Charles A. Barnes, 1979

CHARLES A. BARNES
(b. 1921)

INTERVIEWED BY
HEIDI ASPATURIAN

July and August 1987

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Pasadena, California

Subject area
Physics

Abstract

Interview in six sessions, July and August 1987, with Charles A. Barnes, professor of physics, Caltech. He talks about his childhood and adolescence in Ontario, Canada; his early affinity for mathematics and science; undergraduate years at Canada's McMaster University; wartime work in the British-Canadian atomic energy project at Chalk River, Montreal; and postwar PhD studies in physics at the University of Cambridge, working with O. Frisch and D. Wilkinson. The discussion of his 40-year career in Caltech's Kellogg Radiation Laboratory deals with many aspects of the lab's history, personnel, and research contributions. Barnes talks about his nuclear physics collaborations with W. Fowler, T. Lauritsen, C. Lauritsen, and R. Christy. He gives a detailed account of his and Kellogg's accelerator-based investigations into the nature of the weak nuclear interaction—a key focus of postwar work in quantum field theory and the development of grand unified theories—and describes research collaborations in this area with theorists M. Gell-Mann and R. P. Feynman. He talks about

Kellogg's social and scientific culture, the development of its accelerators and the chronology of its research, its groundbreaking investigations into stellar evolution and stellar nucleosynthesis, and the awarding of the 1983 Nobel Prize in physics to Fowler for his work in nucleosynthesis. Barnes describes his work with J. Bahcall on the solar neutrino flux and discusses the research contributions of K. Thorne, J. DuMond, F. Boehm, M. Schmidt, and G. J. Wasserburg, among others. The interview also covers Kellogg's role in the physics and astrophysics community, both within and beyond Caltech, and the lab's relationship with Caltech administration, including presidents and division chairs; Barnes's work with students; and his views on current trends and future directions in physics and astrophysics.

Administrative information

Access

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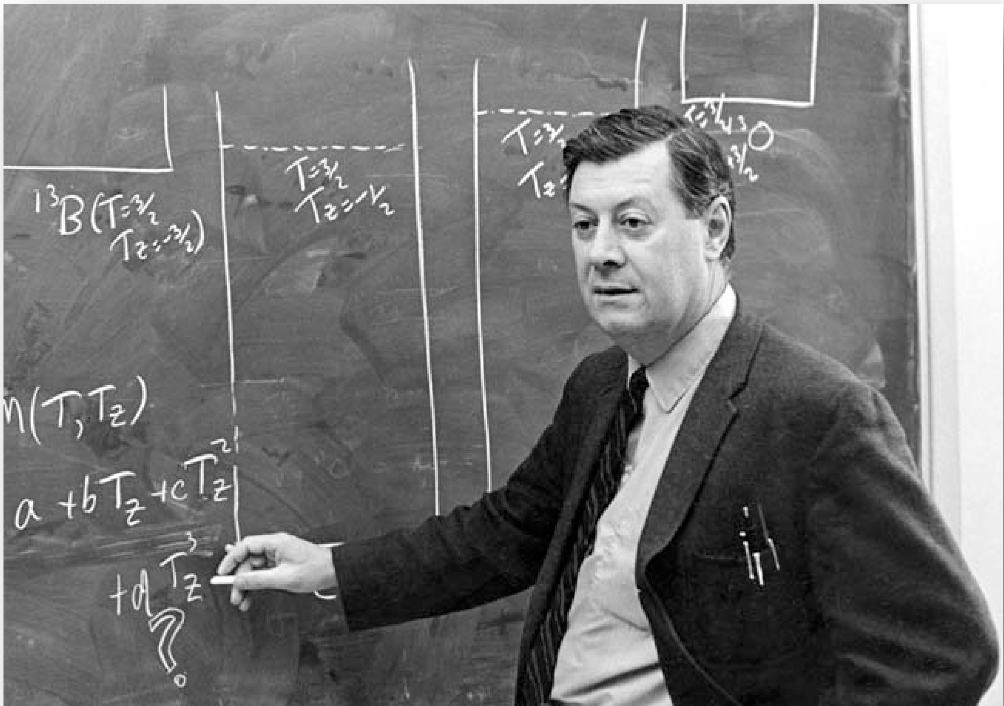

CHARLES BARNES IN 1969. Caltech Archives

WILLIAM FOWLER AND CHARLES BARNES IN 1982, INSPECTING KELLOGG LABORATORY'S THEN NEWEST TANDEM ACCELERATOR, THE PELETRON—DESIGNED BY BARNES ALONG WITH ENGINEERS FROM THE NATIONAL ELECTROSTATIC CORPORATION. Caltech Archives

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH CHARLES A. BARNES

BY HEIDI ASPATURIAN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Charles A. Barnes **by Heidi Aspaturian**
Pasadena, California

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ASPATURIAN: Let's begin with how you became interested in science and what your family background is.

BARNES: I was an eldest child. I don't know if that's significant or not, but I think in some way, eldest children do have a little bit of an advantage because they are the earliest. My father was also a minister, a clergyman—what you might call a northern or moderate Baptist—so he was at home perhaps more than most fathers would be. Anyway, I think he was a scientist manqué, because he had a life-long interest in biology, botany, agriculture, and things that grew out of those subject areas. He spent a lot of time on walks with my two brothers and me, showing us things and talking about how things develop. But he also was pretty quick with arithmetic. I can remember when I was very young he would challenge me to add a column of numbers. I guess I would be about six at the time, maybe six through eight. I think by the time I was eight, I could add a column of numbers faster than he could, but there was a challenge there, and so that became a kind of early skill. It got so I could do arithmetic early. And then we would do mental arithmetic. We would do simple calculations in our heads.

ASPATURIAN: This was just you and your dad? Or did it include the rest of the family?

BARNES: No, it was just my father and me, I think partly because I was the eldest, and probably partly because I just seemed to be more skilled at it. It was more of a game for him, but a challenge for me. I don't want to put down my brothers and my sister in this respect, but I think that there's an advantage possibly to eldest children; there is a time interval before other children compete for their parents' attention. So that gave me a kind of early start, or maybe a bias in the way I looked at things that went all the way through my schooling. The early start in math gave me a tremendous advantage in scientific subjects, and especially in physics because it is so strongly based on mathematics; even today I can still do things like calculate square roots and cube roots, which most people probably can't do anymore. You don't have to do it now, of course, because you have a calculator that will do it. But it sticks with me; I still can do a lot of these arithmetical things.

So there was a kind of early direction that perhaps was an influence. On one hand, there was my father's really genuine interest in some scientific subjects, which did not extend as far as chemistry or physics. I can assure you he was really not very good in physics. He really didn't understand how physics worked, but he was very good at biology and very interested in the subject, and a lot of things that were related to that, as I said earlier, like medicine, agriculture—things that had a connection to human, animal, or plant biology. So, as I went through elementary school and then high school—we didn't have intermediate school, just elementary and high school—I just tended to do better in scientific subjects and mathematics than in other subjects. Although the truth is, I did pretty well in everything and never found it very much of a strain. I had lots of fun going to school because I didn't have to work very hard.

ASPATURIAN: Where were you raised?

BARNES: Well, as you know, I was born in Canada. Because of my father's occupation, we moved around quite a bit. So I started school in northern Ontario, and the rest of my schooling was in different places in southern Ontario, which is a relatively mild version of Canada from the Canadian weather point of view.

ASPATURIAN: Were you near a major population center?

BARNES: Most of these were not such large places. I think all of the places I lived in until I went to university were small cities or towns that wouldn't be any bigger than 20,000 people. Whether that's an advantage or disadvantage, I don't know. I think in fact it may have been an advantage, purely because all of these places had very good schools. I do think that the Canadian school system at that time was actually particularly good, particularly in high school. Almost every high school teacher had a specialist certificate in some subject. You almost couldn't be a high school teacher unless you had an honors degree in history or mathematics or some other subject area. So I think they tended to get pretty capable teachers. I don't know whether that's still true or not. It wasn't true, for example, much later when I spent a few years in Vancouver, where I lived next door to a high school teacher who was really the physical education teacher, and who was teaching mathematics only because there was nobody else to teach it at his school. I didn't have that situation when I was growing up.

Anyway, those were all small places. In fact, I was born very close to Toronto, in a suburban area that has long since been swallowed by Toronto. People don't even recognize the name of the area where I was born anymore. So I just say Toronto.

ASPATURIAN: What was the junior high and high school education like in the scientific areas?

BARNES: Well, it was rather good, I think. As I said before, I think that I had the advantage of really good teachers. A few of them were a bit bizarre. I had a chemistry teacher who was a woman, which was not too usual in those days although no reason why not, who was really quite odd. If we were ever doing chemistry experiments that didn't work the way she planned them to, and occasionally something would blow up, she would say, "Hmm, I wonder why that happened." And that gave me the challenge of trying to figure out why it had happened because it was true that she was having some trouble figuring out the problem. But she worked very hard at teaching, and I think she taught all of us a very great deal. Physics in my high school was taught usually by the principal, who was typically an honors graduate in mathematics and physics. So he really understood the subject. He could challenge me beyond any level that I could match at that time. He could always exceed what I could do, so there was a constant challenge to try to perform. And there was no question about it, I'd get rewards when I was successful—the kind of feeling that it pleases the teacher, perhaps, or it pleases my parents, maybe even pleases me.

When it came time for thinking about universities, my parents made it very clear to me that if I wanted to go to the university, I would be able to do it, but that it was going to be financially a severe strain. And I understood that, because in the particular occupation my father was in, he was never paid very well. Since I was one of four children, I realized that it would be up to me to try to get as much of my own financing as I could. And that meant having a shot at a good scholarship.

ASPATURIAN: Do you recall any specific math and science teachers who encouraged you to go on?

BARNES: I actually got encouragement from many of my teachers, and particularly from the physics teacher and math teachers at various levels, and this chemistry teacher whom I mentioned, because teachers, I believe, tend to like students who are performing well. I can remember even early in high school—what you would call junior high school—having an even more bizarre physics teacher who should have been retired earlier. But he was rather nice, and he taught me quite a lot. He perhaps made a great contribution to me because he asked me physics questions about some things I'd never seen or thought of before. And I answered him quickly, because I knew he wanted an answer. And instead of encouraging me one day, he turned and heaped scorn on me and he said, "You're just not thinking. That's absolutely ridiculous." Then he proceeded to show everybody how what I said was actually ridiculous. That was also educational, too, because I realized that in some way I had failed to meet his expectations, and I decided I wasn't going to say something stupid the next time he asked me a question. The next time he asked me a question, I thought about it before answering, and I never had to struggle that way again; but it was also an important part of my education. But there was the opportunity for an exchange of lots of conversation and ideas with most of my teachers. I guess to be honest, I had less individual contact with teachers in history, literature, English composition, and languages—though I was pretty good in Latin; I still had Latin all the way through high school. I couldn't complain that I had anything but very helpful teachers, but particularly I guess the level of reinforcement was related to my performance. And the things I was best at were the sciences and mathematics.

ASPATURIAN: Was there a lot of outside reading that had an influence as well?

BARNES: I think it was mainly the educational process, but you did mention something that I do remember, that I read a great deal in my life. From the time I was in elementary school, when we lived just two doors away from the public library, I used to spend most of my time there when I wasn't playing ball or something else. A great deal of what I read would be considered very non-educational. You may never have heard of Tom Swift books, but these Tom Swift adventures were all science, or engineering, related. They would be considered as pretty poor reading by some narrow-minded people; but I could read easily and fast, and I could read a Tom Swift in an afternoon, two or three hours, and would be finished with it. I'd go back to the librarian and give it back to her and say, "I need something else to take home." I'm sure that made a difference, the fact that I read a great deal. But it was more of a thing that I learned to do on my own. Unfortunately, I don't do it as much these days as I would like. But certainly all the time I was growing up and in college, I read and read and read. Even if it was really trashy, it didn't bother me. And I always learned something from it.

Of course, I should say my mother was trained as a school teacher. She wasn't teaching during the time that I was around, but she had, of course, a good education. She taught for several years in schools in central Michigan. So I had a good background from both parents. Learning was something that was not especially encouraged, but it was kind of natural in the family. In a way, life is never totally fair to everybody. We can say that everybody should have the same opportunity, but there's no question I had a better opportunity to learn simply because I learned better from other people around me, and there was a reinforcement for this. I can even see it within my own family. My brother, who wasn't as quick to learn, definitely had a much harder time. He completed his bachelor's degree and became a high school teacher with a specialist certification in history, but he never had the same level of enjoyment from going to school that I did. In a way, I sometimes wish that I had worked a little harder when I was in high school when learning things came so easily. But one never gets to run through life by a whole bunch of alternative paths and then compare them. You only go through one path and what you are is from what you did.

Getting back to finishing high school: I guess I realized that I was going to have to win a scholarship; and I thought that the best possibility was to go for what was called a regional scholarship offered by McMaster University, where I happened to go as an undergraduate. McMaster had carved the part of southern Ontario within a few hundred miles in all directions, into six districts, I guess, with six regional scholarships. So I decided to have a shot at one of them, and they were based on six subjects—you had to choose six subjects—in the last year of high school, which meant that I pretty much had to choose to cover everything. I was competing not just in physics, mathematics and chemistry, but also English, history, Latin. I don't remember for sure what my sixth subject was, but it was virtually everything that I was taking. And I was lucky and got one. That paid for almost all of my tuition and a good part of my board during the four academic years, and then I worked every summer. As you've seen from my vita, most of the summer jobs I had were things that I fell into partly because of my subject specialty, and partly because it was wartime and there was a shortage of people. That was a difficult decision for me because I turned eighteen in December of 1939. I had just started college at the age of seventeen in September, 1939, which was the year that World War II started. And Canada was in World War II almost from the first day; the Canadians declared war the same day as the British did, or if not, then maybe it was the next day.

ASPATURIAN: Were you called up for service?

BARNES: Well, there was no immediate draft because they couldn't accommodate people suddenly, but they took the army reserve units and sent them abroad as soon as they could. In fact, the Canadians didn't really have much in the way of large armed forces abroad until about 1941. But the Canadian government did something that I think was quite farsighted: They elected right from the beginning to defer college students, as long as they were doing well in college, and I don't think they drafted any college students until they graduated—at least, until late in the war. So we started military training in college; we had military training a few hours per week, and went to military camp in the summertime. I think it was certainly a generous policy on the part of the Canadian government to take the point of view that they weren't going to draft college students as long as they were doing well—at least sufficiently well to be passing. You didn't have to be exceptional, but you had to stay in good standing. I guess it was natural for me to enroll in mathematics and physics. They had a joint honors program in math and physics, which seemed to be what I really wanted to do at that time. Certainly my tastes later turned more toward the physics and less toward the math, but I certainly started with a completely open mind between the two subjects.

McMaster was not a large university at that time; it was, in fact, really quite small, I think. The undergraduate body then was about the same size as Caltech is now. It's much larger now. It must be many thousands of students now. I think the undergraduate enrollment when I entered was about 900 students. It was coed, and it covered almost all subjects; it wasn't science- and engineering-oriented like Caltech. Classes were small. There were, I think, eight, maybe nine, people in honors mathematics and physics. This meant that we were a kind of club that went through the four years together, mainly in small classes. We had a real opportunity to interact with the faculty. I guess the faculty were pretty overworked; because with a small student body, which meant a small faculty, trying to cover all subjects in arts and sciences meant the teaching load was relatively heavy.

ASPATURIAN: Some of the younger faculty must have chosen to enlist.

BARNES: There weren't too many who had enlisted yet, but there were a lot of them who had moved into war industries. Indeed, there were some who had enlisted, particularly in science-related military areas. But their biggest problem, I would say, was that they were so heavily overloaded in teaching, although I would say no worse than the first teaching job I had years later, which was at UBC [the University of British Columbia], where I carried what seemed to be an enormous teaching load. It was true that the faculty at McMaster, who could only be called truly dedicated under the circumstances, really put out their best, and they all taught four or five courses at once. Of course, this meant that their teaching wasn't always the best in the world, but there was every opportunity for us to do as well as we could and to still be challenged by the treatment.

ASPATURIAN: What was your course work in physics like? I was wondering particularly if quantum mechanics was taught.

BARNES: Not very much. There was a little bit of quantum mechanics in the senior course in modern physics, but no, quantum mechanics was not well taught and was at that time mainly considered to be a graduate subject in Canada universities; maybe also in the U.S. at that time. But the program started with first-year physics—a kind of omnibus course that a large fraction of the student body took, which covered everything in physics. Starting with the second year, the science classes were small classes, and they were much more specialized in electromagnetism, classical mechanics, optics, etc. We did a lot of things that aren't done so much here. We spent a lot of time studying optics and optical instruments. We hit that here at Caltech in an indirect way when teaching some other subjects. Certainly in physics, we didn't have the range of specialized courses we offer here. I guess optics in the general sense would be now taught here mainly in applied physics or laser physics. It's germane to laser programs and such things, so that's where it's taught. But we did have lots of other requirements. In fact, all the way through, we had substantial requirements in humanities, social sciences, languages. Unfortunately, the language thing was for me a kind of farce, because I had had a very good preparation in French in high school; so I passed all of the university exams in French without any hard work, which was really too bad. After I got tired of doing that, I took one course in German, and now I wish I'd taken a lot of German, because now I'm going to Germany for a year. [Laughter] I took a course called Scientific German, of which, remarkably enough, I still remember quite a bit. But I never really learned to speak the language.

Begin Tape 1, Side 2

BARNES: I would give the McMaster program rather good marks as a general education program, and I would give it fair marks as a program in math and physics. Although I had no difficulty later competing with students from other places, I think that some of the larger Canadian universities already had stronger programs, especially in mathematics. I think the physics was pretty good, but I think the math was compromised by the fact that the teaching load was too big.

ASPATURIAN: When you look back on your training in physics at that time and the summer jobs you took, what stands out in your mind?

BARNES: Well, there was a feeling—maybe this is not something that exactly stands out—but there's a kind of feeling I can still recall that the country was very much at war, and I felt a little bit concerned that I was not in the armed services. Meanwhile, my second brother went into the army, and my youngest brother went into the Air Force. In fact, he was overseas, flying bombers from Great Britain in the RCAF—Royal Canadian Air Force. He was an observer in the RCAF when he was nineteen, and only eighteen when he enlisted. Both of them survived intact, I'm glad to say. I guess it did have an effect on me, because everywhere one went, most of the young men were in training, one way or another, or had already gone overseas. We were taking something like six hours a week of military training all the way through the university, and two weeks full-time each summer. But we had the assurance of the federal government that they wanted us to stay in the university and we would go as far as we could. I think it became clear that the attitude of the government may have been a little bit biased, particularly later on, in that they lost some of their concern about trying to keep the humanities students in the university; when they began to get short of officers, they began showing a little bias. But they wanted the people in the science-oriented subjects to stay in the university until they graduated. I think that technically they claimed that the whole thing was administered on an equal basis, but a certain amount of pressure was put on the humanities people, or so it seemed to me. Indeed, my junior year consisted of about 250 students—something of that sort—of which only a small fraction—much less than half—were in science and engineering subjects. Somewhere around this time [August 1942 —Ed.], the British had a raid on the French coast at Dieppe, in which a bunch of commandos were sent across the English Channel to try to find out what the Germans knew about radar. A large fraction of those commandos were Canadians, actually. Quite a few of them were students from my own class, because at the beginning of my junior year, the Canadian armed forces and the universities had made a special offer. They said that for any student who would take a commission immediately, without waiting to graduate, the graduation requirements for the third and fourth years would be waived. So a lot in my class opted for this. Since these guys had already had two or three years' military training, they were immediately sent abroad, and they were in action within a few months of being there. That raid was militarily, I think, a disaster. The casualty rate was enormous. It was done at a time when the armed services really still had no idea how to fight—have they ever learned? They had the officers going into combat with their officers' insignias prominently displayed on their shoulders. And naturally enough, they were selectively destroyed first. It was an unbelievable thing. It shows the lack of thinking on the part of people who should have known better. In any case, a lot of my fellow students from my year and the year ahead were killed in that raid. Out of my small class of about 250 students, about fifteen were killed in this one action. And that had a serious effect on all of us. We didn't go around thinking about it all the time, of course. We had lots of fun and a lot of us played sports. Lots of us had a chance to achieve things in sports that we probably couldn't have done if we'd had a full complement of students, because a lot of the better athletes had already gone into the services. It's hard to say what stands out most in my mind. I may be giving the impression that life was terribly serious and rather grim, but it really wasn't.

ASPATURIAN: Was there any inside knowledge that physicists were involved in the bomb project?

BARNES: Not that early. I knew nothing about it at that time. I had a young and very clever recent PhD graduate teaching modern physics. And I don't think he even knew about it. But he certainly had already told us of these possibilities that might exist, because he really was up-to-date with what nuclear physics had been published. As you know, there was a famous review of the most recent nuclear physics published in 1942, as far as I recall, just before security clamped down in the U.S. And nearly everything that was developed later, or at least what was needed as a starting point for the whole bomb project, was in this article.

ASPATURIAN: Do you recall speculation among your peers or your teachers that this was in fact going on?

BARNES: No, there was no speculation among the students that it was going on. We simply didn't think that far. There was some speculation that a chain reaction might in principle be possible. I don't think any of us fully understood what this would mean. The only thing that was clear was that if it could be made to occur, it could release a very large amount of energy. Of course, we knew that the most easily fissioned uranium isotope—U-235—was only a tiny fraction of naturally-occurring uranium, but the possibility of separating U-235 in large quantities never appeared practical to our limited point of view.

ASPATURIAN: Did any of this have an impact on your decision to veer away from math and concentrate mostly on physics?

BARNES: I don't think it had anything to do with that, although the development of both atomic and nuclear physics, which had been particularly rapid through the early third of the present century, was still relatively fresh stuff; and that certainly affected my interest. But I think I was just technically better at physics than I was at mathematics. Maybe I'm a kind of thing-oriented person, and existence theorems used to bore me, quite honestly. I could do them; I had to do them; I did them well because they were part of learning mathematics. But physics was pure enjoyment; there was no difficulty ever in working hard at physics.

ASPATURIAN: What was the laboratory work like?

BARNES: Well, the labs were a little on the stale side, to be sure. And most of the students hated them; I could see why they found them boring, but I didn't mind them so much. I thought they were kind of fun. I guess my attitude was, "Well, so what; this is really a cut-and-dried experiment; it's been done thousands of times before. Maybe there's some way I can improve on it." I always felt that there was something in it that I might be able to do better than had been done before.

ASPATURIAN: What about your summers' work as an undergraduate? Did you have a chance to apply any of your training?

BARNES: Yes. The less said about my first summer's work, the better. That summer, I simply took a job working on a passenger steamer on the Great Lakes. I was just a regular crew member at sea for three months. I kind of enjoyed that; it was a total change. But as far as my education was concerned, it was totally divorced from it. But starting in my second year at the university, a few of us students were asked to help instruct Air Force trainees in electronics. There was a big demand for radar engineers in England at this time. And radar operators, next to the pilots, had the highest Air Force priority. The pilots, of course, were the most needed for the Battle of Britain. Radar officers were, perhaps, the next highest priority as the British radars were playing a crucial role in the aerial assault on Britain. And the Air Force used to send us large groups of trainees who'd "washed out" of pilot training. Not all of them; some of them were people who had originally enlisted to go into something associated with electronics. But a lot of the Air Force students were people who had washed out of pilot's training, and they were a really mixed bag. I mean, I liked them; they were nice kids. I really enjoyed working with them. Quite a few of them, in fact, were just putting in time, because they realized that if they flunked out of our pre-radar program, then they could do the next best thing to being a pilot for them—they could go into what were called "crash boats." The Air Force had high-powered speedboats that cruised around the English Channel, picking up pilots who had been shot down. And these guys were speed-crazy kids, just as we have now. But they were really delightful. They didn't really cause us any problems. But the students who came to us seemed to be mostly university-educated people themselves, and they were very good. We couldn't teach all the details because they were still highly classified. But the Air Force had sent us all of the basic material that was in principle needed to build or repair a radar set, so we were teaching what was absolutely a state-of-the-art kind of electronics to these people. And there were no electronics' instructors available. We instructors were recruited, as I was, straight out of physics. That I found pretty exciting—learning a near-completely new subject, and knowing that it was actually important and state-of-the-art. At the same time, I guess, it helped a bit to alleviate my concern that I wasn't doing anything for the war effort. So it was full-time in the summers and half-time during the academic year from then on.

ASPATURIAN: The one thing that strikes me is that you had an opportunity to put your training into use immediately.

BARNES: Absolutely immediately. It wasn't so much my training, but it was based on my training. I had a new subject that depended on the things that I already knew how to do. I knew how to do electricity and magnetism, and mathematics, and so on. But a lot of what we were teaching wasn't in any text, because it was so recent.

ASPATURIAN: How was it taught, then?

BARNES: The Air Force sent us some equipment, and they sent us a lot of subject matter. And we made our own lecture and lab courses. There was only a small faculty at the university, and they were heavily overworked. So, as students, a few of us had an absolutely unparalleled opportunity to develop as fast as we could go in the subject, and learn how to teach. We felt good, because the Air Force accredited our training to a level where they would commission officers directly from our course, on our say-so. So that showed a lot of trust in us. They just simply said, "Well, which should be commissioned immediately, just tell us," and these were the ones who were commissioned first. My involvement with the RCAF as a civilian—and as a student—started, I guess, about New Year's when I was a sophomore, which would be the beginning of 1941, and lasted until I graduated in May 1943.

ASPATURIAN: By the time you graduated, were you aware that you wanted to go on in atomic and nuclear physics?

BARNES: Well, it was the newest subject that was taught in modern physics. I was impressed with the enormous speed with which the subject had opened up and promised still to continue. And it was taught by this fresh young professor.

ASPATURIAN: Sounds like he had a fair amount of impact on you.

BARNES: I think he did—only partly because he was particularly good, though. I think it was also because it just was fascinating material, and he had been the most recent one to go through this program himself, so therefore, he was teaching his own best subject. He was a nice fellow. He eventually went into biology a couple of years later, but not for the reasons that you might imagine, that physicists go into biology here. He was diabetic and was taking insulin, intravenously. I guess it was essential to his life. He went to the Banting Institute in Toronto—which was where insulin was first isolated by Best and Banting—to do research. They were also learning how to make penicillin in large quantities at this time. So he also got into the making of penicillin. He certainly did make an impact on me, because he was a nice person and obviously smart. But it was mostly the subject matter that grabbed me.

ASPATURIAN: What stage had nuclear physics reached up to that point?

BARNES: The point was just about what I had alluded to before. People understood basically a little bit about nuclear reactions in a qualitative way. Nuclear fission was discovered in 1939. And as I mentioned, I guess the first paper that showed that there were more neutrons coming out of fission than went in—that one went in to cause it; and something like two or more were coming out—was from work done in France. That was published, I guess about late 1940 or 1941. It was essentially an unclassified subject up to that point. But immediately, that opened up to everybody's recognition the possibility of a chain reaction, at least in principle. Since there was more than one neutron being produced, it meant, at least, that anything greater than one would in principle mean the possibility of a chain reaction. And the fact that fission was producing more than two meant that a chain reaction was even more likely, because that would help to overcome any neutron losses. And this young professor understood that. We talked about it and speculated a little about it, but I don't recall that we ever thought specifically about bombs, quite truthfully; we just thought that there would be a lot of energy released; and in some general way, if you released a lot of energy that could have some military implications.

ASPATURIAN: Did it occur to you to wonder if the government had started something to explore this possibility?

BARNES: We hadn't wondered about it, but it is sure that some people were already involved in thinking about reactors and bombs, even though some people were still publishing freely in the U.S., at least into 1942. There were still open American publications and journals at this time, but I guess Europe was already pretty busy with the war. This stuff about the number of neutrons produced in fission was one of the last things that was published in France, for example. But I guess at the time, if you want to be technical, the idea of separating uranium to make a bomb was pretty hazy to us, and I don't think we thought much about that. Although, we did discuss the point that there was already a conjecture by Niels Bohr and John Wheeler, that of the two most common isotopes of uranium, the one that was most fissionable was uranium 235, which everybody knew was only about one part in 140 of natural uranium. We thought, well, maybe if one could somehow separate out enough of the uranium 235, maybe this would behave really quite differently because it would be much more fissionable. But we weren't really thinking about bombs; it simply wasn't something we thought about, along with classes, military training, and teaching modern electronics to the RCAF. We just didn't think far enough. I think some of us were thinking along the same directions that other people obviously did think about, but we just didn't pursue the full implications of it.

ASPATURIAN: Regarding the war effort, do you remember ever feeling any doubt that the Allies were going to win the war?

