're quite right that neither Lauritsen nor Crane—nor I, when I came—continued studies of neutrons. The production of neutrons and nuclear reactions, the spectra of the neutrons, became the province of Tommy Bonner from Rice, who came here as a National Research Council Fellow. He built a high-pressure cloud chamber which could be filled with hydrogen at high pressure, and then the knock-on protons produced by the neutrons produced in the Lauritsen tubes could actually be seen. He would bombard things, mostly with deuterons, and look at the various neutron groups produced, because this told him something about the excited states in the residual nucleus of the reaction. But that was almost entirely done by Bonner. Crane and Lauritsen were mainly interested—as was Lawrence at Berkeley and Tuve in Washington—in showing that accelerators could be used to produce nuclear reactions at a much faster rate than could be done with natural radioactivity.

With the exception of Bonner's work, and some later work after the war by Staub and Stephens, Kellogg did very little work on neutrons. We were mainly concerned with protons in, gamma rays out, protons in, alpha particles out, because those were the types of reactions where we could learn a great deal about nuclear spectroscopy and those were the reactions that take place in stars during hydrogen burning.

GREENBERG: Is there any connection at all between the paraffin lining the Lauritsen electroscope and Fermi's use of paraffin in his 1934 discovery of the slow neutrons?

FOWLER: Let me think…. My answer to that question has to be based on the purposes of the experiments that Lauritsen was doing contrasted to the experiments that Fermi was doing. It's very difficult to tell who did what first. We know that Lauritsen introduced paraffin into his electroscopes in 1933. We know that Fermi was probably using paraffin to slow down neutrons, if not in 1933, certainly in 1934. The main point that I want to make is that there is no direct connection in physics. Lauritsen was using the paraffin as a converter. It converted neutrons into knock-on protons. The protons produce ionization, and thus they tell you something about the intensity of the neutrons that you are producing. Fermi was using the paraffin to slow down the neutrons, so that when they subsequently struck a target of some heavy element, they gave the remarkable cross section proportional to the $1 / v$ law, and gave much more intense radioactivity when they were slowed down than they did with the energies with which they were produced. So there is no logical connection.

Whether Lauritsen read that Fermi was using paraffin to slow down neutrons.... Because, as I just said, when a neutron hits a proton dead on, it gives it all its energy. When it bounces off at forty-five degrees, it comes away with half the energy. Now, whether Lauritsen read something about Fermi's work in slowing down neutrons with paraffin and immediately said, "Oh, that's also a good way to detect neutrons," I don't know. My feeling, from the dates, is that Charlie's idea must have been independent of Fermi, because by the time I got here in 1933 there was very little talk in Kellogg about Fermi's results.

I suppose in a way we were rather insular, and we were so dead set on doing the things that we had in mind that, for example, when fission came along, we didn't turn a hair. I remember Luis Alvarez coming down and telling us all about this exciting new business and why didn't we get into it. "Well," we said, "we're busy doing all that we can do, and anyhow you fellows can beat us at that game." On the other hand, when Tommy Lauritsen went to Denmark after he got his PhD, Bohr put him to work on fission fragments, because Bohr had ideas about how these fission fragments should lose energy and scatter. Tommy did a beautiful job, working very closely with Niels Bohr in 1939, when he was in Copenhagen. And that's all been published. You can see what Tommy did and the remarks he makes about how Bohr wanted him to be absolutely certain that what results he was getting were right.

GREENBERG: Do you know, roughly, how much time it took Lauritsen to convert the high-voltage X-ray tube to the positive ion accelerator at the very beginning? Are we talking about days, weeks, months?

FOWLER: The answer is, I don't know precisely, because that was all done before I came. My estimate would be that Charlie would have worked night and day with Dick Crane and whoever else—maybe Bill Harper was in on it, and Soltan—Charlie would have worked night and day to accomplish the transfer from an electron accelerator to an ion accelerator in as short a time as possible. I wouldn't be very much surprised if he did it in a period of about a month. It certainly did not take him a year, and I think I could say with equal certainty that it didn't take him six months.

Knowing Charlie, once he found out what Cockcroft and Walton had done, he would just work at getting an ion source into his accelerators, which, you see, was a quite simple operation, in the sense that he didn't have to change the accelerator at all. All he had to do was mount an ion source inside the long reentrant tubes that he had and put a power source up there, a little electric generator, and drive it by an insulated belt from a motor down at the bottom, and he was off and running. So I'd be very much surprised if it took Charlie more than a month.

Unfortunately, there are no records of just what happened, so all I can say is that is what I would guess, based on Charlie's proclivity to work at any idea he had in mind and get it done. Also, he was very certain that Berkeley was up to the same thing.

GREENBERG: You know about Lawrence's hypothesis of the disintegration of the deuteron in early 1934, which Lauritsen repudiated due to contamination. Did Lauritsen rule out the disintegration of the deuteron as a possibility, having shown that Lawrence's account of the whole business was fouled up?

FOWLER: No. The deuteron was subsequently disintegrated, I think by [Maurice] Goldhaber, using gamma rays.

GREENBERG: That's right.

FOWLER: What Lawrence was saying was that when you bombarded targets with deuterons, the deuteron broke up. Now, at that time the mass of the neutron was not known very well. The mass of the deuteron was measured by mass spectroscopy relatively early. So the major question was, What was the binding energy of the deuteron? You see, Ernie could have been right, and in effect he is right. If you smash a deuteron with high enough energy into a target, you can disintegrate it, and you'll get a proton-neutron out, although the whole thing will be very complicated. Because, competing with that kind of a direct effect is the fact that the neutron in the deuteron can penetrate into the nucleus very simply. That's the old Oppenheimer-Phillips process. The protons come off, then the neutrons subsequently come back out, so that the whole thing is kind of a moot question. At the time that he made these claims, Lawrence's cyclotrons did not have enough energy. See, when you fire a deuteron at something, you lose energy by going into the center-of-mass system, and so forth and so on. But that wasn't the main reason. The main point was that Ernie thought that what he was seeing was the disintegration of the deuteron because he found the same effects, the same number of neutrons and protons, regardless of what kind of a target he was bombarding.

The thing that I think Charlie and others mainly showed was that Lawrence had all kinds of contaminations—lithium and boron—around, so that when he bombarded a somewhat heavier nucleus, where this process would a priori be somewhat less probable, he was just seeing the results of the contaminations in his targets on the walls of his cyclotron. It took a long time to learn that nuclear physics was a pretty tricky business, because if you had light nuclei as contaminants in your targets, they could give effects that would mask what you got from the targets that you were trying to study. A proton beam can be contaminated by deuterons, and that was of course what Lawrence and Tuve claimed about Charlie's results from carbon plus protons. Tuve just said, "Well, you've got deuterons in your protons." Charlie was able to show, because the excitation curves were different, that this just was not the case. And incidentally, he was studying one of the first resonance effects—protons on carbon-12 go through a resonance; deuterons on carbon-12 is just a continuum of rising probability. All those things now seem very straightforward and kind of elementary, but it took a long time to disentangle all the effects.

This goof of Ernie's is one that's quoted a great deal. But we made a lot of mistakes, too. Everybody did, everybody did. So that was part of the learning game. You thought you had something straight and then you did another experiment—or someone else did another experiment [laughter]—and showed you that you were completely wrong.

I forget the exact details now. Charlie and I bombarded lithium with deuterons and got two proton groups, and we thought one of the groups was from lithium-6, but it turned out both of them were from lithium-7, because lithium-7 has an excited state—which we later on studied in great detail—roughly 450 kilovolts above the ground state. It was a great game.

GREENBERG: The reason I brought up Lawrence and the deuteron at all is that shortly after Chadwick and Goldhaber, it was pointed out that photoelectric disintegration of the deuteron is the reverse of radiative capture. Did Lauritsen have any such thing on his mind?

FOWLER: I doubt it.

GREENBERG: OK. Well, then, let's go to nuclear astrophysics. It was Oppenheimer who brought Bethe's work on stellar energy generation to your attention.

FOWLER: That's right. Yes, that is correct.

GREENBERG: All right. What, if anything, did the lab do to implement Bethe's work?

FOWLER: Well, Bethe's work came to be known in 1938. He was in character in the amusing first few lines of the letter he wrote about the CN cycle, saying he was being quoted all over the place and he wanted to get something down on paper in this letter and would provide a more detailed discussion later. In fact, I think the letter wasn't published until one of the first issues of the *Physical Review Letters*—which at that time was part of the *Physical Review*—I think it was 1939.

The point I'm trying to make is that Bethe's ideas were well known in 1938, and Oppenheimer heard about them and communicated them to us, because we were just beginning in the latter part of 1938 and 1939 to bring the new Herb-type Van de Graaff electrostatic accelerator into operation, and we were bombarding the isotopes of carbon and nitrogen with protons. As a matter of fact, we had done that previously, just to produce radioactive elements and study their half-lives. That was mainly my thesis, although we would usually use deuterons, because they produce much more activity than the protons did.

As soon as we got our electrostatic accelerator going, we began to do excitation curves of the light nuclei bombarded by protons, looking mainly at the gamma radiation using the cloud chamber that we had built about the same time. So Oppie came and told us that these reactions that we were studying—the isotopes of carbon and nitrogen bombarded by protons—had been incorporated into a marvelous scheme for energy generation in stars by Hans Bethe, who had labeled it "the carbon-nitrogen cycle." Well, we really did not do very much more at that time, because the war came along. We went away for a year. We left here—oh, it must have been January 1941. You might say, "Well, why didn't you do a lot more in 1939 and 1940?" The point was that we were just pretty slow, I guess.

GREENBERG: Did you see the long-range implications?

FOWLER: Whether we saw the long-range implications or not, John, I don't know. What we mainly saw was that what we were doing in the laboratory had implications in the sun. You see, in Bethe's first article the sun operated on the CN cycle. And that alone was enough of a motivation, because we immediately saw that the rate at which energy was generated in the sun depended on the nuclear cross sections translated into reaction rates, and that this was then a playground for the experimentalists, because no theorist could predict the rates a priori. Here was a role that a laboratory could play in a very exciting problem, and one that was especially attractive to us, because, after all, Caltech was next to Mount Wilson. So there was a lot of astronomy going on. After the war, we went back to it and then did quite a bit of work.

Although we, as I've said many, many times, made a conscious decision to work on nuclear reactions that were important in stars, that was not to be the only program. It was clear to us that there were some very exciting things to be done in nuclear physics per se, so that after the war the first things we published were not the cross sections for protons on the carbon-nitrogen cycle. In fact, that didn't come until around 1949, you see.

One of the reasons—I think this is an important point—was that as a result of those seminars that Ike Bowen held in his home on Fridays, right after the war, it became very clear to us that what was needed in the astrophysical applications were the cross sections at extremely low energies. It's paradoxical that the mean energy of a proton at the center of the sun is about 1.5 kilovolts—only 1,500 volts. The effective energy—due to the fact that the higher-energy protons can penetrate Gamow's Coulomb barrier so much more readily—the effective energy is anywhere between 10 and 30 kilovolts. Now, with the first pressure-insulated electrostatic accelerator we built, we were able to go 1.5 *million* volts. And we studied the carbon-nitrogen cross sections.

However, we realized that what Bethe and the other astrophysicists needed was very low-energy data. As you go to low energy, the cross sections become so small that a pressure-insulated electrostatic accelerator is not the optimum way to do the job. In fact, I've never mentioned it, and I forget when it actually took place, but I was very evangelical about this.

We did build, in 1949—when Bob Hall came as a graduate student—a low-energy accelerator that was kind of a Cockcroft-Walton accelerator, which went to a maximum of 150 kilovolts. Bob Hall was able to push the cross section measurements on carbon-12 plus protons down to about 80 kilovolts. Then we also built, very shortly after the war, a 700-kilovolt machine used mainly by Ward Whaling but also by my student, Joe [Joseph L.] Vogl, who did a really bang-up job from 700 kilovolts on down to a couple of 100 kilovolts on those reactions.

The point was, there were so many things in pure nuclear physics that we wanted to do that we did not just concentrate on the astrophysics. We realized that to do what was really needed in the astrophysics, we had to go to very low energies, even lower than we had anticipated, rather than going to the high energies that Lawrence and other people were doing. And it took time to build high-current accelerators that would operate at low energies.

The other point I wanted to emphasize is this business that I was very evangelical about. I found that there were, sometime in the early fifties, very high currents available at Livermore at low energies. And I got Billy Lamb and Ross Hester at Livermore interested in the problem of pushing the carbon-nitrogen cycle reactions down to as low energy as possible. So they kind of bootlegged time on an accelerator that was built for—oh, I forget exactly what dubious purposes. What the dickens was it? Fusion research. Anyhow, they bootlegged time, put in carbon and nitrogen targets and bombarded them with enormous currents. It was an incredible technical feat, because they had to cool the targets with jets of very rapidly moving water back of the target and mount the targets on copper. They pushed the reaction down to very low energies, and fortunately they were able to overlap with what Hall did.

Between what Kellogg did and what Livermore did, we got a pretty clear picture of what the carbon-nitrogen cycle cross sections were like, and to make a long story short, we eventually showed that the carbon-nitrogen cycle did not power the sun, that the alternative that Bethe and his student Charlie Critchfield had presented [*Phys. Rev*. 54: 248 (1938)]—the so-called *p-p* chain—powered the sun, and the carbon-nitrogen-cycle cross sections didn't get large enough until you go to somewhat more massive stars, twenty percent more massive than the sun, which had correspondingly higher central temperatures and densities. And then the carbon-nitrogen cycle, which is very sensitive to temperature, takes over from the proton-proton chain.

GREENBERG: Where had Bethe gotten his data for these reactions, or didn't he bring it down to that level?

FOWLER: Oh, yes. He did bring it down to that level. He scoured the literature. Remember when Bethe wrote his articles in 1936, he had to scour the literature. He knew everybody who was working in nuclear physics, and he had Stan Livingston collaborate with him on the third of those wonderful *Reviews of Modern Physics* articles, in which they looked at all the experimental work that had been done on every nuclear reaction up to that date. So Bethe had quite a bit of information available; even some of the crude earlier work that we had done is referred to in that paper.

I have to say that the major reason that Bethe went wrong in thinking that the carbon-nitrogen cycle powered the sun was because the astronomers told him that the sun was fourteen percent nitrogen by mass. Actually, it's less than half a percent, we know now. So he had too much nitrogen there. It turns out that it's the N^{14} *p*-gamma reaction—the slowest one—that limits the rate of the CN cycle. And he just had so much nitrogen in there that the overall rate was considerably larger than—

GREENBERG: —than it really is.

FOWLER: His cross sections were rather wrong, but not by this enormous factor—wrong by maybe factors of two or three. In addition, he and Critchfield underestimated the rate of the proton-proton reaction. There are some tricky statistical factors that for some reason or other they got wrong, which is not very typical of Bethe. So a combination of circumstances led Bethe to underestimate the effects of the direct proton-proton chain and overestimate the effect of the CN cycle, and so he had the crossover point where the CN cycle replaced the *p-p* chain at a star somewhat less massive than the sun. Now we know that the crossover is just slightly heavier than the sun. But it was our quantitative measurements after the war and the work at Livermore that made it possible to get the whole picture straight.

GREENBERG: This is what brought him out to Caltech for the first time after the war, I guess. It was Hall's work, right?

FOWLER: I think he was greatly interested in that. I think, to be fair, one would say that Hans was interested in so many things that that couldn't have been the only reason he came. I think he visited UCLA and gave some lectures over there at that time. I can't be all that precise. But certainly one of the reasons that he came was to talk to us about our measurements on his cycle and to convince himself that we weren't covering up anything and that what we were doing was essentially correct.

GREENBERG: Was the Staub-Stephens work in 1939 on the problem of stability of elements of mass 5 done with pure nuclear physics in mind? Were you following any of Gamow's cosmology at that time?

Begin Tape 6, Side 2

FOWLER: No. I think one can say fairly definitely that Gamow didn't really begin to talk about the Big Bang until after the war. So Staub and Stephens were mainly interested in the basic nuclear problem: Would mass 5 be stable when you closed the S-wave shell at two protons and two neutrons at mass 4? They were primarily interested in the nuclear physics, just as we were when we got interested in the similar problem at mass 8. We had no idea, as far as I can remember, that it was going to be involved in the production of carbon-12. We were interested in knowing—but this was after the war—whether beryllium-8 was stable, and if it wasn't stable, how much was it unstable? And we did an experiment which found within a few kilovolts the energy instability, which is now known to be the correct value. We got 89 KeV, and it's now known to be around 92 KeV, something like that. So we didn't miss it all that much.

GREENBERG: Why didn't the other labs that had gotten into the business of measuring excitation curves and resonances—Tuve and Crane and Herb—why didn't they go into the astrophysics? Is it because of this irony that the energy goes down instead of up?

FOWLER: I think in large measure it was that boy, they could see that it was going to be an awfully tough racket! And there were so many exciting things in nuclear physics. Herb, for example, was influenced by Gregory Breit, who felt that if you measured the proton-proton force and the proton-neutron force, that you could solve all the secrets of nature. We were convinced that by looking at the excited states of the light nuclei you could do the same thing. Well, Breit got Herb to do an incredible job on studying the scattering of protons, which starts out at low energy, just like Rutherford's scattering, but then very soon shows a deviation due to the intrinsic nuclear force between two protons. And that was the fashionable thing then, John. Nuclear physicists talked about this stuff that those guys at Caltech were doing—"Well, you're doing some nuclear physics, but you're wasting an awful lot of time on this other stuff." We hadn't even invented the term "nuclear astrophysics" then.

So it wasn't until much later that other laboratories began to become involved. It wasn't until the High-Voltage Engineering Company began making Van de Graaffs so people could buy them that other laboratories all around the world and all over the United States were able to buy accelerators. And some of those laboratories became interested in the astrophysical problems.

GREENBERG: When does this date from?

FOWLER: Well, this kind of dates from the sixties.

GREENBERG: Oh! [Laughter]

FOWLER: Yeah, it was much later, much later. I think it was in the late fifties and the sixties that anyplace in the country—largely through NSF [National Science Foundation] and ONR [Office of Naval Research] funding—that wanted an electrostatic accelerator could get one, and most of them are just all gone on the junk heap.

Some of the out-of-the-way places became interested in the astrophysical applications. Actually, a great deal of interest came, considerably later, in Germany. People in the Soviet Union became very much interested in the astrophysical problems. And Japan. Mostly theoretically, not experimentally.

Fig. 10. Jesse Greenstein at Caltech *ca* 1950. Caltech Archives.

Our situation was rather unique, in that we were part of an institution that was very closely related to astronomy. Jesse Greenstein came—what year was it, 1948? And Jesse was a powerful influence in keeping our nose to the grindstone on astrophysics. Here was someone who was interested in what we were doing and who was the new head of the astronomy part of the Division of Physics, Mathematics, and Astronomy. Ike Bowen had been very closely interested in what we were doing, and when he left off, then Jesse Greenstein took over. So we were in a very special position *vis-à-vis* all the other laboratories. Caltech's astronomers were interested in what we were doing, and we didn't have to get our pats on the back from our colleagues in nuclear physics. Nonetheless, we did a lot of rather very specific jobs in nuclear physics. We didn't set up a systematic program, except in nuclear astrophysics, but in nuclear physics we went for—well, first of all, we did a lot of work on measuring the mass of the neutron. And then we hit on this idea of looking at the instability of beryllium-8. Then we looked in on the idea of looking for the recoil given to neutrinos in the decay of lithium-8. We hit a lot of what we thought were very interesting problems, but there was no systematic approach, such as Herb under Breit's guidance established at Wisconsin. That's about the way it was.