BARNES: Never felt any doubt about it. Even before the US came into it, I never doubted that we would win. I just assumed it would happen. But, you know, people are fed this. Of course, by the time the British had successfully weathered the Battle of Britain, it had become clear that Hitler was not going to attack England immediately, and he wasn't in a position to come to North America. It was clear that he had decided he didn't want to go ahead and accept the losses that would have been involved in a direct assault on Britain. I think we all felt very good about that. And then when he decided to turn to attack Russia in the summer of 1941, that struck us as the height of idiocy and must for all time appear to be completely erratic. Up to that time, we had an especially poor view of the Russians, because the Russians had had this neutral, non-aggression treaty with the Germans.

And that didn't go down very well with us, because we were at war; and there were the Russians, carving up Poland along with the Germans. And when Hitler invaded Russia, that seemed to make the eventual result absolutely certain; but even prior to that, we had never considered that we would lose. And what prevailed on the Japanese to attack Pearl Harbor, I don't know. But I guess they must have felt that it was inevitable that the U.S. was sooner or later going to become a full partner in the war. The U.S. strongly supported the British, the Allies, through the Lend-Lease program and in other ways. And nobody in Germany thought that the U.S. was neutral; but there were still lots of Americans who were definitely pro-Germany at the time. I remember—this was not when I was at McMaster but when I went to Montreal a little more than a year later—that one of the things that happened several times was that everybody in the city was watching for German POW escapees. There were German POW camps all over northern Ontario. These weren't really prisons; they were more like big, fenced farms. They were guarded by aging veterans from World War I, who were stronger on belief than on performance, I think. And these POWs were forever escaping. They'd catch them once in a while; but once they got out, they had a good chance of making their way to the U.S. Their aim was to get to the U.S.-Canadian border and somehow get across the border. One of the most direct routes was southeast down through Montreal. In fact, several of them were arrested in Montreal. I know that whenever a POW escaped, it was in the local papers, everybody was watching for the guy whose picture they ran. It seemed that they didn't catch many of them; most of them got to the U.S., where there was immediately a well-organized group of people that supported them. And these ex-POWs in some cases were even treated as heroes, were fed and clothed, and provided with passage back to Germany via some neutral country. But that was not, I think, the majority of the U.S. population. I think the majority honestly supported Lend-Lease, and were pro-British.

ASPATURIAN: Are there any particular people, besides the young teacher you had, that stand out in your mind?

BARNES: Not really. I think that I'd give good, but not excellent marks to all of the faculty that I had. But I think they were really dedicated to teaching, and mostly very helpful. They did their best. **[Tape ends]**

CHARLES A. BARNES

SESSION 2

July 16, 1987

Begin Tape 2, Side 1

ASPATURIAN: I believe we're up to your master's work?

BARNES: Yes. I had just finished my undergraduate work at McMaster. At that point, there was a big decision that had to be made, and that was, what I was going to do next. There was a major project going on in Canada to develop the capability of making optical quality glass, because with the war, the world's primary sources of optical glass, which were in Germany, were no longer available. So in a rather short time, remarkable progress was made in a suburb of Toronto, in developing from scratch a completely first-quality optical glass-fabricating facility. I briefly thought of going there, because they were making all sorts of war-related, weapon-related things that required optical or fiber-optical devices. However, I was invited to go to the University of Toronto by their physics department and was assured that there were important projects for war purposes—you might say for the national war effort—that would be done, and I could do a master's degree at the same time.

I had a very good time the year I spent in Toronto. I found it didn't turn out quite the way that I really expected—nothing ever does. But the principal thing that I found out was that a very large fraction of the Toronto physics faculty was, in fact, already gone. They simply were not in Toronto because of the national emergency. I did have an advisor—a relatively young geophysicist—to work with. I'll say a bit more about him in a moment. There was still some important course work going on, taught by geophysicists, and I became very interested in geophysics at this time. And working for my advisor, I helped them set up a mass spectrometer—actually quite a good one. As an instrumentation job, that was pretty interesting; I really enjoyed doing this. There were a few things that I helped to measure with it, but my personal involvement in the measurements was not at a very high level. I was more a technical assistant. However, one of the large series of samples that I ran on this device were enriched Nitrogen-15 samples. These were part of a nationwide scientific effort to find a more effective, more rapid, and more economical method of producing what was then a very secret explosive, called RDX, which turned out to be one of the major developments during World War II. My role in it was very small; I supplied and operated the mass spectrometer that let them do tracer work in trying to figure out how to make RDX more efficiently.

During that year at Toronto, I also went off with one of our technicians and had a great time collecting and measuring rock samples from one section of northern Ontario. I did the local geology as I went along, collecting samples. And then I correlated the local geology with the radioactive content of these materials, which I took back to the lab to measure. I was looking at a particular kind of symmetrical, roughly elliptical structure, called the Cheddar Batholith. I was amazed to hear later, not too many years ago, that Jerry Wasserburg and other Caltech geologists even knew where the Cheddar Batholith was, so it really exists! But the idea was that this was a kind of structure with elliptical symmetry, kind of rocky, mountainous, overgrown with forest—it was some kind of forest reserve. And what I found was that this elliptical pattern had kind of a ring structure to it, corresponding to different layers of rock that had been laid down at various times. I was able to show that the natural radioactive content of the rocks was highly correlated with what you could tell from the surface geology. I don't think this added anything particularly new, nothing that you couldn't tell by cracking off the rocks and looking at them. But it was interesting to see that the inferences that one might have tried to draw from the geological sampling were completely borne out by the radioactivity levels. At the time, there was a certain amount of interest in tracking uranium deposits, but there was no longer any unclassified information as to why this was interesting. During that year, I also had a bit of combined physics and fun, carrying a magnetometer by foot through some really dense forest in northern Canada.

ASPATURIAN: What is a magnetometer?

BARNES: In this context, a magnetometer is a sensitive, highly accurate instrument that measures the strength of earth's magnetic field. The point of this exercise was that the earth's magnetic field, which we usually think of as being a rather simple kind of magnetic field, has a great deal of structure on a small scale that comes from the fact that the magnetic permeability of the rock varies from place to place, and from one type of rock to another. In fact, in places where you have a little bit of iron in it, the rock becomes very magnetic. So it can have enormous variation from place to place. What I did in those days—this would be 1944—was lug this very delicate, mechanical magnetometer over miles of dense, mosquito-ridden forest. And then each night I would plot the readings that I had got during the day, reading the magnetic field every hundred yards as I went along. What this finally resulted in was a map about the subterranean, subsurface magnetic permeability distribution. The point of this kind of work is that whenever you have a kind of anomaly—that is, some kind of region of variability instead of a kind of bland uniformity—the anomalous magnetic field pattern is telling you where the rocks were seriously disturbed and/or possibly mineralized. This turns out to be extremely valuable economically, because it costs a lot of money to put drill holes down through miles of rock. If you find nothing but a uniform granite for miles, you've learned almost nothing. On the other hand, if you have some advanced knowledge of where the subsurface rock is disturbed, which can make detectable anomalies, you can sink your bore holes directly into those areas and actually pull out some information that might tell you where one might find useful mineral ores.

My background at that time was really not very specialized; it was very general in physics. I was completely undecided what I was going to do. I was somewhat disappointed, and more than a little miffed, to find out later that, rather than some strategic minerals, needed for the war effort, these people were actually mainly looking for gold. And I felt that really wasn't what I expected to have been doing just at that point. The fact that my advisor was also a geophysicist certainly was connected with my interest in the subject. And I remember thinking about him, "This is a strange kind of a physicist, perhaps, but one day he's going to be rich. I just know that he's going to be very wealthy." Now, just by way of a note, many years later, when I next heard of him, I found that he had been fired by the university for lack of attention to his duties as a teacher and scientist at the university while he was busily building his own private geophysics company. And last summer, a friend of mine from Canada sent me a magazine article about this man and his now grown son. It turns out that they are—nobody knows for sure, including them probably, exactly what they're worth, but the best rumored estimates are that the father and son are worth some $500 million at this point. I was thinking one of these days of dropping by! I ought to call him up and see if he still remembers me from the old days when we were all poor together! An interesting side note is that he didn't really become wealthy by simply being a geophysicist. To be sure, he got his start by recognizing that a piece of property that he was surveying was probably mineralogically valuable. He made his first millions out of that. I heard that what happened is that the company that had the mineral rights sold them off to him for some nearly negligible amount. And that turned out to be a multi-million-dollar mistake for them. But this man was a very astute person. And I guess he recognized quickly that the way you really make money, if that's what you're after, is not by doing scientific and technical things, but by becoming a financier. He certainly had the expertise in geophysics to know what things were likely to be good buys and what weren't. But he did not make most of his fortune as a practicing geophysicist.

Anyway, that was a small and very short page in my education, because I decided I was really marking time in this position. I was invited to stay on and continue for a doctorate, but I just decided that was the wrong time to do it, because it was wartime and I didn't feel that what I was then doing was important enough. And coincidentally, a representative from the joint British-Canadian Atomic Energy Project, which was a fission project in Canada at that time—about which I knew absolutely nothing—happened to come by the university and became quite interested when he found out the various things that I had been doing. So, in the spring of 1944, I went to Montreal and joined that project.

ASPATURIAN: What exactly was that?

BARNES: Well, I suppose there were some two hundred or three hundred physicists and chemists, and a lot of engineers, not counting technical staff and other professional people. A large fraction of them were from Europe, most of those British. When the British had decided that they couldn't continue with their atomic energy work in England because of the war, they split their project; some of the people from the British project went to Los Alamos in the U.S., including the well-known theorist, Klaus Fuchs. [In 1950 Fuchs was convicted of supplying information about the Manhattan Project to the Soviet Union. —Ed.] But most of the British staff in fact came to Montreal and set up, together with the Canadians, a new project. The lab started in Montreal about 1943; and at the same time, a major engineering effort was begun to build a nuclear reactor research establishment 140 miles northwest of Ottawa, at a place named Chalk River, on the boundary between southern and northern Ontario. Gradually, as the effort matured, the entire Montreal lab was transferred to Chalk River.

ASPATURIAN: Did Fuchs ever visit your project in Montreal?

BARNES: I don't remember ever seeing Fuchs there. I couldn't say that he never came, but there's no particular reason that I can think of why he would be there. He probably went directly from Britain to Los Alamos. Two people connected with the project in Montreal were later of some concern with respect to espionage. One of them was an Englishman by the name of Allan Nunn May, who was arrested and put in prison in Britain, but not until after the war was over. Also of some interest on this project was a very bright young Italian physicist by the name of Bruno Pontecorvo. When the British mission went back to England after the war, Pontecorvo went with them to their new nuclear research establishment called Harwell. At that point, he had switched completely into unclassified cosmic ray physics. About a year or two after he went to Harwell, he just plain disappeared with his wife, and turned up in Moscow. I think he's dead now, if I'm not mistaken. Their son is now a prominent high-energy physicist in Russia. Pontecorvo's wife has since spent most of her life in mental institutions, I've been told. But I don't know of any evidence that Pontecorvo was ever involved in espionage. I've been asked this many times by people. It's true that he and his wife—it emerged later—had been rabid Communists during the thirties as students in Paris. And that should have been known to the people who hired him for this project. But evidently it slipped through the cracks. But "Ponti" was a great, fun-loving guy and superb tennis player, and also brilliant as a physicist. He'd worked as a very young physicist with Enrico Fermi in Italy, perhaps as early as when he was eighteen or nineteen years old. But I don't think that he was, at the time that we knew him, very political. And the fact that when he got to England, even though he was working for Harwell, he immediately transferred out of anything classified into cosmic ray work makes me think that he really was not involved or interested in espionage at all. Why he went to Russia, I don't know. He continued to do a few interesting scientific things after he went there. He was never ostracized by the rest of the physics world for it. For example, while in Russia he had a bright idea as to why we don't observe more neutrinos from the sun. Pontecorvo was the first person—to my knowledge—to propose that the neutrinos coming from the sun are changed into a different kind of neutrino that is not detectable. We now call this neutrino oscillations. The exact mechanism is a little different from what [Hans] Bethe is working on at the moment. It was before the discovery, I think, of the Tau particle, and certainly its neutrino, but I couldn't be sure of that. The muon, of course, had been known for a long time. In fact, during the time he was at Chalk River, Pontecorvo participated—as a kind of sideline—in an experiment in which he showed, along with other people working in different places in the world, that the particle, then called a meson, that was first identified, I guess, by [Carl] Anderson and [Seth] Neddermeyer, could not possibly be the particle that was postulated by Yukawa as part of the theory of the strong interaction, because its interaction with nuclei was too weak. It was not very long afterward, in 1948 or thereabouts, that the pion was discovered and that finally resolved that particular problem.

ASPATURIAN: He must have been a great loss to the West.

BARNES: Well, in a way he was. He could be accused by some people of being a bit flighty perhaps, but he was smart and he had a lot of clever ideas. In fact, the idea of detecting solar neutrinos by capturing them in chlorine-37 was first postulated by Pontecorvo during the time he was in the Chalk River lab. This idea was later rediscovered independently, and proposed by [Luis] Alvarez as a way to look for solar neutrinos, after World War II. But it became public knowledge quite quickly that the idea had actually been proposed earlier during World War II in Chalk River. But Pontecorvo was an ingenious fellow, there's no question in my mind. Anyway, I don't know why I specially mentioned these two people. They're of public interest, but they were a very small part of the project. There were many impressive people in this project.

Now, my role in this was again a fairly minor one. I mainly did electronics research and development, including participation in the production of instruments for both the physics and chemistry research programs. I was also involved in building instruments to control and monitor the reactors when they were built. Pretty heady stuff for a very young physicist in his first real job out of university, to be associated with this group of really world-class physicists—many of them were very famous—on a project that was both very secret and was clearly an absolutely front-line kind of a project.

ASPATURIAN: Did you at least know what was happening at that time at Los Alamos?

BARNES: Many people in our lab certainly did. The collaboration between the Canadian project and the American project was substantial, but never complete, for security reasons, I think; but at some point it seemed to become considerably weaker. That would be late in '44, perhaps. But the decision was taken quite early that the Canadian project would pursue a different direction in reactor physics, and that there was no reason for them to try to compete with the much larger American project, which was trying several different alternative approaches, aimed, as we now know, at the development of a weapon. So it became clear with the much smaller Canadian project that although it might succeed in doing some significant things in reactor design and operation, it was going to be a relatively small project. There were, in fact, a few things that were first developed there. For example, I was told that a new solvent-extraction technique for separating plutonium and uranium from spent fuel rods was in fact developed in Chalk River. I don't know how they accomplished this, because it seemed to me that half the chemistry department in the first year were at Chalk River trying to discover a better mosquito repellent. You see, in the months of May, June, and July, the mosquitoes are just unbelievable up there in the forests. They're huge and prolific. Then the weather dries out so that by August and September they're gone. But particularly the Englishmen and other Europeans who came from countries where they don't have much in the way of mosquitoes were fantastically sensitive to these North American mosquitoes. They were thus eager to work on insect repellents, but their real job was, of course, actually to develop this new solvent-extraction technique. Anyway, it was a heady experience; all phases of this project were new to me. First of all, there was the development side, getting beyond the research on the instruments, to develop them so they could be used by anyone, which was something I'd never done before. And then, because we had to split our staff between Montreal and Chalk River, at the age of twenty-three, I guess, I was put in charge of the production lab in Montreal to develop and build control and safety instruments for all of the reactors at Chalk River.

Aspaturian: Do you recall anyone else up there who made a particular impression on you?

BARNES: Well, there were dozens of people that I would have anecdotes about, and certainly one of these days I really want to try to do something about correlating my memories of the project with notes that are being written by other people. There's a professor at Queen's University at Kingston by the name of Sargent, who has written a good beginning history of that project, I've been told.

ASPATURIAN: Judy [Goodstein] had asked me to ask you about an anecdote involving Niels

Bohr.

BARNES: I think I know what she's referring to. One evening when we were having Hans and Rose Bethe for dinner, my wife and I invited Judy and her husband, David, to join us because she hadn't met Hans before, I guess. They hit it off wonderfully well. And we had a lot of talk about people we'd known. It turned out that there were a lot of stories that I'd heard part of, and that Bethe had heard part of, while working on the American project. And it was interesting to compare notes. But it is true that I brought up this story and asked him if it was right, and he confirmed it. We had a theorist at the Montreal lab by the name of Placek. He was quite a famous theoretical physicist who was well known to Bethe. Placek, for reasons that I never tried to track down, ended up in the Canadian project and not the American project. In any case, we had all these well-known French and German and British scientists working with us. During the period 1943 to 1945, they lived in Montreal, which is a rather pleasant city to live in, really. And Placek, as I say, was one of the best known. We had a visit from Niels Bohr to our project, during the time he was associated with the American project in Los Alamos. He came up to our lab in Montreal, and he went on to our new Chalk River lab as well, with his son Aage, whom I didn't know at the time and didn't meet until many years later. I met Niels during that visit to the extent that I shook hands with him, but that is about all. But the story was made by the fact that his being in America was very hush-hush; and of course that also held for his visit to Montreal. But one day while he was there, he was walking along the main shopping street in Montreal—a city of about a million plus people—when a woman walked up to him and said, "Excuse me, aren't you Niels Bohr?" And he said, "Madame, you must be mistaken. I'm Nicholas Baker. But aren't you Mrs. Halban?" And she said, "No, you must be mistaken; I'm Mrs. Placek." In fact, she had been Mrs. Halban, but they had been divorced and she had remarried. So there was this funny exchange of people who unexpectedly recognized each other on the streets of Montreal. That was a true story I regret that I never had a chance to talk with Niels Bohr about that visit to Montreal, when I went to Denmark on a sabbatical in 1962, because he died just a few months after I arrived.

We also had a visit on another occasion from Major General Leslie Groves, who was in charge of the American project for the U.S. government. He was very concerned about security. He was brought around and introduced to each of us for a very short chat. **[Tape ends]**

Begin Tape 2, Side 2

ASPATURIAN: What were your security provisions actually like up there?

BARNES: They should really have been better. I remember one security guard in particular. He was quite elderly, but he wasn't required to know very much. As a veteran of World War I, he had a priority in getting such a position. He had one serious shortcoming—at least [laughter]—and that was that he was too fond of the bottle. In fact, he learned at some point about the supplies of alcohol in the chem labs and chem stockrooms. And at one time, they had to go to great limits to disguise all the alcohol so he couldn't actually identify it. But he managed, nevertheless, to occasionally smuggle a bottle out.

ASPATURIAN: In a top-secret security installation?

BARNES: Unbelievable, isn't it. He was, it turns out, a little bit high by eleven o'clock one morning. He sort of skated, slithered, and rolled down a flight of stairs, just as the director of the lab—who was Sir John Cockcroft at that time—and General Groves were walking to the foot of the stairs. And then they were suddenly greeted by this cascade of human being down the stairs. He sort of looked up weakly, saluted Cockcroft, got up and wandered off. [Laughter] Although Cockcroft, who was a member of the British staff, really tried to get rid of him, it turned out to be very hard to do because he was a vet from the First World War. But this guard and I used to get along all right. It turned out that he didn't administer the oath of secrecy to me until a couple of months after I started work there. And I said, "You know, that's really pretty careless. What would you have done if I'd given away some secrets?" He said, "Well, if you had given away secrets before you were sworn, we'd convict you and hang you for espionage. If you did it after you were sworn in, we'd do it for being a traitor. So it doesn't matter." [Laughter] In some ways, having a security officer like that might even have been an advantage because he would certainly, if anybody was investigating things, give them a false sense of what security really was like because he was a joke. In fact, I can't personally vouch for the story about the guard, Sir John, and General Groves, but it came from "a usually reliable source."

Anyway, it was a heady experience for me, and I'm glad to say that some of the things that I worked on seemed to be of some value. I was present when the first Canadian reactor turned on in 1944, the so-called zero energy reactor. It was called ZEEP -- "zero energy experimental pile." It wasn't literally zero energy, but it was intentionally very low energy, for research purposes. But also, many of the things that I worked on, as well as, of course, the efforts of a very large number of other people, went into the first high-flux reactor in Canada, which was called NRX—"nr" standing for National Research Council, and "x" for Experimental. The experience of working with these people, who were world-class scientists, was a very stimulating experience to me; and it persuaded me that I should continue to graduate school as soon as it would be possible.

And after World War II was over, in the summer of 1946, I applied to graduate schools in the United States and in England. And, among other American universities, I was accepted in Cornell, I remember, where R. F. [Robert F.] Bacher, who came here to Caltech later, was then department chairman. Hans Bethe was also there at that time. I received a nice phone call from Dr. Bacher at Chalk River, telling me that I had been accepted, and that they were looking forward to seeing me shortly. It seemed to me that this was a very nice thing for a department chairman to do.

Then I got a letter from England, saying I'd been awarded what was called an Exhibition of 1851 Scholarship. It was a scholarship that didn't sound all that big in terms of dollars, but because of the cost-of-living difference between Canada and England, it meant that I would have all of my expenses paid for three years—tuition, living, and everything. It was a prestigious scholarship that had had a long history of important people who had one, like Ernest Rutherford, and it was given to students from the British dominions who wanted to study in England. It likely wouldn't be known in the U.S. because students from the United States wouldn't be eligible for it.

Now it's highly debatable whether I should have accepted that or not, but it looked good to me at the time, and I'd been working with all of these Englishmen for several years, and I was very impressed by them. So after worrying feverishly about it for a few days, I picked up the telephone and called Professor Bacher at Cornell and said that I regretted that I wouldn't be coming to Cornell after all. And though he couldn't have known me from Adam, he was extremely nice. He said, "I think you will have a very exciting experience in Cambridge, and I wish you good luck." You know, he could have said, "Well, look, we're counting on you here.

You've led us to expect that you're coming." But he didn't; he was a perfect gentleman.

ASPATURIAN: In thinking back to what you had said about your father, he must have been very happy to see this progression of yours culminating in a PhD.

BARNES: I suppose he was. He was certainly a "gentleman of the old school" who really had great difficulty showing his pleasure about anything that had to do with his own family. With anybody outside of the family, he was extremely good about talking with them and congratulating them when they'd done something well, and consoling them when something bad had happened to them, and so on. It was part of his occupation. But somehow, he had difficulty expressing these things in his own family. I think these things partly get passed on to the family, the children as well. There's been nothing but love in our family; it's not an overt thing, but rather a very quiet aspect of our relations. But I'm sure he was immensely proud.

Anyway, in one sense, it was financially advantageous to go to England. I was excited by the prospect of working with some of these English people I'd met in Montreal and at Chalk River. And I was just generally excited at the prospect of going to Europe. I perceived a chance to enlarge my experience a little bit. I went off to England in the early fall of '46, I guess. The Canadian Pacific Steamship Lines had some kind of tie-in that gave free passage across the Atlantic to winners of the 1851. [Laughter] And so another Canadian, who'd also won one, and I, the two of us had a nice trip from Montreal to Liverpool on a Canadian Pacific freight liner—one of those huge freight liners that carried twelve passengers. So it was a kind of exotic experience for me. It took us ten days—not a very fast ship. The other ten passengers were mostly very well-to-do business people who made a hobby of traveling the world on freighters and paying an enormous amount of money for it. I remember landing in England at Liverpool with a great deal of anticipation of taking a train to London. And then I had to get across from one station in London to another station to catch the train to Cambridge. Railroad stations in London are scattered around the city and you have to find your own transportation from one of these main lines to another; that's because historically they were all originally different companies. Anyway, somehow or other, I managed to get myself into Cambridge late at night that day. I had a letter telling me where to go when I got there. It was a long day; I was pretty tired, and I guess I got to bed and slept for about fifteen hours. Then my career in England started the next morning. [Laughter] I don't much remember that trip except there was one businessman who got quite friendly with this other student and me on the way across. About halfway across, he said, "I'll bet we're at mile something-or-other, fifteen hundred and seventy or something." I made a quick calculation and decided that we couldn't be that far yet because I knew how far it was across and I knew it was less than half the time; I knew we couldn't be that far. So I said, "I don't think so." He said, "I'll bet you fifty dollars that we're at mile fifteen hundred and seventy-five." [Laughter] And I said, "You know as well as I do that I've got only about two dollars to put together, and I don't bet anyway." He said, "Well, I'll bet fifty dollars to your fifty cents that we're fifteen hundred and seventy-five miles." [Laughter] And I said, "Well, then, I'll take it." I don't know why to this day he did that, because of course, all I had to do was walk up to the bridge and ask the captain or the officer on duty where we were; and it turns out, of course, we were at twelve hundred and something. So he gave me fifty dollars! That gave me pocket money for the crossing—not that there was much I could spend it on.

He, this business man, had a bad time when we got to Liverpool because he was on a business trip to Europe; and all of his luggage was full of bottles of high-priced Canadian whiskey. He must have had dozens and dozens of these. Anyway, he was flippant with the customs officials in Liverpool. And being flippant with a customs official is something one should never be, I discovered as a result of that incident. Although there was nothing wrong with him bringing this whiskey with him, the customs officers didn't like his attitude. They made him open every single bottle, and then put a float in each bottle to measure its specific gravity and determine whether its alcohol content was what it said on the label. And so, as our train pulled out for London, he was still there with the customs officials measuring his endless bottles of whiskey. I guess he got to London eventually.

Cambridge turned out to be a pretty exciting experience, too, because it was my first opportunity to really undertake independent research, where I had to actually think of the ideas by myself and execute them. There were lots of people around in the same position, so we were all neophytes together. It was great fun and a great challenge. My nominal supervisor, when I first got there, was a gentleman who went off in a different direction very shortly. But I had to have a nominal supervisor. Eventually, Otto Frisch was appointed the Jacksonian Professor of Physics at Cambridge, and he became my official supervisor. The Jacksonian Professor of Physics automatically became the supervisor of approximately one-third of the graduate students, called research students; and I was one of them. That also was in some ways a very pleasant experience, but in others a little bit frustrating because Otto Frisch was a delightful human being, an extremely brilliant, ingenious man. He died just a couple of years ago. But he had no administrative talent whatsoever. Furthermore, he had no wish or desire to supervise graduate students. Although I think part of his salary was for his nominal supervision of his many research students!

The fact that I didn't have very much to do with him wasn't a serious hindrance because I was working a certain amount with a young Englishman by the name of Denys Wilkinson. When Denys got his doctorate, after I'd been there a couple of years, he became my kind of de facto advisor for the remaining two years I was there. In some ways, we were more like colleagues, but he was very helpful to me, in the sense that he had a lot of very good ideas. Otto Frisch, on the other hand, remained a person whom I respected because of his originality and ingenious ideas; but as far as being an advisor to me, I didn't see him for two years at a stretch. In fact, the first time I had a real conversation of any length with him was just before I handed in my thesis. I said, "I suppose I should see my advisor about writing a thesis," and Denys said, "Yes, you should see your advisor about handing in a thesis." Not much like the U.S. system. So I got together all of the stuff I had done—I had actually done two quite significant experiments, either of which could have been my thesis experiment. So Frisch said, "Well, what did you do?" I told him about these two experiments. And he was actually very interested. I knew that he was about to leave for the U.S. on a tour of some weeks, giving lectures all over the United States. He asked me some quite detailed questions about this work and made some notes. And I thought to myself, well, the polite thing to do is ask him a question myself; he was officially my advisor. So I asked him a question—I don't remember what it was—and he looked at me for a minute, and suddenly he opened the top drawer of his desk and pulled out a pair of gray flannel trousers all rolled up into a ball, and he said, "Barnes, I hope you don't mind. You know it's early closing today, and I must get these trousers cleaned and pressed before they close." [Laughter] Whereupon he was gone. I found out the same thing had happened to another graduate student, a young lady whom I knew very well, an Australian student. When she went to see him a few months later, she had the same experience. I guess we decided he had a desk full of gray flannel trousers, waiting to be cleaned, just in case! He was actually a delightful person. You know, he and his aunt, Lise Meitner, provided the first, and in fact correct, explanation of nuclear fission. He narrowly missed discovering fission himself, by an electronic experiment. If fates had been a little kinder, Otto Frisch would have been the discoverer of nuclear fission, not Hahn and Strassmann. Frisch built an ionization chamber in which he had put a thin layer of uranium. What he was going to do was try to find out what happened if you irradiated uranium with neutrons, whether you made some new chemical element. And of course, the main thing that he saw in his ionization chamber was large numbers of alpha particles from the uranium as would be expected. And I guess he hoped to see if there were any alphas of a different energy while you irradiate it with neutrons. The only thing is that every time he irradiated it, his counter developed "electrical breakdown" problems, which gave gigantic pulses. These kinds of electronic problems often plagued people because our insulators weren't all that good at that time. He plowed around for many weeks, with these breakdowns that occurred and he couldn't solve; but they only seemed to occur when he tried to do some serious work and bombarded the chamber with neutrons. It was only after several months passed, when he heard of the discovery of fission by Hahn and Strassmann, that it suddenly dawned on Frisch that these weren't electrical breakdowns at all. The giant pulses he was seeing that were forty or fifty times as big as the alpha particle pulses, were nuclear fission. And you see, he'd seen these giant pulses before Strassmann had figured out what he and Hahn were observing chemically. But, you know, in a way it was a clever thought by Frisch to look at an ionization chamber; what he saw was so unexpected that he looked for a "dirt-effect" explanation, and missed a great discovery. Certainly, he and Meitner immediately understood how fission happened and why it happened as soon as they heard the Hahn and Strassmann result. Although the theory of fission was embellished enormously later on, their simple explanation was basically correct. Frisch was a wonderful person—and also a great pianist. In a sense, he was a concert pianist manqué; he might even have become a better concert pianist than a physicist; who knows.

I did meet Lise Meitner while I was a research student at Cambridge; she came to visit her nephew Otto occasionally, and wandered around the Cavendish lab, talking with students.

ASPATURIAN: Where was she?

BARNES: She went back to Sweden. In fact, I think she lived in Sweden all of the time since she had to leave Germany for her personal safety; and as far as I know Sweden remained her home. But she liked to visit England; and Frisch always brought her around the lab and introduced her to the students. She would occasionally ask a question, usually a very sensible question. She obviously knew what was going on, and understood that it was important to the students to have someone interested enough to ask them questions.

Anyway, that was the situation with my formal advisor, Otto Frisch. I don't want to give the impression that I had anything but great respect for him. He was, however, a poor administrator. I don't blame him for being a nonexistent supervisor, but he was a poor administrator, which in fact, eventually caused the demise of nuclear physics at Cambridge within a few years because all of the best people gradually left and went to other places.

ASPATURIAN: Has it recovered?

BARNES: Not in that area; never did. Oxford suddenly became the British center of nuclear physics. Then later, other universities like Sussex—well, Liverpool also had regained some of its earlier status in nuclear physics. Manchester also, later on, became important in nuclear physics. But Cambridge never recovered from the loss of a handful of extremely talented people. Over here, they would be called associate professors or assistant professors, or something similar. Under the British system, they wouldn't have any such rank. In any case, rank was not the point. Some came to the U.S.; one of them went to Oxford; one of them went to Liverpool. They just disappeared; and with them, the chances that nuclear physics could become in Cambridge what it had been during pre-World War II days vanished. But it was exciting for me. We were the first group of students after the war. In some ways, we had a special challenge, because everything had been pretty much dismantled when the British project went to Canada and the U.S., so we had to start from scratch and build a lab as well as do the physics. But it was fun to do that.

The system there, in the British universities, might be of interest to you. We were not called graduate students but were called research students. And in a way that's consistent with the way the system functioned, at least at Oxford and Cambridge in those days, and probably in all the British universities, which mainly tried to imitate Oxford and Cambridge, as far as I could see. There were no graduate courses for students per se. There were some quite advanced undergraduate courses which, I guess, we could profitably have taken, especially since we'd come from such different university systems. And I did sit in on some of these courses occasionally, when they sounded like fun. But there was a group of young people from other countries outside of England, including the student that I'd come over with from Canada, this young Australian lady whom I mentioned earlier, and two South Africans. The five of us realized after we'd been there a few weeks that there was something badly missing, or as we perceived it, something badly missing in our opportunities to learn. So we formed a kind of club and decided that one of us each week—we'd meet once a week—would study a topic and present the topic to the rest of us, and we'd all ask questions and argue about it. Well, we started right there in the Cavendish Lab, which seemed the reasonable thing to do; that's where we were all together. And it almost instantly became known that we were having these meetings. And numerous British research students, one by one, asked if they could join our group. It seemed the friendly thing to do to say yes. And so we did function this way for a very few weeks. But it became clear to us that their needs and our needs were totally different. There were some things that they knew much more about than we did, and there were some things that we knew a lot more about than they did. There just wasn't any real meeting of the minds on the kind of topics that we should talk about. So we were embarrassed because we'd started this club, and it really wasn't doing what we wanted. And I'm afraid we did something that isn't very nice, looking back on it. We just let it slowly die out in the next couple of weeks by not being there. First one dropped off, and then another. When a third week had gone by, and none of us had showed up, it folded. It just collapsed because we'd started it and just disappeared. In fact, what they didn't know was that we had arranged to go from college to college, and we'd meet in the room in the college of whichever person was presenting the topic. So we'd be in Emmanuel College one night and in Caius College the next week, and Newham College, where this young Australian lady, Joan Freeman, was another night, and so on. We just rotated around the colleges this way. And this kind of extremely informal seminar continued for the whole four years that I was there. In fact, one of the South Africans, Alan Cormack, had funding for only three years and had to leave. And we all kind of pitched in and helped him get his research work finished on time, and he went back to South Africa. The other South African never did go back; he just married in England and became an Englishman. And a few years later, I heard that Cormack had come to the U.S.—I believe he's at Tufts, if I'm not mistaken. A few years ago, Cormack shared the

Nobel Prize for the development of the CAT scanning process—computer assisted tomography. This work was based on calculations he'd done in South Africa between the time he went back from Cambridge and the time he decided that there was no future for him in South Africa. I guess the fact that he was married to an American girl may have had something to do with his emigrating to the U.S. He was married in Cambridge, UK, and I went to his wedding. But there was a clear disaffection among these South African students. They were faced with a terrible dilemma and felt a kind of duty to go back to South Africa to try to make things better in the country. But the rest of us from outside, in our simple-minded but probably correct view of what was going to happen, said no, it's just a matter of time. We thought it would happen faster; but we believed that, at some point, things were going to get nasty in South Africa, and there would be terrible racial trouble. We underestimated how long it would take; but it did happen, and just as we thought. And of course, these two South Africans understood this as well as we did. I think it's interesting that they both ended up overseas, one of them in England and the other in the United States. [CB subsequently added: "In the end, South Africa actually managed a largely non-violent transition to majority black rule, though with many major problems remaining to be solved." —Ed.]

[Tape ends]

CHARLES A. BARNES

SESSION 3

July 20, 1987

Begin Tape 3, Side 1

ASPATURIAN: Last time you were about to talk about the comparative virtues of graduate education in Great Britain and the United States.

BARNES: I did actually want to say that I thoroughly enjoyed the time I had in Cambridge. It was stimulating to work with a group of people, many of whom I had known during the war, and yet were much more senior than I was. It was a complete challenge at all times, trying to excel in this environment. As I said earlier, several of the students who had come from other countries felt that we had had an inadequate preparation for graduate work, compared with at least some of the British students. We chose to remedy that by forming our own little study club. I think this was probably the best thing we did for all of us while we were at Cambridge, because there's no question that we learned far more talking to each other than we did from any of their courses that we ever attended. In fact, this is an area in which the British graduate school and the American graduate school of that time differed very strongly. It may be that the British have now gone some way toward solving this problem in more recent years. But I'm just not familiar with that. Generally, I think that the American system, with a number of important graduate courses for the students, is a good system; i.e., the American custom of having courses designed especially for graduate students really does contribute a lot to their education. However, I do understand that this can be overdone, and maybe some universities do overdo this; i.e., the opposite extreme is having so many graduate courses that the students don't develop any sense of learning things by themselves. In fact, we have some graduate students here that, I think, already suffer from that situation. They would like to be spoon-fed. I think that there's clearly some optimum amount of coursework, beyond which you're wasting the student's time and stunting his development, when he should be developing his own ability to learn. Because most of his life, he is going to have to depend on things he learns by himself, as he goes along, and it's crucially important that he learns how to teach himself. You just cannot coast for very long on what you learned in graduate school.

I think there are advantages of both systems: The strong point of the British system is it puts the graduate students earlier very much on their own. They're forced to sink or swim, and they must find whatever method is best for them to develop themselves. If they fail to do this, they simply vanish from the system. Those who survive are usually very bright and capable.

It was a very pleasant experience for me. Another thing I thought was very different—and perhaps a good idea—was that whenever Cambridge had vacation periods, such as Christmas, Easter, and in the summer, they locked the lab up tight. Nobody could get in; no one had keys. The power was turned off, so even if you could have gotten in you couldn't have turned anything on. The whole point was to insure that the students took vacations. That meant that without any bad conscience, I could travel all over the United Kingdom and the Continent, and just generally have a good time during my vacations. In fact, in the winter, I managed to get on the Cambridge ice hockey team. We were always invited to the Continent to play hockey. To be sure, we were about good enough to play against teams from small towns, not the famous European teams like Davos, in Switzerland. But it was a big change of pace and a lot of fun to do something completely different, and come back completely refreshed to start anew. I recall playing against a team from the town—or small city—of Martigny, in southwestern Switzerland. We arrived about noon to find that there was a civic reception, with the very good local white wine flowing freely, speeches by the local mayor, etc. When we decided it was time to stop the wine and get something to eat, the locals protested, pointing out that the local team was also present at the reception. When we got on the ice about 7:00 pm, I couldn't recognize any of the local team members. It seems as though the locals had separate drinking and hockey teams. It goes without saying that we were not a good match for the local hockey team that evening.

I was pretty lucky, because I actually did two research problems during my doctorate work, either one of which would have been a satisfactory thesis problem. So I wrote them both up, as a single thesis, Parts I and II. They really weren't connected much to one another, and it was pretty transparent that they were two not closely related studies. But that didn't seem to bother my examiners. One of my examiners was most interested in one part of the thesis, and another was most interested in the other part. But, eventually, I realized that I was going to finish soon, and that this highly enjoyable period of my life was going to have to come to an end, and I was going to have to start earning a living, to put it mildly. So I accepted an offer to go to the University of British Columbia in Vancouver. The last week I was in England, I presented two papers at an international nuclear physics congress, held at Oxford, and I got married. I had my final oral examination at the conference, because it was the only place I could get all my examiners together. Then after we got married, my wife and I had two days for a completely unplanned honeymoon in Paris, just in time to get back to England to catch a ship for Canada.

ASPATURIAN: Was your wife British?

BARNES: No, my wife was Canadian. In fact, we had known each other as undergraduates at McMaster, but had kind of lost touch with one another for a good many years. We were reintroduced in England. She had gone to Norway to some summer school some time earlier. She was beginning to run out of money, I guess, and she couldn't speak Norwegian; so she came to England and got a job teaching in a girls' school in London. Some mutual friends in London reintroduced us.

ASPATURIAN: Can we talk about your research at Cambridge?

BARNES: In 1946, when I first went to Cambridge, it seemed to us that one of the most fundamental problems in nuclear physics was the nature of the interaction between neutrons and protons. In fact, that's still one of the most fundamental perennial problems in nuclear physics; but now the uncertainties in the interaction are at high momentum transfers, and the experimental and theoretical techniques have become completely different. It seemed to me and to Denys Wilkinson and a graduate student from South Africa by the name of Godfrey Stafford, that measuring the cross-section for photo-disintegrating the deuteron—that is, splitting a deuteron into a neutron and a proton under the action of electromagnetic radiation at a range of energies—would tell something that wasn't then known about the neutron-proton interaction. The problem divided itself into two nice separate experimental pieces. One of them was developing a suitable detector for the job, and that was undertaken by Stafford. The other half was developing some method of measuring the gamma radiation flux accurately and absolutely over a wide range of gamma ray energies. I undertook that part of it. We eventually got this to work nicely. Both Stafford and I did separate problems using this equipment. I probably could have finished within three years, but I felt that I needed a few more months to finish my thesis to my own standards, so I applied for a few months' more money from a scholarship fund in Canada. Instead of getting another few months, I was given another year. I was determined to make the most of that year; and by this time, I had acquired enough skills to do things a whole lot faster than when I started out on my own.

There was an interesting problem lying around that had been assigned to another unsuccessful student several years earlier. The problem was to look for an angular correlation between the emission of an alpha particle and a gamma ray, emitted one after the other. The quantum mechanical theory of angular momentum predicted that there should in general be a non-isotropic angular correlation between them. In other words, if you detected the alpha particle in some particular direction, then the probability of detecting the following gamma ray would depend on the angle with respect to the initial alpha particle, and would not necessarily be the same at all angles. The student to whom the problem had originally been assigned had made an erroneous conclusion about what kind of radically new instrumental development he had to achieve before he could even attempt the experiment. So he never succeeded in doing it. But I knew immediately how to do it, and I knew that I could very quickly put together an experiment that would actually do this. I went along to the man who had first suggested that experiment, a very bright young faculty member by the name of Sam Devons, who eventually went to Columbia and was a professor there for many years. I said to him, "Do you mind if I do this experiment, since you suggested it and it's really your idea?" And he said, "Well, you can't do it because the instrumental development is too severe." I said, "That's my problem. I know how to do the experiment." And he said, "Oh, you think you can do it?" I said, "Well, yes, of course, I think I can." He said, "It isn't really worth doing anymore." And I said, "Why isn't it worth doing anymore? Nobody has ever looked for such a correlation." And he said, "Oh, well, the correlation must be there. Quantum mechanics has to be all right. But the point is, it's rather boring; you're just going to find out that the gamma radiation is electric dipole radiation. It's going to have a very simple angular correlation; it's going to be proportional to the square of the sine of theta, where theta is the angle between the emission of the alpha particle and the following gamma ray." I was sufficiently simple-minded that I couldn't see that. [Laughter] So I said, "Why are you so sure it's going to be sine squared of theta?" He said, "Well, it might be one plus cosine squared of theta; that's also a dipole pattern." I said, "Why do you think it has to be one of those two?" I had in mind a very simple model of the nucleus Oxygen-16, which was involved in this reaction. It seemed to me that the state in question was very likely to have a different angular momentum than one, and if so, the angular correlation would be much more exciting than a simple one like sine-squared theta or one plus cosine-squared.

I said, "You know, I think it might be an octupole gamma ray with angular momentum-3 alpha-gamma angular correlation, and that would be a whole lot more exciting than what you're predicting." He said, "Well, that can't be, because it was proved long ago by Leonard Schiff in the United States that it had to be an electric dipole." And I thought to myself, even though my nuclear model was very simple, I just don't understand that. So I said, "Do you remember where that reference is, where he published this?" And Devons said, "No, I don't remember things like that. Don't be so lazy; go and find his paper. I can't believe research students have become so lazy." So, of course, I humbly went out and spent hours in the library, trying to find among all of Schiff's papers, where he had published some remark to this effect. I never did find it. But I did, by accident, come across a paper by W. A. Fowler and Charles and Tom Lauritsen from Caltech—whom I didn't know except as names in those days—in which they had been discussing some aspects of the reaction that I was planning to use—fluorine-19 plus a proton emits an alpha particle, leaving an excited state of O-16, which then emits a gamma ray. In the course of their paper, they had said, "We have been informed by Professor Schiff that the gamma rays in question are likely electric dipole gamma rays because the yield of the gamma rays is so strong." I realized immediately that the strength of the gamma ray yield had nothing whatsoever to do with whether they were electric dipole or not. The gammas followed another emission—namely, the emission of the alpha particle—and their strength was completely determined by the strength of the emission of the previous alpha particle. Every alpha particle had to be followed by a gamma ray. And given the strength of the gamma rays, a more interesting problem would be to uncover just what made the alpha particle emission so strong. I don't think that Fowler, Lauritsen, and Lauritsen necessarily subscribed to this remark of Schiff's. I think it was just an offhand remark Schiff had made when he wasn't thinking.

In any case, I was now really determined to do the experiment. It did turn out to be more exciting than I expected. At the end of the first day, I'd measured the correlation at 0°, 45°, 90°, and 135°. And Professor Devons just walked by and saw the preliminary plot as it had developed late in the day. And he said, "Barnes, see, what did I tell you? It's going to look like one plus cosine-squared of theta, just as I said." I countered, "You said it would be sine-squared." He said, "I also told you it was not interesting, anyway." That's as far as I could get the first day, though I felt somewhat chastened.

That night, I went home late, and I laid awake a long time. I was still pretty excited about this experiment and felt that it would still be interesting even if it turned out to be a dipole pattern. And then I realized that my alpha detector and my gamma detector were narrow enough that I could actually do measurements every few degrees and wasn't forced to just do only a few widely spaced measurements. The following morning, I was there as early as I could get into the lab. I checked a couple of angles to make sure that I got the same answers as the day before; and then I started measuring some of the in-between angles. By lunchtime, it was clear that the data were much more exciting than one plus cosine-squared of theta. In fact, it turned out to be not only the first alpha-gamma correlation ever measured, but also a very exciting one. It's what's called a pure electric octupole pattern. It was a very, very strong correlation and had a complicated and beautiful angular correlation. Though it was the work of literally just a few weeks, it actually attracted a lot more attention than my other research, which I had worked very hard on for three years, and which was basically more important. But it was the early days of nuclear physics, and both experiments were strikingly new measurements. That's why two papers were accepted, one on each of these subjects, for this international conference at Oxford, when in fact, they had previously advertised that they weren't going to present more than one paper per person under any circumstances. So I felt pretty good. I felt pretty confident when it came time for my doctoral exam. I had my oral exam at the conference because that was the only way I could get all my examiners together. I couldn't get them together earlier in Cambridge; they all had different things to do. Even at the conference it turned out not to be too easy, but they all agreed that on the last day of the conference there would be a little bit of time between the end of the afternoon session—about five o'clock—and the conference dinner at eight, and they agreed that I could have an oral at this time. We sat down and talked for a little while. They asked a few questions. The only question that really bothered me, to be honest, was a little bit tricky. The fellow who asked it was sitting across from me at the table, where we were all sitting with pads of paper and pencils. It was sufficiently tricky that he couldn't, or maybe just didn't, look me straight in the eye when he asked the question. By pure happenstance, he happened to be looking in the direction of one of the other examiners, who thought that the question had been addressed to him and started to answer it. [Laughter] The examiner who asked the question exploded, "No, no, no! I want the candidate to answer it." Of course, by the time he got this other examiner stopped, I'd had a pretty good hint as to how to go about answering the question." [Laughter]

But I was even luckier than that, because about forty-five minutes into the oral, one of the examiners said, "You know, I heard a rumor that His Majesty's Government is going to throw a cocktail party before the dinner." Apparently, that wasn't a common thing in England; and I guess it was done because they had all these foreign visitors, especially from America! Another examiner said, "Well, hell, we can't miss that! When is it supposed to start?" And somebody said, "Well, I don't have any idea when it's supposed to start—probably six o'clock." [Laughter] So at about two minutes to six, somebody said, "You know, if we're going to get to that cocktail party on time, we'd better finish this up pretty quickly." So that was my oral; it was pretty easy. Life is full of such amusing occasions. It didn't seem to be amusing at the time, really; but looking back, it wasn't that difficult.

After dinner that night, I went out with one of my examiners to visit the pubs of Oxford, called "pub crawling." It's more of a social thing than a drinking thing, to be sure. In one of these pubs, we ran into a bunch of people from the conference, one of whom was Tom Lauritsen from Caltech, whom I'd never met before. The examiner who was with me said, "I'd like you to meet this student of ours, Barnes, whose oral examination we had this afternoon." Tommy Lauritsen was always a very friendly, kind person. He turned to me immediately and said, "Well, I assume you passed." And I said, "Well, Professor Lauritsen, I wish I could tell you that, but the custom here is that they don't tell you at the end of your Ph.D. exam whether you've passed or not. You have to wait until you're notified by the registrar of the university." I knew I was going to be abroad by that time, in Canada. Tommy Lauritsen said, "Oh, that's absolutely unthinkable! We can't have things like that going on. You must tell the student whether he's passed or not." My examiner, standing beside me, looked a little bit sheepish, and he said, "Well, you know, it's the rules." So Tommy said to the examiner, "Well, should we be buying Barnes drinks to make him feel a little better, or should he be buying us drinks in celebration?" And the examiner said, "Well, I guess he should be buying us drinks." So I knew immediately that I'd passed. But that was sort of typical Tommy Lauritsen. It was an accident that it came up in a conversation; but nobody in America would ever have a student wait around for several weeks to find out whether he'd passed his doctoral oral exam, and Tommy wasn't going to let it happen in England either, if he could do anything to do about it.

Life at the University of British Columbia was very different. This was now 1950. It was a relatively new university, and they had just started a graduate program in physics a year before. That wasn't too unusual, perhaps, but compared to nowadays, there was a very great difference in teaching loads, both undergraduate and graduate.

ASPATURIAN: Were they hiring mainly younger faculty for their new department and graduate program?

BARNES: Not really. They did hire some younger faculty, such as I. But the head of the department there, an unusually shrewd gentleman by the name of Gordon Shrum, had a great idea. He knew that after World War II, there would be a lot of scientists in England who would be released from various wartime jobs, and perhaps even more in Germany. Especially in post-war Germany, there wouldn't be any jobs for a lot of these people. So he went every summer to Europe and just hired the best physicists you could find, from all over Germany, Holland, and England, and brought them back to Vancouver. He rapidly accumulated a faculty that was the envy of all the other universities in Canada. A few of them were young people, but most were quite experienced and some were even senior physicists. I was very much the junior man on the totem pole when I got there, but I think everybody had a big teaching load. My teaching load was four courses, averaging three class hours each a week, so I had twelve hours in the classroom per week. That was a kind of standard teaching load in 1950 in Canada. I think the American universities had already started to reduce the teaching load, but that hadn't gotten to Canada yet.

On top of this, I was also put in charge of an undergraduate lab with about two hundred students in it. It was my job to see that the lab functioned, and to replace as quickly as was practical a lot of the experiments from the old days that were just dreadful and really should have been abandoned long ago. But they couldn't be abandoned until new experiments were worked out. So I had this twelve-hour class teaching load, and perhaps a six-hour-a-week load with this lab. Finally, I was also responsible for bringing into operation a Van de Graaff accelerator that they had started to build and hadn't finished. The man in charge of this machine had just gone off on sabbatical to Australia, leaving me to finish it. Looking back, it seems very hard to understand how I managed to do all of those things at once. Clearly, I must not have done them as well as I would have if I'd had fewer things to do.

Also, the salary was unbelievably small, and there was no way that my wife and I could exist on it without her working. She immediately got a job in the Red Cross blood clinic in Vancouver, typing blood. In Canada during World War II, she had been an ambulance driver for the Red Cross, and had then become a blood technician. It wasn't a high-paying occupation, and yet it paid her nearly as much as I was getting in the university. So neither of those were what you would call lucrative professions.

Anyway, it was a very joyful time for us—young, fresh from a degree, taking up a new job, big challenges everywhere, a new accelerator to be brought into operation. A large number of graduate students were waiting for problems; I had the pleasant task of thinking up important experiments for them to work on. By the time the accelerator did operate, they were all ready to get going, doing physics research.

ASPATURIAN: Did this program emphasize experimental research over theoretical? Was there a special emphasis?

BARNES: I don't know that there was a special emphasis any more than in most universities; most physics graduate schools in the country have both theory and experiment, with different students in each. I think that the UBC was trying to become a really comprehensive graduate school in physics, and had recruited several very good theoretical physicists from Germany. There were some extraordinary things in the program, because as I mentioned before, we had all these European faculty members. We also had faculty who had taken their graduate work earlier in the United States and faculty who'd taken their graduate work in Canada. It was a very diverse group of people, and everybody had his own ideas about what the PhD program ought to contain. Right away, among other prolific requirements, they insisted on a proficient knowledge of two foreign languages. In fact, we had that here when I came here to Caltech, until not too long ago. They had set up an unreasonably heavy course-load requirement; they had a series of oral examinations asking students about their preparation, or perhaps taxing their preparation, as well as the final oral exam. It was a very exciting time because there were perhaps twenty-five new students taking this PhD program for the first time. And the program grew more crowded as time went by.

ASPATURIAN: Since World War II had only ended five years earlier, I was wondering if the British and Canadian faculty had any antagonism toward the many German scientists who were brought in?

BARNES: I don't really think there was too much of a feeling of that sort. I couldn't speak for some of the older members of the department; but as a relatively young person, I got along just fine. There was also a Polish professor and a Dutch professor; and there were two or three British professors. I got along with the Germans as well as with everybody else. They seemed to get along with everybody, as far as I could see. No, there wasn't any really bad feeling, I think. We were just a bunch of people who were starting a new program and were overloaded with teaching.

There were lots of things that had to be changed in mid-course. One of the first was the language requirement, because the person who had proposed the two-language requirement had included in his proposal that the proficiency testing be done by that particular language department in the university. That meant, for example, that French proficiency would be evaluated by the French department, and German proficiency by the German department. About one year after I got there, the first attempt of the graduate students to pass the German language requirement came up. Out of the twenty or so physics graduate students who took the German language requirement test, none of them passed, including two students who had been born in Germany and had lived there until they were sixteen or seventeen years old. They couldn't pass this German exam. [Laughter]

It really wasn't much of my business, since I didn't have to take the test. But I felt this was pretty unjust, and I really made a bit of a fuss about it. It emerged that this examination consisted almost entirely of pages and pages of really turgid German philosophy. There's nothing that is more opaque to try to understand than German philosophy. Even after it's been translated into English, it's totally opaque! So it wasn't too surprising that nobody could pass it. I happened to be in the right position at the right moment to make a suggestion—that the examination should be graded by the German language department, since they felt this was their prerogative. But the examination should consist mainly of translation, and the items to be translated would be picked by the physics department, preferably by the student's thesis advisor. We were ourselves supposed to be very familiar with the foreign literature, so it was assumed that we could pick suitable things. That suggestion was accepted and became the rule for all foreign language tests after that. The change materially helped the students; at least the next time the students tried the German test, most of them passed it, even the two whose native language it had been. [Laughter]

There were lots of mishaps along the way, in trying to develop a PhD program *ab initio*. The first PhDs began coming up about 1951. They were mostly a pretty good crop of students. I remember one fellow who, I think, had done a particularly fine problem in radio physics and had a very fine thesis, who came up a few months after I arrived in Vancouver. But the poor fellow hadn't really paid much attention to general physics, at least for several years, and the entire physics faculty—about twenty-five professors—was the examining committee for the student—a committee of the whole! Most of them neither understood, nor cared, about the candidate's thesis problem, and so the discussion and acceptance of the thesis took place quite quickly. But each of them had his own idea of what was important in physics, and insisted on asking a question of the student. It turned out very quickly that the student hadn't paid any attention to even quite elementary physics for years. He tripped immediately they started asking questions, and rapidly became such a basket case that he couldn't answer anything. And of course, as soon as people found he couldn't answer sophisticated physics questions, they started asking him sophomore, and then freshman, physics. But as fast as they could reduce the level of the questions, his state of mind and his ability to function were also being reduced; and it was pretty gruesome. That was something that had to be changed, because only a student with an iron constitution could stand to be questioned by twenty-five professors who could ask questions on anything at all in the world of physics. That experience was an education to me about how not to do things. I tried, when I came to Caltech, to remember this, and how easy it would be for a student to choke. Indeed, I saw a few examples of that at Caltech after I came here.

ASPATURIAN: With the teaching load and the laboratories, did you have time to do much original research?

BARNES: Not a whole lot. I actually did a couple of things that turned out to be pretty interesting, because, while I was a graduate student at Cambridge, I'd learned this totally new technique of angular distributions and angular correlations of particles or gamma rays. And there were dozens of these that one could do with the new accelerator. So, as soon as I got the Van de Graaff accelerator working in Vancouver, I immediately started doing some of that kind of work.

Some other nice things came up at that time, too, but in a way it was a bit of a rat race and I really didn't have a chance to think deeply about these things. I had to produce good ideas for a large group of graduate students who had been building this accelerator for years, and none of them had had a chance to think about experiments or had any experience in thinking about them. Although that was a stimulating and challenging thing to have to do, I felt a little bit like a cat on a hot tin roof perhaps, leaping from one thing to another; and I really wanted to do something with my own hands.