GREENBERG: I couldn't find any specific references to work in mass spectroscopy in Bethe's statement in 1939. Is the determination of masses important?

FOWLER: Of course. It's very important, but that aspect of the astrophysical problem was well understood. The mass spectroscopy had shown that four protons or four hydrogen atoms are heavier than the alpha particle of a helium atom by an amount of about 0.7 of a percent, which would give plenty of energy over a very long timescale. So that part of it was known—in the twenties.

What Bethe did—and there had been previous work: Atkinson, Houtermans, and others—was pinpoint the specific reactions that could work in the sun. Most people don't realize that what he showed was that lithium, beryllium, and boron were too rare to be a fuel. Things heavier than the carbon-nitrogen isotopes can't be disintegrated at the temperatures that are available in the sun, and so the CN cycle was it, not only because of the cyclic property—which was the marvelous thing—but because he was able to dig out the rates even from the crude estimates that we made. He knew the rates were right, because all you had to do was increase the temperature very slightly, once you were in the right ballpark. In fact, I think he thought the temperature of the sun was something around 20 million degrees instead of 15 million. In addition, he also realized that the basic proton-proton reaction was another possibility. That, of course, was a great disappointment to us, in a sense, because the rate is so small that even to this day we haven't been able to measure it in the laboratory. On the other hand, it can be calculated with extremely high precision using nuclear parameters that are determined indirectly, so that in a sense it's the best known of the nuclear processes in the world, and Bethe and Critchfield, as I said, got it wrong. It was Ed Salpeter who found their diddly little errors and got it all straightened out and confirmed, about the same time with what we were finding—that the CN cycle just didn't work in the sun but the proton-proton chain did. That's still what we think, although the number of neutrinos that we expect aren't found. We know the CN cycle can't do it. If that was the full source of energy in the sun, the neutrino detection rate that [Raymond] Davis gets should be another factor of ten higher than what one gets from the *p-p* chain. So we know the carbon-nitrogen cycle can't do any more than a few percent of the energy generation of the sun. That's just what the nuclear reaction rates tell us, when you work them all out.

I think it's fair to say that that's largely the result of work here in Kellogg—getting all the details of the relative rates of the carbon-nitrogen cycle. We extended it to the carbon-nitrogen-oxygen bi-cycle. And then there's a tri-cycle. So all of the details, for both the CN cycle and the subsequent reactions in the *p-p* chain, are largely due to work here in Kellogg.

GREENBERG: In his paper in 1939, Bethe divorced stellar energy generation from stellar abundances, for the most part. And then right after the war, at the Bowen seminars, you found that the astronomers were interested in stellar abundances and that this looked like there might be a way to hook this up with what you'd learned from Bethe. I'm trying to straighten out these two things that Bethe divorced.

FOWLER: Well, what Bethe said was that he didn't have very good information on the stability or instability of beryllium-8. He wasn't able to make a very good calculation, although he did make a calculation of the rate of the three-alpha reaction. It didn't occur to him to put a resonance in. In the second stage, Hoyle came up later with the beryllium-8 plus an alpha particle. So Bethe just concluded in that paper of his that there would be no further nucleosynthesis beyond helium in stars, but he qualified it—it's very clear, because I had the paragraph all typed up and had a slide made of it. He qualifies what he says with the phrase "under the present conditions." And for a main-sequence star, he's absolutely right. The only thing that happens in a main-sequence star is hydrogen burning.

It's a little surprising that he didn't come up with the idea that you could start with carbon and, through his CN cycle, make carbon-13, nitrogen-14, and nitrogen-15. I don't think that's in there. So it's a little surprising that he didn't realize that there was a chance that the two rare isotopes—carbon-13, which is one percent of carbon-12, and nitrogen-15, which is 10^{-4} of the nitrogen—might be made in the CN cycle. Of course, we're not absolutely certain nowadays that that's the way they're made. It's a little strange that he didn't put that all together. But then, I have to say that neither did we.

You have reminded me of the fact that the astronomers here were interested in the abundance problem. I'm afraid that just sailed right over my head, until I met the Burbidges, and I think over Charlie's and Tommy's too, because I don't find any place that we started thinking about abundances. That came much later, and of course Ed Salpeter started it by working out the nonresonant three-alpha reaction to make carbon-12, using in part our value for the instability of beryllium-8, which showed that since beryllium-8 was only unstable by 90 kilovolts, it would build up to a substantial concentration in a red giant star, where the temperatures correspond to 10 kilovolts or so. So the Boltzmann factor is not all that small.

Then Fred Hoyle came along with the resonance idea. It wasn't until then that it was borne home, to me at least, that this general area had much broader horizons than I had been thinking about. As soon as Salpeter showed that in later-generation stars, in the red giants, some new nuclear physics could take place—Boy, that just opened up a whole new ball game! But you have brought out that the astronomers in those seminars they gave us seemed to anticipate this to a certain extent. I think it would be fair to say, not in detail. They were just interested in where the hell did the elements come from.

GREENBERG: In your notes [Fowler's 1946 seminar notebooks] you state that stars like the sun seem to have the same relative abundances of the elements. Was it puzzling that all the stars seem to have the same?

FOWLER: That was what led many people to think that George Gamow was right. Gamow's work was going on all through 1946, and by 1947 and 1948 he was publishing Big Bang articles. Because the astronomers saw abundances in other stars that were so similar to the sun, a universal cosmic source seemed like just the answer. It wasn't until we appreciated the fact that in George's Big Bang the conditions were never right to get over the mass-5 and the mass-8 gap. At first, George was unwilling to accept that—not the fact that the gap would kill his scheme, but he just didn't think, for a while, that the gaps existed. He felt all this nonsense about there being no mass 5 and no mass 8 was for the birds. But he eventually realized that the gaps were there. I have a famous quotation from one of his papers where he admits that the lion's share of the production of elements beyond helium had to be done in stars.

GREENBERG: Was Bethe skeptical of Gamow's Big Bang? Was he skeptical of primeval nucleosynthesis? Gamow put his name on a paper: "Alpher, Bethe, Gamow." [Laughter]

FOWLER: Well, the story that Hans tells is that he was not even consulted, and that he was furious when he saw the paper [R. Alpher, H. Bethe, G. Gamow, "The Origin of Chemical Elements," *Phys. Rev.* 73: 803 (1948)]. It's typical of Gamow that he would love to have a paper on the theory of Alpher, Bethe, and Gamow, which could then be called the alpha, beta, gamma or the ABC's of nucleosynthesis. I have a feeling that Bethe was conned, that George asked him in some kind of an offhand way if he could put his name on a paper just for this "alpha, beta, gamma" reason. Hans probably shrugged his shoulders and George probably took that as a "yes," and that's what happened. But you see, Bethe, of all the people, knew about the mass gaps. He knew about mass 5 and 8. So he was skeptical of Gamow's trying to make the elements in the Big Bang. On the other hand, he was not skeptical of the general idea of the Big Bang.

GREENBERG: That's what I wanted to get straight.

FOWLER: All of science is divided into two kinds of people. There are those who like beginnings or origins, and then there are people—and I'd say Hans Bethe is one of those, and the great majority of astronomers, certainly—who don't like the idea of a discontinuity and who, at one time, tended to favor the Steady State theory: for example, Hoyle, [Hermann] Bondi, and [Thomas] Gold. Vic [Victor F.] Weisskopf, for example, was always very sympathetic to the Steady State theory. He just thought that it had to be right. Of course, once the microwave radiation was found, that cinched the argument for the Big Bang. And once we were able to supplement the Big Bang production of helium with element synthesis in stars, there was no reason to rule out the Big Bang because it couldn't make the heavy elements, because stars would do that. [Note added by William A. Fowler, August 14, 1985: "Recently the inflationary model suggests that our observable universe is just an expanding bubble in an otherwise Steady State universe!"]

GREENBERG: Was Tollestrup's work done with cosmology in mind?

FOWLER: No, no. It wasn't until I met Fred Hoyle that any of this stuff came home to me. But the minute we were able to show that the Salpeter-Hoyle process worked after Ward Whaling found that the excited state in carbon-12 existed, then we had to show that it had the proper spin and parity combination so that it could be made by putting three alpha particles together. If it had had an abnormal parity rather than.... That's not quite the term people use. If it had been a zero minus state, zero angular momentum and minus parity, negative parity and odd parity, it couldn't have been made by three alpha particles. In fact, it had to be zero plus, two plus, or four plus. You can't put three alphas together—I think it's true—to make a state with angular momentum 1.

GREENBERG: I wanted to ask about this. In the Weiner interview [interview of William A

Fowler by Charles Weiner for the American Institute of Physics, June 3 and 9, 1972] I got the impression that you did this work right after Hoyle, and this was what you went and talked about at the Cavendish when you went over there.

FOWLER: That's right.

GREENBERG: Now, this was the work with Charlie Cook, which wasn't published until 1957 [C. W. Cook, W. A. Fowler, C. C. Lauritsen, T. Lauritsen, *Phys. Rev.* 107: 508 (1957)]. Was the work done immediately after Hoyle, or was it done sometime later?

FOWLER: No. That work was done before I went to the Cavendish in 1954, because, as you just got through saying, I talked about it in the Cavendish colloquium. It wasn't published probably because the year that I spent in the Cavendish I got so much excited by other things that I didn't write up what Charlie Cook had done.

There was an article in the *Bulletin of the American Physical Society* in 1956: Fowler, Cook, Lauritsen, Lauritsen, and Mosher. It's really strange that it took us so long to publish those, but I swear that we had these curves and had seen these alpha particles from boron-12 by the time I went to Cambridge in 1954. Now, probably we didn't have a very good idea of the energy; just let me see if I can figure it out here. In 1956 in the *Bulletin of the American Physical Society*—it was in the very first one of series II, so it probably won't be in these *Physical Reviews*, because that's when the *Bulletin* was made a separate volume.

My recollection is that I was talking about boron-12 and the red giants when I was in Cambridge in 1954. And the only explanation I can give is that that must have been very preliminary results and then, while I was away, Cook continued to work on the problem. Then after I got back in 1955 I remember we continued to try to tie down the energy as accurately as we could, because by looking at the alpha particles you get an independent measure of the energy of the carbon-12 state. We wanted to make sure that what we got by this independent way agreed with what Ward had found from the nitrogen-14 *d*-alpha reaction. So I can only say there was probably a lot more precision work to be done. Then, it took time to get it all written up, and it took from March 26, 1957, until July 15, 1957, to get it published. And we clearly knew the answer in 1956—I would say a precision answer in 1956. I'm claiming that I don't think I have really any reason to doubt it. We did something *immediately* after Ward Whaling's work that showed that the state could break up into three alpha particles. We may not have measured the energy of breakup precisely, but we knew that the state had the correct properties, and if you look at this paper of ours, that's mainly what it's all about. It says, "It is, of course, crucial to the theory that the carbon-12 level be of such a character that it can be formed by beryllium-8 plus helium-4; that is, that it have even spin and even parity, or odd spin and odd parity, and a nonvanishing alpha particle width. It must have a reasonable probability for gamma or *e* plus/minus decay if carbon-12 is to be formed."

We go on to talk about the reversibility—that you can make carbon-12 and it breaks up into three alphas, which shows, on the principle of reversibility, that three alphas can form the carbon-12. My guess is that we must have done a preliminary experiment that convinced us that the alphas were there, and then it took a couple of years to set up the complicated apparatus that it took to really measure precisely what the energy of the alpha particles was.

GREENBERG: I want to jump back to something earlier, for a second. At the Minneapolis conference, [Symposium on the History of Nuclear Physics at the University of Minnesota, May 1977] you reminded people, "You must remember that Millikan believed that the cosmic rays were gamma rays. He had, very early, suggested essentially nuclear energy by saying that it was silicon nuclei which are annihilating." I asked you earlier about Millikan's talk of electron-proton annihilation and atom synthesis and things like that, and you replied, "CCL [Charles C. Lauritsen] was embarrassed, Oppie was furious, and Tolman puffed his pipe." What's your opinion of Millikan's ideas of the twenties. Are they underdeveloped? Are they premature? Are they half-baked?

FOWLER: Well, they certainly were underdeveloped. He never made a detailed presentation with any mathematical backup for his ideas. He wrote a lot of rather long-winded essays on the subject.

GREENBERG: In the course of the cosmic-ray dispute, he wrote a long paper on the synthesis of elements from hydrogen, or something like that.

FOWLER: Yes. Well, he wasn't entirely wrong, and he, of course, realized that there was a problem.

GREENBERG: What he claimed was that it couldn't be taking place in stellar interiors. It had to be going on in interstellar space, or something like that.

FOWLER: Yes. Well, there he was just wrong. It's hard for me to say, but I think the statement I made was that Lauritsen and Oppenheimer and Tolman were embarrassed and just didn't believe a word of what the Chief was saying. I think if you're interested in that, the person you've got to go to is Vic [Henry Victor] Neher, because Vic was much closer to Millikan than all the rest of us.

I think Carl Anderson might also be helpful, although Carl was also very skeptical. You see, he, to a certain extent, was the father of the annihilation idea. He discovered the positron and realized that when one found an electron, they'd annihilate. Charlie Lauritsen did a very cute experiment which showed the effect of the annihilation of positrons producing half-million-volt radiation. Millikan seized on Carl Anderson's discovery, is the way I would say it now; it's a little bit of a hindsight. Millikan realized that an electron and a positron, when they annihilated, gave off the sum of their rest masses in gamma rays. So he said, "Well, if I can annihilate something heavier, I can get more energy." His estimate of the energy of the gamma rays that he thought constituted the cosmic rays pretty well matched the energy that you would get from the annihilation of two silicon nuclei. Now, where he was going to get the anti-silicon from is the question. He may have addressed that problem. He was just so far off, because most people—even though they were in general quite loyal to Millikan—sided with [Arthur Holly] Compton, who thought the cosmic rays were electrons. So in that sense, Compton was closer to the truth—although neither of them was right. It wasn't until much later that it began to be realized that the cosmic rays were mostly fast protons.

There was no question that everyone was embarrassed; everyone at Caltech, with maybe the possible exception of Vic Neher, was acutely embarrassed by Millikan's belief 1) that the cosmic rays were gamma rays and 2) that the gamma rays came from the birth cries of [atoms], or death cries—for some reason or another he got birth cries in, and I don't know how that came along with the idea of annihilation. But he was never clear about all that. He had these vague ideas.

There were many things on which Millikan was really quite expert and knew what was going on and knew all the details and knew the theory. But on this business of the origin of the cosmic rays and their constitution, he was just off base right from the start.

WILLIAM A. FOWLER

SESSION 7

May 30, 1984

[The Nobel Prize in physics was awarded in December 1983 to William A. Fowler and the astrophysicist Subrahmanyan Chandrasekhar. The award citation noted Dr. Fowler's "theoretical and experimental studies of the nuclear reactions of importance in the formation of the chemical elements in the universe."]

Begin Tape 7, Side 1

GREENBERG: [leading into discussion of astrophysics at Caltech after World War II] I'm trying to fish around to see what kind of activity there was going on before Salpeter and Hoyle. We know that there was a series of Bowen seminars beginning in 1946, and there were also a lot of colloquia going on in the late forties, some of which seemed to be taking place at Santa Barbara Street [headquarters of Mount Wilson (Carnegie Observatories) astronomers]. We know, for example, that Jesse Greenstein talked on determination of the abundances of the elements in stars. Bowen talked on astronomy and nuclear physics at the series of colloquia, and you talked in 1948 on the rates of nuclear reactions, and Christy talked in 1948 on nuclear physics of the carbon cycle and its connection with astrophysics. You talked again in 1949 a couple of times, on nuclear physics and on measuring nuclear cross sections in the Kellogg Lab. Harrison Brown came and talked on determination of cosmic abundances. [James] Donald O'Reilly—who I guess was a student of Christy's?

FOWLER: Father O'Reilly.

GREENBERG: Father O'Reilly finished a PhD thesis on "A Study of the Physical and Chemical Composition of Homogenous and Inhomogenous Models of the Sun" [1950]. Also in 1949, Lawrence Aller came and talked on abundance determinations; Jesse Greenstein talked in 1950 on isotope abundances, and so on. If we look at the written record, we get the impression that the problem of abundances was being talked about on the campus during the late forties.

FOWLER: Well, John, I would like to begin—because I've been thinking about it—with the role that Charlie Lauritsen and Ike Bowen played in coming to the ultimate decision that Kellogg would remain in low-energy nuclear physics. Because the more I think of it now, I see it as the crucial time when the whole future of nuclear physics and nuclear astrophysics at Caltech was decided. And I think it's very interesting to look at how fast it happened. I must say, of course, that our interest was primarily stimulated in 1939 when Bethe published the carbon-nitrogen cycle. Charlie Lauritsen, Tommy Lauritsen, and I were actually bombarding the isotopes of carbon and nitrogen with protons, which were just the reactions that occur in Bethe's CN cycle. For us it was a revelation that something that goes on in stars could be duplicated in the laboratory. It was with a real sense of excitement and enthusiasm that we realized that what we were doing had an astronomical significance, especially in view of the fact that Caltech and Mount Wilson were so closely associated, even then.

Well, of course, the war came along, and during the war Ike Bowen, Horace Babcock, Olin Wilson, and Bob King all worked with us on the rocket ordnance project. So we got to know them extremely well, although Charlie and I had known Ike Bowen quite closely because he was a member of the faculty before he went to become director of Mount Wilson. But it's quite interesting to see how fast it all happened. On May 7, 1945, Germany surrendered; August the 6th, 1945, was Hiroshima; August the 9th, 1945 (my birthday! I was thirty-four), was Nagasaki; Japan surrendered on August 14th. On January the first, 1946, Bowen succeeded Walter Adams as director of Mount Wilson Observatory, and from January 21, 1946, to April 1, 1946, five seminars, arranged by Charlie and Ike Bowen, were held in Bowen's home. I've got the dates here, which I think are interesting. You know, I have a notebook of the notes I took. Someday that'll go into the Archives.

Five seminars were given. In the very first one, on Monday, January 21, 1946, Bowen spoke on abundances in stars. This was at his home. After the talks, we all sat around and drank beer. On Monday, February 4, Olin Wilson spoke on stellar structure, essentially from [Bengt] Stromgren but trying to explain to us what Bethe had been talking about in his 1939 article. Then on Monday, February 25, I spoke on the nuclear reactions that Hans Bethe had used; Bethe's article was already our bible, and we were trying to understand it. Bowen talked on abundances, Wilson talked on stellar structure, I talked on nuclear reactions. Then Bowen talked again—and this is interesting—on March 18, on equilibrium theories of abundances; even

Chandrasekhar had worked on equilibrium theories, and they had completely failed. It wasn't until much later that, primarily through Fred Hoyle, we realized that the abundances were not made by equilibrium conditions at one temperature and density but that nucleosynthesis occurred under a whole series of situations. Finally, on April 1, April Fool's Day, 1946, Rudolph Minkowski talked on supernovae. That showed that we already were thinking of supernovae as one of the mechanisms by which the elements produced in stars were getting into the interstellar medium, which is a key part of the whole grand scheme.