Well, about 1953, after three years at Vancouver, as spring came along toward the end of the quarter, I was beginning to feel pretty washed up. By this time, the accelerator was working, and the graduate students were all working on their problems. The guy who had started the building of it had come back from Australia, ending his sabbatical. I was just exhausted and I felt that I had to get out and do something different for a while. So I remembered several papers I'd read while I was still a graduate student, in a 1948 special addition of *Reviews of Modern Physics*; and some of the papers were by Fowler, Lauritsen, and Lauritsen, of Caltech. So in a vague way, I knew these people. And I had actually met Charlie Lauritsen at an APS meeting in Vancouver. And I had met Tommy after my doctoral exam, as I mentioned earlier. **[Tape ends]**

Begin Tape 3, Side 2

BARNES: The opportunity to meet Charlie Lauritsen arose from the fact that there'd been an American Physical Society meeting in Vancouver, in late '52, I think, or early '53. During that meeting as one of the hosts in the physics department at UBC, I met a good many people, including Charlie Lauritsen, and I liked what I saw when I talked to him. I also remember meeting Ray Herb from the University of Wisconsin, who is really the father of the electrostatic accelerator—not R. J. Van de Graaff, after whom it was named. Herb was the first person to put one of these into a highly pressurized tank to withstand high voltages and to make a stable instrument out of it that could do physics. In Van de Graaff's hands, the thing was largely something for making artificial lightning bolts for impressing the public. Van de Graaff, going back to the mid-thirties, had built these giant open-air machines in an immense building, with one insulating column going up to 2½ million volts positive, and the other one to 2½ million volts negative, and he made huge 5-million volt flashes of lightning between them. But though this was plenty spectacular, it was not of tremendous scientific importance. I don't want to put Mr. Van de Graaff down; he had a clever idea for generating high DC voltages, but it really had no strong scientific impact at the time, until that type of accelerator was "tamed" by Ray Herb.

Getting back to Ray Herb, we persuaded him to stay for a couple of weeks after the APS meeting, because it was just at that point that I had finally got this accelerator ready to run. Since I knew that he was essentially the father of such machines, I thought it would be fun to have him around. Our department chairman agreed, and made the kind of offer that counts the most in this business. He simply walked up to Ray Herb and said, "I'll give you a hundred dollars if you stay for a week or so and help us start this machine up." Ray Herb accepted, and we had a great time together. Of course, he knew how to test our machine instantly and verify that the performance we thought we were getting was, in fact, being achieved. And that was very important.

My opportunity to meet all these famous American physicists at the APS meeting came about because there was a garden party scheduled for all the physics meeting guests and wives at the home of one of the lumber barons in Vancouver. There was a thirsty bunch of guys at this garden party, and it just happened to be a pretty "dry" period in Vancouver—I think you could get a cocktail with a meal, but that was about the only thing you could do in the entire city in the way of social drinking. Several of the visiting physicists had asked the head of the department at Vancouver whether there would be liquid refreshments at the garden party. And the chairman of the department, who was in a sense the local host, said that since it hadn't been advertised as a tea party, he assumed that a garden party by distinction would have real cocktails. But it turned out this was incorrect. Several hundred thirsty American physicists and more than a few thirsty Canadian physicists turned up at this party, and there was nothing but tea and sandwiches. It was a delightful tea party, but I think somewhat of a disappointment to the visitors.

I knew our accelerator was about to operate, and since people seemed to be at a kind of loose end for things to do that evening, I thought it would be rather interesting to invite a few of the nuclear physics people over to our little wartime-housing place for drinks and a chat about nuclear physics. The first five people I invited said, "Yes, we'd love to come to your place," and they wrote down my address. My wife and I had this little, tiny house with a living room about ten feet by ten feet, the kitchen about the same size, and one bedroom and one bathroom. I thought, well, we can probably invite a couple more. Some of them had wives with them and some didn't. So I proceeded selectively and carefully to pick out another couple of people at the party and ask them to come to our place for the evening. But actually, before I completed that task, people I had already invited started catching me in the crowd and saying, "Oh, Dr. Barnes, you know I'm really here with Dr. So-and-so and Dr. So-and-so, and would you mind if I brought them along to your place tonight, too?" And the first couple of times I said, "Well, okay, that would be wonderful; we'd be very glad to have them." But as the minutes wore on at this party, I realized with horror that the number of people coming to our little shack for cocktails was growing exponentially. I realized that my wife and I were in deep trouble, so I caught the chairman of my department at some point and explained what was happening. Of course, he also immediately recognized that a truly major crisis was developing. [Laughter] What he did was to go around and draft all the physics professors from UBC, on whom he could lay hands, to come to our place that evening and bring refreshments and booze in large quantities. And so this thing snowballed into a giant party. I think some 150 people turned up, of which, perhaps, fifteen could have gotten into our living room. But it was a warm time of year, so the guests just spread over the whole neighborhood. And that was where I first met Charlie Lauritsen. [Laughter]

Anyway, at this point, as my third academic year in Vancouver came to a close, I wrote to Charlie Lauritsen and asked if I could come to Caltech as a research fellow. I think research fellows were, in fact, a rather new innovation at that time, though they're actually the rule now. I was delighted to get a letter back, inviting me to come to Pasadena. So my wife and I, and first baby, packed up and drove down here early in the summer of 1953.

ASPATURIAN: Did you know much about Caltech beyond what you'd read by Fowler and Lauritsen and Lauritsen?

BARNES: Not really very much. I'd heard of Caltech; I knew it was considered to be a rather good university in science and engineering. I'm not sure that it had—well, I think that it did actually have—the outstanding reputation then that it does now, but I wasn't as much aware of it.

My real reason for coming here was, of course, that the Lauritsens and Willy Fowler were here. We found a nice little apartment on the south side of Pasadena, just on the boundary with South Pasadena. I came as a research fellow and expected to stay here and work very hard for a year, and then go back to Vancouver. Well, things were going so well by the time that year was over that I wrote to Vancouver and said, "I'm in the middle of some things that I think are quite important. Could I take leave for another year?" They didn't object in the slightest degree, so I stayed for a second year, as a senior research fellow, having pretty well committed myself to going back at the end of it.

Lee DuBridge was Caltech's president by then. I knew Dr. DuBridge by his reputation as a physicist as well as by the enormous reputation he had acquired during the war as director of the so-called Radiation Lab at MIT, which was the principal place where radar was developed in the United States. I knew a lot about the British radar development because a lot of the British radar people had come to join us in the British-Canadian Atomic Energy Project late in World War II, but I didn't know much about the American radar labs, except that, of course, they had produced a lot of wonderful radar sets, especially shorter wavelength radars, and contributed very strongly to the Allied effort in World War II.

ASPATURIAN: When you first came here as a research fellow, what projects did you get involved in?

BARNES: There were three electrostatic accelerators already in operation when I came here—all of them now deceased, long ago. [Laughter] But all home built. I immediately started working, first on the earliest, which I called the "2-million volt machine." It really wouldn't quite go to 2 million volts. I also very shortly started working on the one that we came to call the "3-million volt machine." These were the two larger ones of the three that had been built at Caltech. I see by my notes that the first publications from that period were actually pretty exciting, because the scintillation counter—a completely new tool—had really just come into use as a method of detecting gamma rays. Being rather new in that game myself, it was natural for me to take it up and learn how it worked. One day, when I was looking at the gamma rays that come from proton bombardment of fluorine, I remembered that there had been some earlier weak indication—in an experiment done by Ward Whaling and a Swedish physicist by the name of Katarina Ahnlund—that possibly there were some totally unexpected low-lying excited states in the nucleus fluorine-

19. What had been studied previously, and what was quite familiar to me from my work at Cambridge, were the high-energy, band 7MeV, gamma rays that you get from bombarding fluorine with protons. But just for the fun of it, I kept turning the gain on my scintillation counter amplifier up, and trimming the electronic constants so it didn't overload, to see if anything new was really there. And I was just startled out of my skin to see—it just immediately popped into sight—incontrovertible evidence of new low-lying states in fluorine, with excitation energies of about 100 and 200 keV, that had only been a kind of suspicion before. If anybody else had taken the same simple steps that I did, which was just to look for electromagnetic radiation with high gain on a scintillation counter, they couldn't have failed to have discovered these low-lying states.

That was a new and unexpected thrill. Suddenly we had this fluorine-19 nucleus that had two excited states so close to the ground state that nature might have easily made either of those the ground state. That is due to a very subtle interplay of the forces that set the order for these three states. In fact, the state near 100 KeV even has the opposite parity to the actual ground state. I embarked on a series of measurements, first with Willy Fowler and Charlie Lauritsen and a graduate student by the name of Peterson, to exploit these two unexpected states by studying the inelastic protons that are given off when you excite the nucleus. If you bombard it with protons, the protons come back with a little bit less energy, corresponding to the energies of these excited states. We studied them with the magnetic spectrometer system that Fowler and Lauritsen had worked on for so long. That was immediately extremely fruitful. But we also had a French research fellow in the lab by the name of Jacques Thirion. He and Valentine Telegdi—another research fellow who had overlapped slightly with me for a couple of months, but by this time was gone—had invented a simple, new method of measuring the lifetime of very short-lived nuclear states. So one of the first things that I did—since I could see these states so clearly with the scintillation counter—was to get together with Thirion to measure the lifetimes of these states. That turned out to be an exciting and amusing experiment to do, but it wasn't difficult. It was brand new; there was really no competition. Nobody else in the world suspected there were any states there. We had the field to ourselves, and it was really quite fun.

Meanwhile, a couple of other people in the lab—Rubby Sherr, from Princeton, who was here as a visiting professor, and Bob Christy, who was then our in-house theorist—collaborated to look at the Coulomb excitation of these states. That was a completely new electromagnetic process that had only been hypothesized about the year before. And the combination, particularly of the Coulomb excitation work and the life-time work that I did with Thirion, uniquely identified both the spins and parities of these newly discovered states. Remarkably enough, for the first time in any nucleus that had been studied up to this point, we found that there was an excited state of the fluorine nucleus that just missed being the ground state by only a hundred kilo-electron volts, and had the opposite parity to the ground state, as though it was formed from a totally different structure of the nucleus. With such a subtle interplay of forces, it could easily have been the ground state of the fluorine-19 nucleus. So we rushed into print with a whole bunch of letters, covering these various things. I had the satisfaction of getting a letter from my former colleague and supervisor in Cambridge, Denys Wilkinson, saying, “Your preposterous suggestion that the first excited state of fluorine-19 has odd parity has just sent me into a spin. I don’t believe it for one minute.” Denys subsequently wrote to me and said, “I have actually, in fact, just confirmed what you say, that this state has odd parity.”

This was one of the most exciting things that happened to me during my first two years at Caltech—the discovery that fluorine-19 had two entirely unexpected low-lying states, and that one of them even had a parity different from the ground state. This was a totally new phenomenon. In retrospect, it’s now completely understandable how this can happen. But it was certainly neither known nor expected at that time.

ASPATURIAN: Did this, then, prompt a search for similar phenomena in other elements?

BARNES: We secretly and quietly looked at a whole bunch of other nuclei to see if we could find such low-lying gamma rays in other nuclei. We found a few that were nearly as low-energy, but nothing that generated the same level of excitement and total unpreparedness that we’d had in the fluorine case.

I also had the pleasure of doing some other good experiments during that time. They were, I think, good nuclear physics, but probably didn’t cause too many people to raise their eyebrows.

In the summer of 1955, I went back to Vancouver feeling very happy that I’d had an extremely productive time. I was now expecting certainly to spend the next few years of my career at the University of British Columbia, if not my whole career. I was quite happy at UBC;

I thought it was a rather nice place, and it is certainly one of the more beautiful areas of the world. By this time, the teaching load had also dropped by about one course. [Laughter] That was some improvement, but still not a very large one. Although I didn't have to teach when I was at Caltech, I was invited to teach during my second year there if I wished, and I had decided to do that.

But I was just barely back at UBC when, about the first of December, I got a letter from Willy Fowler, saying, "Dear Charlie: Charlie and Tommy Lauritsen and I have been talking about the productive times that we had together here, and how well things went with our research program. And we think it would be rather nice if you'd come back again. Would you accept a research appointment?" I wrote, "Willy, that sounds very attractive to me. I have to warn you or tell you that UCLA had expressed some mild interest in me last year. Of course, I haven't heard anything from them since then, so obviously nothing is going to come of it, but I just thought I should mention that to you." I guess Willy, being wise in the ways of this business, interpreted that—not that I was putting pressure on him, which I certainly wasn't—to mean that maybe he shouldn't waste any time doing something more than an inquiry. Because just days after that, I got a formal offer from Caltech. In the same mail, I also got a formal offer from UCLA. That put me in a terrible spot, because when they had first inquired about it during my second year at Caltech, I hadn't really put them off or said no. And then, suddenly, I had these two offers. I realized there was going to be a problem, because it was a time—this was 1955—when physics faculties were beginning to expand in response to the growing enrollments and improvements in funding. I felt pretty badly about it, and I worried that there might be some hard feelings one way or the other. That's the last thing I wanted to have on my conscience—or on my record, for that matter. So I told both parties that I was coming down to Pasadena and to Los Angeles to try to work this out. Maybe that was a naïve, simple-minded thing for me to do, but I didn't know any other way to deal with it at that time. The day after Christmas, I got on a plane in Vancouver without any reservations, and I flew down to Los Angeles, all the way on stand-by flights. I came straight to Pasadena, as soon as I got here, because that was the area I knew best.

In the morning, the first person I went to see was Willy Fowler, of course, since he'd sent the letter to me. And then immediately to the division chairman, who at that time was Bob Bacher. I explained to him that I realized that it might look a little bit funny that I was in this position, and I wasn't trying to hold them up or anything of that sort, but that I felt that I had a serious problem. So I talked with Bob Bacher for quite a long time. And then later in the day, I went over to UCLA and talked to the physicists there, and found out that there was perhaps an undercurrent of feeling that maybe I hadn't been straightforward enough with them; and maybe they felt that I had made more of a commitment to them than I thought I had actually made. In fact, though I had been noncommittal when the subject had come up the year before, I couldn't recall actually making any commitment at all to the UCLA physicists.

I must say that both groups of people behaved absolutely magnificently. They phoned each other and talked to one another. And they both agreed to start from scratch, to assume that no commitments had been made by anybody, or to anybody, and see what I would like to do. Though it was extremely hard for me to make a decision, since I was really strongly attracted to both places, I chose Caltech. I knew it better; I knew what I'd be doing; and I knew it was going to be an exciting time ahead. Though I'm sure I would have been welcome at UCLA and probably very happy there as well, I chose Caltech.

I came back here in the summer of 1956—of course, I had to finish the academic year in Vancouver. And I've been associated with Caltech ever since.

ASPATURIAN: I'd like to ask you to talk about your early impressions of some of your colleagues.

BARNES: My colleagues mostly consisted of research fellows in Kellogg—in fact, research fellows throughout physics. My immediate faculty associates were, of course, Charlie and Tommy Lauritsen and Willy Fowler and Bob Christy, who, you will recall, was in Kellogg at that time. And they were indeed a wonderful group of people. When I first came to Caltech in 1953, I think everybody was still in a state of some euphoria over new opportunities that were opening up rapidly in physics, partly driven by advances in nuclear physics that had happened during World War II, and partly by improvements in accelerators, although not all these came after World War II. Mixed in, perhaps, was even a little bit of proudness and pleasure that World War II had been completed, and that the United States had come out of it rather well. Certainly there were lots of casualties, but the number, considering how serious the war was, was smaller than had been feared originally. To say a kind of euphoria still permeated people into the fifties might be a little too strong, but perhaps it isn't. I think people felt that they had really given their best efforts in the war and that they had been rewarded by the defeat of what everyone felt was a pretty virulent kind of poison in the world. Everybody felt pretty good about it.

The department was also smaller in those days, and there seemed to be lots of partying. We got together among ourselves often. Every Friday evening Kellogg held a seminar in which a lot of outside people used to participate, and these were a great success, too. These were group seminars on various topics in nuclear physics, sometimes given by visitors, sometimes by in-house people, occasionally by people from elsewhere on campus—maybe astrophysics. Astrophysics really didn't exist. There was astronomy, but astrophysics as such was still pretty non-existent. These seminars had been an established part of life in Kellogg, going back to the 1930s, from what people told me. Originally, I think they used to meet at Charlie Lauritsen's house, sit around the outdoor fireplace he'd built, and talk physics and astronomy and everything else that came into their minds late into the evening. They were a very congenial group of people among themselves and with others, and also a bunch of people who were very interested, even though they joked about it, in the pursuit of knowledge and of new information in science.

Bob Christy was a particular education to me, because every single morning he would come into the lab and look over my shoulder—and anybody else's who was doing experiments in the lab—and would want to know what we were doing. And, of course, it was always a pleasure to explain our work to him. Remarkably often, he would come back the next day and say, "You know, that's really great, because it shows so-and-so, and I think we should do the following." We would pursue this subject, and a paper would emerge within a week or two. So things were happening very rapidly. Part of the credit for that, of course, must go to Bob Christy, because he is a very good general theorist and has had a lot of experience in many fields of physics. He was a particularly effective communicator with us. I think it served him well, too, because in those days, he knew what was going on in the lab before anybody else did, because he saw it as it was developing. In nuclear physics, he was really ahead of the world. He wasn't the only one, but he was one of the very best in the world.

ASPATURIAN: Did he ever reflect on his experiences at Los Alamos?

BARNES: I don't really think I'm qualified to talk about how Bob felt about his Los Alamos experience—at least at that particular time. Bob is quite a serious fellow, as most people know, and was always concerned about the implications of physics and what he did. But I don't think I ever got into any intimate kind of discussion with him about his feelings about the development of the bomb. I knew before too long, although I certainly don't think it was the general public knowledge—it is now—that Bob had made a particularly unique contribution to the Los Alamos effort. There were some serious problems about how to make some of these bombs work; and, I guess, all I want to say is that Bob had contributed a very original and brilliant idea about how to make one of them function. So, as well as coming here with this enormous reputation as a general physicist, he had actually made a major contribution on a number of occasions at Los Alamos.

ASPATURIAN: Could he also do instrumentation?

BARNES: No. But Bob understood instrumentation. In fact, he was unusual among theorists in that respect. If I'm not mistaken, while he was still a master's student at UBC, before he went to work with Robert Oppenheimer at Berkeley in the pre-World War II days, he had actually done a problem as a master's student, with significant experimental connections. He certainly understood experimental equipment, and in fact, one day he embarrassed [Professor Physics Ralph] Kavanagh and me because for two days we had been hunting a pesky leak in our vacuum system that was really holding us up. We'd been going over and over the same areas, trying to find this leak, and we just couldn't see it. Bob came and he sort of looked at us and laughed a little bit, and said, "Well, what have you done? Where have you looked for it?" Partly perhaps to make conversation, and maybe partly to humor him a little bit, we told him in detail the many places that we had looked for it and how we had verified that there was no leak there, and yet there was clearly a leak in the system. Bob looked thoughtfully at us for a minute and he said, "Well, obviously the leak is in there." What he pointed to was something that we hadn't even thought of, something totally internal to the system that couldn't be tested from the outside world. What Bob had done actually was to put more faith in us than we had in ourselves. He had just simply said to himself, "All right, these guys have been over and over this. Obviously the leak isn't where they'd been looking; it has to be somewhere else. There's only one other place, where they couldn't look." He was smart enough to think of it. We did immediately think of an indirect way of checking this hypothesis, and he was absolutely right. Bob certainly also had an outstanding grasp of what was possible in the experimental physics, and he understood what experimentalists did. You know, that gave him and us both an advantage. He could get results long before they were published by talking to people, and he could also provide guidance and help to the experimentalists in mid-course as they were doing an experiment. It was a highly valuable and enjoyable association between Bob Christy and the rest of us. **[Tape ends]**

Begin Tape 4, Side 1

ASPATURIAN: Can you tell me a little bit about the Lauritsens, father and son?

BARNES: Well, we all got along very well. They were certainly very congenial people. And the relationship with them was based both on a kind of personal regard for them as people, and also a respect for their abilities as physicists.

ASPATURIAN: I was wondering what qualities Charles Lauritsen had. He seems to have been, as well as a good scientist, a very good administrator, and that's a rare combination.

BARNES: Well, I think he was. I didn't see too many signs of Charlie actually administrating the lab after I came here, partly because he had a quiet way of persuading people to do what he thought should be done, and partly because he was already very busy doing things in Washington and other places, as well as working on national questions when he was here. I think Charlie was nominally the head of the lab when I first came here, but I think Willy was the operational head for quite a while, and then Tommy Lauritsen after Willy decided he didn't want to be head of the lab any more, in an administrative sense. But it is true that Charlie was certainly a very nice human being. I think he understood the importance and value of people getting along well, personally as well as scientifically. I can remember once we were busily doing an experiment—Charlie, Tommy, Willy, and myself. My contribution to that work was a fairly large fraction of it, because I came along just enough later than they did in the pipeline of physics to have relied more on electronics. In fact, I'd worked on electronics in the Canadian atomic energy project, so I played an important role in that experiment because it involved the development and use of a lot of electronics. But I can remember at some point when there were long hours of data-taking; and I guess Tommy and I got just a little bit scrappy. It probably started innocently enough when we were baiting each other about something or other—being humorous—but it got just a little bit sharp. I can remember wondering where this was going to go. I didn't like the sound of it too much, but I really didn't have to worry about it because before it got very far, I remember Charlie leaning over and grinning at us and saying, "All right, try to get along, fellas," and that was the end of it; it was finished. And he realized that the time to stop it was before it got serious. It was a minor event, but he just had a natural feeling for things like that—maintaining a smoothly operating group effort. I'm sure that he would be equally effective in his work with the Presidential Science Advisory Committee, keeping his colleagues focused on the important items.

So he was a pretty wonderful person. He certainly was very good to me. Whenever he had visiting physicists come to visit him, which was quite often—senior physicists from everywhere in the country, like [I. I.] Rabi or dozens of others I could mention—he nearly always would have a small dinner for these people, and then they would sit outside by the outdoor fireplace in the summer, sometimes almost till morning—certainly often up to midnight or even later—just talking. And he very often invited me to join these parties, although there was no particular reason why he had to do that. So, of course, I did have the opportunity to meet a lot of people I would otherwise never have met. But he was like that; he included people, and so did his wife, Sigrid. I recall him taking me aside shortly after I came to Caltech as a postdoctoral research fellow, to discuss the future research program in Kellogg. He pointed out the historical strength of the lab in designing and building precision equipment such as electrostatic analyzers, double-focusing magnetic spectrometers, and beta spectrometers. He then pointed out that there would be a national effort in plasma physics, with ample funding, to advance nuclear fusion research aimed at large scale energy production. He then asked me if I thought we should transfer to that area. I answered that I was more enthusiastic about the nascent nuclear astrophysics area recently opened by Willy Fowler. It was typical of Charlie that he included us all in making such an important decision, even a beginning research fellow.

ASPATURIAN: Was there a lot of interaction between Kellogg and the other branches? Was there actually then a division of physics and astronomy?

BARNES: Yes, I think the divisional structure of Caltech is quite old. But one of the things that was decided even before I came to Kellogg, and was certainly continually emphasized while I was here, was that Kellogg, although in some ways it was a kind of club and it had a family feeling about it, was not a separate institute. We stressed that repeatedly and we never tried to call ourselves an institute within the Institute, as indeed some existing programs on the campus do now. We always insisted that we were a regular part of the physics, math, and astronomy division. There were suggestions, occasionally from junior people, that maybe there should be a director, but there never has been an official "director of the Kellogg Radiation Lab." There have always been one or more principal investigators of the contract that we had from the government, but these were purely financial positions of responsibility that were forced on us by the National Science Foundation or whatever funding agency we were with that requires a principal investigator. But the position of director of the Kellogg Lab, per se, formally does not exist. I can remember being consulted on that point by both Charlie and Willy, even when I was quite new here. They included people right from the beginning without concern for rank or seniority, and I certainly shared their opinions on that matter.

ASPATURIAN: Was there a feeling that having a director would over-institutionalize the place?

BARNES: Probably. But I think it was more that they felt that Caltech was a very special place because it was small, and people got to know each other, and that it would be a shame under the circumstances to start compartmentalizing Caltech any more than it already was by the division structure.

ASPATURIAN: At that fairly early point in your career here, was there much association between you and the astronomy or astrophysics faculty. Were there many high-energy experimentalists here yet? Or was Kellogg basically the center of experimentation and then you had the theorists and a nucleus of astronomers?

BARNES: There was quite a bit of association with other people, because in a way, the high-energy physics group grew out of Kellogg. Charlie and Willy were among those who brought Bob Bacher here, first as professor. He told me—I just checked with him the other day on that—he had been invited to come here, either as professor of physics or as chairman of the division, whichever he wished. And I guess he elected to be chairman of the division, eventually, if not immediately. One of the objectives in bringing Bacher here was to have a senior physicist who would have a special interest in, and be in charge of, developing a high-energy program. Charlie and Willy were among the people who pushed most strongly for getting Bob Bacher here. I guess at about the same time, we also got Bob Walker, who had just finished his PhD with Boyce McDaniel at Cornell. When he came out here, just before I came, the first research he did was in Kellogg, because there was nothing yet in high-energy physics here. Also, one of the other persons who played a leading role in starting up the high-energy physics program here, was Alvin Tollestrup, who was a PhD from Kellogg. So the nucleus of the high-energy physics program was Alvin Tollestrup and Bob Walker from Cornell, and I should include Matt Sands, who had come here, I think, also from Cornell. Certainly, we also got Richard Feynman about that same time from Cornell. Caltech obviously must have devastated Cornell over a period of a few years with all of these acquisitions. Anyway, I know that Bob Walker came at the time Bacher did explicitly for the purposes of starting up a high-energy physics program, which, as I say, was being sparked and pushed by Charlie Lauritsen and Willy Fowler, because they felt that we, Caltech, had to embark on high-energy physics, and that the way to do it was to get some new people here; this was absolutely the correct thing to do. These people all came out of nuclear physics originally, when there was no separate high-energy physics.

ASPATURIAN: Were you ever tempted to move over into that realm?

BARNES: Yes.

ASPATURIAN: So to some extent, your work has been on the border?

BARNES: Well, to some extent. I can remember Matt Sands and Alvin Tollestrup, at different times, made it clear that if I wanted to make that transfer, it would be instantaneous. Though I felt very honored; both of these invitations came at times when I was in the middle of doing something I thought was very important and really couldn't see that I could move just then. If I had made the transfer, life would have been different, but I couldn't possibly imagine whether it would have been a good thing or a bad thing for me, or for physics even, whether I did that. But certainly I thought about it. And the two subjects, although they tended to grow apart more and more as time went by; they may, as I'll say later, end up by growing together again.

ASPATURIAN: There's a sense that you can argue that a lot of high-energy physics is technically an aspect of nuclear physics.

BARNES: Well, we could go into that subject. The basic difference now between high-energy physics or particle physics and what we'd call intermediate or high-energy nuclear physics is simply the targets that people use experimentally. The particle physicists find that nature is sufficiently complicated that they need to bombard protons with protons, and they make plenty of new particles that they don't yet know how to really understand or assimilate. They do bombard deuterium sometimes, which is actually a nucleus in the sense that it has two particles in it—a proton and a neutron—simply because there is no such thing as a pure neutron target. Now the nuclear physicist in this developing area of intermediate and high-energy nuclear physics operates to some extent in the same energy range of traditional particle physics—mostly the lower part of it. The only difference is that they don't feel bound to bombard only protons. They bombard anything they feel like. [Laughter] Interestingly enough, the prediction was made long ago that this was not a useful thing to do, because all people would find was that bombarding a nucleus at high energies just gave you the same answers as bombarding the same number of protons and the same number of neutrons separately and adding the results together. And it turns out that that's not correct; we know that now. So naturally, nature has once again told people not to jump to conclusions. I think the subjects are tending to grow together somewhat, but how that will develop in the future, of course, we don't really know. It may actually be that ultimately we won't have two separate terms for the disciplines. Often in Europe, they just use the phrase "nuclear physics," implying both particle physics and traditional nuclear physics in the same term. It's just different branches of the same subject. Sometimes they use the term "subatomic physics" for either or both.

ASPATURIAN: I guess the other branch I was wondering about was astronomy. Was there much connection during the middle fifties?

BARNES: I didn't have very much connection at that time in any detailed way with the astronomers. There had long been contacts between Willy Fowler and the Lauritsens, and some of the observational astronomers, going back to the 1930s. Of course, you have to remember that the first really significant connection that I would call nuclear astrophysics occurred with the publication of Hans Bethe's papers on the burning of hydrogen, and von Weizsäcker's papers, similarly on the burning of hydrogen.

ASPATURIAN: What year was this?

BARNES: These were published in 1937 and 1938. But, of course, when Bethe's paper was published, he was able to make quantitative calculations of hydrogen-burning, first by what we call the CNO [Carbon-Nitrogen-Oxygen] cycle, and then by what we call the proton-proton chain of reactions, which we believe is the most important in producing the sun's energy. The reason Bethe was able to make these calculations is because people had already been studying exactly those reactions, or at least most of them, here in Kellogg and to some extent in other places. I don't think Bethe really was thinking of stars when Kellogg did the first work in the early 1930s on protons plus carbon-12 and other light nuclei. But certainly by the time Bethe's articles were published, the Kellogg group realized fully that there was a very direct application of their work to astronomy and astrophysics.