Those five seminars were attended by Bowen, Olin Wilson, myself, Minkowski, Walter Baade, Horace Babcock, Paul Merrill (who came to play a very important role), Charlie Lauritsen, and Tommy Lauritsen, and it was as a consequence of those five seminars that Charlie Lauritsen made a deliberate decision, quite contrary to the fashionable trend at the time, to stay in low-energy nuclear physics. He decided to give up the old AC transformers and build high-pressure electrostatic accelerators of the Herb model in Kellogg; Tommy and I had put the first one together under Charlie's general supervision in 1938 and 1939. We had already decided, even before the war, that we had to have more precise accelerators than the tubes driven by AC transformers. After the war, there was pressure on everybody either to stay in nuclear physics using electrostatic accelerators—commonly called Van de Graaffs—or to go into high-energy physics using cyclotrons.

It's my recollection that Ernie Lawrence came down from Berkeley—Ernie was a great advocate of going to higher and higher energies—and told Millikan, "You've just got to get Lauritsen to go into high-energy nuclear physics and stop all this nonsense that was going on before the war." And in fact it's my recollection that Lawrence offered to give Millikan one of the cyclotrons. I distinctly remember Charlie being, in a sense, as furious as Charlie ever got about anything, over the fact that Lawrence went over his head to Millikan to try to determine the future of Kellogg [laughter] as well as the future of Berkeley. Well, anyhow, Millikan didn't bite, because he trusted Charlie.

The seminars that Ike arranged showed us, in a sense, all the possibilities. They made it very clear that Caltech was too small to really go in for great big accelerators and Kellogg was certainly not the place to do it. On the other hand, Lauritsen did realize that Caltech should perhaps go into high-energy nuclear physics in a *modest* way. And we arranged for Joe [Robert] Langmuir to come out. I think he's now retired in engineering. He'd been building betatrons at

General Electric, and we arranged for him to come out. DuBridge came in 1946, and he was gung-ho to go into some kind of high-energy physics, but he agreed with Charlie that Kellogg would not be the place to do it, so that we first got Langmuir and then Bob Walker, who was in Kellogg when he first came. He actually did a few low-energy experiments, because that had been his field; however, he was looked on as someone who was going to go into the high-energy field. Then the big thing was in 1949, when Bob Bacher came. He took over and started building the synchrotron and the synchrotron lab in the old optics lab.

So the history was this: that Kellogg, on the specific decision of Lauritsen—he consulted with me and with Tommy, and we certainly agreed—Kellogg was to stay in low-energy nuclear physics with emphasis on reactions of importance in the stars. But *Caltech* was to get into high-energy physics under Bacher. So that's, I think, an extremely important part of the history which I want to get down, as I just have, explicitly.

Then, of course, Oppenheimer came back to Caltech after the war in November of 1945, after a great deal of debate in his mind about whether to go back to Berkeley or to come to Caltech. So Oppie came in November of 1945 and, as far as I can make out, stayed until April of 1946, when he went back to Berkeley, planning to spend a few days a month at Caltech, as he had before the war. Finally, in April 1947, he went to the Institute for Advanced Study. But I must emphasize that Oppie was around. As far as I remember, he did not attend these Bowen seminars. Although he was at Caltech, he spent most of his time in Washington. He did give a course in nuclear physics during that period that he was at Caltech, and I've got notes on that. That was also a great help in what we were doing, because we had just *no* training, neither Charlie nor Tommy nor I, in modern theory of nuclear physics, which to a certain extent Oppenheimer founded, although there were a lot of other people who also made contributions. But Oppenheimer played a great role. Richard Tolman and DuBridge supported us in this decision.

Then in 1948 Jesse Greenstein came to build an astronomy department, although we never call it "department" at Caltech. Jesse was a great deal of help. Jesse and I talked all the time. In fact, I want to get on the record that one of the first papers, even before Burbidge, Burbidge, Fowler and Hoyle, was a paper, "Element Building Reactions in Stars," *Proceedings of the National Academy of Sciences*, volume 42, 1956, page 173, W. A. F. and J. L. G. So Jesse and I were working together and that paper was written just before [Hans] Suess and [Harold]

Urey came out with an abundance curve which made clear that the heavy elements were made with neutrons. [*Rev. Mod. Phys.*, 28: 53 (1956).] So all Jesse and I talked about was building up to iron, and, of course, we were greatly dependent on what Fred Hoyle had already done [in 1946]. Then—just to get it on the record—when we realized that Oppie was going to leave, we got Bob Christy to come to be the theorist in Kellogg. That was 1946; then Ward Whaling came in 1949, Charlie Barnes in 1953, [Ralph W.] Kavanagh in 1956, [Thomas A.] Tombrello in 1965, and [Steven E.] Koonin in 1975.

The one other thing that really contributed a great deal to our enthusiasm in the field was the discovery of technetium in S-type stars by Paul Merrill in 1952. The technetium isotope involved—I think it's technetium-99—has a half-life of 200,000 years, so it became very clear that the technetium had been made in recent times and that nucleosynthesis was indeed going on in giant stars. The technetium, which was made probably fairly deeply, somehow or other was brought to the surface where Paul Merrill was able to see its characteristic lines. Technetium had been made artificially in one of the big reactor labs.

GREENBERG: Was that after the Salpeter visit?

FOWLER: It all was almost simultaneous. Salpeter came in—I have 1951 and also 1953. I think he came to Mount Wilson first in 1951, but I don't remember. Anyhow, eventually Salpeter came out, and he spent part of his time at Mount Wilson and part of his time in Kellogg. Alvin Tollestrup and Charlie and I had just shown that beryllium-8 was unstable, but we got a good value for the energy in the instability—something like 89 kilovolts. It's now known to be 92. Salpeter immediately realized that that amount of instability was small enough that in red giant stars there would be a high enough concentration of beryllium-8 constantly being made, constantly disintegrating, that it could be hit while it was beryllium-8 by another helium nucleus to make carbon-12.

GREENBERG: Is it fair to say that this is really the first time that the Kellogg Lab gets involved with the astrophysical problem?

FOWLER: Well, [pause] no; because right after the war, starting in 1946, we went back to measuring the carbon-nitrogen cycle reactions with much higher precision. So we were in the business.

GREENBERG: But is this the first time that the lab really gets involved in research with nucleosynthesis specifically in mind?

FOWLER: I think we made it clear—that in 1946, when Lauritsen made his decision, then we immediately began to do precise measurements on the carbon and nitrogen and also the oxygen isotopes: bombarding them with protons, getting the cross sections, getting the reaction rates which we could then give the astronomers for substitution into calculating the reaction rates averaged over the thermal distribution in energy at a given temperature in a star. And we showed, among other things, that the carbon-nitrogen cycle did not work in the sun.

Bethe and Critchfield, in 1938, had proposed the proton-proton chain. For some reason or other, that had not caused the excitement that his announcement of the CN cycle in 1939 did, one of the reasons being that the basic reaction in the proton-proton chain is so rare that it hasn't been measured experimentally even to this day. It has to be calculated. Now it's calculated on such firm grounds with indirect experimental input that I jokingly say, "Well, the rate of the *p-p* reaction which can't be measured is probably one of the most accurately known reaction rates in nuclear astrophysics." But it has to be calculated indirectly.

I don't think we were even aware of Bethe and Critchfield's paper until we saw Bethe's CN cycle. But then after the war, we measured the CN cycle, compared it with the theoretical calculations for the proton-proton chain, and showed that the CN cycle took over for stars slightly heavier than the sun, and that the sun, contrary to Bethe's original statement in his first paper, operated on a *p-p* chain rather than on the CN cycle. Now, there were other theorists at the time—a man named Epstein, as I remember—who came to realize this. But the experimental work that tied the CN cycle rates down was, I claim, the crucial input to showing that the sun operates on the proton-proton chain and not on the CN cycle. And of course, that's accepted now.

The most powerful evidence is that if the CN cycle operated in the sun—I mean a hundred percent—then the solar neutrino flux from the sun would be even ten times the value that one gets for the *p-p* chain using the chlorine detector that Raymond Davis uses. So we know just from the solar neutrino detection that the carbon-nitrogen cycle cannot be more than five percent. If you say that everything that Davis sees is due to the CN cycle, then it's at most five percent of what's going on in the sun. Theoretically it's about two percent. The CN cycle goes around, but very, very slowly. And it generates about two percent of the energy, compared to ninety-eight percent by the proton-proton chain.

If you go to a slightly more massive star, the central temperature is higher, and the CN cycle—which is much more sensitive to temperature than the *p-p* chain—then comes to be the dominant one; the crossover is very sudden. So we were definitely involved in nuclear astrophysics. We were still trying also to do nuclear physics, which mainly meant measuring the energy levels of the light nuclei. But then one of the problems that came up was, Is the nucleus beryllium-8 stable? That was stimulated by Gamow's claim around 1946, too, that you could build all of the elements in the Big Bang if there were stable isotopes at mass 5 and at mass 8.

GREENBERG: You did the work on beryllium-8 with that problem in mind?

FOWLER: Yes, we did. Oh, yes, yes, yes. But you see, even before the war, Hans Staub and Bill Stephens, in Kellogg—and I think this was around 1939—confirmed that there was no stable nucleus at mass 5. I would have to say that the primary discovery was made at Minnesota by Johnny Williams's group, but Staub and Stephens did a much more complete job and showed that the state of helium-5 that they could produce was indeed what we call a *p*-shell state, and the reason it was unstable was because when you put a particle into the *p*-shell it's not bound very tightly to the remainder. But that was all unconscious. Before the war, we didn't connect the work on the instability of mass 5 at all with nucleosynthesis.

George Gamow knew that mass 5 was unstable, and he ultimately learned that mass 8 was unstable from our work and other work. He thought that somehow or other you could get around that, but then it just turned out that you couldn't. George himself finally had to admit that the lack of stable nuclei at mass 8 meant that nucleosynthesis in the Big Bang essentially stopped at mass 4.

GREENBERG: Were you and Tollestrup familiar in 1949 with Hoyle's work? I mean, the alternative to Gamow.

FOWLER: Yes. But we were only familiar with it in terms of the controversy between the Steady State theory and Gamow's Big Bang.

GREENBERG: Not so much the nucleosynthesis?

FOWLER: I have to say that Hoyle's 1946 paper [*Mon. Not. R. Astron. Soc.* 106: 343 (1946)] was really the first one that definitely said the heavy elements have to be made in stars, although in that he was primarily motivated because he believed in the Steady State and he didn't have a high-temperature, high-density Big Bang, period. His paper in 1946 was really an attempt to do a number of equilibrium calculations at different temperatures. Now, actually that's not a bad idea, although it turns out that to really do things properly you have to know the nuclear reaction rates. In 1946, they probably didn't have any.

So he used equilibrium arguments, but contrary to what had always been done in the past by Tolman and [Mario] Schenberg and Chandrasekhar, he just said, "If I want to make higher masses, like oxygen and magnesium, I have to have this temperature. If I want to make silicon, I have to have a still higher temperature. If I want to make iron, I have to have even higher temperature." And of course, Hoyle used the big peak for the iron group nuclei, which can be adequately described by an equilibrium calculation, as one of his powerful arguments. But he only went to iron, you see, in 1946. Well, I would say we weren't familiar with that paper.

It really wasn't until Hoyle came to Caltech—When was it, 1953?—and told us that he had used Ed Salpeter's calculation on the, by that time we were calling it the three-alpha reaction forming carbon-12, that he paid attention. He had used Ed Salpeter's calculations, which put the beryllium-8 state in as a resonance, but then when Salpeter added helium-4 to beryllium-8 to make carbon-12 he used a nonresonant calculation. That rate came out so slow when Hoyle and Schwarzchild—following an earlier paper by [Allan] Sandage and Schwarzchild [*Astrophys. J.* 116: 463 (1952)] which was very explicit on the point also—found that from the astronomical evidence in the red giant stars for the turn-on of the three-alpha reaction, which terminates the red giant branch, the temperatures they got from their evolutionary and structure calculations were just too low. Hoyle then said, "If Salpeter's rate at what we know to be the correct temperatures is too slow, then there has to be a resonance in the reaction between beryllium-8 and the alpha particle." He predicted where the resonance would be, and at Caltech in 1953 he tried to talk Charlie and Tommy and me into working on it. We were busy, told him to go away and not bother us.

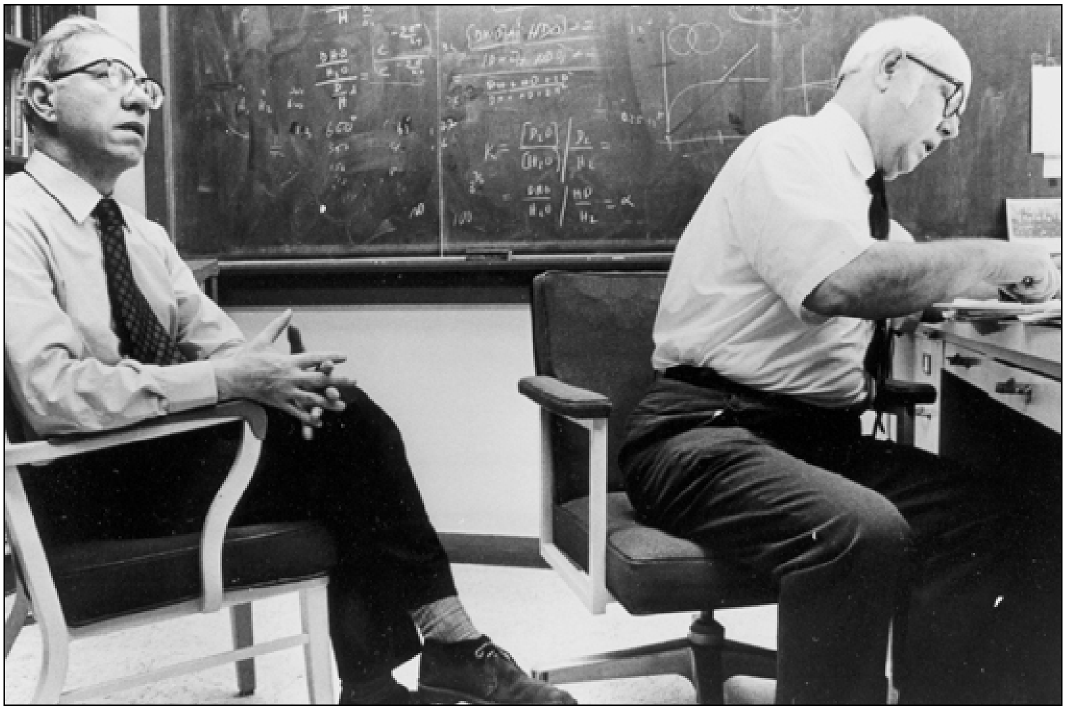

Fig. 11. Fred Hoyle and William A. Fowler in Fowler's office in the W.K. Kellogg Laboratory, Caltech. *ca* 1953. Caltech Archives.

Ward Whaling, however, had sat in on one of the conversations, and he actually went to some seminars that Hoyle gave. I didn't even go to Hoyle's seminars, you see. We were so damn busy in the lab. Whaling was convinced there might be something in it. He and his postdocs and grad students looked for Hoyle's state, found it, and that just changed everything. As I say, that made a believer out of me.

The next year I got a Fulbright and went to England to work with Fred to find out what this was all about. There I met the Burbidges, brought them back to Caltech in, it must have been the fall of 1955. I was busy then working with Jesse Greenstein on the paper that appeared in the *Proceedings* of the National Academy in 1956. Then all of a sudden the Suess and Urey paper came out, which made it clear that Gamow's scheme of neutron capture worked fine above iron, all the way up to the heavy elements. Then Hoyle came over, and Hoyle and the Burbidges and I wrote a preliminary paper that was published in 1956, and we wrote the big paper published in 1957. [E. M. Burbidge, G. R. Burbidge, W. A. Fowler, and F. Hoyle, "Synthesis of the Elements in Stars," *Rev. Mod. Phys.* 29: 547 (1957).] Al [Alastair G. W.] Cameron independently came across the same ideas, and he published in 1957, in addition [*Publ. Astron. Soc. Pac.* 69: 201 (1957)]. But I really think that, whether consciously or unconsciously, Kellogg had been working on nuclear astrophysics in 1939, showing that mass 5 was unstable and working on the CN cycle reactions.

It wasn't until after Bethe's paper and then after the war that we began *consciously* to devote our effort, in part at least, to nuclear astrophysics. We mainly tried to get the CN cycle reaction rates correct. When we did, we found that the sun couldn't be shining on the CN cycle; it had to be shining on the *p-p* chain. And then we also decided to look at the instability of beryllium-8. We almost certainly knew it was important in Gamow's scheme. But it also was a terrifically important nuclear physics problem, because by that time the shell structure had taken hold; it was simple and we all could understand it; we believed in it.

It was clear that one filled the lowest shell with two protons and two neutrons. That made helium-4. You tried to add one more nucleon to mass 5. It just wasn't bound tightly enough to be stable. If you added two, you got enough binding between the two neutrons so lithium-6 was stable. Then the whole question is, Well, what happens at beryllium-8? You might argue, Well, the additional binding will continue. But the point is, beryllium-8, if it decays, can decay into two alpha particles which are both very tightly bound. And sure enough, the beryllium-8 turned out to be unbound. And that *really* put paid to Gamow's theory, because even though he might have been able to get around the mass 5 gap by some hook or crook, he certainly couldn't get around both mass 5 and mass 8 instabilities.

GREENBERG: A year later came the work of Fermi and [Anthony] Turkevich.

FOWLER: Yes.

GREENBERG: Was that anticlimatic for you in the sense that, again, it was shown that Gamow's primeval nucleosynthesis was going to fail?

FOWLER: We made no detailed calculations on the effect of the instability at mass 5 and the instability at mass 8. We made no detailed calculations. We were busy doing other things.

Fermi and Turkevich did a very detailed calculation trying their best to make Gamow—and by that time Ralph Alpher and Robert Herman were in on it—to make that scheme work, but they showed that you just could not do it. And so it was Fermi and Turkevich who really put paid in a quantitative way to Gamow's scheme.

Kellogg had been in on the mass 5 breakup and in on the mass 8 breakup, but we had not done any quantitative calculations. We were much more interested in the fact that Ed Salpeter showed, in principle, that in a red giant star you had densities as much as 10^5 times the density of water—10^5 grams. The densities were high enough that three helium nuclei had enough probability of colliding to make the carbon-12, whereas in Gamow's Big Bang, you see, after the helium is made and the universe is expanding, the temperature and the density are dropping. So it was clear, but it took Fermi and Turkevich to prove it, that the three-helium-to-carbon-12 would not work in the Big Bang. Then of course, Hoyle came along and added the resonance, which then satisfied the astronomical evidence about red giants. That's when I became a believer.