Another group that was spawned, in a sense, from Kellogg, was Kip Thorne's group. We partially supported Kip Thorne financially for a while until he was able to get his own grants. So, for a while, Kip was part of our lab and he continued to have his papers and his preprints prepared in Kellogg, for a long time after he had started his own separate project. There wasn't all that much astrophysics at Caltech at the time. Astronomy was more pure observational astronomy and less astrophysics than it is now. I personally think—maybe this is parochial—that it's been very good for the development of astronomy that it's been taken up more and more by physicists, and that astrophysics has boomed and is still booming, because the affinity between the subjects is very strong. It means that the character of astronomy, of course, changes under this impact, because, whereas astronomy at one time was almost purely an observational science and attracted many people who were impressed by the beauty of the universe in some abstract way, it now also attracts a lot of people who are much more quantitatively oriented and who are trained as physicists. In fact, even as early as World War II, some astronomers were very good at a wide range of instrumentation. Some of these made the same kinds of major contributions to instrumentation during the war that the physicists made, such as streak cameras for very high-speed photography, and things like that. So, astronomy has been changing during this period, too.

But I didn't have that much contact with the astronomers when I first started here. And indeed, most of the experiments I did when I first came here weren't very directly connected to astronomy.

ASPATURIAN: Did you also have teaching responsibilities?

BARNES: During the first two-year visit to Caltech, I taught in the second year—1954-1955—though it was not required. Of course, I had already taught three years at UBC. I came back in 1956 as a senior research fellow—better salary, but the same rank as I had previously. I stayed as a senior research fellow for two years, but because I had already taught, I was invited to teach again as soon as I got here. I couldn't be the official advisor of graduate students, but I was, in fact, already directing graduate students when I came here first as a research fellow—helping with the direction of the graduate students. To some extent, Professor R. W. Kavanagh—who most people now call Ralph Kavanagh, and I have always called Bill Kavanagh because that's how I first knew him—was my first graduate student here.

He got his PhD degree under the official supervision of Willy Fowler, I believe; and he actually completed it the year I was back in Vancouver. So the question didn't even arise. But a significant part of his work was done in association with me when I first came here. And then when I came back, I also had the good fortune to work with two or three of the graduate students, some of whom later became my official advisees, but I was already advising them before I was officially allowed to do it. So I was teaching and advising graduate students all this time. I was made an associate professor in '58.

About the most exciting thing that I got into when I came back from Vancouver was the weak interaction development, because the whole theory of weak interactions was being rewritten.

ASPATURIAN: Could you place that in a little bit of historical context?

BARNES: Well, there was a gradually accumulating body of knowledge about some weak interaction situations that people felt uncomfortable about, where things didn't fit together too well. And I really didn't play much of a role in that before this time. But I guess about the mid-fifties or slightly after the mid-fifties, there was a famous paper by [T. D.] Lee and [C. N.] Yang on what was called the tau-theta puzzle. It had been discovered that particles which we now call kaons or k-mesons could decay into two pions or into three pions. Apparently there were two different kinds of particles, since two pions and three pions have opposite parities. One of these was named the tau meson and the other was named the theta meson. The more accurately that people measured the masses of these two kinds of particles, the closer their masses seemed to be the same. And it turned out that they must therefore be the same particle. Eventually, Lee and Yang suggested that these weak interaction decays were caused by a universal weak interaction that had the property of violating parity conservation—maximally as it turned out. A lot more went into the picture because people much earlier than this had recognized that in the theory of beta decay—which stemmed originally from Enrico Fermi—there were five forms of the interaction consistent with special relativity; for certain beta decays, called allowed decays, there were just four—scalar, vector, axial-vector, and tensor. From the absence of certain interference terms in beta spectra, it was recognized that these four kinds of beta decay interaction were restricted to occur in two groupings—vector with axial-vector, or scalar with tensor. No considerable effort over several years, with a variety of different beta-decaying nuclei, was expended to track down which interaction forms actually occurred in nature. One of the ways that people attacked this problem, long before Lee and Yang's paper, was by measuring the angular correlation between the emission of a beta ray and its accompanying neutrino. They couldn't, of course, detect the neutrino, but they could indirectly detect where it went by looking at the recoiling final nucleus, resulting from the beta decay. It was a technically very hard experiment to do because the energy of the nucleus is quite small. But, in principle, they could do this. It was one of the main activities, certainly shortly after World War II—I guess there may have been some attempts to do this in the late 1930s. But the picture was still pretty wide open until somewhere around 1955, when a team from Columbia working at Brookhaven did an experiment measuring the beta-neutrino angular correlation in the decay of helium-6, which had to be either a tensor or an axial-vector interaction because of the particular angular momenta involved. They published a *Physical Review* paper in 1955, which nobody contested apparently, proving conclusively that the interaction was tensor. About the mid-fifties, a physicist by the name of Allen measured the beta-neutrino angular correlation of neon-23, what we would call a mixed beta decay that could have two of the interactions in it, one from each group. Allen was able to show clearly that no combination of tensor and scalar could give an angular correlation in agreement with his result. It was then clear that either the Brookhaven helium-6 result, or Allen's new result, must be wrong since we already knew that the beta decay interaction in Allen's experiment could only involve tensor with scalar, or vector with axial vector. So that much was already known and was folded into the synthesis by Lee and Yang. And in fact, it was particularly the vector-axial-vector—VA—character of the theory that was exploited and proposed by these people that led eventually to the maximal parity violation. But there were a number of people in the country working on the actual detail of the theory, and perhaps abroad as well, that I wasn't aware of.

The subject was very actively pursued here at Caltech by Murray Gell-Mann and Dick Feynman. In fact, Dick had apparently long wondered about the validity of the common practice of assuming that parity simply had to be conserved as the starting point to any theoretical attempt to formulate a theory of beta decay. But I guess he hadn't previously thought seriously of contesting this common assumption until it was suggested by Lee and Yang. But the V-A theory—that is, the beautiful theory that was developed by building the vector and axial-vector interactions into the theory—was fundamentally characteristic of the theory developed by Feynman and Gell-Mann, and made a most convincing and most beautiful theory.

ASPATURIAN: It must have been an interesting collaboration, because they have kind of different styles.

BARNES: Yes; although, up to that particular time they seemed to get along well enough. They haven't been so close since their joint work on the theory of beta decay.

In any case, the whole redoing of the theory of beta decay was developing in real time right here on this campus. Remembering exactly the order in which things happened isn't too easy, though it is well covered in the literature. But I can remember that a number of things were more or less going on simultaneously. I happened to have what I thought was one of my better inspirations during this period. Because I already understood about the beta-neutrino angular correlation, it suddenly occurred to me that if there was a non-isotropic angular emission of betas from polarized radioactive nuclei, as predicted by Lee and Yang, following from a violation of parity conservation, the beta rays would also have to be polarized. And I don't believe that had previously been suggested.

We had three very smart young theorists in the lab at that time: Aage Winther from Denmark, Kurt Alder from Switzerland, and Berthold Stech from Germany. They shared a basement office here in Kellogg. The one I probably knew best was Aage Winther, who's been back here many times since. In fact, I think that about this time a group of people, including Wu at Columbia and a team at the National Bureau of Standards Hayward, Hoppes and others—had just verified experimentally Lee and Yang's prediction that in the beta decay of polarized cobalt-60 nuclei, there would be a forward-backward asymmetry with respect to the axis of nuclear polarization—a direct confirmation of parity violation. I suddenly realized that if that was really true, and if—as was verified earlier, there would be a non-isotropic beta-neutrino angular correlation—the betas would themselves, of necessity, be polarized. I remember suggesting this to Aage Winther one morning. I said, "Maybe that's too simple-minded, but it seems absolutely unavoidable to me." And Aage said, "Well, it probably isn't true. It probably works out some way so that the betas aren't polarized at all when you do the whole calculation properly." And I thought, "Oh, that's too bad." And I sat there trying to figure out how the betas could avoid being polarized. About two hours later, Aage came rushing back up to the lab where I was working and said, "Yes, we have worked out that the beta rays will be polarized, and they'll be polarized to the extent v over c. And what's more, we've worked out the complete theory of it." These three guys understood the theory of beta decay, even in those more primitive times, so well that in two hours they'd worked the whole thing out. And I realized, "Boy, we'd better get busy and verify that." I started taking steps to try to get a strong pure beta source and get a method of measuring beta polarization set up. Felix Boehm was very helpful. He arranged to get a very strong thulium source made for me in the next few months at one of the new reactors in Idaho. Unfortunately, I just didn't move fast enough, because eventually, the three theorists published their paper in which they predicted that beta rays would be polarized. And electrons would be polarized one way and positrons would be polarized the opposite way. I was naive enough to think that I still had a good chance of actually measuring this before anybody else could. But I didn't bargain on the fact that there was a team of several people at the University of Illinois at Urbana who also heard about this prediction quite early and certainly went to it much faster than I did, even using cobalt-60, a potent gamma emitter. They actually were the first people to discover that betas were actually polarized in beta decay. So it was a disappointment to me, but it was a lesson.

ASPATURIAN: Did you receive credit for the idea in the letter?

BARNES: I never looked to see, I was so disappointed. I didn't think it was that important. But it was an education, though, and I should have been a lot more energetic in pursuing it, and I should have just plain dropped whatever else I could drop. Because, just as I was beginning to see the polarization—I think the very first results—I got a preprint of a letter from the University of Illinois, which said they had actually done it. Well, I had to admit that I had been thinking along good lines anyway, but that was little compensation for not being the first to demonstrate experimentally that betas were polarized.

Also, as noted above, during this period, people were saying, "Well, there must be something wrong with the earlier Brookhaven measurements that were made on helium-6." While that didn't really bear directly on the subject of whether parity would be violated or not in beta decay, it was essential to the arguments that were being made by Feynman and Gell-Mann, that the proper theory of beta decay was a vector, axial-vector theory. Thus the measurements that claimed that the helium-6 decay was tensor had to be just plain wrong, even though the Brookhaven helium-6 experiment was a high statistics experiment, done by competent people.

Well, I sat down with a student of mine, Don Kohler, and we looked very hard at the paper that had been published. It was several years after everybody had really accepted the helium-6 paper as truly definitive. We suddenly realized that the authors had not supplied any detail in the paper as to how they had actually reduced the data. They'd given a very general theoretical expression for interpreting the data, but they didn't say how they actually evaluated the data. And I realized, by looking at the curve shown in the paper, that the power of their experiment to discriminate between the two possible alternatives—tensor and axial-vector—depended on only one of the angles measured—that is, the measurement at 180 degrees. They'd measured, as I recall, beta-recoil coincidences at four or five angles, but the difference between the predictions for these two interactions was slight for all of their measurements, except the one measurement at 180 degrees. If they'd been able to measure at angles less than 90 degrees as well as at the backward angles, they probably would never have gotten in this trouble. But because of the nature of their experiment, they could only measure angles from about 110 to 180 degrees, all in the backward hemisphere. And only one of these, namely the 180 degree measurement, really had a substantial ability to tell the difference. Then it dawned on me that there was one place they might have made a ghastly serious mistake. At that one angle, at 180 degrees, if the helium-6 gas hadn't been pumped away as fast as was evidently assumed by the experimenters, it was possible that the effective amount of helium-6 was much greater for that 180-degree measurement than for any other angle. And that would have made the number of beta-recoil coincidences erroneously too high at that 180-degree point.

So I suggested to Don to calculate the pressure of helium-6 in the region where it seemed possible to me that the experimenters had erroneously assumed that the pressure was essentially zero. And, sure enough, it turned out, within an uncertainty of about 30 percent, as well as we could calculate the pressure with the information on hand, that there was a considerably greater amount of helium-6 that would produce coincidence counts for the 180 degrees measurement than at any of the other angles in their measurements. It completely explained why they got the erroneous result of a high reading at 180 degrees, and were then driven to conclude that the beta decay interaction was tensor.

What happened after that actually was a bit foolish on my part—this is in the nature of an anecdote more than anything else. Kohler and I, in retrospect, certainly did the wrong thing. What we should have done was to have immediately written a *Physical Review* letter and sent it in for immediate publication as a possible explanation of the discrepancy between the way the theory was developing in people's minds at the time, and the helium-6 paper that had been published several years before. Instead of doing that, I figured it would be good experience for my graduate student to get a bit more involved in this business. So I got him to write a letter to C. S. [Chien-Shiung] Wu, Madame Wu, at Columbia. The reason for the letter to her was that it was Columbia people who had done this experiment at Brookhaven. And although she wasn't an author on the Brookhaven paper, she was thanked for advising them in the experiment. So I got Kohler to write a letter with our helium-6 pressure calculations to Madame Wu, asking if it was really possible that this error had actually happened, pointing out that this would be the likely explanation of the Brookhaven results choosing the tensor interaction instead of axial vector. Well, I never did hear from Madame Wu, nor did Don Kohler. But about two weeks later, we had a visitor come through the lab who told me that Dr. Wu at Columbia had figured out what was wrong with the Rustad and Ruby Brookhaven experiment published in 1955, [laughter] and that the erroneous result was due to a miscalculation of the amount of helium-6, et cetera. So I tried to correct this mistaken impression as to the source of the discovery of the problem with the Brookhaven experiment. I repeated the story at intervals of a few days to numerous travelers typically passing through Caltech on visits to U.S. nuclear labs from coast to coast. I also told Felix Boehm about it. Of course we kept in touch quite often. And Felix evidently told Feynman, although—I don't remember—I might have told Feynman, too. But I know Feynman's book credits Felix as the source of that information—not that Felix discovered it, but that it was Felix who had told Feynman that the Rustad and Ruby experiment was all wrong and why it was wrong. So that was a second time when I didn't handle things very swiftly. I should not have tried to be quite so clever in writing the letter to Madame Wu. We just should have written a letter to the *Physical Review* as quickly as possible. After acceptance, we could well have written Wu and sent her a copy of our letter to *Physical Review*. However, we were obviously in pay dirt country at that time, doing the right things, and thinking about the right things, and really enjoying life to the full.

I was collaborating with Felix and a couple of visitors to Caltech at about this time, to measure the level of polarization of the beta rays from the decay of nitrogen-13. At this point in time, physicists at MIT had first measured a pure Fermi beta decay—that is, one that should have gone by the vector interaction, according to the new beta decay theories being developed. However, although it had become known by this time that beta rays should be polarized, MIT had reported no polarization in the Fermi decay they were studying. This result caused some major concerns for the theorists because the elegance of the developing theory would have been seriously damaged. Fortunately, Caltech was able to solve this worry because we could make very strong nitrogen-13 beta decay sources in Kellogg, Felix Boehm had a set-up working that could measure the polarization of the positrons from N-13, and our own theorists knew that the "mixed" decay of N-13 proceeded mainly by the Fermi Vector interaction. The result was that the beta were fully polarized to the theoretical value which was nearly one. Case closed! **[Tape ends]**

CHARLES A. BARNES

SESSION 4

July 27, 1987

Begin Tape 5, Side 1

BARNES: Among physicists, there were differences between Jesse DuMond on the one hand and Charlie Lauritsen and Willy Fowler on the other, that went back to long before the time I first came to Caltech in 1953. I never really did find out what the origin of these problems was. I just remember that at some point, when I hadn't been here very long, I found it necessary to borrow some minor equipment from Jesse DuMond's group over in West Bridge, which at the time consisted of DuMond, Pierre Marmier, and Felix Boehm. The latter two were both Swiss research fellows with whom I got along pretty well. In fact, I got along pretty well with Jesse, too, for that matter. I remember being quite startled when Willy Fowler informed me that borrowing equipment from them was a "no-no," and I was never to borrow anything more over there. I didn't really pay any attention to this, because it didn't affect me much one way or another. I guess I really was never even very curious about why there seemed to be this separateness, and I never pursued the matter. And, at least from my point of view, it didn't affect my relationships with those people. We continued to talk physics on occasion—in fact, sometimes even frequently—and to see each other occasionally socially as well. When I was about to go back to Vancouver in 1955, Jesse even made me some vague kind of junior faculty offer and asked if I would consider joining his group. I recall feeling at the time that although that was of interest to me, it wasn't really something I could seriously consider because I felt that I was firmly committed to going back to Vancouver.

ASPATURIAN: What was DuMond doing?

BARNES: He made his fame in X-rays, as you know. He had built a succession of better spectrometers for measuring X-rays more accurately, as well as being a compiler of data who tried to make the best reconciliation possible in determining the values of the fundamental constants of nature. He was basically a builder of equipment, and he built a succession of instruments that were very good. One of these was a new curved crystal diffraction spectrometer for making measurements of X-ray quantum energies, and to a lesser extent, intensities, but mainly it was a very precise energy-measuring device. I believe that both Boehm and Marmier were also associated with this work. In addition, Jesse at this point decided to build a novel kind of beta ray spectrometer that could be used to measure either beta ray energy spectra, or gamma ray energies—in the latter case by interposing a gamma ray "converter" to make secondary electrons and focusing them in the spectrometer. It was roughly the shape of a giant egg; and we used to call it the "egg spectrometer." I think it still exists but my guess is it's probably with the old equipment that is out in the Caltech warehouse in Azusa or somewhere like that. In fact, the other day a message came around that maybe Caltech was going to give up that space; and it occurred to me that I really ought to inquire as to whether this beta spectrometer was still there and, if it was, alert people in case anybody wanted it. It was offered to us at one time; and I thought it would be useful for a particular kind of experiment on the tandem accelerator, but we actually never used it. While I was on one of my sabbatical visits to Copenhagen, this old spectrometer was finally moved out to very cold storage, and it eventually must have been scrapped.

Jesse loved building instruments. The data taking and the experiments that were done with his various instruments were, I think, mainly the work of Marmier and Boehm. At some point, Marmier returned to Switzerland. He became a full professor, and I think director, of the ETH, the technical university in Zurich. I think that there was a little bit of competition between him and Boehm, but Marmier was actually the senior by two or three years. When this position in Zurich became vacant, he went to it.

Boehm was, I would say, a little more reserved than Marmier, at least at that time. I didn't mind that; we often talked about physics, and I figured that it was just some European, if not specifically Swiss, kind of reserve. But I think that Marmier might have become somewhat more Americanized while he was here. Marmier died, unfortunately, when he was not very old—I've forgotten which year that was. I was very sad to hear that.

In any case, Boehm continued to work in DuMond's group and gradually inherited it and very ably carried on the research. If I'm not mistaken, one of the last ventures that Jesse DuMond sparked—although there was obviously a lot of participation by Boehm—was building a new beta spectrometer of a type that would bend much higher energy beta rays. I think its actual purpose was very specifically to look at gamma ray energies by putting a converter—

something that produces electrons when struck by gamma rays—into the source position on the spectrometer and then measuring the energy of these electrons very accurately, from which one could then very accurately infer the energy of the gamma rays.

ASPATURIAN: How much building were you doing at this time?

BARNES: We were undoubtedly building things on and off, but, you know, without going back and looking at papers and so on, it's a little bit hard to reconstruct specific dates. The major project that the Kellogg group undertook during this period was establishing a new laboratory, equipped with a new, considerably higher energy particle accelerator called a tandem accelerator, purchased from High Voltage Engineering Company, an early outgrowth of MIT. We got the funding for this development in 1959 or 1960. We had started the planning for it earlier, in the middle fifties, when it seemed likely that it would be possible to build electrostatic accelerators of considerably higher energy than any of the home-built ones we had. So we certainly were thinking of experiments that we could do with such a machine if and when it became available. It was installed in the sub-basement of Sloan, and the conversion of the Sloan building from the old high-voltage lab to physics-plus-mathematics, was actually done while I was here. Tommy Lauritsen and I paid quite a lot of attention to this, especially Tommy—partly as a kind of hobby. We probably walked through the construction site so often that we were certainly in danger of being mistaken for belonging to the construction company.

ASPATURIAN: Who initiated the conversion? Did that come out of Kellogg?

BARNES: No, not entirely Kellogg. It must have been pushed by the entire physics, math, and astronomy division, because the occupants eventually were several groups in physics and mathematics. I guess I didn't know too much about the politics of it, if I could use that word. I know quite a lot about the construction of it because it became almost a daily pleasure for Tommy and me to walk through the place after lunch, and see what was happening. In fact, even before any conversion of the building was done, we started to pore over the drawings from the architects.

ASPATURIAN: Did you make revisions, or suggest some changes?

BARNES: Certainly we suggested a lot of minor revisions, and probably others did, as well. But Tommy probably knew more about the plans of that building than anyone except possibly the architects. Indeed, at some point, he discovered that the architects' plans had not provided enough ventilation for the new tandem accelerator lab, which we were planning at the same time. We were thinking about what kind of installation and space we would need, while the machine was actually being built by HVEC. Tommy discovered, to his horror, that the architect hadn't really understood about scientific equipment. The principal thing that he evidently understood was people. He found out that the personnel occupancy of the EN tandem lab wouldn't be very big—the number of people divided by the large volume would be a very small number—and so he hadn't provided much air-conditioning for the space. I guess that he thought of people as being the principal source of heat—you know, 100 watts per person. We could easily, of course, visualize an extreme case of what the heat situation might be. Imagine that the total installed electrical power that we had requested for the lab was all turned into heat in the room. Of course, that's an over-exaggeration of what would be likely to happen; but if we were to do that, with the small amount of air-conditioning that had been called for, we found that the air temperature would go up to 150° Fahrenheit in about two hours. There was a big mismatch between what had been supplied and what could happen.

ASPATURIAN: If Lauritsen hadn't been looking at the blueprints, do you think the lab would have been pretty well built before someone finally realized that there was a problem?

BARNES: In this particular case, it was already pretty well built. The building had advanced far enough that there was no longer enough more room to install more air-conditioning ducts. The problem was solved—not a very elegant solution—by putting another big air-conditioning compressor in the utilities tunnels outside the lab. That supplied most of the cooling for the tandem lab. It worked out all right in the end; but I'd say it was typical of Tommy that he was the person who found this. There were a lot of sheepish faces when he pointed this out to the architect-engineers for the project.

ASPATURIAN: One of the things I occasionally hear from a grad student or postdoc is that for twenty years there's been radiation leaking out of every pore of Kellogg Laboratory. Is there?

BARNES: That's absolute nonsense. The radiation level outside Kellogg is absolutely and truly undetectable—only cosmic rays. It's very low even inside the lab, because people who work here are very concerned about the health aspects of radiation, dating back to Charlie Lauritsen's pioneering work in the early thirties on radiation dosimetry. No, that's absolutely without the slightest foundation. I think it's a very interesting rumor, but I'd never heard it myself. It is true that there is a truly gross misunderstanding by lay people of what radiation is; but offhand one would expect that people at Caltech would be a little better informed. I have no reason for concealing anything because the EN tandem lab is about the best shielded lab that exists in the world. It's built, as you know, well outside the shell of the Sloan building, and it's deep underground.

ASPATURIAN: Did it turn out to be a model for future designs of this sort?

BARNES: Well, it might be. Certainly, quite a number of people came and visited the lab after it was built to see how it had been designed and built. But, you know, anybody who builds an accelerator has to go through the procedure of trying to design an appropriate lab to exploit the machine as well as possible, and make the installation safe for occupants and surrounding people. I don't know that we would claim that we had any enormous insight. There are lots of other solutions to the accelerator lab situation that look very different from ours. I'd have to admit that some of those ways might be better than ours, and some aren't. I think we did a pretty good job, because at that time, money was available to build a pretty good lab. We were both happy with, and proud of, our new lab.

ASPATURIAN: Was this project primarily supported by outside or inside funding?

BARNES: The accelerator was purchased for Caltech by the Office of Naval Research [ONR], which was our first major external funding agency. Building the lab was part of the cost of converting Sloan from what had been just an empty shell into a complete building with floors and rooms and all the other things that buildings normally have. I think that the construction must have been funded by donors to Caltech. The purchase price of the accelerator was $1 million, which in those days was considered a very large project.

ASPATURIAN: I take it this was a state-of-the-art accelerator.

BARNES: It was the state-of-the-art electrostatic accelerator at the time, and it was the basis for a very diverse research program for a large number of years. It's still a beautifully engineered accelerator and it still runs marvelously well.

ASPATURIAN: Was there at some point, probably in the sixties, an articulated decision that Kellogg—or Caltech—was not going to get into the great big team accelerator projects that cropped up at Berkeley and Stanford?

BARNES: I think there was some recognition at Caltech that we were a small university, though not so small in science and engineering. As I mentioned earlier, I think Kellogg played an early role in spawning the high-energy physics group. In fact, Alvin Tollestrup, as I said, was a grad student in Kellogg; and Bob Walker came here expressly from Cornell, along with Bob Bacher, and others. Bob Walker did some experiments in Kellogg before the high-energy lab was built. The synchrotron at Caltech was built from a large ring magnet that was a one-quarter-scale model of the bevatron at Berkeley, built originally to test the magnet design for the bevatron. The bevatron was built at Berkeley and was one of the first of the really big machines, accelerating protons up to a bit over six GeV—6 x 10^9eV. Before this, just at the end of the thirties, Berkeley had built a 400 MEV cyclotron, which was a pretty big machine in those days. At the end of World War II, the cyclotron was briefly raised to a 700 MEV machine. But then they really made a big jump to the bevatron. It was a 6.2 BEV, or GEV—depending on whether you prefer American or international usage machine—whose energy was explicitly chosen to be high enough to make anti-protons and anti-neutrons. And in fact, when they finally made anti-protons, it won Nobel Prizes for the group who happened to be scheduled to run just at that particular time. The poor guys who were scheduled a few weeks later and who were the first to make anti-neutrons received no Nobels for getting there in second place.

In any case, before Berkeley actually constructed that machine, they built a magnet model that was one-quarter-scale. Once that magnet had served its purpose of letting them measure magnetic fields and calculating a little more accurately what they wanted to do for the bevatron, it became totally surplus. I don't know what financial arrangements if any were involved; but it came down here. At least we saved them the cost of providing storage space for the quarter-scale magnet. [CB subsequently added: "After many years at Caltech, that magnet had yet another career as a storage ring at Los Alamos Meson Physics Facility—LAMPF."]

ASPATURIAN: Did Kellogg request it?

BARNES: No, it wasn't Kellogg. We're talking about Caltech now. You see, the particle physics group was being formed, and the magnet was requested by members of that group before some of them ever arrived here. One of the first to arrive was R. V. Langmuir, who became a professor of electrical engineering at Caltech. I suppose that would also include Bacher, who came here at the encouragement of [Caltech president] Lee DuBridge, and also very much at the urging of Fowler and the Lauritsens. It was clear that Caltech was going into particle physics, and the decision was made to build an electron accelerator that would allow the production of very high-energy photons so that one could do photonuclear work. That was a project on a scale that Caltech could really competently handle. Just taking the quarter-scale Berkeley magnet as it stood, it was capable of accelerating electrons up to about 550 MEV. Getting up to that energy level was Phase I of the project. Later, the electron synchrotron was taken out of commission for an extended period, and the aperture in the magnet was made much smaller. In the intervening years, physicists had discovered a radically new principle of focusing previously unknown, called strong focusing.

ASPATURIAN: Was that discovered here?

BARNES: No, the principle was discovered by E. D. Courant, M. S. Livingston, and H. S. Snyder at Brookhaven. Actually, it was first discovered earlier, possibly during World War II by a Greek elevator engineer, N. Christophilos. He had been doing some calculations to occupy his mind apparently, and discovered the principle of strong focusing, which was considerably later rediscovered. Christophilos had even sent a copy of his calculations to Berkeley about 1949, just after World War II; and the people there were too busy to recognize the remarkable discovery this man had made. I guess they just treated his letter like crank mail. He ultimately did get the credit, though several years after World War II. He went to Livermore National Laboratory and built a device called the Astron there. He must have been awarded American citizenship at some point, I suppose, to work at Livermore.

Coming back to the synchrotron, and this strong focusing principle, our accelerator and high-energy physicists realized that the electron paths in the synchrotron would be much more tightly controlled. Thus they wouldn't need such large gaps in the magnet, so they could bring the pole faces in much closer and thereby get higher magnetic fields, and thus bend higher energy electrons. So Phase II of the synchrotron, when it was carried out, was designed to bring the machine up from 550 to 1500 MEV. I think they never succeeded in getting enough radio frequency power to get that high, but it certainly went to 1350 MEV or something like that. That was a leading rank machine at the time. Both phases of the synchrotron were very successful, because the first phase of 550 MEV was just enough higher than any other electron accelerator in the country—especially Cornell's—to be the only one to find out something quite startling about how the photopion-production cross section of hydrogen varied with energy. It turned out to have a giant peak in it. The only accelerator that could go high enough to show that it was a peak and not just a continuously rising yield curve was the Caltech machine. A similar thing happened when the machine was pushed into Phase II. It was just enough ahead of the game to discover a whole bunch of new resonance states in the photopion cross-section. By the time Phase II began to wind down, much, much later, it was clear that a small campus like Caltech could not support the next large jump in the size of accelerators. So Caltech's high-energy program became a users' program at institutions where such big machines were planned or already existed.

By this time, the high-energy project was really quite separate and distinct from Kellogg, although Kellogg in a way, fathered—or mothered— it to begin with; hatched it in a sense. It was clearly destined to function on its own, and it became a big project. Financially, it became the largest project on the campus, and perhaps it still is.

ASPATURIAN: Did it originally have ONR funding as well?

BARNES: No. It was funded from the beginning by what was originally the Atomic Energy Commission and gradually became DOE—Department of Energy. Kellogg remained funded by the ONR for many years, doing purely unclassified basic research. Kellogg has never done any classified work except for some projects during World War II, as far as I know.

ASPATURIAN: Was that an explicit policy decision after the war, on the part of the Lauritsens?

BARNES: That was an explicit policy decision by both Fowler and the Lauritsens, and by Caltech as well. By and large, during the time I've been here, it has been Institute policy—reaffirmed in stronger and stronger terms over the years—that classified work will not be done on campus.

[Tape ends]

Begin Tape, 5, Side 2

ASPATURIAN: How different were the students between Vancouver and Caltech?

BARNES: The principal difference that I see between Caltech students and students in other universities—including the University of British Columbia—is that almost all of the students here are extremely capable and technically very good. They're strong in mathematics and they're strong in science. I don't know the specific situation in engineering subjects so well, but it is likely similar. I would not be one to put down other universities, although I've heard a few people talk rather snidely about them—fortunately a very few. Even in universities where one sometimes wonders about the general standard, usually some really significant fraction of the faculty and students are truly first-rate. We see that when we accept graduate students coming here from other universities. The few Caltech undergraduates who stay on here as grad students generally do very well, but not significantly better than our other graduate students. The point is that there are really first-rate, talented individuals everywhere you look. It's just that on average, I guess, the standard would be higher here.

Some of our students, I think, would have profited from having more social life than they did. But our powers to do something about that are somewhat limited. We can try to help them get a little more enjoyment out of life by inviting them to social events and having parties and including them, and so on. I think that the undergraduates tend to mix substantially less with the faculty outside of classes than the grad students. In some ways, that may be too bad. I certainly get the impression that the undergraduates have a kind of tightly knit society that's built mainly around the student houses. They have a language they develop of their own and a kind of culture that goes along with this. In some sense, they may even have a feeling that it's kind of us against them—in terms of students versus faculty, perhaps. They work together trying to psych out what the professors are likely to ask them on exams, keep wonderful files of all the professors' previous examinations, study them assiduously, and try and guess what the faculty are going to do in a given course. They're very, very good at analyzing such things. I think I've sensed that for most undergraduate students the bond of association that they form within their house is very close, and most of them really never forget this bond of friendship the rest of their lives. But I do feel that they're somewhat more compartmentalized this way, partly out of choice and partly because of the way they live together in the houses. Graduate students, on the other hand, are a greater mixture; and a lot of them have come from places where they didn't have to work quite as hard as undergrads do here. They're generally, I would think, somewhat better able to mix socially among themselves and with others.

ASPATURIAN: Do you enjoy teaching; did you enjoy it when you started?

BARNES: I've always enjoyed teaching. I won't say that it hasn't sometimes been hard work, because I've always found it very challenging. Somebody once said to me, "Since you had all these courses to teach at Vancouver, and you never taught more than one course at a time at Caltech, that must have made a big difference in your life." Indeed, it did, because most of us at Caltech are on what might be called half-time teaching and half-time research. It's a very informal and somewhat nominal breakdown—teaching one course here is kind of a half-time teaching appointment. The four courses plus the lab that I taught at Vancouver typically were considered full-time there, at that time. However, somehow or other you were also expected to do research at Vancouver just as you are expected to do research here. The difference wasn't that astounding; I guess I was able to put in a lot more time on research when I came here than I could at Vancouver; and that would be natural. On the other hand, I think I really worked nearly as hard on the one course at a time that I taught here as I did on at least two or three courses at Vancouver. I just felt more was expected of me here, and I worked harder at it. But I did have more time to get fired up about research and to spend more time in the lab, to think about experiments and develop and build things here.

The determination of the polarization of the positrons from nitrogen-13 that I mentioned earlier certainly helped to tie up what the form of a new theory of beta decay would have to accomplish. Simultaneously, we were carrying out a program in Kellogg to determine the nature of the interaction in the beta decay of lithium-8. This was a nucleus that had been worked on, I guess, in 1948, in a really memorable cloud chamber experiment by the Lauritsens, Willy Fowler, and Bob Christy, who consulted with them as a theorist on that experiment. The results of that experiment were completely correct, but the theory of beta decay wasn't far enough advanced at that time to draw the kind of conclusions that we were able to draw later. In any case, the first thing we tried to do was measure the angular of correlation between beta rays and neutrinos because we realized that the nature of this correlation would tell us what the nature of the beta decay interaction was. First, we embarked on a measurement to determine the nature of this interaction by looking at how the combined recoil from the beta ray and neutrino would affect the energy of the residual nucleus. In this case, the kind of residual nucleus is a little different from most beta decays; here the final nucleus is beryllium-8, which breaks up in 10^{-21} of a second into two alpha particles. So the method by which we could look for the beta neutrino correlation's contribution to the recoil was just to look at the energy partition between the two alpha particles.

I think, as it turned out later, that was a really worthwhile thing to do. During this period, Dick Feynman, who was furiously working away and thinking about beta decay, was an almost daily visitor to the lab to see how we were getting along. One day, in the course of discussion—in fact, he was sitting there, helping us take data at this point—he said, "I just thought of something last night. If the beta decay of lithium-8 is a Gamow-Teller decay, the experiment you are doing will confirm whether the decay is Fermi—scalar or vector—or Gamow-Teller—axial-vector or tensor. But if it is Gamow-Teller, as seems likely, it cannot tell the difference between the two different Gamow-Teller alternatives, which are that the decay is driven either by the tensor interaction or by the axial- vector interaction."

So, as we continued to finish that experiment, I started a different experiment with colleagues to measure the beta-neutrino contribution to the recoil of the beryllium-8 in the direction parallel to the direction in which it breaks up into alphas. I had, by this time, realized that such a measurement should make it possible for one to make the distinction between tensor and axial-vector, once the first experiment has confirmed that the beta decay of lithium-8 is a Gamow-Teller transition. I had the pleasure of doing that experiment also, along with Willy Fowler, Charlie Lauritsen, Emery Nordberg, who was a graduate student of mine, and Howard Greenstein, who was Fowler's graduate student, I think. We published those two experiments in 1958 as adjacent letters; the first of the letters was based on the first experiment, which confirmed that the beta decay of lithium-8 did not proceed via either the scalar or the vector interaction—that is to say, it was a Gamow-Teller interaction. The second experiment, with that information in hand, unequivocally showed that it was an axial-vector decay.

In a way, the determination that the lithium-8 decay was occurring via the axial-vector interaction confirmed results that were being obtained elsewhere on other nuclei, first in the repudiation of the result from the helium-6 experiment by the work by James Allen, who was doing the beta-neutrino angular correlation in the neon-23 beta-decay. Although this is a mixed Fermi, Gamow-Teller decay, his work showed clearly that no possible mixture of scalar and tensor could give his experimental answer. The lithium-8 study was a little later in the game than some of the earlier experiments, but it was a very clear-cut confirmation that the Gamow-Teller beta-decay proceeded by the axial-vector mechanism. In combination with the known absence of certain Fierz interference terms in beta decay, that also meant that the so-called Fermi decays occur by the vector interaction and not by the scalar interaction. All of that was folded into the new, developing theory by Feynman and Gell-Mann, and it all made a beautifully consistent picture, which we now know as the V-A theory—that is, vector, axial-vector. Fermi transitions go by vector and the Gamow-Teller transitions go by axial-vector.

ASPATURIAN: Correct me if I'm wrong, but I think Feynman and Gell-Mann maintained, on the basis of their theory, that the helium-6 results had to have been wrong to start with. Did they get the news from you, that in fact, this was the case?

BARNES: I'm told that Dick said in his book [*Surely You're Joking, Mr. Feynman*] that one of the things that contributed to his confidence that they were going in the right direction was a remark from Felix Boehm, who had heard from Madame Wu that she understood why the helium-6 experiment was wrong. In fact, though it isn't a very serious matter, I think that Dick probably wasn't remembering that event completely. We had told people widely at Caltech—including Felix Boehm, and probably Dick also—what was wrong with that experiment at the time that Don Kohler sent his letter to Madame Wu. It was only a week or two after we sent that letter that we started having visitors, who had just previously visited Columbia, come through Caltech, telling us what was wrong with the original helium-6 experiment. We were getting our own words back to us by all these people touring across the country. I'm pretty sure Felix knew that

that result had come from us. But there's no particular reason why Dick would have made a point of noting that, because as I said earlier, we didn't publish it. We took the slightly malicious course of sending this letter to Madame Wu to tease her a little about how good graduate students at Caltech really were, instead of publishing it immediately, as we should have. We got well and truly repaid for that by being largely ignored as the source of this information. But people around Caltech were, of course, informed immediately of our conclusion that the helium-6 experiment was wrong as well as just how the experiment had gone wrong. It gradually became nationwide knowledge, just through diffusion, not least of which was diffusion by people who visited Columbia during this period and picked it up. [Laughter] Well, it's not a big deal, really and truly, but we really should have handled it better.

ASPATURIAN: While you were working on the lithium-8 problem, did you have Gell-Mann, as well as Feynman, in the lab haunting the premises? Were they looking for confirmation?

BARNES: Well, to be honest, during this period, although we were good friends with both Murray and Dick, Murray really wasn't around the lab as Dick was. It was Dick who was in the lab many, many times over a period of several months, to see what was new. As I said earlier, we even had Dick taking data for a day or two.

ASPATURIAN: Can he do instrumentation?

BARNES: Well, I can imagine that he could. I don't think he ever worked at it as a bona fide experimentalist. But the oldest story from Dick Feynman that I know of—or one of the oldest stories—is of how he fixed a radio for one of his mother's friends when he was in the early grades of high school. No, but Dick did ask a lot of questions. As I told you earlier, one of my own contributions to the lab in those days was that having worked in electronics during World War II, I was really up to date on the state-of-the-art, and was able to contribute a lot to our instrumentation by building circuits to do precisely what we wanted. In the course of a day's work, we might decide that it would be nice if we could do something slightly different the next day and measure this in coincidence with something else, and so on. The subject was sufficiently familiar to me that I could come back into the lab after we'd shut down the experiment in the evening and work for three or four hours late at night, and in the morning be ready to go with a completely new set-up in electronics. It was just the state-of-the-art and the state of my training.

Anyway, when Dick would come into the lab, he'd see these new circuits, and he'd say, "What's this?" I would explain what it was in words and then I'd sometimes show him the circuit diagram of it. Since a detailed circuit diagram looks a little mysterious to anybody who isn't familiar with that art, I would then draw the circuit diagram in blocks, and label what each block did, instead of drawing in all the wires, resistors, et cetera. Of course, Feynman immediately understood that kind of approach because he understood the logic elements. As soon as one removed the detail of the way the individual circuit elements were put together, Dick could understand extremely quickly the logic of how the circuit worked and what it was supposed to do. So I think that Dick could have been a great instrumentalist, if he'd chosen to go that way. I think he just had a superb understanding of the logic of how things worked and the basic physics of why they worked. He just never had the intimate exposure to the detail of instruments as far as I know; but he certainly had no difficulty understanding in a logical way what all of these instruments did.

ASPATURIAN: Gell-Mann is not that interested in that sort of thing, I believe.

BARNES: I wouldn't have any information on that. I never actually got into that kind of discussion with Murray. I think Murray's interests were in other directions. I know Murray was more mathematically interested, perhaps. I've heard that some physics departments actually turned him down as a graduate student because they thought he wasn't a physicist but rather a mathematician. Of course, that was a grievous error on their part. But that attitude just testifies to Murray's enormous interest in and capability in mathematics, which certainly helped to make him a great theoretical physicist. I don't want to take anything away from Dick, who is also, of course, a tremendous mathematician; it's just that he had his own original way of doing things. When Dick was faced with a problem in mathematics, he was tremendously intuitive. He would find ways to solve the problem, or to prove something that he'd conjectured was true. These ways would be quite original and would usually be considered completely unorthodox by the mathematical fraternity. But they worked. He understood mathematics well enough to invent new mathematics that was intrinsically correct. He didn't make mistakes at it; it's just that he developed novel ways to do things that fitted in with his experience, and achieved results that sometimes took other people quite a while to understand how he'd achieved them. In fact, in a way that's related to Dick's whole remarkable developments in quantum electrodynamics. Before he went off to Sweden to receive the Nobel Prize, he gave an absolutely wonderful lecture on the campus to the local people in the little theatre—Culbertson—that used to exist here on campus. It was a beautiful little building, and it held two or three hundred people, I guess. We were just fascinated while Dick made a very clear and extremely modest, typical Dick Feynman exposition of how he had got to the position that finally ended up with him being invited to Sweden to get the Nobel Prize. He explained that the way he got to the point of being able to formulate his rules for quantum electrodynamics was by doing every difficult quantum electrodynamics problem that people would bring to him. There were other methods that people had been using to solve many of them, which were grossly unsatisfactory, terribly clumsy, and full of inconsistencies. But whenever Dick went somewhere for discussions of difficulties with then standard theory, he would invite people to dig out problems that they couldn't solve or could only solve with incredible difficulty. The ones that were particularly useful to him were those that other people had solved, but only with enormous difficulty. By looking at these problems and learning how to solve them by his own methods, Dick evolved his own set of rules. By the time he'd done a large number of problems—in fact, every problem that he could lay his hands on—according to his own methods, he became convinced that his rules must be correct. And indeed, they were correct; and they gave him an enormous advantage at that time over any other theoretical physicist in the world in the speed with which he could do quantum electrodynamics calculations. I think it was typical of Dick that he had the intuition to develop this set of rules. He knew they were correct, because they gave the right answer every time on every problem where the answer was known. But it was quite a long time before others could convince themselves of the fact. It took a few years before other people, notably Freeman Dyson, were able to prove, using more conventional methods, that Feynman's rules were indeed correct. Dyson has written some very illuminating things about this period. I met him, actually, when I was a graduate student in England.

ASPATURIAN: What was he like then?

BARNES: He wasn't a graduate student by this time. He was already working in the United States and was back in Cambridge for a visit over Christmas, as I remember. He was very interested in one of the problems I was working on—the cross section for the photodisintegration of the deuteron. It was fun to talk with him. He was very patient about his ideas about the nucleon-nucleon force, and so on. Of course, he was by this time quite a few years senior to me, at least, in terms of education. He wasn't around Cambridge all that long, as I remember, so I really didn't get to know him personally.

Anyway, we were talking about the differences between Dick and Murray. I had great respect for both of them, and I wouldn't want to draw any conclusions about their relative merits. They're just two different people; they thought differently and they operated differently; and their styles in theoretical physics were different. And I think I said earlier, it was a disappointment to many of us, but not a great surprise, when they didn't continue their collaboration after the development of the theory of beta decay. On the other hand, the world may be a richer place because they went their separate routes, and both continued to be prolific researchers. **[Tape ends]**

CHARLES A. BARNES

SESSION 5

August 3, 1987

Begin Tape 6, Side 1

ASPATURIAN: During the period of the weak interaction work, was there a lot of excitement over the fact that Kellogg had triumphed on a key question in nuclear physics? Was there a sense of being on top of the heap?

BARNES: It would be a bit extreme to put things this strongly, because a lot of labs were contributing at this time. It was an exciting time, I think, in physics everywhere, and certainly an exciting time at Caltech, in general, both in theory and in experiment. Felix Boehm was also doing things in experiment that were very relevant to sorting out the weak interaction at the same time. Naturally we always felt pretty pleased about the kind of things that had historically been done in Kellogg, as well as the research we were then currently doing, which was actually coming to the forefront as being important. Indeed, for a period, we were contributing at the cutting edge, not just in nuclear physics, but you might say, also in particle physics, because the most important thing to be settled in that particular era was the nature of the weak interaction. The experiments that we were doing with our own obsolescent accelerators were right on the cutting edge.

ASPATURIAN: I think the weak interaction is now interesting a lot of chemists and biologists. Are you following any of that research?

BARNES: Well, I haven't followed it in much detail, but I can understand why they might be interested. Almost as soon as we knew about parity violation, I—and many others, I'm sure—recognized that it might have some implications for biology, because of the known asymmetries in the handedness of many biological molecules, which tend to be left-handed. That seemed clear to me from the beginning, but I guess I never felt I knew enough about biology to really work on that. The step to go from the fundamental result of a weak interaction to getting a handedness orientation in biological molecules is however not a simple step. There have been lots and lots of attempts to try to do it. Some of them sound plausible for a short time, and then eventually sound implausible; some of them never sound plausible. [Laughter] But I'm not aware that anybody has made an absolutely convincing story on that yet, because it's not simple. It may even turn out that the cause of a favored handedness in biological molecules lies somewhere else than the handedness of beta-rays. The implications in biology are exciting in a general sense. Those of us who work in physics are parochial enough to think it is a kind of fundamental science, because it really deals with the properties of matter at the fundamental level. In some way, the properties of matter at the most fundamental level must exert some influence—obviously they have to exert some influence—when you start dealing with more complicated manifestations of matter. There's no reason that life shouldn't be included in that. I knew that at one time there was a great philosophical debate about whether life involved some entirely different type of force that wasn't amenable to measurement by physical techniques. This was in the thirties. There were people who felt that there would be no way, in principle—not because of complexity, but even in principle—to find out what you would like to know about biology purely through physics and chemistry. It was thought that there were additional, entirely unknown fundamental forces at work, and some people even believed that these would never be related to physics and chemistry. That was a doubly mystical idea. I don't think that there's very much of that surviving, at least not among the people I talk to. [Laughter] But I couldn't say the belief no longer exists. It is generally exciting to know, although I never doubted it, that the fundamental properties of matter will impact on more complicated organizations of matter—inanimate as well as animate. It's inevitable. My view of physics is that it's trying to understand and describe matter in the world and what its properties are. Physicists, on the other hand, generally are reductionists, in the sense that they try to deal with as simple systems as possible. To a large extent, that usually saves them from the business of trying to explain complicated things. But most of us persisted in believing that even in complicated things, there's often still an element of simplicity if one can find it, and that this element of simplicity probably will be related to simple properties of matter, as far as we can find out what these are.

ASPATURIAN: Would you say that this period in the fifties and sixties was kind of a golden age of nuclear physics; or would that be overstating it?

BARNES: It's not a concept that I really like, because you might have said that the golden age of nuclear physics was 1919, or 1932, or something like that. I think it is true, though, that a whole bunch of new things were found out, not just about the weak interaction, but about nuclear physics in general in the fifties and sixties, partly driven by the great improvement in experimental equipment. I don't think that one should attach more significance to this than it deserves, because the development of knowledge is and always has been a partnership between the experimentalist on the one hand and the theorist on the other. What the experimentalists can do is very strongly influenced by the state of science itself and the state of the technology that's built on it. It is true that one always has a burst of discovery of new knowledge—and sometimes even fundamentally new principles—whenever major advances in technology take place that make it possible to study matter in a way that hasn't been possible before.

ASPATURIAN: During that period, would you say that the theory was driving the experiments, or the experiments were driving the theory; or was it basically a pretty equal partnership?

BARNES: I would say it was a pretty equal partnership in nuclear physics, and probably in physics in general. A theorist might tell you that it wasn't; and his perception of it could be different. But my perception of it is that it would be that way. In fact, nuclear physics continues to be largely an empirical science. There is, certainly, a lot of marvelous theoretical work that's being done; it's getting better all the time. However, it's made more difficult than some other branches of physics by the fact that the nucleus is a many-body system without some of the simplifications that occur, for example, in crystals, or perhaps in some of the systems with very small numbers of degrees of freedom or very small complexity. I think at one time, particle physicists believed that particle physics was going to turn out to be simple. That seemed unlikely to me at the time, to be sure. And I think that's been verified—as the knowledge of particle physics has expanded, we have found that it is not simple either. When we raise the energy of the accelerators we use—and possibly the intensity level as well, though that's not so obvious yet—it results in the discovery of previously unsuspected things. I see no reason to think that has stopped. We've heard the particle physicists talk about the great intellectual desert that will exist between certain ranges of energies, which never include the range of energies in which they're working, or where they plan to work in the near future, but are always at higher energies. The safest predictions are the ones where people say, "Well, the desert of knowledge will be in the energy region that we're very unlikely to gain access to in the foreseeable future." That's a statement that you can be sure isn't going to be proven wrong for some so-far undefined time, barring the accidental discovery of something that we don't or can't anticipate.

I think it's fair to say that nuclear physics is a complicated subject, and that at the present time, it is not generating really fundamentally, striking new concepts with any regularity. That could change overnight, of course, but it's not likely to occur soon. Our group in Kellogg under the joint leadership of Steve Koonin and myself opted a few years ago to branch out into what's called intermediate and high-energy nuclear physics. I think that was a good choice. More than half of our effort now is in that area. That conceivably could produce some startling, unexpected result. We know already that there are some differences between what people would have said a few years ago about the physics of nuclei and what is being learned by studying nuclei with very high-energy electrons. At any time, this could develop into some really unsuspected new understanding. On the other hand, it may not do that. The fastest way to discover something new is clearly to work in a field or an area where nobody else has been. You may still not find anything especially interesting, but you'll find something new, that's for sure, by definition.

It's also true that physics has by its very success worked itself into a situation where it is sometimes very difficult to get into a region that is drastically new. In particle physics, the problem is the cost and complexity of big accelerators. But it's generally true across the board, that for one reason or another, physics has been successful in explaining most of what has been found. Other areas are so murky that we don't really know how to start on them, to some extent.

ASPATURIAN: Such as?

BARNES: Well, many-body problems, in general. If there isn't some real overriding simplicity in them, as there is, say, in the translational or rotational invariances of crystals, many-body problems are really a hard way to make progress. But I don't think that problem is restricted to particle physics or nuclear physics. I think it's true across the board that in what is often called fundamental physics—although, one man's fundamental is another man's applied, as they say—it is hard at the moment to get into a totally untouched area, because of the very success that physics has enjoyed for many decades. Because of its intrinsic complexity, condensed matter physics—which used to be solid state physics—probably still has many nearly untouched areas.

ASPATURIAN: In the particle realm, it often seems that physics is now being carried out on the industrial scale.

BARNES: Well, certainly the scale of efforts is industrial, to be sure. But even where the number of scientists on an experiment tends to be large—sometimes sixty or a hundred people—it's still possible for a recognizably bright person to exert a lot of leadership in the field.

ASPATURIAN: But is it harder?

BARNES: It is harder in the sense that one doesn't have the situation anymore that one had in the earliest days of the development of the science. Then each individual was sure to discover something new, because nobody had been there before; and if he was even moderately smart, he might make a really fundamental discovery, just by the act of doing something first. I think it is true that perhaps a lot of students are actually shying away from particle physics—and probably also from nuclear physics—because they see this trend: in order to get into virgin territory in experimental particle physics, they're going to have to become members of large groups. And, in a way, it's a pity to see nuclear physics also following in this direction.

ASPATURIAN: Is that also happening in nuclear physics?

BARNES: It certainly is. In intermediate-energy and high-energy nuclear physics—perhaps not quite on the scale of the largest particle physics groups so far. Our intermediate energy group currently consists of two experimental faculty, one theorist, three postdocs, and maybe five students, which in the old days we would have called a really very sizable effort. In some of our experiments now we're also partners with three or four or five other universities or groups in national labs, some of which have groups that are much larger than ours. It's like particle physics group sizes of maybe twenty years ago. But, you know, there are some people who like that kind of work, who look at this and think, "I would like to work in this kind of environment," for a great many different reasons. Some of them just like a lot of company because they're sociable, but I'm leaving them out of consideration. Some people feel that they like to work with a lot of people, because they like to compete with the people that are close to them. Others like to be members of a team and compete as a team with other people in the search for something new. But you would have to admit that a lot of scientists have a substantial personal ego, and that they are, to some extent, driven by this. There are a lot of people who really want to do something by themselves, singly, with nobody else. Unfortunately, nature doesn't provide very many such opportunities in experimental nuclear or particle physics these days. It's still, in principle, possible for a theorist to do this, but not so obvious for an experimentalist.

I do think physics will continue to attract people. I think we're in an era where applied physics and condensed matter physics are looking more popular than they used to. We see that in the preferences expressed by incoming students. It's hard to judge how much of that is really driven by the fact that people see the possibility of significant or fundamental new advances in that area. A lot of students—one has to face it—are vocationally minded, and they have not missed the fact that the employment of physicists in the country is much the largest in the condensed matter, solid-state field. It's industrially such an active region that it's not even very easy for universities to find enough really good people, because they're competing financially with industry. You could certainly understand the student saying, "Well, gee, it looks as though maybe something nice might come out of this. It might even be useful to humanity. Or it might not"; but they don't think of the might not. "But above everything else, it's sure to get me a good job." But, you know, the pendulum swings back and forth. I personally still find nuclear physics an interesting subject. There are many more important things in nuclear astrophysics that really must be measured than I will ever have time to do. Part of the reason, of course, why these things have to be measured, is because we really don't have any kind of exact theory of the nucleus. We only have a qualitative theory.

ASPATURIAN: Do you think there ever will be an exact theory? Is it possible, given the nature of the subject?

BARNES: No. Maybe that's a definition of exact, that there won't ever be a truly exact model in any complicated many-body problem. But one can certainly imagine a theory that would be more than adequate for almost any purposes one could imagine—either applied or industrial purposes, or for testing subtle aspects of nuclei. I think that better theories will develop. We thought at one time that a model theory called the nuclear shell model would prove to be a sufficiently quantitative and detailed model to go a long way towards providing a reasonably complete theory of the nucleus. And indeed, it is a marvelously successful theory. The embellishment of the shell model by Aage Bohr and Ben Mottelson by adding collective nucleon motions makes it a really very useful and versatile theory. However, even at the time we were developing this theory, it was abundantly clear to people working in the field that such understandable structure in the pattern of excited states of nuclei, and in the properties of nuclei in these various collective states, would probably only be simple to describe for low-lying excitations. Since there are a large number of particles in the nucleus, there would remain a large range of phenomena at higher excitation energies that would really be far outside the capability of the shell model, or the so-called unified model that included collective motions, to describe. At these energies, the theory would be pretty helpless, and we would have to continue to describe things in very qualitative statistical terms.

ASPATURIAN: Were these limitations, for the most part, discovered through experiment or from qualifications coming from other theories?

BARNES: I think they were discovered in both experimental and theoretical ways. It was readily understandable that, at least in some approximation, nuclei consist of nucleons, and that there are a lot of nucleons in most nuclei. When you get a lot of particles, and you don't have some obvious, intrinsic, physical structure that positions them at nice uniform intervals in space, or is simple in some way, you know that describing them is going to be complicated. I don't think any of us ever doubted that quantum mechanics would be valid for nuclei, but the nature of the forces involved are not known to us in complete detail. In fact, they are still not completely known to us, and may never be completely known to us because of the complexity of the problem and the difficulty of extracting fine details. For instance, up until a few years ago, it was assumed by many people that the physics of nuclei at high energies could be obtained just by taking the sum of the effects from the individual neutrons and individual protons, and adding them all together in some simple way. The ultimate reductionists, such as the particle physicists, certainly felt that this would be so. We've now discovered that is not correct; and most nuclear physicists, I think, had already realized that it would not be correct. Where that knowledge will lead us, it is too early to say. The kind of revelations that occur from experiment and clever interpretation or understanding of it are very unpredictable, and those could come soon or not for a long time, but they remain totally unpredictable.

Returning to the weak interaction for a little while, I recall that after the V-A theory of the weak interaction was already largely complete, Murray Gell-Mann pursued an analogy that appealed to him between the new beta-decay theory and electromagnetism. He proposed that there should be a new interaction term that was related to leading weak interaction in beta decay in the same way as magnetism is related to the electromagnetic interaction; and it was natural that he proposed the name weak magnetism for the proposed new interaction.