As I've told you, when Hoyle came and talked to Charlie and Tommy and me, even though we had been in on the determination of the instability of beryllium-8, we just didn't think that what he was talking about was important enough. We thought, "Well, Ed Salpeter's done it, and Hoyle and Schwarzchild—and earlier, Sandage with Schwarzchild—their calculations for the temperatures could be off." Then when Hoyle predicted the state and Ward Whaling found it, it was clear that there was something in what Hoyle was talking about. Then we went back and read his 1946 paper and saw that by studying charged particle reactions, which we could do in Kellogg, you could go all the way up to iron. So we had a program. We had a future. We had a future.

GREENBERG: Did you discover how nucleosynthesis took place as a result of Salpeter and Hoyle's visits to campus? Weren't you pretty well primed for it by that point?

FOWLER: Well, John, I think it's fair to say that our main preoccupation before Salpeter and Hoyle was with nuclear energy generation in stars. The nucleosynthesis was clearly floating about because of the Gamow Big Bang controversy with Hoyle, Gold, and Bondi, and the Steady State theory. We were, I think, much more interested in the energy generation in stars. We knew just from first principles that if a star generates energy it has to create new nuclei. But our vision, I must say—at least, mine—was terribly limited. You have to remember that we were really operating the laboratory day in and day out, trying to accumulate data. I think the first paper I wrote that specifically addressed the problem was "Element Building Reactions in Stars," [*Proc. Nat. Acad. Sci.* 42: 4, 173, 1956] which I wrote with Jesse Greenstein. If you look at that paper with Jesse, we give full credit to Hoyle.

Begin Tape 7, Side 2

FOWLER: When I was in Cambridge for my sabbatical in 1954 and 1955, Fred Hoyle was very busy fighting the Steady State battle, which I assiduously avoided—cowardly but prudent! Out of that came papers between myself and the Burbidges, without Hoyle's participation, in which we tried to explain anomalous abundances in stars. We were looking for neutron sources. That was certainly nucleosynthesis, but it was complicated by the fact that no one really knew what the so-called cosmic abundances—really, the solar system abundances—were. What the Burbidges were finding—especially Margaret, with her observations—was that there were some stars that showed abundances of barium and other things, for example, which were quite different than in other stars. So we tried to work on how you made anomalous abundances by exposing normal abundances to neutrons.

But the key thing, really, that led to nucleosynthesis in stars all the way from helium, going to carbon, all the way up to uranium and thorium, the parents of the radioactive series, was when Suess and Urey made a systematic study of the abundances and showed us the so-called double peaks among the heavy elements. We then immediately realized that these were due to two neutron processes, one of which we called the *r*-process, because it happened rapidly, and one we called the *s*-process, because it happened slowly.

It's awfully hard to remember, but I think it's fair to say that we were primarily interested in nuclear energy generation in stars—in taking what was already there and modifying it by neutron captures. It wasn't until Suess and Urey came out that we realized that you could cover the whole periodic table.

GREENBERG: There are a couple of things here that I want to ask about. You've recently talked about Jesse Greenstein's 1952 invention of a helium-burning reaction $C^{13}(\alpha, n)O^{16}$—to produce neutrons. I noted that back in your paper with the Burbidges in 1955 [*Astrophys. J.* 122: 271 (1955)] you cite the co-inventor, Cameron, but not Greenstein. I wondered about this.

FOWLER: Well, the problem was that Cameron actually published a paper. Greenstein's suggestion was incorporated in a chapter he wrote for a book called *Modern Physics for Engineers* [New York: McGraw Hill, 1954]. Now, Jesse had talked about it. As far as I know he never wrote a very refereed paper, but you'll have to check with Jesse. And you know what the rules are about such things; whereas Cameron actually came out with a paper. Then the Burbidges and I decided, in work that we did in England in 1954-55, that carbon-13 didn't work very well for our purposes, and we suggested neon-21 and neon-22 as, let's see—those are alpha-*n* reactions. It's clear that Jesse had the idea but Cameron actually published a paper. Later on, I gave full credit to Jesse's priority.

GREENBERG: Well, the thing that intrigued me was that this is before Hoyle; this is in 1952.

FOWLER: No, the—

GREENBERG: Well, Jesse claims it was in 1952 that he did this.

FOWLER: Well, I guess when we came to write our paper on sources of neutrons, we just decided that the valid reference was Cameron's paper. There are, however, other places where I'm sure I wrote a paper—I mean commenting on the fact that Greenstein had also done this. It's just one of those things. When someone writes a refereed paper, then you use that as a reference rather than a reference to a short comment in a book. But that is all, to a certain extent, irrelevant. I think it only fair to say that Greenstein and Cameron independently invented the C^{13} alpha-*n* reaction as a source of neutrons in red giant stars and that's now by far the favored method for making neutrons.

GREENBERG: All I really was after was to get an idea of how involved Greenstein was, whether or not he was involved with these sorts of problems. It sounds as if he was.

FOWLER: Oh, Jesse was working on these things all of the time. He and I talked at great length, and I learned an enormous amount about the astrophysics from Jesse. If I had to do it over again, Jesse's chapter in that book—I also wrote a chapter for it—would have been mentioned. It was the fact that Jesse had contributed so much that led Jesse and me to write that 1956 paper. Now, it's very interesting how that paper came about. It was written at the suggestion of Paul Merrill, and I don't mind saying that what Paul was up to was to get Jesse and me into the National Academy. And in fact right after that paper I was elected—in, I think, 1956—and Jesse was elected in 1957. It took the astronomy part of the election a year longer. [Laughter]

Greenstein's contributions were just enormous, because by that time he had taken over the position that Bowen had kind of had for that short period after the war. All of the Mount Wilson people were busy doing their thing, and they weren't working on nucleosynthesis at all. In fact, there was a pretty strong feeling at Mount Wilson that the Steady State theory was just wrong. This came about because the radio observations and [Martin] Ryle's work began to show that things were different in the past than they are now and that the Steady State theory was just untenable. But people didn't really give up the Steady State theory until [Arno] Penzias and [Robert W.] Wilson discovered the microwave radiation, essentially predicted by Gamow and Herman and Alpher, although on grounds that are not at all tenable. The funny fact was that [Robert H.] Dicke had never read their paper; in fact, he was looking for the microwave radiation, too. So when Penzias and Wilson found it, they referred to Dicke. I don't think that they referred to the work that Gamow, Alpher, and Herman had done.

The amazing thing was that Alpher and Herman corrected Gamow's very [laughter] inaccurate numerical calculations and came up with the prediction of a present background radiation of 5° K. Well, if you look at their calculation, although they were numerically correct, the basis for it is not really valid. That doesn't discredit them; the fact was that they knew that if there was a Big Bang, by god, there had to be some temperature now that represented the temperature after the great drop in temperature due to the expansion of the universe.

I don't know. To tell you the truth, John, we never put two and two together, Charlie and Tommy and I, the people in Kellogg. We had to have the help of Salpeter and Hoyle and the Burbidges to really get going on beyond the very lightest elements to studying reactions that would produce the intermediate mass elements with proton and alpha bombardment. In fact, Charlie Lauritsen always spoke of the heavy elements as "the transneon elements," and Kellogg was only to work on things up to neon. In fact, when Bob Christy came, we then spent a lot of time studying the bombardment of fluorine with protons, because it was very interesting nuclear physics. It had absolutely *nothing* to do with nuclear astrophysics—fluorine-19 is very rare.

The real breakthrough—well, the three breakthroughs were Hoyle's demonstration that there existed an excited state in carbon-12 which no one in nuclear physics could predict, and that was so exciting and showed that you could jump from helium to carbon-12. And then there was his 1946 paper that showed that you could continue right on up to iron. Then along came Suess and Urey in 1956 and showed that then, with neutrons, you could go all the way up to uranium and thorium. By that time, we began to realize that nucleosynthesis was the thing, rather than nuclear energy generation in stars.

GREENBERG: Talking about the Lauritsens: One of the other things that intrigued me was that apparently at Greenstein's provocation, Lauritsen invented the helium-3-on-helium-3 reaction that explains why you don't find helium-3 in the sun and closes out the *p*-*p* chain.

FOWLER: Yes. It's amazing that that reaction was not mentioned by Bethe and Critchfield in their original paper on the proton-proton chain. They did not think of helium-3 plus helium-3 going to two protons and helium-4.

I'm sure that Jesse played a role in it. On the other hand, I became interested—and Charlie did too. All we ever published was a little abstract of a talk I gave at a Physical Society meeting, I think out here, or maybe in Berkeley. I tried to get to helium-4. See, we knew if you bombard helium-3 with protons, it doesn't work; you don't get to helium-4 that way. Then I worked on the capture of electrons by helium-3 to make tritium, because the tritium *p*-gamma reaction makes helium-4 very, very fast. But the helium-3 plus an electron to go to tritium is a weak interaction with a threshold, and I soon found that it was just too slow. I talked to Charlie about this—that I had been trying and trying to get the helium-4 in the *p*-*p* chain, and it was he who told me, "Well, look, why don't you use helium-3 plus helium-3 going to helium-4 plus two protons?" That was a very special reaction. It involved three products. Who the hell ever heard of such nonsense? But I realized right away that he was right.

Then, of course, [Evry] Schatzman, in France, came to the same conclusion in much the same way as the Cameron-Greenstein business on carbon 13 alpha-*n*. Schatzman published a paper on helium-3 going to helium-4 plus two protons [*Comptes Rendus* 232:19, 1740 (1951)]. All Charlie and I did was publish an abstract. But Charlie had the idea independently of Schatzman. He may have gotten it from Jesse, but it was Charlie who told me. And once he told me I said, "My god, that's it!" And then, of course, we eventually, in Kellogg, studied that reaction to a fare-thee-well.

GREENBERG: Yes. I was wondering why he doesn't play too much of a role in the nuclear astrophysics of the fifties.

FOWLER: Charlie was....

GREENBERG: Too busy?

FOWLER: Charlie was out of town most of the time. Right after the war he spent a lot of time in Washington, helping to found the Office of Naval Research, and that was a godsend for us, because we got one of the first grants from the Office of Naval Research, once it was set up. We asked for $90,000 and got it. Boy, that was an *enormous* amount of money in those days, compared to what the lab had been running on!. But Charlie was on the advisory committee to the air force, and he played a role in all the politics of what went on after the war—the whole question of the atomic bomb and the fusion bomb.

When he was back in Pasadena, he worked hard with Tommy and me in the lab. Because he was away so much, his main contribution was to help us with the laboratory instrumentation, in which he was just superb. Without him, most of the experiments that we did just couldn't have been done. But the decision of what to do next was largely up to Tommy and me.

Bob Christy played a big role. Bob Christy played a very big role—but mainly in the nuclear physics. Bob wasn't sold on nucleosynthesis until the results of the Eniwetok bomb were announced and it was shown that in the bomb very heavy elements were made. Christy was excited enough that he joined Walter Baade, Fred Hoyle, the Burbidges, and me in a paper that said, "Oh, boy! Supernova is where the action is." [*Pub. Ast. Soc. Pac*. 68: 296 (1956)] Walter came into it because he had a supernova light curve which had the same decay period as the decay period of californium-254. Once we got the information that californium-254 was made in the Eniwetok bomb, it was mainly Geoff Burbidge who said, "By golly, I think that's the same lifetime that Walter's been talking about for one of his supernovae." So we all got together and wrote a paper real fast. That, I think Bob Christy would say, was the first time he really got interested in the nucleosynthesis problem.

GREENBERG: Was Father O'Reilly the first nuclear astrophysics PhD student at Caltech?

FOWLER: Did you tell me what year he got his Ph.D.?

GREENBERG: Yes. In 1949 he gave his final doctorate examination—on the physical and chemical composition of the sun.

FOWLER: Well, I suppose that shows that I'm not completely right, that Christy was—in the theoretical work that he was doing—independent of a lot of the other stuff in Kellogg. There again, I think it was mainly in connection with the *structure* of the sun. That's a whole different kettle of fish. You see, the reason, really, that Bethe went wrong in saying that the CN cycle worked in the sun was because his work showed that the nitrogen-14 *p*-gamma reaction was the one that dictated how fast the CN cycle operated. But the astronomers told him at the time that the sun consisted of fourteen percent nitrogen. We now know that it's less than a percent. So it was not only our work in showing that the N^{14} *p*-gamma was very, very slow indeed, but also the fact that there's very little N^{14} in the sun. Christy was obviously interested in these problems, if he put a student on it. I would go so far as to say that the primary purpose of Father O'Reilly's thesis was to get the structure and the energy generation in the sun correct, not so much to worry about any nucleosynthesis, because all the sun is doing is converting hydrogen into helium. Stars do make a little helium, but most of the helium comes out of the Big Bang. There's always this dichotomy of effort in nuclear astrophysics. One, we are looking at what generates energy in stars, and two, we look for the nucleosynthesis that thereby results. That second part involves getting the material out of the star into the interstellar medium, where later on it can then condense to form new stars.

GREENBERG: Most of the helium comes out of the Big Bang?

FOWLER: Yes. The amount of helium that has been produced in the galaxy since the lifetime of the galaxy can be calculated fairly accurately just from the luminosity of the galaxy, because it comes from the conversion of hydrogen into helium. We can calculate how much helium has been made just to give the light that the stars in the galaxy emit. I think the current statement would be that the Big Bang helium, by mass, was something like twenty-three percent of the total, with the rest being hydrogen. Then, by the time the sun formed, the helium content in the interstellar medium had been increased to twenty-seven or twenty-eight percent, and now it may be as high as twenty-nine or thirty percent. But stars—ordinary stars, main sequence stars—shine on the conversion of hydrogen into helium, but the production doesn't require very much conversion to give the energy generation in the whole history of stars in the galaxy.

GREENBERG: Some stars shine on the CN cycle? Where did the carbon come from?

FOWLER: They have to have inherited that from a previous generation of stars in which the hydrogen went to helium through the *p-p* chain. In the red giant stage, the helium went to carbon, and then the next-generation star, if it's massive and hot enough, can begin converting hydrogen to helium with the carbon and the oxygen that's also made. Nowadays, that's one of the key problems being worked on in Kellogg and elsewhere: Out of the helium burning, how much carbon do you make and how much oxygen do you make? That's still one of the unsolved problems in nuclear astrophysics. There's a big controversy about it at the moment, because the results that Charlie Barnes and Peggy Dyer got a few years ago [*Nuc. Phys. A* 233: 495 (1974)] have been challenged by a group at Münster. So both teams are now repeating the measurements, trying to go to still lower energies. That's one of the things that our little yellow submarine's being used for by Charlie Barnes and Brad Filippone.

It's a key problem, because the rates just have to be such that, for example, in the sun there is about twice as much oxygen as carbon, but that ratio comes as a result of essentially what happens in helium burning. It turns out that if you use the Münster results, you make practically nothing but oxygen and don't make any carbon. So most of us think that their results must be wrong. With Charlie Barnes and Peggy Dyer's results, you get a reasonable ratio of oxygen and carbon coming out of helium burning. So there, really, in the whole scheme is a big problem, and it's being worked on very hard.

GREENBERG: The abundance project on the campus that Greenstein oversaw: In reading what he says about it, he's rather cryptic. Was that a successful project?

FOWLER: Well, I would say so. My recollection is that, again, Jesse and the abundance project group were more interested in anomalous abundances than they were in the production of the so-called cosmic abundances, which are really the abundances in the sun and other main sequence stars that are similar to the sun. The determination of the solar system abundances was largely the work of [Leo] Goldberg and [Lawrence H.] Aller, from the astronomical side, and of the geochemists looking at meteorites. I remember Jesse from time to time showing comparisons between astronomical abundances and the meteoritic abundances. So in that sense he was very interested. There was the longstanding controversy about the abundance of iron. As I remember it, there seemed to be more iron calculated from the spectroscopy of the sun than the geochemists found in meteorites. And that problem was in part solved by Ward Whaling, although other groups definitely contributed. What Whaling did was use the so-called beam-foil spectroscopy method with the accelerators in Kellogg to determine what astronomers called the *f*-values—the strength of the lines radiated by excited atoms in the surface of the sun. And Ward, as well as others, showed that the old *f*-values were just wrong as the spectroscopists had determined them, and that when you use his new values then the amount of iron determined in the surface of the sun relative to the total is just what the geochemists had been saying for years was the iron abundance relative to everything else.

GREENBERG: Did Hoyle and the Burbidges know each other before you came along?

FOWLER: I would say yes, because Geoff Burbidge had come to Cambridge to work with Ryle even before I got to Cambridge in the fall of 1954—it couldn't have been very much sooner. I gave a colloquium in the Cavendish on the results that had been obtained on the three-alpha-to-carbon-12 reaction after what Ward Whaling had done, and then Charlie Cook and Charlie Lauritsen and Tommy Lauritsen and I showed that the state that Whaling had found had the proper spin and parity to be formed from three alpha particles.

I gave a colloquium, and I know Hoyle was there, and Burbidge was there, because the next day he came in to my office in the Cavendish and said, "Well, that was very interesting stuff, but there are much more important problems of that nature in astronomy. My wife and I are finding anomalous abundances in the barium stars"—and so forth and so on—"and there must be some explanation in terms of what you're talking about." So that was just amazing. When Geoff walked into my office I thought he was Charles Laughton—same girth, same double chin. And then he said, "Well, I want you to meet my wife." So we arranged to meet, and my god, Margaret showed up and she was so *incredibly* beautiful! She still is—she's an incredibly beautiful woman. I could hardly believe that this beautiful girl had married this character with a double chin and a waist folded over his belt, but they were a perfect team, she the observationist and he the theorist.

Hoyle and Burbidge must have known each other, because they were clearly very friendly right from the beginning. They were mainly friendly because Geoff was convinced that the Steady State theory was right, and he agreed with Hoyle that Ryle's interpretation of Ryle's data was just wrong. Well, after one year of this, Ryle said, "I don't want you around any more; you're not the theorist that I want working on my results."

So when I found that out**,** that's when I invited Geoff and Margaret to come back to Caltech. They had been in the United States before, at Yerkes and at MacDonald. There is this, I think, very funny story about what I wrote to Ike Bowen. I said, "Ike, I've met this couple who are very much interested in something that's of mutual interest to us—namely, nucleosynthesis and anomalous abundances in stars—and Mrs. Burbidge is an observer. Could you make her a Carnegie Fellow and we'll make Geoff a research fellow in Kellogg." And Ike wrote back and says, "Willy, I'm sorry, there are no toilet facilities for women [laughter] at Mount Wilson." I told Margaret this and Margaret said, "Well I'll use the bushes." I'll never forget that. The eventual result was that Ike gave Burbidge a Carnegie Fellowship and Kellogg gave Margaret a half-time research fellowship.

GREENBERG: I am curious about how you viewed Cameron. Was this a serious competition, or were you pretty much going on about your own business independently of each other?

FOWLER: I had considerable correspondence with Cameron, and we exchanged preprints. The correspondence was mainly over the neutron source in stars. Cameron, having invented C^{13} alpha-*n*, tended to stick to it. I had come to the conclusion—for reasons I'm not clear about now—that the C^{13} alpha-*n* wasn't a very effective source, and had plugged for neon-21 alpha-*n* and neon-22 alpha-*n*, so there is some correspondence between Cameron and myself arguing mainly about that point.

Fig. 12. Margaret and Geoffrey Burbidge in March 1956. Caltech Archives; photograph by W.W. Girdner.