As Murray had earlier had a good background in nuclear physics, it was natural that he would search for an experimental situation in nuclear physics where experimentalists could try to confirm quantitatively his proposed new interaction term. He knew about "mirror" states in nuclei, where one finds similar sets of excited states in "mirror nuclei"—that is, nuclei in which one member of the pair of nuclei differs from the other member by having a neutron in one and a proton in the other. The earliest well-studied mirror pair were probably the nuclei lithium-7 and beryllium-7—studied in the late 1930s—where one of the neutrons in the lithium-7 is replaced by a proton in the beryllium-7. This led naturally to the concept that the neutron and the proton were members of a doublet of states differing in a new quantum number—later called isospin.

After the resumption of the study of nuclear physics at the end of World War II, many studies of isospin doublet nuclei were made, and then the search began for isospin-triplets, and quartets, where the isospin analog states were assumed to have the same physical structure across a "mulitplet" of states, differing only in the substitution of neutrons for protons, or vice versa.

In any case, isospin eventually became an even more important quantum number in particle physics than in nuclear physics. Murray understood isospin in nuclei very well, long before it became particularly important in particle physics; and it was natural that he would think that isospin multiplets could somehow provide a testing ground for this new weak magnetism.

So Murray came over to our lab one day and was telling me about his ideas regarding weak magnetism, and asked if there was any place among the light nuclei—which was the area of nuclear physics that we had mainly concentrated on here in Kellogg—where these ideas could be tested. I explored with him a little bit what kind of things would be desirable for such a test, to be sure that I really understood what would be needed. One thing that would be good would be a high-energy beta decay, because the predicted effects would be proportional to the beta energy. Another point—and this was the most important point—was that the test would require complete isospin triplet. Namely, we would want to be able to compare the mirror positron and electron decays—isospin plus one and isospin minus one—and to compare these two with the corresponding gamma ray decay in the isospin zero nucleus.

Isospin conservation is, of course, believed to be a conserved quantity for the strong interaction. It also plays an important role in the weak interactions, but it's very different there. In particular, the conserved vector current theory said that there would be a quantitative relationship between the three transitions—the energy spectra of the electron and positron emitters and the gamma ray emission. What I realized was that while there were a number of high-energy beta decays where we knew quite a bit about some of the members of these isospin triplets of nuclei, there was only one case that was uniquely suited for such a test. That would be a comparison of the negative beta-decay of boron-12, the positive beta-decay of nitrogen-12, and the corresponding gamma-ray decay from the highly excited state of carbon-12, which was the isospin analog of the nitrogen-12 and boron-12 ground states, to the carbon-12 ground state.

I suggested this case to Murray and explained what was known and how to get the latest values on the gamma-ray energy and intensity, who to talk to for other data, and so on. Murray incorporated this in the paper he was already writing and very kindly offered to include my name as an author. But I felt that my contribution to it had been rather small and was simply a result of the fact that I was familiar with my own field of physics, and therefore I knew exactly where to look. So I declined his offer of joint authorship. The paper was published and it did arouse a great deal of interest. Murray, of course, acknowledged my suggestion of the boron-12, nitrogen-12, carbon-12 isospin triplet.

A lot of people started working on tests of weak magnetism with the mass-12 isospin triplet, including our own group at Caltech. I wasn't directly involved in the local measurements, but I was advising people doing it.

ASPATURIAN: Was that by your own choice, that you weren't involved?

BARNES: It was partly because I was doing other things, but it was mainly that there were already enough people available to do this measurement. To carry out this work we used a beta ray spectrometer that had been built originally by Tommy Lauritsen and some of his earlier grad students, which I'm sad to say no longer exists. It was really a wonderfully opportune piece of equipment to have at that moment. We had people already doing thesis work on this beta spectrometer, and also a postdoctoral research fellow from Germany who worked with it. There was plenty of experienced manpower to carry out the measurements. In fact, it became the thesis project of one of the graduate students already working on it. I think he was supervised by Tommy Lauritsen, although one of the great things about Kellogg has always been the very close relationships among the faculty and between the faculty and the students. It's certainly been a guiding principle of mine, and I think truly of all the faculty in Kellogg, that the students should feel at home and perfectly free to go to any faculty members to ask for ideas or advice, or discuss subjects with them. I think that it was very profitable for the students, to have this kind of extended family of advisors.

ASPATURIAN: Do you think this was true to a greater extent in Kellogg than, for example, in some other areas of physics or, indeed, in some of the other disciplines on campus?

BARNES: I really do. I think it was partly due to the influence of Charlie and Tommy Lauritsen and Willy Fowler, who got along so marvelously well together and had worked together for so long; this spirit just spread to the other people who worked in the lab. I think that it definitely was much more true of Kellogg than most other places on the campus.

ASPATURIAN: Is it still true?

BARNES: I regret to say that it may not be quite as true, although we try to maintain and strengthen this kind of feeling. It's not only productive from the point of view of training graduate students, but also from the point of view of the advance of science, because people do have different ideas; they think about things differently, and contribute complementary ideas. A lot was gained then, and a lot could still be gained in the future, by maintaining this kind of close and friendly cooperation. If people simply compartmentalize themselves, a lot is lost. So, in fact, I paid a lot of attention to this experiment. I probably advised this student nearly as much as his own advisor, possibly more; I sat on his final oral, helped him write his thesis just as his own advisor did, and I was probably more directly concerned with the experimental details of the experiment than his advisor, because I was in the lab more in those days than his advisor. In particular, a problem emerged late in the experiment that had to do with the particular design of the spectrometer. I addressed this problem myself and puzzled over it for quite a while, and eventually understood what was causing it. It made it necessary to take another step in the understanding of the experimental results that, I guess, we really hadn't foreseen when we started. It had to do with the penetration of the beta rays through the edges of the analyzing slits that defined the beta ray energy. I suppose this was an effect that we all should have foreseen. But I was able to analyze it quantitatively; and more importantly, to show that the difference in this effect that would occur between electrons and positrons was sufficiently small that it would not vitiate the principal result of the experiment, which was to compare the shapes of the beta spectra of boron-12 and nitrogen-12. I was invited to present an invited paper on this experiment at a New York American Physical Society meeting. I can't remember the year, but I think it was one of the more important papers at the meeting. Although this particular experiment was only partly my work, my supervision of the experiment and the fact that I had contributed the crucial idea made it possible to verify that our result had correctly tested the quantity it was intended to test. That was fun, invigorating, and stimulating, but I guess it also worried me a bit before the paper; although my impression is that the paper was very well received.

ASPATURIAN: And the results held up?

BARNES: The results held up. They were verified in other labs later on. There were minor variations in the shapes of the electron and positron spectra from experiment to experiment; but the comparison of the two beta spectra that we had made—which was the important result of the experiment—has been verified a number of times since. Ironically, before we could actually redesign the apparatus to eliminate this slit-edge effect altogether, the beta spectrometer died. It didn't just "fade away"; it died for an interesting but really frustrating reason. At that time, because of the growing demand for water in California, it became public policy not to run cooling water through a piece of apparatus and then discard it into the drains, but rather to re-circulate the cooling water.

Thus, in the early sixties or the late fifties, it became Caltech policy to build closed cooling-water systems and re-circulate the water through cooling towers. A little bit of water was lost in the evaporative process to get the cooling, but most of the water was just re-circulated. At the same time, we felt it was advisable—and I think correctly—to clean up the water because we'd always had trouble with the large amount of lime and other stuff in the Pasadena water. A lot of it is still there; I think we're still drinking the bottom sand and gravel from the Colorado River part of the time. But in any case, there was so much lime and solid matter in the water that we decided to clean the water—purify it—at the same time it was re-circulated. What happened next was not exactly anticipated.

Apparently we had developed some water leaks in the cooling coils of the beta spectrometer that had been plugged by lime and sand as fast as the leaks developed. When we put in the clean, purified water, it ate away the lime and washed away the gravel and, in a matter of a few months, a substantial water leak opened up when the magnet was operating at maximum electrical power. This wasn't noticed, and the overheating that resulted just burned the spectrometer coils up and completely destroyed them. It would have been a truly major undertaking at this point to completely rebuild the spectrometer, especially since we did not have the funds to do it at that time. That's why we actually couldn't go back and redo the experiment with a redesigned spectrometer. But the life of an experimentalist contains bizarre happenings like this once in a while. It's fortunate that this accident happened after we had finished the measurements and I had a chance to calculate the effect of the slit-edge penetration and show that the differences between electron and positron slit-edge penetrations were so small that they did not negate the experiment as a valid test of weak magnetism.

I also started about the same time trying to think if any of the other isospin triplets of high-energy beta-decays and corresponding gamma rays could provide similar tests of weak magnetism.

Begin Tape 6, Side 2

BARNES: The effect of "weak magnetism" had been described in Murray's paper. It occurred to me shortly after the boron-12, nitrogen-12 experiment that a similar situation might occur in comparing lithium-8, beryllium-8, and boron-8. As good luck would have it, just at the point when I was thinking about this case, a theorist at Princeton had the same idea and actually published the theory of how it might work. His calculation was based on the same fundamental idea published by Murray. But, of course, the details could be different in different nuclei. The only differences that emerged here is that in the boron-12, nitrogen-12 case, we had to measure and compare the spectra of the electron emitter and the positron emitter with the "mirror" gamma ray in carbon-12. In the lithium-8, boron-8 case, we had another option, which was to measure the angular correlation of the emitted beta-rays and the alpha particles that would be produced when the unstable daughter product—beryllium-8—fissioned into two alpha particles. In some ways, measuring an angular distribution is easier than trying to do an absolute spectrum shape measurement, although the effects were predicted to be relatively large in the mass-12 case—about one-half percent per MeV of the beta spectrum. I felt that, even though I foresaw some difficulties in trying to measure the beta-alpha angular correlation with these rather low-energy alpha particles, we could do it accurately enough to actually make a significant test of weak magnetism. Unfortunately, in the mass-8 case, the corresponding gamma-ray was unmeasured and would have to be calculated instead. However, it seemed that this would be good enough.

ASPATURIAN: It must have been very encouraging for you to be able also to place such faith in the instrumentation you had here at Caltech.

BARNES: The faith in the instrumentation comes from using it, and having had to build a lot of it. [Laughter] Making perhaps a short detour to answer your question, I think that we have every reason to be very proud of our total experimental program. To be sure, there have been a few results over all this time in which later work with better equipment has shown that the best answer was slightly different from what we got—perhaps even outside the range of our estimated errors. Willy Fowler used to keep his own private statistics on this kind of thing, if you think of assigning a standard deviation on a measurement result as being a kind of estimate of its likely error. One should be able to assign such an "experimental" uncertainty well enough that if it proved possible to unambiguously get a more accurate result later, the new result should be found within the standard deviation of the original result roughly two-thirds of the time, and outside about one-third, just on the basis of statistical theory. What Willy used to point out was that, on the basis of keeping records on the cases it was possible to look back at, we had been within our assigned standard deviation something like 95 percent of the time. That's a result that's statistically very improbable. What it was showing, of course, if you really believe in using statistics this way, is that we were always a little too conservative in quoting errors just to be sure that we were really not overestimating the quality of our results. Of course, Willy used to use this to argue the opposite way around. He would come into the lab and use this as a kind of verbal club to make sure we didn't make the errors too big. He'd keep beating us down on the errors to the minimum amount that we would accept as an adequate error estimate, simply to try to reduce this timid overestimating of errors. But to a large extent, our ability to do this—that is, to get answers over the years that, on the whole, were pretty good, stems from the fact that most of the equipment we were using—both electronic and otherwise—we had designed and built ourselves in the lab and understood its operation very well. That wasn't always the case for all of our competitors, although obviously we had lots of really competent competitors, too.

But in the case of the lithium-8, boron-8 experiment, I started with two graduate students—Emery Nordberg and Fernando Morinigo—to actually compare these two beta-alpha angular correlations. As it turns out, two or three other groups—I can't remember for sure—actually finished doing the lithium-8 beta-alpha correlation, but much later than we did, with results that were pretty similar to ours. As far as I know, none of the people during this same era ever succeeded in doing the boron-8 as well. The most significant result required a comparison of these two angular correlations.

I guess we finished the experiment about 1959, and our first publication on the results was in 1960. At that time, we were the only people who had a result that would enable one to compare the two beta-alpha angular correlations. There again, the result was a strong confirmation of the weak magnetism prediction. If it had turned out to be different, there would have had to be a serious reexamination of the theory. The lithium-8, boron-8 angular correlation comparison was redone by a group of physicists at Princeton more recently—I think their results were published in 1980, or perhaps even later. Although they had pursued the problem a little bit further than we did in the late 1950s, they got about the same answer as we got in 1959, with much more sophisticated equipment. So we've always had this reason for feeling pretty good about that experiment.

At the same time as we were doing these other measurements, Willy Fowler and I were testing another of the predicted consequences of the new beta-decay theory, which was that the theory was universal and should apply to all weak interactions. The conserved vector current part of the theory particularly implied that, aside from some small but more or less calculable corrections, one should get the same coupling constant in the beta-decay of muons as one gets in the vector type decays in nuclear beta-decay. We could already tell at that time from looking at the existing values that this might well be true to within an accuracy of a few percent. Willy and I realized that the nuclear physics measurements that went into this comparison were at that time far from adequate to do the best job of this comparison. Although other people continued to make more such comparisons years later, we were a long way ahead of them in recognizing that the first thing to do was to look at the nucleus oxygen-14, which has a pure vector decay, and compare its coupling constant with the muon coupling constant. We were gratified to find out in our experiment with graduate students Keith Bardin and Phil Seeger that, within an accuracy of about one part in a thousand, the two coupling constants were the same—exactly as required by the conserved vector current part of the new theory. In a way, that wasn't any longer a great surprise to people, but it was a nice additional confirmation of the new theory.

During this same period of time, I was becoming more involved in the nuclear astrophysics part of our program. Though a lot of what I was doing then really consisted more of expressing my ideas about what measurement should be done and encouraging others who were working directly on these problems, my own interest certainly was increasing.

ASPATURIAN: Who were the major people in nuclear astrophysics research at that time?

BARNES: Willy, of course, was heavily involved on the theoretical-plus-phenomenological side of the game. My colleague, Bill Kavanagh—or Ralph Kavanagh as he is mostly known more recently—was actively measuring the rates of reactions that are important in hydrogen burning in stars with his students. In Germany, Claus Rolfs and his students were our principal competitors. During this period, we were all very concerned about the question of trying to improve the predictions of the neutrino flux from the sun. We had worked on most of the reactions involved in hydrogen burning. On the strength of even earlier results on the subject, and a lot of interest by the physics and astrophysics communities, Ray Davis and associates at Brookhaven had started an experiment to actually try to detect solar neutrinos by capturing them in chlorine-37. This was certainly the first large-scale neutrino detector, and it was really the first significant attempt to detect the neutrinos from the sun. But Davis had previously been investigating chlorine-37 as a neutrino detector, following the independent rediscovery in the late forties by

Luis Alvarez, I guess, of the possibility of detecting neutrinos by capturing them in chlorine-37, which would be converted into argon-37. I knew, and it was shortly general knowledge, that the same idea had been independently proposed by Bruno Pontecorvo, then working in the British-Canadian Atomic Energy Project, where I'd worked during World War II. It was a clever idea for anybody to think of, and I think Alvarez, who's a very ingenious, productive fellow, deserves credit for discovering that, even if it was actually a rediscovery.

In any case, Kavanagh and his colleagues were concentrating on trying to get better and better values for the rates of the nuclear reactions that are the source of energy from hydrogen burning in the sun, and thereby to get better predictions of what the solar neutrino flux should be. But, of course, in order to predict the results of Davis' experiment, one not only needs to know what the neutrino flux is, but also the sensitivity of the detector for counting neutrinos. This had mainly been worked out by John Bahcall and collaborators in a series of papers extending over several years, starting with rather poor reaction data and involving a certain amount of guesswork as to what the chlorine-37 capture rate for neutrinos would really be—that is, what its intrinsic efficiency would be as a neutrino detector. We already knew one of the branches—the chlorine-37 ground-state to argon-37 ground-state branch—which is just the reverse reaction to the electron-capture beta-decay of argon-37. Since we know experimentally the rate at which argon-37 captures electrons to turn into chlorine-37, by an elementary and simple law of quantum mechanics, we could turn it around and get the same accuracy in predicting that neutrino capture rate. But we didn't know very much about what other contributions there might be, and there was a fair amount of guesswork involved in that. In fact, I don't think very much was considered, in the earliest predictions of the chlorine-37 neutrino capture cross section probability to excited states of argon-37.

By the way, there is a marvelous history of John Bahcall and Ray Davis's project to detect solar neutrinos in their contribution to a book, for which I was the senior scientific editor, in honor of Willy Fowler's 70th birthday. You know, I picked the name for the book; I just thought it sounded nice—*Essays in Nuclear Astrophysics*. That's a plug, of course. [Laughter]

I guess in 1963 or 1964, we had a visit from John Bahcall, who by this time had gone on to work elsewhere but was still interested in the problems of trying to predict the solar neutrino flux and calculating the chlorine-37 neutrino detection efficiency. John happened to come back to Caltech and agreed to give us a seminar on the present status of his work on this subject. As soon as he started to talk about chlorine-37 and argon-37, I immediately was struck by the recognition that chlorine-37, argon-37, potassium-37, and calcium-37 would form an isospin quartet of nuclei that would be exactly similar to the isospin quartets of nuclei that my graduate students and I were then, and had been for some years, investigating, using the new model EN tandem accelerator in the basement of Sloan. By this time, we'd become pretty familiar with these things and we're just dashing through all the light nuclei, identifying these isospin multiplets way ahead of most of our competitors. Not very hard, but lots of fun. It was just wonderful; we were having a great feast! In a way, you could think of them as successive hurdles, and it was lots of fun; it was not only good physics but highly recreational. But as a result of this, I realized immediately—and I couldn't wait to get a chance to tell John—that the major contribution would most likely come from the neutrino capture that would transform chlorine-37 to the excited state of argon-37 that was the analog of the chlorine-37ground state, because the quantum mechanical matrix element, that controls the probability of the neutrino capture, would have a perfect overlap between these two states. The states would be structurally the same—with a maximum matrix element and therefore a maximum contribution—even though it was an excited state. I was somewhat dismayed and let down a little bit when, within a few minutes, John said, "One of the most exciting things that's happened recently is that I just came through Copenhagen and I was busy talking with Ben Mottelson; and he pointed out to me that one should not overlook the importance of the chlorine-37 analog state in the neutrino capture." Ben had obviously immediately also recognized the importance of the analog state from his very great knowledge of nuclear physics. In fact, because we knew that these states would be essentially perfect analogs, we knew within a very small uncertainty what the capture rate to the analog state would contribute. Since we knew this would be almost certainly the biggest contribution, the contribution between the ground states, which we knew already from the argon-27 decay, plus the analog state contribution, which we knew exactly from theory, would most likely dominate the neutrino capture rate for chlorine-37. Even if we were slightly wrong on the other unknown capture branches, we couldn't now be very far wrong. The chance of being wrong by an order of magnitude had suddenly vanished with the recognition of the importance of the analog state. However, I managed to sit there and listen to the remainder of John's talk. I realized that as Ben Mottelson was a theorist, it was reasonable that he would indeed understand this point about the neutrino capture to the isospin analog of chlorine-37.

However, not being an experimentalist, there was something that he wouldn't naturally think of, that I did naturally think of. I could hardly wait for John to finish his seminar to point out to him that it was not only the analog state, which we could predict on the basis of theory. We could measure all the various branches of the positron decay of calcium-37, which would be the isospin analog of chlorine-37. Because of Coulomb energy differences, calcium-37 could beta decay to the analogs of all of the relevant argon-37 states in potassium-37. By measuring the branching fractions for the calcium-37 decays to all of these potassium-37 states, we could get the matrix elements for all of them and transfer them to the problem of predicting the total rate of the neutrino capture on chlorine-37 leading to argon-37.

I did wait until the end of John's talk, of course, and then explained to him how all these decays would work. With his background in nuclear physics, once I started talking about calcium-37, he understood immediately; in fact, I think that John might have already thought about calcium-37; he knew exactly where I was going, and we both knew it was a really good idea. I realized also that the states in potassium-37, that were in the range of interest for the neutrino capture in chlorine-37, would all be beta-delayed proton emitters, making them easier to detect as branches in the calcium-37 decay. I realized also, to my disappointment, that we would not be able to make calcium-37 with the low accelerator energies we had available in Kellogg. There was just no way we could do it here.

What John and I then did was to sit down and write a short letter to *Physical Review Letters* to get it into print as fast as possible, pointing out the importance of studying positron decays of calcium-37 to the problem of determining the neutrino capture efficiency of chlorine-37. Our hope was that other people who could make calcium-37 would actually undertake to do this. As soon as we had the letter written and sent off, we sent preprints to many of the labs that we thought might be able to do it. In the letter we explained how people could go about trying to do it using the beta-delayed protons resulting from the calcium-37. I remember I also phoned our nuclear physics colleagues at UCLA, particularly Reginald Richardson and people working with him, because I knew they had a cyclotron that was capable of making calcium-37.

Their response was that they were certainly very interested in this possibility. Unfortunately, the effort and manpower that they were able to put into the experiment turned out not to be enough to win the race. There were other groups consisting of a lot more people who perhaps had even better accelerators. A group from Brookhaven was the first to get there, and to claim the bottle of champagne that John Bahcall and I had promised the winners of the competition. The proposed calcium-37 experiment was actually published simultaneously by groups at McGill University and Brookhaven. But it was, again, a minor piece of fun and excitement. This kind of fun and excitement really adds to the zest of doing science! Let's face it, we enjoyed it. I don't remember all of the authors of the Brookhaven paper; but I remember one of them, for example, was A. M. Poskanzer, whose subsequent career has been mostly at Berkeley, I think. [CB subsequently added: "After checking the journals, I see that those involved were P. L. Reeder, Poskanzer, and R. I. Esterlund at Brookhaven, and J. C. Hardy and R. I. Verrall at McGill." —Ed.] We had no idea that within about a month of the appearance of our letter, the experiment was going to be done and done very well. My role in all this was really minor; but it was particularly good fun to sit down with John, who was just visiting here for a few days at that time, and in as few days write this letter, work out all the consequences of it, figure out how to make calcium-37, figure out how to do the experiments, put it all in print, and get preprints off to all these people, before John had to move on to somewhere else. Science usually doesn't proceed with that kind of speed and personal feeling of satisfaction and reward, but it's nice that it happens once in a while. In the big scheme of things it's not a huge thing, but it did mean that literally within a matter of a few weeks, we were able to completely eliminate uncertainties in the neutrino capture rate of chlorine-37 as a solar neutrino detector. That doesn't say anything about whether the sun would make neutrinos at the rate we were predicting; that's a different matter.

ASPATURIAN: Was this for a particular type of neutrino?

BARNES: We're talking about electron neutrinos, and we're talking specifically about electron neutrinos as distinct from electron anti-neutrinos, because the predominant radioactive elements made in hydrogen burning are positron emitters. Because of conservation of leptons, and particularly conservation of electron-leptons in this case, the thing that comes with a positron—which is an anti-lepton—must be a lepton. And so it's an electron neutrino that is emitted along with the positron. That's one of the marvelous simplicities that nature seems to adhere to. It seems that the weaker the interaction, the easier it is for nature to be simple. [Laughter] And maybe there's a good reason for that.

ASPATURIAN: Was the center of all the nuclear astrophysics research also in Kellogg? Were you, in effect, on the spot?

BARNES: We certainly weren't the only people in the world who were trying to do nuclear astrophysics. We were the only people on this campus doing it. But I started composing review articles on the subject, and I can remember giving a number of invited reviews over the years, in several places, on nuclear astrophysics. I guess about 1966 is the first major one I gave at some nuclear astrophysics conference as an invited speaker. That was stimulating and profitable for me because it gave me the incentive, or putting it in a different way, it made it absolutely necessary for me to become thoroughly familiar with everything that was going on, and what the possibilities were.

We had a lot of general interest in nuclear astrophysics, part of it sparked simply by the beginning of the Davis experiment designed to try to find solar neutrinos. We recognized, and this point has been made many times by John Bahcall and others, that the only known kind of radiation that can in principle actually get to us from the central region of the sun, where the nuclear reactions are occurring is the radiation of neutrinos. They are hard to detect, and it's extremely difficult to do experiments with them for this reason. But as John Bahcall used to say, the solar neutrino flux turns out to be an extraordinarily sensitive thermometer for measuring the temperature at the center of the sun; because chlorine-37, by virtue of the importance of the neutrino captures to high-flying excited states of argon-37—especially the important analog state—the neutrino captures are strongly weighted to the high-energy neutrinos. The highest energy solar neutrinos come from the beta decay of the nucleus boron-8, and the boron-8 comes from a nuclear reaction whose rate is especially steeply dependent on the temperature. So one way of looking at the detection of solar neutrinos, at least with chlorine-37, is to recognize that the reaction that you're most sensitive to is really also the reaction in hydrogen burning that's most sensitive to the temperature. Though the rate of producing neutrinos depends on the density at the center of the sun and depends somewhat on the constituent contents present in the center of the sun, the thing it depends most critically on is the temperature. Even a rather moderately accurate measurement of solar neutrinos would give an accurate measurement of the temperature at the center of the sun, just because of this steep temperature dependence.

ASPATURIAN: I may be jumping ahead here, but at what point did it become clear that the number of solar neutrinos that was being collected in this particular experiment was not consistent with what was predicted by the theory?

BARNES: That was something that became clear gradually, as Davis's experiments improved in precision, because the experiment originally had a poor signal to background ratio. Over the years there was a very important technical improvement in Davis's technique, for which, in fact, we should give credit to Gordon Garmire, who was a professor here at Caltech before he went elsewhere to continue his work in space physics. He was an expert in using proportional counters for detecting very low-energy events, and he suggested to Ray Davis on one of his visits to Caltech that if he, Davis, would look at the pulse shape of the events in his argon-37 detector as well as at the pulse height, he would be able to eliminate a large fraction of the background counts in his detector, and thereby make his argon-37 detector of, several times more sensitive. And right away, this one relatively simple step improved the quality of Davis's data immensely.

There were other changes in experiments, such as doing most of the measurements deep in a mine in South Dakota, because that also helped to reduce the background in the detector. This was also an important step. Davis had been doing his earliest measurements at Brookhaven, but it was clear that taking these into the mine would also help. There was thus a gradual improvement of the sensitivity of the experiments, and an accompanying parallel development in the accuracy of the theoretical predictions. Originally, at the very earliest days, the predictions would have been that the solar neutrino capture rate was far too small to detect. Then the accepted rate of one of the nuclear reactions involved in the production of solar neutrinos was found to be grossly too small, which immediately bumped the predicted rate up a lot. But there were some other nuclear reaction rates that were known poorly, and these caused the neutrino capture rate to be greatly over predicted. The successive history of the theoretical side—that is, the predictions of the solar neutrino capture rate in chlorine-37—I guess more or less monotonically declined from that point on. That meant the experiment was getting continuously harder than had originally been contemplated. I think if Ray Davis had known how small the predicted rate would eventually turn out to be, it's entirely conceivable he would never have tried the experiment. But perhaps encouraged artificially by a predicted rate that for reasons that were nobody's fault was too high, it seemed to be a smart experiment to try. Anyhow, both of these things—the experimental techniques and the theoretical predictions—progressed simultaneously. I would say that the theoretical predictions of the solar neutrino flux in the simplest model, where nothing happens to the neutrinos during their path to the detector, have not really changed by more than perhaps 20 or 30 percent for many years, although the predictions still bounce around a little bit from time to time. Similarly, the results that Davis is getting for the argon-37 produced by neutrino capture in his gigantic tank of chlorine—actually perchloroethylene—have continued to show that the upper limit for the measured rate—a one sigma upper limit—is only about a third of the present predicted rate. That's been a stronger and stronger conclusion that hasn't changed significantly over recent years. It's unfortunately a low statistics experiment because the number of captures is not all that big, and it takes a long time to get good counting statistics. The errors on each individual month data are very large. But by doing the experiment repeatedly, month after month, the fractional error in the result gets smaller, and it's becoming more and more clear that the average rate at which he's detecting neutrino capture events is about only a third of the prediction. So as Ray Davis is very careful to point out, that all he's saying is that at most, the rate of events he sees is about a third of the prediction. Maybe none of the events he sees are from solar neutrinos; they could be produced from some other cause we don't know of at all. As you know, there have been a number of very exciting alternative explanations of why the solar neutrino rate might really be so small at the earth, including electron neutrinos oscillating into other kinds, but that's another story that I haven't personally been much involved in, except as an eager spectator watching it developing.