On the grand scheme of nucleosynthesis, the work was entirely independent—it was entirely independent. The minute we saw Suess and Urey's paper, we started writing B^2FH [Burbidge, Burbidge, Fowler, and Hoyle, "Synthesis of the Elements in Stars"]. We actually wrote a previous paper—Hoyle, Fowler, Burbidge, and Burbidge—which appeared in *Science,* [124: 3223, 611 (1956)], but then Al was at the same time going along very much the same lines, writing for the Astronomical Society of the Pacific. He also had a Chalk River preprint which we got after we had submitted our papers. We didn't send him our stuff until we were ready to submit for publication, because there was a sense of competition, in the sense that we knew Al Cameron was a hell of a smart guy and that if we had seen the significance of Suess and

Urey's paper Al would almost certainly have seen it, too. In all fairness, I think our paper was much more detailed than his, and furthermore we published first in *Science* and then in the *Reviews of Modern Physics*, which people read, and Al, for some reason or other, published in the *Publications of the Astronomical Society of the Pacific*, which no one reads. So the upshot has been that most of the people who refer to B^2FH just ignore Al's piece. But I have made no bones about the fact that Cameron's work was completely independent, because there was no correspondence on that aspect of the problem.

We all clearly—the Burbidges and Hoyle and I—just spent every waking hour getting that goddamned paper, which is a pretty big thick thing, written up. We did write a preliminary sketch first and then we got down to writing the big one. Fred and I were invited by Walter Baade to the Semaine d'Étude 2 in the Vatican. That must have been in 1957, and I remember taking a big thick preprint of B^2FH along with me and actually giving a talk about it there.

There's just no doubt that Cameron had the ideas quite independently, and although he and I had corresponded, mainly in argumentation about what was the source of neutrons in stars, that was about it. He came to Caltech, and I forget what year that was, to work with Jesse's group. Out of that came the fact that Jesse's son, George Greenstein, actually did his PhD work under Cameron at Yale. So Cameron came to work on the abundance project with Jesse. I saw a great deal of him, and the story, I think, is given fairly in the chapter that [John N.] Bahcall and [Raymond] Davis wrote in the book *Essays in Nuclear Astrophysics* [edited by C. A. Barnes, D. D. Clayton, and D. N. Schramm, and presented to William A. Fowler on the occasion of his seventieth birthday; Cambridge University Press, 1982].

While Al was here, Harry Holmgren at Maryland showed that the helium-3 alpha-gamma reaction was a hell of a lot faster than the calculations that Ed Salpeter had made earlier. This meant that you could make beryllium-7 in stars, and then the beryllium-7 could make boron-8, and that gave neutrinos that Davis could detect with his chlorine detector. I think it's fair to say, and according to what's in that book, Cameron admits that I told him—because I was a little bit faster on reading the literature on experimental data—about the helium-3 alpha-gamma. Again, quite independently, he realized that that would make a lot of beryllium-7 and the beryllium-7 would then make a lot of boron-8. So, again, we published independently; we didn't work together. I just told him one day, "Look, Al, there's something very exciting in the last *Physical Review,"* and so he wrote a paper and I wrote a paper. Again, I think it's fair to say that my paper was much more detailed than his. But again, he quite independently came to the detailed completion of the *p*-*p* chain, just as I did.

WILLIAM A. FOWLER

SESSION 8

May 31, 1984

Begin Tape 8, Side 1

GREENBERG: When we left off yesterday, we were talking a little bit about Al Cameron, who, for a while anyway, I guess was something of a competitor.

FOWLER: And still is.

GREENBERG: And still is. You said some interesting things. I was interested to learn, for example, that you collaborated with him—you wrote a paper with him, after the dust settled.

FOWLER: Yes. We wrote a paper together on the production of lithium in stars that show lithium, but that was fairly late in the game [*Astrophys. J.*, 164:111 (1971)]. I forget the exact time. But as I remember, even while I was in England in 1954-55, mostly working with the Burbidges, trying to find a source of neutrons in stars that would produce the anomalous abundances that they were finding in various types of stars, I corresponded with Al, because he, along with Jesse Greenstein, had pushed carbon-13 alpha-*n* as the basic source of neutrons in red giant stars. That's now generally accepted, although it's still one of the major problems. Anyhow, I corresponded with Al on the ideas that I had, coming out of the work with the Burbidges, that maybe neon-21 alpha-*n* and neon-22 alpha-*n* could be sources. So we corresponded very early in the game, and the letters were to a certain extent controversial. Then I met Al, and I think it's fair to say that he's fairly outspoken, and at that time, at least, I thought that he wouldn't listen really intently to what I was trying to say. So there was a period when we weren't especially friendly.

GREENBERG: I have here in my notes, as you put it, "He tells you how it is, hands out the truth in the form of graphs."

FOWLER: Yes. He always carried around a little pocketful of photographs of the graphs of his work. He would just insist on showing you this and explaining everything. But that's Al. But I soon began to realize, especially when he sent me a preprint of his work on nucleosynthesis based on the discoveries of Suess and Urey, that it was very similar to what the Burbidges and Hoyle and I were doing. I realized right then that anyone who could duplicate single-handedly what the four of us had been doing was a pretty smart guy. The other thing that led to the friendship that we have today was that he began sending his graduate students to me as postdocs—Jim Truran and Dave Arnett, who are now right at the top in nuclear astrophysics. It was through them that I learned that Al, under his exterior, had a heart of gold. Truran and Arnett were two of the best postdocs I ever had, and now that I think about it, they gave further evidence to me that Cameron really knew what he was doing and that I should listen to what he said. So one thing led to another; we met at meetings; he came out here for a year to work with Jesse .

GREENBERG: On the abundance project?

FOWLER: Yes, he worked on the abundance project with Jesse, and, of course, we were busy still trying to measure cross sections, and I was interested in the applications of those cross sections. So I became a very strong supporter of Al Cameron's position in the field. Eventually we wrote a paper together. We have continued to correspond, not as intensively as those first few years, but he sends me all his preprints and I send him mine, and we exchange comments and so forth and so on. He continues to be a leader in the field. He covers a much wider range of subjects than I have even done. He is probably looked on, if not as *the* authority, certainly as one of the authorities on the origin of the solar system. He's done a great deal of work that is extremely good, and he continues to work in that field.

GREENBERG: You said that you felt he was more of a theoretician than you were.

FOWLER: Much more. He is much more of a theoretician in the strict sense of the word than I am. Most of my theory has been developing methods of theoretical analysis of cross sections measured in the laboratory so that they can be translated into reaction rates in stars, whereas Al has produced much broader theoretical concepts than I ever have.

The high-energy physicists have a name for people who analyze laboratory experimental data to put it in such a form that it can be used in various astrophysical or cosmological circumstances. They're called phenomenologists. So if the word were used in nuclear physics I think that I would be called a phenomenologist rather than a theorist. For example, Geoffrey Fox at Caltech is one of the leading phenomenologists in elementary particle theory, and much of the work that he's been doing on developing supercomputers now is just so that all of this complicated data that pours out of CERN [European Organization for Nuclear Research] and Fermilab and SLAC can be put into a form that is useful to the theorist. So whereas Al Cameron is what you would call a pure theorist, I think that I would be characterized as a phenomenologist, although that term has never been applied in nuclear physics.

GREENBERG: One other thing you mentioned was that Jesse Greenstein's son—the one who is the astronomer—was a graduate student of Cameron's.

FOWLER: George Greenstein is a really topnotch theorist now, and that's another evidence that Cameron has made a great impact on the field, through his theoretical graduate students. I think it's fair to say that I have made an impact mainly through graduate students and postdocs who were experimentalists, although some of my students—for example, Don Clayton—have become theorists. I could mention others.

GREENBERG: All right. Is this the time to talk about [Gerald J.] Wasserburg?

FOWLER: I would think so. Gerry Wasserburg came to what was then the Geology Division—it's now Geological and Planetary Sciences—in 1955, and immediately started setting up a very clean laboratory, which is necessary to do precise measurements of isotopic abundances in meteorites. I think he's even done it with the microdust that falls on the earth's atmosphere from outer space that's collected by very high-flying U2s. Wasserburg was really a shot in the arm for me and the rest of us in Kellogg, because here we had someone who was actively engaged in experimental determinations of anomalies in abundances in meteorites.

His research was then and continues to be a key contribution to our ideas of nucleosynthesis in stars and how the solar system, when it formed from a solar nebula, was not completely mixed. It was mixed down to an almost incredible level in a sense, so that the anomalies in different parts of the solar system, such as that part that formed the Allende meteorite, had slightly different abundances than the solar system had on average. By comparing meteoritic abundances with isotopic abundances on the earth, Gerry's been able to show that when the solar system formed, the material was not completely mixed, so that material that formed in the vicinity of the earth was slightly different than the material that formed the asteroid belt, from which, presumably, meteorites come. We've learned an enormous amount from Gerry's measurements.

One of the papers of which I'm proudest is one that I wrote with Dave Sandler and Steve Koonin, trying to explain, and I think fairly successfully, the anomalies that Gerry had found in the calcium and titanium isotopes in the Allende meteorite [*Astrophys. J.*, 259 (2): 908 (1982)]. So for me, Wasserburg's work has, in kind of an incredible way—by finding very small differences—allowed us to learn a great deal about the details of nucleosynthesis which we otherwise would not have known. You see, the entrenched belief in astronomy was that the element abundances and the isotopic abundances were the same throughout the solar system and were the same throughout all stars that were similar to the sun. Well, that's true to a certain extent, but not completely true. And the deviations have been very, very informative.

Gerry has been a constant prod, in a way, to get us to do some things in the lab which we otherwise would not have done. So he has had an enormous influence, ever since he came, on the direction of exactly which reactions we were going to study in the laboratory. Also he's a very close friend of mine. We discuss these problems all the time and I learn an enormous amount from him.

Of course his laboratory is recognized now as *the* greatest in the world in the field. He, as everyone knows, calls it the Lunatic Asylum, because—I forgot to mention—he not only looked at meteoritic samples but once samples were brought back from the moon his lab took the lead in looking again at isotopic abundances in the lunar samples. His greatest contribution there was to show, looking at the ratio of radioactive products to their parents, that the moon was 4.5 billion years old—the same age as the meteorites. And that established that, along with the fact that the oldest rocks on the earth—which are very rare, because the earth has had so much tectonic activity—also give ages of at least 4 billion years. The earth has wiped out so much of its early history that we can't really trust the earth as a true indicator of the element abundances in the solar system.

On the other hand, all the chemical processes that separated the elements here on the earth did not disturb the isotopic abundances very much. So we take terrestrial isotopic abundances as standards, and then when one finds something different in meteorites and something different in the moon, that just shows that the material of the big gas cloud—the solar nebula, as we call it—from which the earth formed was not completely mixed.

I've done some thinking on it already and I plan in the future to make that one of my major projects, because the fact that the solar nebula wasn't completely mixed is a key to how the interstellar medium collected material from novae and supernovae and red giants. There is turbulence in the interstellar medium which tends to mix things, but it didn't mix it completely. And that's really one of the biggest things that I think has happened in the last decade, and Gerry Wasserburg has taken the lead in that.

The other thing is that at one time Gerry was offered a very prestigious position at Harvard University. I realized at the time that one of the reasons that he might go to Harvard was because they promised him better funding than he was getting here at Caltech, so we decided at that time that part of the Kellogg grant—several hundred thousand dollars a year—would go to Gerry's lab, because he was doing things in which we were so greatly interested and because he was stimulating work in our lab. So he has been a member of the Kellogg grant from NSF for at least a decade and continues to receive funds along with the rest of us. As a consequence, he's considered a member of the staff of the grant, and he meets with the rest of us at our weekly or biweekly meetings and makes his input, just like the rest of us do. [By 1985, Wasserburg had sufficient other funding to give up his share of the Kellogg funding—W. A. F.]

GREENBERG: Is there anything concerning postwar rebuilding of the Kellogg lab that we might have missed?

FOWLER: Yes. In thinking about what I said to you yesterday—tthat right after the war we didn't really play much of a role in theoretical developments—the reason was that Kellogg had to be completely rebuilt as a nuclear physics lab after the war. The one electrostatic accelerator we had operating—the one that Tommy Lauritsen and I had built in the High Voltage Laboratory—had been moved into Kellogg and had been our workhorse, but when the war came it was moved over to a corner of the big lab on the second floor, and the space it had occupied was turned into the drafting room for the rocket project. We had just purchased a still larger pressure vessel in order to build a still higher energy machine. That pressure vessel was actually taken out of Kellogg at the start of the war and was used at Morris Dam as a pressure vessel to project torpedos into the water there, so that the exact angle and entry of the torpedo in speed could be known. A great deal of the developments on torpedos during the war was done as part of the rocket project, mainly under the direction of Fred Lindvall and Max Mason. Previous to that, all the testing of torpedos had been done by actually dropping them from airplanes into an underwater array of detectors, but in spite of the skill of the pilots it was just random whether or not the torpedo fell where they could study its characteristics. So the Morris Dam project on torpedos was part of the rocket project. The [Naval] Bureau of Ordnance asked us to do it, so we did it.

Then after the war, as soon as the war was over, that big pressure vessel had to be moved back from Morris Dam, reinstalled in room 200 of Kellogg, where it still is, and it had to be rebuilt from scratch, just like the smaller one had to be completely refurbished. Then Ward Whaling built a low energy/high current accelerator to study nuclear reactions at very low energies, which is where you want to have the information for application in stars. So we were so busy for two or three or four years completely rebuilding Kellogg that all we had time to do, as we got the accelerators going, was to use them. We didn't do all that much thinking about what the results meant until.... With the possible exception of the CN cycle reactions; we knew what that meant and what we were doing there. But the implications, as I said before, of what we were doing were brought forcibly home to us by Ed Salpeter and Fred Hoyle in 1951-53.

GREENBERG: Did the change of command from Millikan to DuBridge have any effect on directions?

FOWLER: I don't think it had much effect on Kellogg. Millikan really never regained his dictatorial position at Caltech after the war. The man who really ran things until DuBridge came [1946] was Earnest Watson. Then when DuBridge came—you must remember that DuBridge was a nuclear physicist at Rochester. He had gone to MIT to run the radar project. So when DuBridge came to succeed Millikan, I think there were problems. Millikan, as far as I can remember, wasn't too happy about being replaced. The old man thought he could just go on forever, and that's quite natural and very admirable.

DuBridge supported what we were doing in Kellogg hook, line, and sinker. You have to remember that Charlie Lauritsen was a very powerful figure. His role during the war had made him quite well known to people such as DuBridge, and he had this idea of what he was going to do. I think DuBridge just realized, "Well, here's one lab at Caltech that I don't have to worry about. Let them go about their business." He went about getting Greenstein here, for example, to start some astronomy, and all the other things that Lee did. But he supported us, and in fact, just didn't bother us.

GREENBERG: Where did the money come from after the war?

FOWLER: Well, after the war, in 1946, we immediately made one of the first proposals to the Office of Naval Research, which Lauritsen had helped Admiral Robert Conrad set up. So we got a grant sometime in 1946 for $90,000, which, as I've said before, was an incredible sum of money in those days. And that money was what enabled us to rebuild Kellogg. Those grants continued, and by 1968, the last year that we were supported by the ONR, the funding had gone up to around $900,000 a year.

Then in 1968 the National Science Foundation took over the support of Kellogg, and there were a couple of periods of overlap when ONR was supplying some of the money and the National Science Foundation the rest, but about that time the National Science Foundation took over the entire grant. So there was no discontinuity in our support when it became national policy for the National Science Foundation to take over many of the grants that the ONR had been supporting.

One complication did arise. I had been the principal investigator for ten years or so on the ONR grant. By 1968 the National Science Foundation was the sole support, and I was appointed to the National Science Board. One of the rules is that if you're a member of the National Science Board you cannot be principal investigator of one of their grants. So when I was appointed to the board, there had to be a lot of quick dealing, and Tommy Lauritsen was made the principal investigator, which was a good thing because Tommy had essentially been doing all of the dirty work while I was the principal investigator. Tommy continued until his death in

1973, and then Tom Tombrello became the principal investigator.

So the change in command at Caltech did not involve Kellogg, although it was a wrench in many ways. The man who played the leading role in bringing DuBridge here was Max Mason. Millikan finally gracefully agreed, and he acted properly when DuBridge was inaugurated and all that sort of thing. But we all knew that the Chief, as he was called, was kind of unhappy about it. He was kind of unhappy during the whole war, because there were the two big projects at Caltech—the one in Kellogg on rocket ordnance and the one in Guggenheim on jet-assisted takeoff, and Millikan had practically nothing to do with those projects. Earnest Watson was the principal investigator for the Kellogg project—Charlie was the scientific director, and I was assistant scientific director—and Clark Millikan and [Theodore] von Kármán and Frank Malina essentially ran the jet-assisted takeoff. Millikan, although he was still head of Caltech, had very, very little to do with what was going on.

GREENBERG: All right. Let's go to the sixties, when you effectively begin a second career.

FOWLER: Yes. Well, I guess I did my last experimental work in the lab, in which I actively participated, in 1964. At that time I decided, "Well, I've been working in the laboratory for thirty years, and it's about time that I begin to think a little bit about what all this work means." I began to see that we had accumulated so much data that needed putting into a form that astrophysicists could use, so at that time I essentially became a phenomenologist.

One of the first indications of that was that Jan [Georgeanne R.] Caughlan, who is just this year retiring as professor of physics at Montana State, wanted to finish her PhD work at the University of Washington, where she had taken all of the graduate courses but had not finished a thesis. So she came down to Kellogg, I think for a year [1961-1963], essentially under my supervision, wrote a thesis, and the University of Washington accepted it. Then she and I, along with Barbara Zimmerman, began a collaboration which continues to this day in publishing in the *Annual Reviews of Astronomy and Astrophysics* our analysis of nuclear cross sections in which we express the results in terms of reaction rates as a function of temperature, which the theoretical astrophysicists could use. We published periodically, in 1967, in 1975, and then again in 1982. Mike Harris, who was a postdoc with me then, collaborated in that third one. We call them "Thermonuclear Reaction Rates I," "Thermonuclear Reaction Rates II," "Thermonuclear Reaction Rates III" [*Annual Reviews of Astronomy and Astrophysics,* 5: 525 (1967); 13: 69 (1975); 21: 165 (1983)].

All of those results were expressed in terms of analytical formulas which we had to develop to account for all of the kinds of idiosyncrasies in the cross sections—cross sections and this, that, and the other thing. We got more and more demands from people who said they would like to have numerical tables as well as these analytical expressions, because some people prefer to put numerical tables into their computer programs rather than analytical expressions, which they have to call and solve every time they want an answer. So we published preprints for a number of years, giving analytical expressions, and now we've just submitted to *Atomic Data and Nuclear Data Tables* the numerical expressions of the very latest results. It's been submitted and accepted; the referee has approved it. So it will appear within the next few months or so. So that became one of my major preoccupations.

It's a little strange that it took us eight years each time. I think I said 1967, 1975, 1982, and then 1983, and now, either late in 1984 or early in 1985, will be this numerical thing, which I think people will find extremely useful. It's probably the last that I will do in that regard, because with a few exceptions most of the cross sections and the analysis have been kind of agreed upon, and so that paper in *Atomic Data and Nuclear Data Tables* [32: 197 (1985)] will put paid to that part of my research activities.

As modifications come from new experiments, most people have now learned from our discussions in those papers how to do it themselves. If they make a measurement of cross sections in the laboratory, we have given prescriptions that cover practically any eventuality. So I think that field is now one that will just go on, on its own. There are still many problems. Our results only go up to silicon. Something similar could be done for all reactions from silicon to iron; to a certain extent, that has been done by Graham Sargood at Melbourne, who spent several years as a research fellow at Caltech. So I think that's in very good hands. The neutron cross sections have been mainly measured at Oak Ridge under [Richard] Macklin and [John] Gibbons—Jack Gibbons is now head of the Office of Technology Assessment in Washington. They have published—again, in agreement with the methods that we used—all of the neutron reaction rates that run from iron all the way up to uranium and thorium. So the reaction rate business, I think, is in good hands, although probably someone should codify in some way all of the stuff from silicon up to iron, but I don't intend to do that. That will have to be someone else.

There's a group at Livermore under Grant Mathews, who was a postdoc here, who probably will do that job. But as I just said, Sargood has essentially done it in such a way that people can get all the information that they really need. [W.A.F.: August 1985—Caughlan and Fowler are doing it!]

GREENBERG: What about the relativistic astrophysics? That's really what I had in mind when I said a "whole new line of work."

FOWLER: Well, [pause] Fred Hoyle and I became interested in the enormous amounts of energy being developed in the strong radio sources. So we decided to look at supermassive objects—stars, if you want to call them that, in the range of 10^6 to 10^{12} solar masses.

Begin Tape 8, Side 2

FOWLER: [continues] These solar masses are capable of generating—just because they're so large—enormous amounts of nuclear energy. So Fred and I developed the generation of nuclear energy by supermassive objects.

Then, just after we had published our first papers, one in *Nature* [197 (486): 533 (1963)] and one in the *Monthly Notices of the Royal Astronomical Society* [125: 169 (1963)], I think it was, the quasars were discovered. After we had done all of this work on supermassive objects, we realized that although they could supply energy comparable to what the strong radio sources were putting out, it was clear that the strong radio sources were very extended objects and that supenmassive objects that were stars didn't seem to have much connection in that sense with strong radio sources. But when quasars—which were named that after the full term, "quasi-stellar objects"—were discovered, we immediately thought, "Oh, by god, we've explained quasars!" That euphoria lasted for only a brief time, because it soon became clear that nuclear energy would power supermassive objects only for about a million years, and here were quasars, which gave more energy than you really could get from nuclear reactions unless you went to stars of the order of 10^{13} solar masses. Furthermore, it became clear that quasars lived longer than a million years—maybe a billion years, although there's still some controversy about that.

The upshot is that although we mentioned it in a subsequent paper, many other people got into the business. It became clear that the quasars must somehow or other be generating gravitational energy, and the current model is that quasars probably have a supermassive black hole at their center which is accreting material from the interstellar medium. As that material falls in, it generates enormous amounts of gravitational energy due to the great gravitational fields of the presumed black hole. And it generates that energy outside of the black hole by collisions as the stuff's falling, and that energy can get away.

The present model is that there's a black hole which rotates; the material kind of spirals in and forms what's called an accretion disk. It's the constant falling in, developing kinetic energy due to the great gravitational field; then the collisions of the particles generate X rays, gamma rays, optical light, and so forth and so on. So our attempt to explain quasars by nuclear energy just does not work, and the accepted point of view now is that, well, the thing may have started as a supermassive star, lived for a million years, and then collapsed to a black hole. The real energy generation began as that black hole accumulated material from the interstellar medium.

We had a year or so where we thought we had explained quasars, but no one else believed it, because the astronomers were beginning to find out that the quasars, if they're cosmological, were generating much more energy than you can get from nuclear energy. You see, nuclear energy—the conversion of hydrogen into helium gives you one percent, roughly, of the rest mass. That's just not enough, whereas the gravitational energies can be as much as ten to twenty percent of the rest mass.

GREENBERG: How long did you work on this problem?

FOWLER: Well, I continued to work for some time on it. I tried to keep the supermassive objects living longer, before they collapsed, by having them rotate. By rotating them, you generate centrifugal forces which oppose the gravitational collapse. So I continued to work for quite a few years on rotating supermassive objects. Fred played practically no role in that; he was off doing other things. But even with large amounts of rotation, I found that the amount of mass that a rotating supermassive object had to have was something like 10^{13} solar masses, and that's a 100 times the mass of a galaxy. I finally concluded that that was just unreasonable and gave up.

GREENBERG: Did you work in the laboratory in connection with this project?

FOWLER: No. No, the only thing I was doing in the laboratory was still helping to supervise several graduate students, along with Charlie Barnes, but Barnes was their real supervisor. I talked to them mainly about what their results meant. So I kept an interest in what was going on in the lab, and I participated mainly in the theoretical aspects of a number of graduate students. But that had nothing to do with supermassive objects. That was something I was doing entirely on my own, kind of just for the hell of it.

GREENBERG: Well, the supermassive objects sound a lot different than the kind of thing you'd been doing up to then.

FOWLER: It was, because it was kind of a new venture for me. I had to learn a lot of general relativity. At the same time that I was doing things on supermassive objects, I used what we call the post-Newtonian approximation. That is, you use Newton's laws with just the next terms in the equations that you get from Einstein's equations. At the same time that I was doing it that way—that's perfectly sufficient—it turned out [that Subrahmanyan] Chandrasekhar was solving the structure of supermassive objects using the full panoply of general relativity. He got essentially the same results that I got using the post-Newtonian approximation, but his work was of course much more elegant and beautiful. But people didn't believe him anymore than they believed me.

On the other hand, Chandrasekhar got interested in black holes, and of course he has now written a book [*The Mathematical Theory of Black Holes* (Oxford: Clarendon Press, 1983)], which is considered to be *the* authoritative book on the application of general relativity to massive objects. He's extremely proud of that, and I don't blame him; it's a wonderful book. But in the precollapsed stage, the post-Newtonian approximation is sufficient. Once collapse starts—even rotation can prevent it only for a certain amount of time—once collapse starts, then you've got to use the full panoply of general relativity, and I just wasn't capable of doing that, but Chandrasekhar was.

GREENBERG: Did Feynman point out something along these lines?

FOWLER: Yes. Feynman played a very key role. He bumped into me one day, and he says, "Willy, you know those supermassive objects that you and Fred have been working on are unstable. They'll collapse, due to general relativity." And I was just shocked, because Fred and I, in our first papers, had made a completely Newtonian solution of the problem—just standard stellar structure, although Fred had written a few kind of cryptic remarks about the possibility of instability. But I hadn't understood really what Fred was talking about, and we hadn't emphasized it. But it was Dick Feynman who pointed out that if you put in general relativity, then a supermassive object was unstable.

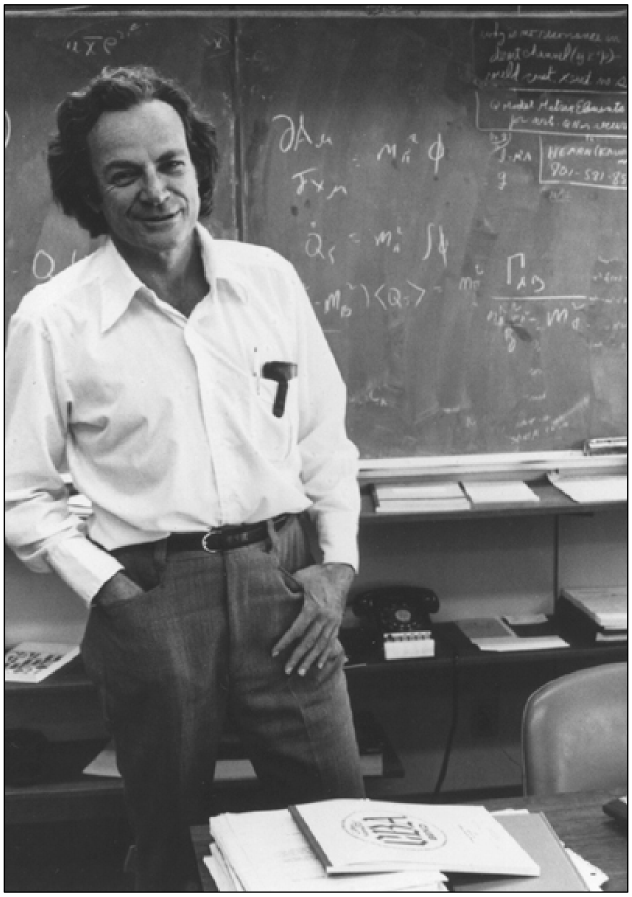

Fig. 13. Richard Feynman in his office at Caltech in 1974. Caltech Archives; photograph by Floyd Clark.

The general relativistic terms reduce the effective gamma of the equation of state below the value for which a star can be stable. So he actually then gave a lecture in one of his classes and Icko Iben, who was a postdoc with me at the time, was attending Dick's classes. So Icko Iben immediately began making numerical solutions of the problems, using the Caltech computer, and convinced me that Dick was right. I immediately began to put general relativity in, but only in terms of the next approximation after Newton, and found right away that Dick was right, that the damn things would collapse.

I had a period where I tried to save it by having the thing partially collapse, burn some more hydrogen, and get back into the main sequence—I called them relaxation oscillations—but that would only work for about a million years. Then I started putting in rotation, so in addition to the post-Newtonian relativistic terms, I added essentially the centrifugal forces due to rotation, and that helped, but really not enough.

Now, of course, most quasars have been found to have a little fuzz around them; they're not quite like objects. Better observations with telescopes show that they probably are in the center of a galaxy; there is probably a black hole there that's accreting material and powering the whole thing, essentially using gravitational energy.

It was interesting, during the Nobel week [in Stockholm, December 5-10, 1983], Chandrasekhar and I went to the Stockholm Observatory. We had both known Bertil Lindblad, who was a very great Swedish astronomer. He's dead, but Mrs. Lindblad, whom we both knew, is living near the observatory, where their son is now the director. So one of the things we did was go over and see Mrs. Lindblad, which was a very nice occasion.

They had a meeting in the observatory with all the observatory staff and students there, and Chandra and I sat up in front, and we got into a fairly heated argument about black holes, because Chandra was expounding on what he had done and written in his book, and I, in kind of a malicious way, was pointing out, "Well, there are several candidates for black holes but none has been completely accepted, even today, by most of the astronomical community." I emphasized that the important thing was to somehow or other make some observations which would prove that these candidates were really black holes. And of course Chandra took the attitude, "Well, the theory is so elegant and so beautiful [that] there just have to be black holes."

It was a friendly enough discussion, but it was just quite in keeping with the fact that Chandra is a pure theorist, whereas I still want to see observational evidence in astronomy, just like I want to see experimental evidence in physics for theories. Anyhow, the students and the staff had a great time listening to us, and I would be the first to admit that in a sense Chandra won the argument, because everybody *wants* to believe in black holes. But then there are some of us who want to see unequivocal evidence that a black hole exists. There's one in Cygnus A that's a very strong candidate.

The quasar theories are all very elegant, but the behavior of quasars as found observationally is strange and wonderful. How the energy gets from this accreting black hole out into the associated radio sources is still, I would say, a mystery, although in particular Roger Blandford at Caltech has done a lot of work to show that some of these very peculiar phenomena can be understood.

GREENBERG: I guess Hoyle to this day doesn't believe that the quasars are cosmological, is that right?

FOWLER: No, he did recant at one time, when the black body radiation was found; it pointed so clearly to the Big Bang as the correct cosmological model. So there was a period when Fred, as I said, recanted, and to the extent that he joined with Robert Wagoner and me in redoing all that Fermi and Turkevich had done on the element synthesis in the Big Bang. Murray Gell-Mann had been prodding me to do that for years, and I just hadn't had any real motivation to do it, but when the microwave radiation was discovered, Bob Wagoner—who was a graduate student of Leonard Schiff, who was an undergraduate with me at Ohio State—came down as a postdoc, and I said, "Bob, I've got all these new reaction rates available that have been accumulating over the years. Why don't we redo what Fermi and Turkevich did?" Fred joined us in that paper and was very helpful [*Astrophys. J.*, 148: 3 (1967)]. We did what is now the accepted work, although Wagoner has written several papers in addition, when still new data came along.

It's one of the papers of which I'm proudest, because it's used, time and time again, to put constraints on the baryon density in the universe. We showed that to get the observed abundance of deuterium, helium-3, helium-4, and lithium-7 out of the Big Bang, using the current black body temperature extrapolated back in time to the very high temperatures at that time, we were able to specify the density of the universe that gave the right answers. And it turns out that the baryon density is only, at most, ten percent of the critical density necessary to close the universe.

Right at the moment, that's one of the things that I'm most excited about, because the new model of the expanding universe, called the inflationary universe, has explained so many things, like the homogeneity and isotropy of the universe, so beautifully that I just have a feeling, and many people have, that it must be true. It predicts that the universe is a flat Euclidian one, with *exactly* what's called the critical density, which you can calculate from the Hubble constant. So the current situation is that a really very elegant theory, which has had an incredible number of successes, predicts that the current density of the universe on average is five times 10^{-30} grams per centimeter cubed, whereas our work on the production of the light isotopes in the Big Bang gives a baryon density—that's ordinary matter—of only five times 10^{-31}. So there's a deficiency, ninety percent, and one of the fashionable suggestions for what makes up the deficiency is massive neutrinos. By "massive," I mean something of the order of 1/100,000 of the mass of the electron.

That's the problem that—when I'm able to do so—I'm mainly working on. If neutrinos are massive, that can also explain the solar neutrino problem, because if neutrinos are to oscillate or transform from one form to the other, which would explain the solar neutrino problem, they have to have a mass and they have to have slightly *different* masses. And Felix Boehm, by looking for oscillations on a terrestrial scale—a few meters—has shown that the mass differences have to be very small; but the differences could be incredibly smaller and still give oscillations in the great distance between the sun and the earth. I'm working on that at the present time, and if I can get around to it, I'm going to write a paper giving what I think are the conditions for neutrinos to "close the universe," as it's called, and also to solve the solar neutrino problem. There's still plenty of work to be done, and that's one of my major projects.

GREENBERG: Was deuterium production the last thing that Hoyle and you did together?

FOWLER: I think that was the last thing. We decided to see if deuterium could be made in stars. I forget all the details, but we felt that we succeeded to a certain extent. The conditions under which deuterium was made were so tricky that my feeling now is that it really doesn't work, and that the deuterium and the helium-3, along with helium-4, are all primordial. Fred has gone back to using supermassive objects to make the primordial helium and also to make the baryon excess in the universe.

One of the big developments in the inflationary universe—and the elementary particle physics goes along with it—is that at ordinary laboratory energies going up to as high as CERN can go, the so-called conservation law of baryons is always observed. But at the extreme temperatures, and thus extreme energies, certain particles were formed—they're called X particles—and their decay can violate the conservation of baryons. So one of the really big problems in cosmology has been solved: Why is the universe made of matter, and why didn't it just produce equal amounts of matter and antimatter which would have annihilated by now so there wouldn't be anything but radiation? That's one of the great triumphs of this inflationary universe scheme. To me, *that* has been the basic cosmological problem, not all the arguments about Steady State or about the Big Bang. The real problem is why the universe has wound up with an excess of matter. The inflationary universe explains that, using the current elementary particle physics in a just beautiful way, and that's one of the reasons I'm so enthusiastic about it. It's one of its great triumphs.

Of course, there is a test. If, for example, more protons than antiprotons came out of the very early stages of the Big Bang, and then what antiprotons there were annihilated an equal number of protons but left an excess of protons, that means you can create protons. Then, by the laws of physics, that means that protons can decay, and one of the big things that's going on now is that everyone and his brother are looking for proton decay. If it is found, that will really clinch the argument for the inflationary universe.

Unfortunately, the most straightforward theory predicts a lifetime for protons of some 10^{30} years, which just seems incredible. But the point is that you can get 10^{30} protons in a swimming pool full of water, and if their lifetime is 10^{30} years, then every year will mean that one of them will decay. Well, the experiments so far have pushed the limit to about 10^{32} years.

The simplest of the elementary particle theories, called SU(5), which you'll have to get details on from Feynman or Gell-Mann or [Steven] Frautschi, doesn't give the right answer. People are working on another group called SU(10), and there an answer may come out that is consistent with the limits that have been put on the proton lifetime. But the experiments are continuing, and there are hopes to build bigger and bigger and more sensitive detectors and be able to push the proton decay lifetime to a point where either the theory will stand or it will fall.

To me, as I said, the theory also indicates that there's a missing mass in the universe. I would love that to be neutrinos with a small mass. All three of the neutrinos—electron, muon, tauon neutrinos—all practically degenerate, but with small enough mass differences that oscillations could occur as the electron neutrinos come out of the sun. Before they get to the earth, they'll be one-third muon neutrinos, one-third tauon neutrinos, and then they won't trigger the detectors: Only the one-third of the electron neutrinos left would trigger the detectors. That's just the factor that is missing in the solar neutrino problem. So it's... Oh, boy, it's a really exciting time now! But again, it's a thing which has very little connection with the experimental work in Kellogg. It's a thing I'm working on kind of on my own.

GREENBERG: Caltech is a center for general relativity, is it not?

FOWLER: Yes. In large extent, the work is done in Kip Thorne's group, which includes Roger Blandford and others.

GREENBERG: Was Kellogg involved in starting this group?

FOWLER: We were involved only in the sense that when Kip Thorne came to Caltech, Carl Anderson was very anxious to get him. Carl Anderson was head of the Physics, Mathematics and Astronomy Division.

GREENBERG: Kip was an undergraduate, I think .

FOWLER: Yes, I think he was an undergraduate here, and then he did his graduate work with Johnny Wheeler at Princeton. We all realized he was one hell of a smart guy, and so Carl Anderson wanted him to come to Caltech and some money was needed. So again, we in Kellogg agreed to put some of our funds into seed money for Kip Thorne's general relativity group. After all, he was just a graduate student. Maybe he'd had the postdoc years; I don't know. So we funded Kip Thorne's first few years, until he was able to stand on his own feet and get a grant from the foundation on his own.

I take great pride in the fact that we seeded Wasserburg's work and continued to contribute a part of what he uses to run his lab; we also seeded Kip Thorne's work, and at one time in the game we seeded Jim Mercereau in his work. But that's in large extent, John, due to the fact that Kellogg was fortunate in having funds of the order, in those years, of a million dollars a year, and so we could afford to be generous in making it possible for new things to get started at Caltech. And they've all worked out. They've all worked out.

We continued to contribute to Wasserburg's work specifically in connection with what he was doing that we thought was relevant to the Kellogg work. And of course, Kip's entirely on his own now. Ron Drever is doing all that wonderful work, attempting to detect gravitational waves, and Mercereau's lab is on its own. We got involved with Mercereau because he was interested in building superconducting accelerators, which at one time we thought we might want to put on the back end of one of our accelerators to increase the energy. We eventually decided that we didn't want to do that, so most of his work has been in connection with Stony Brook, and I think Stony Brook has a superconducting stage running now. Tombrello is the one who had the basic ideas, and there was Mercereau and—oh, a young man whose name I forget. He's a research fellow there, and he actually did the work. And Tom eventually lost interest in it because he became more interested in using our accelerators at the energies that existed to do things other than nuclear physics.

GREENBERG: That raises a question. Do you foresee that the Kellogg will continue on in your tradition, or will it change in the near future?

FOWLER: Well, there are enormous pressures nowadays to cut down in the NSF on the support of accelerator groups. Nuclear physics is not the glamorous subject that it once was. So accelerator labs like ours have been closed down all over the country. We're one of the last ones, and we're the one with by far the largest budget, although Princeton is fairly close to us in the amount of support. But the trend now is for people to want to form users' groups, even in nuclear physics, to go to places like [Los Alamos National Laboratory], and to do work at still higher energy accelerators if this big new electron accelerator [CEBAF: Continuous Electron Beam Accelerator Facility] that's being built near Norfolk, at enormous expense, eventually comes into being. That's not absolutely certain yet, but if it does [get built], there will be pressures for people to want to go there to do their research, because it's thought to be much more exciting than carrying on the work in low-energy nuclear physics as applied to nuclear astrophysics.

So I think that Kellogg will continue to do low-energy nuclear astrophysics: certainly [Ralph W.] Kavanagh will continue in that area. Barnes will certainly continue in part, but I think that, for example, Steve Koonin is much more interested in the theory of what's going to be happening at Norfolk. I think Bob McKeown wants to get into [LANL], and he's already doing some users' group work there. So what the future holds is problematical. My feeling is that there will be a tendency for the younger people to form users' groups, while Barnes and Kavanagh continue to do nuclear astrophysics. There are plenty of problems, but they amount to using established techniques to accumulate more and more information, and it's pretty hard to get graduate students and postdocs interested in kind of gilding the lily, as it were.

The biggest problem in nuclear astrophysics at the moment, other than the solar neutrinos problem, is the rate of the carbon-12 alpha-gamma reaction, which makes 0^{16} and essentially determines the abundance of oxygen. Barnes and Brad Filippone are working night and day trying to make a better determination of the rate of that reaction, because the group in Münster, Germany, repeated the work that Barnes and Peggy Dyer did a few years ago and got a different answer. So both laboratories are working very hard to get that rate established. There are several other key problems, and I'm sure that both Kavanagh and Barnes will continue to work on them.

Fig. 14. William A. Fowler with Hans Bethe in Cambridge, England, in July 1981. Caltech Archives.

There's still a lot of work going on around the world—Toronto, Alberta, Münster, Melbourne, to a certain extent Yale, and four or five other places are still doing nuclear astrophysics. But the big pressure is coming from the federal government, which says, "Well, we just can't continue to support expensive projects at individual universities." People have got to form teams and go to these big installations that are being strongly supported by the nuclear physics community. You just can't do elementary particle physics at a university any more. The Caltech synchrotron was shut down a long time ago. You either have to go to CERN or Fermilab or to SLAC.

Begin Tape 9, Side 1

FOWLER: [continues] . . . and this user group technique is now extending into nuclear physics and to other areas. If one wants to use synchrotron radiation for, oh, an *enormous* number of applications in the solid state physics and this, that, and the other thing, you just have to go to SLAC. I have used, frankly, every bit of influence that I have in Washington to say that I think this is a great mistake, because if hands-on physics eventually disappears from university campuses, we're going to be the worse for it.

What will happen is that university campuses will become the places where research in chemistry and biology and geology are done, and more and more branches of physics, just like elementary particle physics, will have to be done at big central installations. I just think that that's a problem that ought to be studied pretty carefully and not just permitted to be solved by default. I've used every bit of influence that I've got to tell people in the National Science Board and people in the Department of Energy that they've got to be very careful in following a policy that is going to mean essentially that no hands-on physics research which gets results will be done on university campuses.

Now, it's perfectly true that the users' groups build a lot of equipment on their campuses and then take that equipment and go to the big accelerators. It's the same in the space program. Ed Stone's program, which is just superb, builds equipment on the campus, but then they take it and put it on a satellite. That's a completely different mode of operation than actually having data coming out right in a laboratory on the campus—of having graduate students not only do their graduate work on the campus but get their results on the campus. I think it's a major problem in the United States, and as I said, I've used all the influence I have to point out that I think there's a problem. But, boy, the pressures are certainly going in the other way as the physics becomes more and more sophisticated, requiring larger and larger facilities. It's almost inevitable that physics is going to be done in big central locations. But it worries me. It worries me.

GREENBERG: I gather that in some fields the computer is actually used to simulate experiments. Has that entered into the kind of work that you do?

FOWLER: I don't think that that use of computers has had very much impact in Kellogg. We have a VAX—and there must be twenty VAXes on the campus now. Every laboratory that's at all operative has a VAX, which can be used in a great number of ways. It's perfectly true that the supercomputers can be used to simulate experimental results and sometimes at a great saving. They can do things much more cheaply than building another great big accelerator could do. But I don't think that's had a very important impact in Kellogg. That may not be true in the elementary particle group at Caltech, but you'd have to talk to Geoff Fox and Steve Frautschi about that. The computer in Kellogg is a tool; it's used to operate the machines. Steve Koonin's theoretical work uses it all the time, and the experimentalists use it to translate their data. The computer is used to accumulate data which you can analyze when you don't have running time. You see, in the old days before computers, when we were operating in the laboratory, we had to write all the results down in a notebook. Now all you put down in a notebook is what you were doing and when and some notation about where the results will be in the VAX. Of course, that has meant that the use of the accelerators is *enormously* efficient and much more accurate. Boy, when we were writing everything down by hand in the old days and trying to keep everything running, I mean, we made mistakes! Well, the computer just doesn't make mistakes, in general.

So the whole operation is *incredibly* more efficient. Furthermore, even in a place like Kellogg, the experiments are so incredibly sophisticated and difficult compared to what was my experience, even in the sixties. They just have to have computers to take in this enormous amount of data and to help them analyze it.

I don't oppose the use of computers to simulate experiments—although I'm skeptical about it, in that I don't think it should substitute for hands-on laboratory work on university campuses. I was in Washington three weeks ago for the National Science Week that the National Science Foundation put on. I visited one of the high schools—Bannaker High School—and talked to the students about the excitement of science, and I gave the first Benjamin Franklin Lecture. Then I had a press conference where I raised these problems, and Lew Branscomb, who has just stepped down as chairman of the National Science Board, was there at the press conference along with Ed [Edward A.] Knapp—all the National Science Foundation big shots—and I brought up this problem. And to my great surprise, Lew Branscomb, chief scientist of IBM, supported me hook, line, and sinker. He said, "We must not let our students learn by computer simulation." He said, "I agree with you, Willy, we've got to have our colleges and universities equipped with laboratory equipment with which youngsters can do relevant experiments, not just do pulleys and wedges; they've got to be doing relevant experiments."

Caltech handles this problem by having undergraduates work in the operating labs. Kellogg has about ten, and in the summertime they may have twenty or thirty undergraduates around. However, the trend in physics, and for that matter even in chemistry nowadays, is to bigger and bigger central installations, where graduate students will do their course work in a couple of years and then disappear at Fermilab for three years to do their theses. I think it's going to change the whole character of university research, and I'm not sure it's for the better. And I think that some thought should be given to it, and I've been trying to get the National Science Board to get a study project going that will look into just what is the best thing to do.

GREENBERG: Even Caltech won't be able to stay away from that.

FOWLER: I'm not sure this is completely true, but in a way Kellogg is the only physics lab at Caltech which is turning out data in the laboratory. And that worries me. It worries me. There are pressures, because Kellogg's in nuclear physics, and there are pressures to start doing nuclear physics in big central installations rather than in campus laboratories. I have taken a very definite stand that we've got to keep physics live in the universities, otherwise the whole system that's been developed in the United States will change. Now physics research is done on university campuses, in industrial laboratories, and in the national labs. All three have made substantial contributions, and for us to give up one of them may turn out to be disastrous.

Of course, the example that one always brings up is the Soviet Union. The Soviet Union has practically no laboratories in their universities. The student goes to the University of Moscow for a couple of years to learn his graduate work, and then he goes to one of the big institutes to do his experimental work. And I've said, and I say it in public, experimental work in the Soviet Union is definitely third class. Now, when the Soviets put their mind to doing something, like building bombs or a space program, they can do it. But their contributions in experimental physics in the last decade have been, as I've said, third class. Theoretical, they're very good; tops in mathematics; very good in theoretical astrophysics. [Yakov] Zel'dovich is one of the really great ones. Kip Thorne will tell you that. [Vitaly L.] Ginzburg, Novikov—in theory they're fine. But, boy, not only is their experimental work third class, but they just do wrong things. They get results that are wrong, and we've had more trouble with things coming out of the Soviet Union that are just crazy. Someone over here has to spend a lot of money to do the experiments all over again. Well, the Soviet Union isn't a very good example, because they also don't have industrial [laughter] labs.

GREENBERG: Well, we've spent about three-quarters of our interviews talking about physics at Caltech before the war, and I'm sure this is doing a great injustice to physics at Caltech since the war. What have we missed? What about Caltech, the institute?

FOWLER: Well, I have taken it that our discussions have been mainly about Kellogg, which has continued, in my book, to be enormously successful in what it did. It has been well funded by the National Science Foundation. When we decided that we needed a new low-energy accelerator, the NSF provided the accelerator at a cost of roughly a million dollars, and the institute built the new laboratory in which it's installed at roughly a cost of a million dollars. So the support of the work in Kellogg has really been, I would say, just tremendous. It's not only been that, but we've had our fair share of funds from the institute for special pieces of equipment that we couldn't get out of our National Science Foundation budget, so we have really been supported magnificently, I would say.

And I've made no bones about it—that the awarding of the Nobel Prize to me was essentially an award to the Kellogg Lab. It was made very clear to me in Stockholm that the fact that they were able to cite experimental as well as theoretical work on the nuclear reactions which produce the chemical elements in the universe—the fact that they were able to cite the experimental work was the reason that I was chosen among a great number of other possible candidates. Once they had decided on Chandrasekhar, purely theoretical, they had to place some emphasis on experimental work in astrophysics. The people I talked to wouldn't be very explicit, because they can't be, but there was no doubt that the citation says "for experimental and theoretical results," and that's what turned the vote, I'm sure, in my favor. When you mention experimental results, that means that it was the laboratory effort, starting with Charlie Lauritsen and Tommy and continuing with Kavanagh and Barnes and Whaling and—until he found other things to do—Tombrello.

The other thing that I have been very fortunate in is the fact that the teaching load at Caltech is so low. John, in my whole career I taught one class—met with a class three hours a week—and I had the rest of the time to do experimental work. I gave all my classes at eight o'clock in the morning until the very last few years—at eight o'clock in the morning, because I found that if I had a class at eleven and got into the lab in the morning and got the accelerators started, I had to turn them off or let some dumb graduate student try to run the thing; it just wasted a day. So I gave all my classes at eight o'clock in the morning so that by nine o'clock I could get into the lab and get to work. I was a very poor citizen at Caltech, in the sense that I allowed them to appoint me to faculty committees from time to time but I just never went. I just decided that any time I had outside of my work I would spend in Washington, trying to do what I could—essentially lobbying. No question about it. So I served on all kinds of committees in the early days: ONR, then NSF, and then the National Science Board and American Physical Society—I became president of that.

GREENBERG: Do you feel you made a dent?

FOWLER: I feel I've made a dent. I feel I made a dent. It's frustrating. My experience on the

National Science Board was extremely frustrating in that—is this thing still on?

GREENBERG: Yes.

FOWLER: It was extremely frustrating because—of the twenty-six members of the National Science Board when I was on it, only five of us were active scientists. [Recorder off; turned back on] In a way, my service on the National Science Board was very frustrating to me, because here were all these deans and chancellors and college presidents who were mainly concerned about what I considered to be vague national policy. I've never been much of one for that sort of thing. What I was primarily interested in was, What are we going to do about instrumentation in American universities and in American colleges? I've always—even before this recent business, I've always gone around giving lectures at small colleges, and when I do I ask to see their laboratories, and I find small colleges, good colleges, giving bachelor of science degrees in physics and they don't have a physics laboratory! It's just, to me, just insane. Well, that is being turned around.

When I was on the National Science Board, I got the board to authorize a study by the National Academy of Sciences about university instrumentation, and the study was made and the board did nothing about it! And then, a few years ago, I got an academy study, independent of the science foundation, to look into scientific instrumentation. We put out a booklet called "Instrumentation Revitalized" and pointed out all the problems that I testified to before Congress, and actually things have turned around. There are funds being dedicated to instrumentation in colleges and universities. I think things are going in the right direction now, but, boy, it just takes constant, constant pressure to do this!

And then there is this other pressure to concentrate everything in big central facilities. What I'm recommending now is that before it just keeps going the way it's been going over the years, a study be made of the situation to see what is the best thing to do. I think people are beginning to think that maybe that's a good idea. But, boy, the wheels in Washington grind very slowly!

There have been, outside of Kellogg, just incredible developments in physics at Caltech. I would tend to single out the work that Ron Drever has done on the detection of gravitational radiation. I think that laboratory that he's built is just one of the most beautiful things in the world. And as you know, it's a prototype of a much larger detector. But, gee, that's an incredibly exciting problem. And there again, you have hands-on research being done on the campus. Eventually it will have to wind up in much the same way that astronomy has done—that you've got to put your observatory out somewhere where you're free of this or that type of interference. But that's an entirely different situation than sharing time at some big central facility with somebody else. So I'm very much in favor of what Drever is doing. I just know that Ron Drever sooner or later will discover gravitational radiation. I'm just willing to bet good money. Maybe not with the 40-meter arms that he's got on his laser apparatus now, but when he gets a kilometer arm, kilometer arms out on the desert, he'll find something. You just watch.

GREENBERG: And when is that due to—

FOWLER: Oh, it'll come within the next four or five years, I should think. So Drever's work is, to me, just *exactly* the sort of thing that should be done on a university campus. And the funding is pretty large. I think it's even more than we're getting in Kellogg now, but I don't know exactly. Maybe now that the installation is built, the operating funding is reasonable, but it took some three million dollars or so, I think, to build that detector.

I must emphasize that there's no question of why Caltech has won so many Nobel Prizes. Caltech is a place where you can come and work and do your own thing, and without essentially any interruptions at all—I mean, look at Roger Sperry. He was able to work in much the same way I was, without getting involved—I don't think Roger *ever* served on a faculty committee. Of course, people could take the choice. It's fine that there are some people who serve on faculty committees and do this and that, and it's important. But you *can* work at Caltech with a minimum teaching load, a minimum bureaucratic load, and essentially spend your full time on your own work. And, boy, that just makes an enormous difference! God, I've been around to places where guys teach three hours every day! I just don't understand how they could even do that, and they certainly can't do any research in addition, you see. And, boy, Caltech has been very generous in supplementing the funds we get from the federal government. Of course, it takes its overhead, but I think that's fair enough. So I am sure that if I had been in any other place, I could never have accomplished what I did. It just wouldn't have been possible. It just would not have been possible. So in my book, the atmosphere here—there are problems, I know, in some areas, but in the main, I know of no other place in the world where a person can devote himself to his own career in the sense that one can do at Caltech.

CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

SUPPLEMENTAL INTERVIEW WITH WILLIAM A. FOWLER

BY CAROL BUGÉ

PASADENA, CALIFORNIA

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by Carol Bugé

Pasadena, California October 3, 1986

[EDITOR'S NOTE: The following brief interview concerns the Kellogg Radiation Laboratory's work on rocket development for the Naval Bureau of Ordnance during World War II. This work began under Charles C. Lauritsen in September 1941. It continued in parallel with (but independent of) the JATO (jet-assisted takeoff) rocket project for the Army Air Corps, conducted by Caltech's Guggenheim Aeronautical Laboratory, and later by the newly established Jet Propulsion Laboratory, under Theodore von Kármán and Frank Malina. Fowler goes on to discuss Kellogg's interaction with Los Alamos and the Manhattan Project at the end of the war. Bugé begins by asking Fowler about the degree of opposition on campus to Caltech's role in the rocket program.]

Begin Tape 1, Side 1

FOWLER: Well, I don't really know, and I don't think there was any overt criticism. There may have been faculty members, not involved in either the rocket ordnance work or the JPL work, who were a little concerned. What they didn't realize was that even before the war ended, the Lauritsens and I definitely decided that Caltech was not to continue in the production of rocket ordnance, and that at the earliest possible moment, even before the war ended, we had to transfer all the activities to what was then called the Naval Ordnance Test Station, now the Naval Weapons Center, at China Lake. So I spent a good part of my time in '44 and '45 establishing the Naval Ordnance Test Station, with the help of a great number of other people. Charlie Lauritsen was very thoroughly involved at the higher levels in Washington. But I even served a period as director of research at the Naval Ordnance Test Station, until they were able to get someone to come on permanently. I don't know whether he came immediately, but it wasn't very long until Bill [William B.] McLean took over. We were very conscious that it was not the right thing for Caltech to do, to stay in rocket ordnance. In fact, my own feeling was that Caltech should not have stayed in the production of ordnance for the army, as JPL did. On the other hand, when the space program started, and JPL essentially transferred all of the ordnance work to Huntsville, or wherever it was, and concentrated only on launchers for the space program, then, I very enthusiastically—and I think most of the faculty very enthusiastically—supported that.

The greatest difficulty was that our people at the Naval Ordnance Test Station—and I forget how many thousands we had—wanted to stay under Caltech. So I had just a dreadful time getting our Caltech people at the Naval Ordnance Test Station to go on to civil service, which they had to do if they were going to work for the navy. But I just had to be very clear and positive, every time someone asked me, that there was zero probability that Caltech would continue to operate the Naval Ordnance Test Station or the production of rocket ordnance. You see, Trevor Gardner, who later on became assistant secretary of the air force for research and development, was the one we got to take over all this production work. And there was absolutely no other alternative. You wanted to see what you had worked so hard on in the research and testing areas used in the war. But we just had to produce them; there was no way that the navy could do it. And it was the same way with JPL. There was just no way that army ordnance could take over the production of the big ordnance rockets that JPL was making. You see, all of our work was on relatively small stuff. The biggest rocket we made, which we called Tiny Tim, had a diameter of eleven inches and was about six feet long; whereas everything that JPL made, once they got out of the jet-assisted takeoff business, was much larger than that.

BUGÉ: They were producing ordnance rockets during the war?

FOWLER: Well, there I have to be a little careful, because I don't know enough of the details. I just don't know. You'd have to find out how many they made here and how many were made in Alabama. It may have been that all the production was done in Alabama. But that would be neither here nor there, because I'm sure there had to be someone from JPL there—because the services then, and I would say now, are not very effective at the development, the testing, and the production of new weapons. It has been shown time and time again. It's one of the things that's characteristic of our military—that they are, in general, you can almost say incompetent, and that's because they have no training, except for a very few, in science and technology. And the same thing would happen all over again if we got into another war. Places like Caltech and MIT would have to fall to, and do everything, like MIT did with the radar business. And we did small rockets, JPL did large rockets, and the University of California essentially operated Los Alamos, and it made the atomic bombs. I have no recollection of overt criticism in this regard.

BUGÉ: With hindsight, would you speculate on it?

FOWLER: I can only say that there clearly were some members of the faculty who were afraid that we were going to continue in rocket production. But if they looked into it, they would have found that we were doing everything we could to transfer the work to civil servants at the Naval Ordnance Test Station.

Of course, it also got very complicated, because once it was clear that the atomic bombs were going to work after the Trinity test, Robert Oppenheimer, the director of Los Alamos, asked us to start producing vast quantities of nonnuclear components for the atomic bombs. You see, once they knew that "Fat Man" was going to work, they needed a great number of tests to establish the ballistics of the weapon, because it was a very ungainly thing. It was essentially round, with a great big tail to stabilize it on the back end. No one could calculate the ballistics. So we made hundreds of dummies—we called them "pumpkins"—and shipped them up to Wendover, Utah, where the air force dropped enough of them at their big range in Wendover until they could establish something about, given the point of release and the velocity of the airplane and all that kind of stuff, where the darn things would hit. We had a substantial fraction of all the machine shops in LA County tied up on the rocket work. So when Robert saw that he was going to need a lot of ballistics tests, he asked Charlie Lauritsen, and the word eventually got down to me, to make nonnuclear components in the Los Angeles area. And so that actually was another motivation for transferring the rocket work to the navy at the Naval Ordnance Test Station as fast as possible.

BUGÉ: Was there ever a conflict of interest between what NDRC [National Defense Research Committee] wanted from you in the way of rockets, and what Los Alamos needed?

FOWLER: Well, the NDRC wanted nothing from us in the way of rockets. We told them, very early on, what we were going to do. In fact, Charlie Lauritsen pretty much had to wage a big effort in Washington to get the OSRD [Office of Scientific Research and Development] and the

NDRC to do such a plebeian thing as make barrage rockets for navy landing craft. The first thing we made were what we called target rockets. They were just rockets that had great big fins on, and we made them and shipped them down to San Clemente, where the marines would fire a rocket and then the marine gunners would try to hit it. Because they just couldn't make enough dummy planes to give the marines sufficient practice tracking a fast-moving object. We did all kinds of what were very plebeian [things] compared to the radar work and the atomic bomb work. I'm sure Charlie had to work pretty hard to convince Van [Vannevar] Bush and Fred Hovde that what we wanted to do, although it didn't seem very exciting scientifically, was really important for the troops and the sailors. But they had nothing to do with our going into production. And the Bureau of Ordnance, if I remember correctly, paid the suppliers and the fabricators directly. I don't think any of that money—it may have gone rapidly through the Caltech business office, or the office that we had set up to handle such things. But that was all pro forma. We knew what we were going to do. We talked to people in the Bureau of Ordnance, not people in the NDRC. You see, once we built NOTS, then the navy sent a commander, Evy [Sherman E.] Burroughs; an experimental officer, Chick [John T.] Hayward; and a whole bunch of officers. So we had someone to talk to. All of these men had been in combat. Chick Hayward was injured in aerial combat, so he had to be grounded, and he was made experimental officer at the Naval Ordnance Test Station. And he had a good feeling for what the combat problems were, and so did Evy Burroughs and the other officers there. So by discussing with them, we found out what we really needed, although we had pretty good ideas of our own. No one in Washington had the slightest idea of what was needed. I went out to the Pacific for three months—I guess it was in '44—to see how things were really going, and I learned a great deal about problems that the marines and the sailors were having using our equipment. And then we had direct connections to a desk in the Bureau of Ordnance. We informed Fred Hovde—who was head of the section, or the division, in the NDRC of which we were part—of what we were doing. It was only possible because Charlie Lauritsen was such a dynamic person and had so many ideas that people in the services recognized were good ideas; and then he had a good team. A person who contributed an enormous amount was Earnest Watson, who served as the local administrator. And Earnest just protected the rest of us from all the Washington paperwork.

BUGÉ: That's what he said in his oral history, by the way.

FOWLER: Yes. And he did. We didn't have to justify what we were doing; we just wrote a short memo to Earnest, and he handled all the paperwork.

BUGÉ: I want to show you something, because it's a bit ironic, in light of what you say about JPL, but this is a quote taken from an oral history with Frank Malina; evidently the JPL group was critical of your group. "I felt, and Kármán did too, very strongly that Caltech was not an appropriate institution for production. We also were watching Willie Fowler and Lauritsen and Sage and some of these others who were doing production of armament rockets. And we thought that was a great mistake, because we just didn't think that was the kind of thing that Caltech should do. I mean, it should stick to basic research and instruction and so forth." My next question was going to be about what you thought of JPL and what the relationship was between your group and Kármán's group during this period.

FOWLER: Well, I should start at the beginning. When we decided to set up the rocket ordnance project here at Caltech, it must have been in August of '41; we'd been in Washington for almost a year, working on proximity fuses. And one of the things we did was put proximity fuses on the rockets that the navy was then using. It was heartbreaking, because we worked very hard to put a proximity fuse together, take it down to Dahlgren or Indian Head [Naval Surface Warfare Centers] and have the navy fire it, and half the time the navy rockets would blow up on the launcher, and there'd go our hand-built proximity fuse.

BUGÉ: How long did it take to make a fuse?

FOWLER: A couple of weeks, and for a test we needed twenty. And then Charlie went to England and saw the success the English were having with the use of rockets. They called them the zed [Z] batteries, which just fired rockets at V-1s and V-2s. Even though rockets aren't intrinsically accurate, they'd fire a battery of them, roughly fifty of them, in the hope that one of them might hit the damn thing. Charlie was convinced that the proximity fuse stuff was pretty well all worked out. He decided what this country needed was some good rockets, and especially the navy.

There was another group in the NDRC working at the Allegheny Ballistics Lab, then part of Johns Hopkins. They made rockets for the army. They made the bazooka and other things. But we were quite independent of them, and there wasn't much cooperation.

So we came back here in August of '41. And the first thing I did was go see Frank Malina, whom I had known, and said, "Frank, I don't know the first thing about rockets. Where do I learn how a rocket works?" So he gave me some references, as I remember. They had nothing to do with ordnance rockets. But I only saw him once. And I saw von Kármán around, but I don't ever remember talking to von Kármán. I did, at later stages, talk to Clark Millikan, because Clark and I were quite close friends; and Clark eventually became the director of Guggenheim [Aeronautical Laboratory]. Let's see, Malina left right after the war [1946], as I remember. Anyhow, during the war there was very little interplay between what we were doing and what they were doing, partly because Charlie Lauritsen was very independent; he knew exactly what we were to do. Tommy Lauritsen, his son, headed the group that did the design and development of our ordnance rockets. I was mainly concerned with the testing, which we did on our own at the range we had built at Goldstone, north of Barstow. And when it became too small for firing rockets from aircraft, that's when we had to build China Lake for the navy. So there was very little cooperation between the two groups. They were doing jet-assisted takeoff devices; we were building rocket weapons.

BUGÉ: Was there a lack of cooperation, or just no interaction?

FOWLER: There certainly was no interaction at the level of the two Lauritsens and myself. There may have been—because we don't know everything that went on—there may have been between some of our theorists, like Leverett Davis, for example, and some of the people in Guggenheim who were working on theory. But the design and testing was carried on completely independent of any parallel work going on at what became JPL. But of course, there really wasn't all that much scientific and technological overlap. They were doing a different thing than we were. Just the fact that their rockets were so much bigger made the whole design an entirely different problem. The motivations were different.

BUGÉ: But the testing of propellants that came in the very early stages? It would seem logical that there would have been more cooperation than there was, unless there were reasons not to be, which is of course the interesting part.

FOWLER: Well, you see, when we started out, they were still working on propellants for jet-assisted takeoff. And that was not at all satisfactory for what we wanted to do. Now, eventually I suppose they came to this, too. But we had to decide on what propellant we were going to use, and Bruce Sage had to set up the extrusion presses to extrude the cylindrical propellants, which would go inside the rockets, which we essentially made here or in the shops in this area. So Sage had to set up extrusion presses in Eaton Canyon. And then he built an enormous part of China Lake, which was nothing but extrusion presses. And again, Sage was a very independent character and I doubt if he learned very much from what the Guggenheim people were doing. But you'd have to look to see what he might have had to say about that. But I think in all fairness the main point was that the applications of what we were building were entirely different than what JPL was building. The scale of the rockets—their rockets were ten times as big as ours, you see. The purposes were different, and that made the design problems quite different. So I doubt, even if we'd wanted to, if either side could have learned very much from the other. And there was also a problem that we did have security, and we were working for the navy, and they were working for the army; and as everyone knows, the navy doesn't tell the army what it's doing and the army doesn't tell the navy what it's doing. So I never had a pass to get into Guggenheim during the war, and I don't remember Frank Malina ever asking for one to get into Kellogg.

BUGÉ: There's been some speculation in recent years about the politics of some of the people, including Frank Malina, who were associated with JPL—whether they were Communists, whether they were just Communist sympathizers. There were articles written in the fifties about Malina, when he was long gone to Paris. But Sidney Weinbaum, who was around JPL in the earlier years, actually went to jail. Did you know anything about that, or did you have any suspicions at the time?

FOWLER: Well, I knew that Frank Malina was very liberal, but I didn't know him well enough to know to what extent his political views went. My strong feeling was that that was nobody's business. It was very clear that he was doing an excellent job, and that just because his politics didn't agree with someone else's politics was no reason why he shouldn't serve in the war effort. I felt the same way about Robert Oppenheimer. There were a lot of people who felt that Robert's politics were too liberal. But all the rest of us strongly supported him, because he was the man who could do the job. And in wartime, if you only use conservatives, then you're clearly going to lose the war, as our military showed in Vietnam, where they didn't ask for any help from the scientific community, except on levels like the Jason[i] level, which is essentially useless in my book.

BUGÉ: I think you've answered a lot of the questions I had [about the two groups]. Because apparently there wasn't sharing of information particularly, or socializing, or much of a relationship to speak of, with the possible exception of Clark Millikan. I know there are some letters that Clark Millikan exchanged with Charlie Lauritsen early on, before the project got started, volunteering information about a testing range that would be available that would [encourage] the government [in] the possibility of locating the project on the West Coast.

FOWLER: You see, Clark and Charlie were very close friends, and I was a close friend. So Clark was the person that Charlie certainly looked to, and that explains why there could have been an exchange of correspondence between them.

BUGÉ: Though Clark's role was mostly administrative. I don't think he was as involved as Frank Malina in the production or even the research end.

FOWLER: That's true. But just that very fact, however, would have meant that Charlie was willing to talk to him, even though he might not have wanted to discuss things with Malina. Malina had been in the rocket business, I think, from the days of Goddard, and there were some people who didn't think he was very good. And it could well have been that Charlie shared that view. My own feeling was that the few times I talked to him before the war—and then this once I went to see him—was that Frank really knew what he was doing. But there certainly were people who were very skeptical of his ability. I did not share that. And just how Charlie

Lauritsen felt, I don't know. But Charlie never encouraged us to have much to do with Malina's efforts. And the major contacts: I had social contacts with Clark during the war, and Charlie and Clark may have had more top-level contacts, because Clark was an administrator and in a sense, Charlie was administering things. He left the actual details up to Tommy Lauritsen and up to me.

BUGÉ: So when you socialized during that period, did you socialize mainly within the group that you worked with?

FOWLER: Yes.

BUGÉ: That's the first time I've heard it stated that there were people who didn't think very much of Malina's ability. Though it often seems just under the surface.

FOWLER: Well, let me just make clear that I did not share that. I'm only saying that I got this feeling, not from the Lauritsens but from some of the people in Guggenheim after the war, if my recollection is correct. You'd have to ask Hans Liepmann and some of them what they thought about Malina.

BUGÉ: There is a 1945 letter from you in Robert Oppenheimer's papers which indicates that there was some friction, maybe not very important, between the hierarchy at Los Alamos and those of you who were working from Caltech—just about chain of command and communication, and how much freedom, how much autonomy, you had. I wonder if you want to shed any light on that?

FOWLER: [Reads letter] Yes, well, we soon found out that Los Alamos, or "Y," was operated in quite a different way than we worked with the navy here in Kellogg. There was a navy office here, which was located over in Arden House, under Admiral Holmes. Holmes was a wonderful old gentleman, a retired admiral, who had come back for duty during the war. And he never interfered in any way with what we did. In fact, he was very helpful, and when problems would arise that Watson felt he couldn't handle, he'd go to Holmes to go straight at his pals in

Washington and settle it. Well, when I and Tommy got to Los Alamos, we found that the military there essentially reported directly to General [Leslie] Groves. And they not only—well, they bugged Robert all the time. When they started bugging me, I just wanted to make it clear to Robert that we weren't going to take any commands or orders from people whom we considered nitwits. That's the way we felt, and I know he felt the same way, only he couldn't say it then. And I wanted him to know that if any problems arose with this Colonel Lockridge, who somehow or other was assigned through Groves's chain of command to be our contact with the military at Los Alamos—I can't remember who Burton was; I do remember this character Colonel Lockridge. But there was a little bit of—for example, when we started producing these pumpkins in quantity, he wanted to have oversight over placing the orders and inspecting the things. On the other hand, we were getting frantic calls from the people at Wendover, Utah, in the air corps, saying, "Get these things up here so that we can start determining the ballistics, because we're going to be dropping them on Japs in a couple of months; and we want to at least hit Japan."

As I recall, nothing ever came of it. I would guess that Robert went to Lockridge and said, "Now look, these guys are doing something that's really essential at this moment. So leave them alone." And I don't remember that we had any trouble. In fact, somewhere in all the stuff that I gave Judy [Judith R. Goodstein, Caltech archivist], there's a picture of me at Wendover. I went up to Wendover to make sure that the pumpkins were getting there and to make sure that the air corps was studying the ballistics in a sensible way. In fact, they had some good people up there. They were doing an excellent job. One of the reasons I went there was that they needed so many of them that it occurred to someone—maybe me, I don't know—that after they'd dropped one, couldn't we haul it back here and repair it? Well, I got up there and I saw that when they dropped them from high altitude out on the desert, they'd go in so deep that they'd just rip the whole tail structure off, and the whole business of recovering them and shipping them back here would have cost more than making new ones. So we just kept on. See, they were dummies; they had the same exterior shape as the bomb dropped on Nagasaki. We filled them with cement so that they had the right weight, and we put in the cement in such a way that they had the same center of gravity as the real thing. So from the outside, as far as the atmosphere of the earth is concerned through which these things were being dropped, the atmosphere just didn't know the difference. So we provided them with these dummies that ballistically were identical with the real thing. And the major thing was that they were fairly expensive objects, and they could only drop them once. So I went up there to make sure that their claim that they could only drop them once was justified. And I was convinced that it was. Sometimes, in the drops, the darn things would actually kind of bounce on the surface and really tear up the tail fins and break the walls and dislodge the ballast inside.

BUGÉ: Are there some still lying around in the ground up there?

FOWLER: I would think not, because after the war they wanted to get rid of any evidence of such things. And so I'm sure that after the war they dug them all up and buried them, or something. They probably, my guess is, took them out to sea and dumped them overboard.

BUGÉ: Did you do most of your work here, then, for the Los Alamos project, or did you spend a fair amount of time in Los Alamos?

FOWLER: Oh, I would say I spent about a third to a half of the last year of the war at Los Alamos.

BUGÉ: And the Lauritsens also?

FOWLER: No. Well, it's hard to know about Charlie. I don't really know. He was gone so much, either to Washington or to Los Alamos, that it would be hard for me to say what fraction of his time he spent at Los Alamos. But it was always on a level that had to do with getting things done. That letter I wrote to Robert—I would guess that I went one day to Charlie and griped to him about this fool who was bugging me, this Colonel Lockridge, and Charlie said, "Well, write Robert a letter, I'm busy." [Laughter]

BUGÉ: So, when you were actually at Los Alamos, did you run into more problems like this more of the time?

FOWLER: Oh, no! You see, most of the people there—Ed [Edwin M.] McMillan, Hans Staub, Robert, Frank—were all friends of mine; I had no problems.

BUGÉ: Who's Frank?

FOWLER: Frank Oppenheimer. He was there part of the time, anyhow. Bob Serber was there. Bob Christy was there. If I'd stop to think about it, I could name others. Bob [Robert R.] Wilson was one.

BUGÉ : A lot of those people came back to Caltech after the war, didn't they? Or several of them?

FOWLER: Hans Staub came back, and Bob Christy came. Of course, Bob Bacher was there, and I had known him from the time when I was a graduate student. Of course Bacher came. So there were a great number of physicists at Los Alamos whom I had known ever since I'd been a graduate student. Luis Alvarez. Bob Brode. So I had no problem at all with the scientific staff, because I knew them all.

BUGÉ: And the military people didn't interfere at that level?

FOWLER: Well, the military only got involved when they saw that we were producing things as well as doing research.

BUGÉ: Ah, research. So scientists are only supposed to think.

FOWLER: That's right. So that when they found out we were producing all these things and shipping them directly to the air force, this Colonel Lockridge got up on his hind legs and decided he wanted to have something to do with it, and there just wasn't any time to consult with him every time we responded to a request to do this or do that. But on the scientific end, getting the design, getting all the details about this supersecret thing so we could make dummies that were identical to it; I had no problems at all.

Begin Tape 1, Side 2

FOWLER: Actually, I had a Q clearance, which I got in Washington right at the start of the war. The Q clearance made it possible for me to get in and out of Los Alamos without any trouble at all. But the cooperation with the people at Los Alamos was just wonderful. Again, we were doing kind of mundane things. We weren't working on the nuclear secrets of the atomic bombs; we were making what we called pumpkins—dummy bombs for testing the ballistics. Well, some people might say that's not very scientific, and we just didn't give a damn. We weren't doing science during the war, we were trying to help win the war; because we were all convinced, rightly or wrongly, that Hitler and the Nazis were really SOBs, and that everything had to be done to protect the future world from such people. We were really convinced. And when it came to the question of whether or not the damn bomb should be dropped, there were a few people at Chicago who thought there should be a test first, but frankly, ninety percent of us were gung-ho to see what we had built used.

BUGÉ: Of course you're not talking about Nazis anymore at this time.

FOWLER: Well, the Japanese under that awful general [Tojo] they had were just as bad, if not worse. When I went out in the Pacific in '44, I went all over both commands, both under Halsey and under MacArthur and it was very clear to me that if we had to try to defeat the Japanese by conventional weapons, that we were going to lose at least a million men. So there was no doubt in my mind when I came back that the only solution was to use the bombs. I still think Truman made the right decision. Where he made the wrong decision was to go ahead with the hydrogen bombs, because I don't think they were needed, and we might have been saved a lot of problems with the Soviet Union if the hydrogen bombs hadn't been developed. But that is another story.

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[i] Proposed in 1958, established in 1960, and still in existence, "Jason" is the code name for an organization created to enable scientists to contribute in an ongoing way to problems of national security: to study basic research problems; to make conceptual contributions toward the solution of technical problems; to identify basic research problems not under study; and to indirectly advise the secretary of defense. The project name was proposed by project chairman Marvin Goldberger as a better choice than the computer-selected "Sunrise."—ed.