My own major launch into nuclear astrophysics experiments—as distinct from talking with all my colleagues about it over the years—came about like this: I had recognized for some time that it was very likely that the rate of the nuclear reaction in which carbon-12 and an alpha particle react to form oxygen-16 plus a gamma ray was likely to be a very important reaction in the second stage of stellar nuclear-burning, namely the process now called helium burning. In fact, it became clearer as Willy and other people, myself included, worked on this, that to a large extent there were just two nuclear reactions that were going to be really important in helium burning. In principle, there could have been a whole chain of them, but the chain is mostly broken after the first two reactions. The first one is the reaction that produces carbon-12, and the second one converts carbon-12 into oxygen-16. The relative rates of these reactions would determine the relative amounts of C-12 and O-16 that would be produced at the end of helium burning. It was verified by the astrophysical theorists, that indeed the relative rates and the relative production of C-12 and O-16 would have very important consequences, not just for the subsequent nuclear physics evolution of a star, but also for the gross dynamical evolution of the star, and would even affect the percentage of stars, or the range of original masses of stars, that would end up as black holes versus those that would end up as neutron stars, or white dwarfs. I had a kind of lucky intuition, perhaps, that these two reactions would both be important. The C-12 alpha, gamma reaction was one where I happened to think of a little trick that I thought might be useful. That little trick ties back to one of the first devices that a student of mine and I built when we first got the EN tandem accelerator. I realized even before we got this particular kind of accelerator, that its configuration would lend itself very readily and very neatly to building what we call a time-of-flight neutron spectrometer. That is, we could actually do energy measurements in nuclear reactions that emitted neutrons quite accurately, by pulsing the beam from the accelerator into short-time bursts. Then we would measure the time the neutrons, emitted from the target, required to cover some distance, such as a meter perhaps, to reach a neutron detector—accurately timing down to a nanosecond, a billionth of a second—or better still, some tenths of a billionth of a second. From how long it took the neutrons to get to the detector, we'd get the neutron velocity, and hence the neutron energy. So we could separate complex energy spectra of neutrons by this so-called time-of-flight method. The method had been invented earlier and had been used several times for single-ended accelerators. But the new tandem accelerator lent itself marvelously well to a really elegant way to do this. So almost the first thing we did after we got the thing working, literally within two days, was to start to build the system we'd already planned, the neutron time-of-flight system. **[Tape ends]**

Begin Tape 7, Side 1

ASPATURIAN: We were discussing the neutron time-of-flight apparatus.

BARNES: As I mentioned earlier, one of the first modifications that we made to the new EN tandem accelerator when it was installed here in 1960 was to develop the capability of doing neutron time-of-flight work. I was very fortunate to have at that time a student, Frank Dietrich, who had helped to build a neutron time-of-flight apparatus on a single-ended accelerator at Los

Alamos a year or two earlier. This device was not too different from what I wanted to do on the tandem accelerator, although the tandem gave us the opportunity of developing a neutron time-of-flight system in a particularly elegant way. In fact, when we were contemplating buying the tandem accelerator, I recognized that it would be easy to modify the accelerator layout slightly and obtain a neutron time-of-flight system for measuring neutron energies that would be both elegant and straightforward.

ASPATURIAN: When you say elegant in this context, what exactly are you referring to?

BARNES: Measuring and detecting neutrons quantitatively is a really difficult business experimentally because, not being charged, neutrons themselves don't directly give any signal when they go into a detector. There are only two ways they can make a signal in a detector. One is by capturing on some element and making a gamma ray, which is an extremely clumsy way to do things. It certainly is possible to detect neutrons this way, but it doesn't allow you to do a decent job of measuring their energies. The other thing one can do relatively easily is to let the neutrons collide with hydrogen gas in a detector. The neutrons thus produce proton recoils, much like billiard ball collisions, by colliding with the protons in the hydrogen gas. So the neutron is replaced by a charged particle whose presence and energy you can easily measure. Unfortunately, even if the neutrons are mono-energetic—that is, even if they all have the same kinetic energy, the same velocity—the proton recoils that they make in these collisions will have a uniform continuum of energies ranging from zero up to the neutron energy. For this reason a complicated spectrum of neutron energies is further complicated by the effect of each discrete neutron energy producing its own recoil continuum. The idea of the time-of-flight apparatus is that you pulse the beam in pulses about a nanosecond long—that is, one billionth of a second—so that all the neutrons emitted from the target during the beam pulse are made within that particular short time interval. You know that they have to be made at that time, and that's the only thing you know so far. But now you can set a detector containing hydrogen—for example, a plastic scintillation detector—at some known measured distance away from the target, and you can time the neutrons till they get to this detector. Measuring their time of arrival at the detector, relative to the beam burst, gives you the neutron velocity, which then gives you its energy. You count every individual neutron this way; every neutron that's detected in the detector gives you its own signature on what its flight-time was.

ASPATURIAN: What is the source of the neutrons?

BARNES: The source of the neutrons is whatever nuclear reaction you really want to study. In fact, much of the work that my students and I did, through the 1960s after the new accelerator was installed, consisted of observing and measuring for the first time the energies of the higher isospin states of nuclei. We had really a very good deal of fun doing that because we were constantly in competition with people in other labs who were looking for these same states by different methods. It just happened that our technique of looking for them with helium-3, p and helium-3, n reactions was a particularly selective way of finding such states and, that, moreover, we could measure their intensities and energies with very high accuracy. Although we often had disagreements with colleagues elsewhere about the energies and/or intensities of these higher isospin states, I think it's true that in every single case our competitors eventually had to withdraw. It wasn't that we were particularly clever about it; it was just that the techniques we had chosen to employ really made it impossible to make much of a mistake. Our work was elegant in the sense that, with only a straightforward application of a modest amount of care, we just couldn't be wrong on the excitation energies of the so-called higher isospin states. But this work did provide a lot of important new information about nuclear structure. After we—the graduate students working with me during this period, Eric Adelberger, Art McDonald, Dave Hensley, and Pat Nettles—laid out the energies at which these higher isospin states occur, we proceeded to study the dynamics of these states—that is, to see quantitatively how large the isospin symmetry breaking was. That's a subject that's still continuing elsewhere; I was talking to a visiting physicist just a few days ago who's continuing this kind of work at Princeton.

However, there was an unexpected thing that I noticed in every one of the time-spectra that we used to take. If we had several different groups of mono-energetic neutrons, each group of different energy, we would get a peak in the time spectrum corresponding to each neutron energy, whose height we could then interpret as being the number of neutrons of that energy, as expected. But, in the flight-time plot, the largest peak was always one that didn't have anything to do with the neutrons at all, but had to do with the fact that when the short beam burst hit the target, it essentially always made a burst of gamma rays as well. And since all of the gamma rays travel at the speed of light, typically about 30 times as fast as the neutrons, this burst of gamma rays always got to the detector first, and it always gave a large and extremely sharp, well-defined peak. That peak was useful in our neutron-energy spectroscopy, because we know that the gammas propagated at the velocity of light, and so that peak calibrated the zero of our time scale very beautifully. But it occurred to me many times, as I looked at this gamma-ray peak, that we were not using it in a very deep way, and that it probably should be useful for something more important. It did dawn on me eventually that we might be able to use this peak as a method of detecting gamma rays in the presence of a background of neutrons.

ASPATURIAN: What would be the significance of that?

BARNES: It's very difficult to detect and measure the energies of gamma rays in the presence of neutrons because the neutrons also interact with the gamma ray detectors by producing capture gamma-rays or nuclear recoils in the detectors. One's ability to figure out the energy distribution of the gamma rays coming from some nuclear reaction can be largely destroyed by a significant background of neutrons. Since the neutrons also interact with the gamma-ray detector, you simply can't see the things you're really trying to study. It occurred to me one day that maybe one way around this neutron problem, which occurs in many nuclear reactions, would be to again resort to beam pulsing. Only this time, instead of looking at the neutrons, what we would do is look at the energies of the first particles that get to the detector, which are the gamma rays. We called this time-of-flight neutron, gamma-ray separation, for obvious reasons.

As far as I know, that had never been done before. It wasn't really a difficult idea to come up with; it was only a matter of really needing it for another experiment that caused me to take the next step, which was to actually employ pulsed beams and flight times to look at gamma rays in the presence of a higher level of neutron background.

The first reaction we attempted to do this on was the radiative capture of alpha particles on oxygen-18—above a beam energy of about 700 kilo-electron volts; detecting the gamma rays was plagued by the onset of a neutron-producing reaction that was very much stronger than the gamma rays. We reported our preliminary successful results at an American Physical Society meeting in Oak Ridge. As far as I can remember, that was in 1968. What we really wanted to do was use this new technique to measure the reaction carbon-12 (alpha gamma) oxygen-16, and I'll come to why that's an important reaction in a moment. But in some ways, it turned out that Oak Ridge wasn't the smartest place and time to give the first public presentation of this new technique. In Oak Ridge at that time there was a recently arrived physicist who'd spent his previous career trying to measure C-12 (alpha gamma) O-16, without much success, just because of this terrible problem with the neutrons—which, in this case, come from C-13 (alpha,n) O-16 on the C-13 in the target. He'd recently moved to Oak Ridge, after having spent years trying to do this reaction elsewhere. It wasn't very difficult for him to make the connection that this new technique might be just the trick that was needed to do C-12 (alpha, gamma) O-16. The other point was that Oak Ridge already had in operation one of the best neutron time-of-flight systems in the United States, which had been used only for neutron energy spectra, as in our work at Caltech. As it turned out, only a couple of weeks after the APS meeting, this man started to measure C-12 (alpha, gamma), using the time-of-flight gamma, neutron separation technique we had presented, and we weren't that quickly off the mark, as we hadn't anticipated anyone else using the new technique so quickly. This was a bad guess on our part!

At that time, I was very fortunate to have a very good graduate student by the name of Peggy Dyer, who was the first woman graduate student in Kellogg, I think.

ASPATURIAN: When she was admitted as a graduate student in physics, was there a feeling that a risk was being taken admitting a woman, or was it viewed as a positive step, or was it just treated as an everyday thing?

BARNES: To me it was just treated as a straightforward step. In fact, I think she was preceded by one or maybe two women grad students in other areas of physics here. I knew that she was a very promising student because I'd seen her admission records, and I knew that she'd done experimental work as an undergraduate at the University of Texas, helping in data reduction—in fact, even helping in some theoretical calculations. Naturally, I was delighted when she elected to come to Kellogg, and particularly when she decided that she'd like to take on this very challenging experiment to measure carbon-12 (alpha, gamma). I had by this time realized that C-12 (alpha, gamma) would be an important reaction in nuclear astrophysics, because the calculations that the theorists were doing indicated repeatedly that the subsequent evolution of stars after helium burning definitely depended strongly on the relative amounts of C-12 and O-16 left over at the end of helium-burning. Since helium-burning mainly produces just those two nuclei, the carbon-12 (alpha, gamma) reaction is terribly important, because it's the one that turns carbon into oxygen and it has an absolutely direct bearing on the relative amounts of carbon and oxygen at the end of helium-burning.

ASPATURIAN: What happens when your amounts of carbon and oxygen vary?

BARNES: It's a quite complex technical point. Carbon-12 is by itself a very explosive nuclear fuel at temperatures just above those for helium burning. If helium burning produces mainly C-12 in the core and very little O-16, carbon-burning ignites under what are called degenerate conditions and an explosion is likely to result. The energy release can be enough to totally bypass the next burning stages and go directly to the build-up to iron, followed by a supernova explosion. If the star is massive enough, the whole star may go into a black hole. If less massive, a neutron star—pulsar—may be left. The range of stellar masses that result in black holes or neutron stars, or even no remnant at all, is very sensitive to the O-16, C-12 ratio at the end of helium burning. In other words, it's probable that some supernovae leave black hole remnants; some probably leave neutron star remnants—pulsars; some probably leave white dwarf remnants, and some probably leave no remnant at all. If there's enough energy release, supernovae may just dismember the entire star. What happens depends both on the mass of the star and, as the work of a good number of very capable theorists had shown, on the relative amounts of carbon and oxygen.

Peggy Dyer and I started out to measure carbon-12 (alpha, gamma). I guess this work was pretty well complete by 1973, when, as I remember, Peggy obtained her PhD. In fact, she stayed on for another year as a post-doctoral research fellow.

ASPATURIAN: She went through fast—four years?

BARNES: She was a fast grad student, definitely. She worked very hard. That wasn't the only thing she did, either. On top of this, she sat down and finished up another complete, difficult experiment before she even started the carbon-12 (alpha, gamma), which was itself a tour de force. But her thesis problem was the carbon-12 (alpha, gamma). It's not an exaggeration to say she's internationally famous in the nuclear astrophysics community as the first person to make truly credible measurements of carbon-12 (alpha, gamma). Initially, competitors at Oak Ridge did in fact get the jump on us, because it took us a while to get started on this reaction, at least partly because we were already doing other things that needed to be completed. Nevertheless, I don't think our competitors fully understood the kind of precision and the perseverance that would be needed in the experiment, and they just plain stopped before they had obtained a credible result.

ASPATURIAN: Besides Dyer, who were your chief colleagues during this period?

BARNES: I had a good number. Peggy was a graduate student, and I had a lot of other very good graduate students. We also had visiting research fellows or postdocs at various times. I remember working with Davis Nichols, who eventually went to work for Boeing; Danny Ashery from Israel, who returned to Israel, I believe, and also does most of his research at Los Alamos in intermediate energy physics; Bibiana Čujec, a physics professor from Laval University in Quebec, Canada; Ziggy Switkowski, from Australia. Ziggy is now high up in the Australian branch of the Kodak Company, and well on the way to becoming very wealthy.

ASPATURIAN: These are all the students who evolved into Caltech's chief competitors in this field?

BARNES: A lot of them did, indeed. What I've given you is only a very small fraction of the whole list. There were so many, and it was a really exciting group. In fact, throughout the sixties and seventies, I think Kellogg really attracted the cream of the crop in the graduate student category into nuclear physics, at least for experimentalists. I think that's partly because we had the brand new EN tandem accelerator, which opened up a completely new and exciting energy region of nuclear study, and partly because there was a kind of lull in particle physics and some of the other competing areas at this time; so we just got a fantastically good group of people. We happened to get a large number of absolutely first-rate students, and out of that group came our principal competitors, to be sure. I can think of a host of them, like Andy Bacher, for example, who is now professor of physics at Indiana. He's Bob Bacher's son. He got his bachelor's degree at Harvard, and came to Caltech as a doctoral student. Peter Parker is a professor of physics at Yale University. Cary Davids is a senior scientist at Argonne National

Lab and also, I guess, he may be with the University of Chicago. Bob Stockstad is a senior scientist at Lawrence Berkeley National Lab. The list goes on and on. Not all of our students and postdoctoral fellows ended up as our direct competitors. A lot of them also went into industry and national labs. At least two I can think of became medical doctors. But enough of them went into direct competition with us to have really kept us on our toes over the years, and they still do so today. Working with Peggy was an absolutely remarkable experience that I'll never forget, because she was just so very competent and determined to do an exceptionally good job, and she really did. She picked an experiment that was very difficult. Her results were the first ones that had any real significance as far as trying to settle the question of the relative amounts of carbon and oxygen made in helium burning in the stars.

ASPATURIAN: Did she have especially finely honed skills in certain areas that allowed her to do this? Or was it partially a matter of timing?

BARNES: You know, it's a matter of being smart. [Laughter] Life is a matter of being smart, let's face it. It's also a matter of being really determined and working very hard. And Peggy really did work very hard. On top of everything, while she was a graduate student here, I happened to learn that she had been doing a lot of other things outside as well. She had a troop of African-American Girl Scouts that she used to spend hours a week with, from an underprivileged area of Pasadena. Peggy herself was physically quite petite. These eleven- and twelve- and thirteen-year-old Girl Scouts all seemed to be about 50 percent taller than she was. But they just adored Peggy. She may well have been a saving factor in the lives and careers of some of these young ladies. During the time she was a grad student here, I often wondered what would happen when she left, because I couldn't really imagine them ever finding anybody to take her place. But she did a lot of outside things like that, as well as turning in an outstanding performance as a young physicist.

ASPATURIAN: If there had been reservations about bringing women in as experimentalists before she came, she must have helped to bring down some of those barriers.

BARNES: I don't think there were any reservations in the Kellogg lab, as far as I know. I can't speak for all my colleagues, naturally, but I happen to be married to a woman who's always been a feminist, and I've shared her point of view. So I didn't myself have the slightest problem. In fact, I was delighted to have Peggy work with us, because she was just so good. She may herself have encountered some problems earlier in her career. I do know that the very first time I met her, she said, "Dr. Barnes," and I said, "Yes, Miss Dyer." "There's one thing that I want you to know," she said. "I want you to know that I don't want any special consideration because I'm a woman. And furthermore, I want you to know that I consider that I am competing with all of the graduate students in this Institute. It doesn't matter to me what sex they are." I said, "Miss Dyer, I don't know how to answer you. It never occurred to me that there would be any special treatment for you, and therefore I don't know how to answer this. I can assure you there won't be any special treatment." She said, "That's all I want to get clear, right at the beginning." You have to forgive people things they say when they first come into a place, because, of course, they don't really know what to expect. I think that she must have had some previous experiences—possibly at Texas or somewhere else—that had predisposed her to making sure that there wouldn't be any special treatment. But we simply aren't the kind of place that expects special treatment, and we don't expect to give it either.

I think it was kind of funny, because when she got her PhD, she was actually much sought after because of her ability. It's also true that a lot of places were under pressure at this time, on the grounds that they might be discriminating against women, or at least may have given the appearance of discriminating against women. People were quite nervous about this. I can remember one day getting a phone call from a university that is not in the Los Angeles area but is in southern California. The man first asked me about Peggy, about her work and her ability to teach and such things. I was able to, of course, give her a very high recommendation on the basis of her performance in all of these categories. But he made one remark which, to me, sounded a bit weird. He said something about, "We have a slot in our department for a woman physicist." I thought, this is going to be fun, because Peggy was standing right there. I said, "Well, I'll put Miss Dyer on the phone and you can talk to her and see what happens." Peggy had heard this remark, I guess, or judged from my response something about it. So she decided that since he was paying for the phone call, he was going to pay a lot for the phone call. She kept him on the phone for what seemed to be about forty-five minutes, and then told him that she wasn't the slightest bit interested in accepting a position just because there was a slot for a woman physicist. I don't know whether that contributed to this department head's education, but

I'd like to think it did. I think she probably did do some good. The next time he tried to hire a woman physicist, I think he would at the very least leave out that qualification about having a slot for a woman.

ASPATURIAN: Or ask first, "Are there any students in the office with you now?"

BARNES: [Laughter] I loved it because Peggy had, in fact, already told me that she thought she would like to spend a couple of years doing research before she started to teach. And indeed, true to that, she went to the University of Washington at Seattle as a research fellow instead of taking an assistant professorship, which she could have had, I think. From there, she went later to Michigan State University, where she was a research professor for a while, and there she met Hamish Robertson, whom she later married. He's an extraordinarily good nuclear physicist also. They both went to Los Alamos later to do research, and I guess they found that a pretty congenial research atmosphere and never did go back to MSU.

ASPATURIAN: During this period, whom were you collaborating with among the astronomers and the theoretical astrophysicists, since your work moved more in that direction?

BARNES: We didn't have very direct work with the astronomers, per se. But most of the theoretical astrophysicists who were working in our area by this time were people like David Arnett at the University of Chicago, Icko Iben and Jim Truran at the University of Illinois, Stan Woosley now at the University of California at Santa Cruz. There were a host of others. And almost every single one of them had been in Kellogg as a postdoc in earlier years, mostly working with Willy Fowler. But we knew them all well, and we continued to have a close correspondence with them as they sort of spread over the United States. Some even went abroad.

ASPATURIAN: Was there anyone here on campus?

BARNES: The astrophysics people on campus at this time were mostly working in different areas of astrophysics. We certainly had good relationships and often talked with Jesse Greenstein and Wal Sargent. We discussed some questions with Leonard Searle; he belonged to the Mount Wilson group. There are probably others I'm leaving out. Of course we talked often with

Maarten Schmidt, because of his work on the discovery of quasars. Most of us felt that quasars must have some association with extremely massive objects, and we were pretty interested in the possibility that there might be an inter-relation between nuclear energy, the centers of galaxies, and gravitational energies. Although, I guess we already recognized at that time that if you bring enough mass together, the gravitational energy can, in fact, exceed the nuclear energy. So it wasn't clear to us exactly what role, if any, nuclear physics would actually play in these extraordinarily brilliant objects. People are still arguing, as you know, about these objects with very large red shifts, the so-called quasars. I think most people now feel that they're associated with galactic centers and perhaps collisions of galaxies, and perhaps they are more likely the result of gravitational energy release near black holes in the centers of massive galaxies.

ASPATURIAN: When Schmidt discovered the significance of the red shift, do you recall there being a lot of excitement around here?

BARNES: Yes, there was a lot of excitement around here, and it has continued, as you know, to be a topic of great interest. And there are still people today who don't accept the cosmological explanation for the red shifts. The idea that the red shifts are mainly Doppler shifts from the Hubble expansion of the universe—therefore they are very distant and very early objects—seemed to us the most direct and simple explanation. Somehow the arguments produced on the opposite side of this question, though a little bit unsettling in a couple of cases, have mostly been not very convincing. Obviously people tend to think of things, not only in terms of their own intrinsic importance, but also insofar as how they relate to their own interests. We probably felt that the energy output of quasars was in the region where it had to be gravitational energy that was being converted into radiant energy one way or another. Although such objects would certainly produce high temperatures, and therefore lots of nuclear reactions and nucleosynthesis, we felt the principal energy source for them probably wasn't nuclear. I still think that's likely to be the case.

ASPATURIAN: Was Gerald Wasserburg at all involved in your work? Did the results feed into what he was working on at that time?

BARNES: We did have very close relationships with Jerry Wasserburg's group. For a long time, part of his funding, in fact, came through a joint grant shared between him and Kellogg. He was very much a part of Kellogg through a great many years, extending up until just a few years ago, at which point his own funding got big enough that he didn't need any external funding from us. Jerry has done extremely fine work, and it was always a pleasure to have this close relationship with him. There is a very direct relationship between nuclear physics and Jerry's work. His explanation—or at least one of the explanations—of the many isotopic anomalies that he and his colleagues have found is that they are nuclear in origin, and that is directly related to our work, right across the whole range of nuclei, from light to heavy nuclei. In some ways, although Jerry knows quite a lot about nuclear physics, too, it is our main subject area, so we also have occasionally had some ideas that might have been useful to him. It works both ways, because if somebody is working on nuclei that would be made in, say, a supernova explosion or in a nova, or perhaps in the course of ordinary stellar-burning, it would likely have consequences for the kinds of research that Jerry is doing. In fact, there are lots of isotopic anomalies that remain poorly understood at this time. Perhaps the most striking thing that Jerry's group discovered was the result of work by Typhoon Lee and Jerry. Typhoon Lee was actually a graduate student of Don Clayton's, who, by the way, I should have mentioned earlier as a colleague of ours, and a former student and postdoc. Typhoon was sent to Caltech to do his thesis work with Jerry, because he wanted to work on isotopic anomalies; and his advisor realized correctly that the best place in the world for him to do that was right here at Caltech.

The task that Jerry set him to was looking for magnesium isotope anomalies, something most of us had long worried about. There are three isotopes of magnesium, so it's a particularly favorable case to work on. We know that there are lots of physical and chemical processes that give some small amount of fractionation of isotopes based on isotopic mass, and that these small fractionations are interesting to some people perhaps, but also in another way less exciting than the possibility of anomalies caused by nuclear physics, in some special environment. But in order to subtract out the effects of chemical or physical isotopic fractionation and still be able to say something about whether there's anything interesting left over, you need at least three stable isotopes. Magnesium satisfies that requirement with the isotopes 24, 25, and 26. Aluminum-26, a radioactive isotope of aluminum, which has only one stable isotope—aluminum-27—decays into magnesium-26 with a mean life of about one million years. That is short, compared with the age of the earth. At the time this particular anomaly search started, it was thought that the mean-life of Al-26 might be much shorter than the time it would take to form the solar system. It's still difficult to understand, although even shorter-lived radioactive elements have been detected by their decay products as isotopic anomalies. Coming back to Al-26, other people had looked for magnesium-26 anomalies, and, in fact, they'd also been looked for here unsuccessfully by David Schramm, who worked as a graduate student with Wasserburg. It fell to Typhoon Lee to find these isotopic anomalies in some extremely primitive meteorites, particularly Allende, which Jerry Wasserburg carefully got large amounts of by driving down to Mexico after the thing fell. He was Johnny-on-the-spot collecting great chunks of the meteorite, which broke up as it fell from the heating in passing through the atmosphere.

ASPATURIAN: They just let him take it away?

BARNES: Oh, I suspect that all the local villagers were busy selling pieces of the meteorite, of which there were a large number. I've never gotten into this question with Jerry—and don't plan to—but I can't think of a more worthwhile place for pieces of the meteorite to go than to Caltech for serious scientific study.

In any case, Jerry had obtained a lot of the meteorite Allende. It turned out that similar anomalies were also found later in a few other very primitive meteorites. That's the crucial point—they are carbonaceous chondrite meteorites, so you could tell by their content that they haven't been subject to too much processing at elevated temperatures. Also, they can be inferred to be very primitive from their uranium ages and other such things. Jerry's group found first, some relatively small magnesium-26 anomalies, but larger than isotopic fractionation could produce. Eventually, they found magnesium-26 anomalies in some mineral grains that were indeed very large. I mean, they were on the order of many percent. Their precision was such that they could easily measure an anomaly that was well below one part in a thousand. In fact, I think their measurement uncertainty in good samples was down in the few parts per 10,000 level. They were sometimes seeing anomalies that were hundreds of times bigger than the uncertainties in their measurements.

Anyway, that was a marvelous discovery. The most exciting feature of this particular discovery was that the excess magnesium-26 turned out to be perfectly correlated with the aluminum content of the minerals. This meant that the precursor to this excess magnesium 26 had to be aluminum-26, and it had to have been incorporated in the meteorite grains as aluminum, because it was so well correlated with the aluminum-27 content of these objects. It was a fantastic discovery and had been justly celebrated.

We worried a lot about aluminum-26. A lot of the cross-sections that we had measured and continued to measure bear on such questions as where and how could you make aluminum-26. It's nontrivial because it turns out that aluminum-26 would be destroyed very easily by neutrons. So the first and obvious answer—that you make aluminum-26 in supernovae—may turn out not to be very promising because in supernovae you tend to make lots of neutrons as well, at least in some regions. Of course, there are regions of the supernova where there may not be so many neutrons, but it seems at this time very difficult to actually make that work. The most likely prospects now are novae. If you have enough hydrogen present you may ignite a burst of nuclear reactions, and they start with some stuff like carbon, oxygen, and neon, you may have a hydrogen-rich chain of nuclear reactions, in which you could make aluminum-26. If there's lots of hydrogen present, the hydrogen also tends to eat any neutrons that you might form, so it may prevent the destruction of the aluminum-26.

So, such discoveries are tremendous boosts to our morale, naturally, and our research is clearly motivated to trying to understand such discoveries. Right now, we're trying to understand how sodium-22 might be made, as there appear to be some meteoritic neon measurements that could conceivably arise from the radioactive nucleus sodium-22. **[Tape ends]**

Begin Tape 7, Side 2

BARNES: In some meteorite mineral grains, one finds isotopic anomalies in neon, where there are also three stable isotopes, and the same game can be played. In fact, one finds perhaps even more remarkable anomalies there. One finds samples in which the neon appears to be almost pure neon-22, although neon-20 is normally the most abundant isotope in nature. It's difficult to imagine how you can produce this without some nuclear physics being involved. One of the possible scenarios, just like the one suggested for the production of magnesium-26, would be that you first of all, make sodium-22, which is a radioactive element, and that the sodium is somehow captured into mineral grains, which become part of the meteorite that you pick up many years later. The sodium-22 in these mineral grains would decay in situ into neon-22, which is trapped to a fairly good extent in the crystal lattice of the mineral grains. The thing that is perhaps most puzzling at this time about the sodium-22, neon-22 hypothesis is that the half-life of sodium-22 is only two-and-a-half years. That really rules out a whole host of scenarios that one might imagine. The process probably is still possible in novae, because we know from observational evidence that in novae one starts to observe grain formation after a few months. And it seems quite possible that one could incorporate sodium-22 into these grains as sodium and that these grains might sometimes have a subsequent history that lets them become part of a meteorite that we pick up.

There's a lot still to be done about understanding such anomalies, and no doubt a lot of information can still be obtained from these observations about conditions in the solar system as a function of time, particularly in the early history of the solar system. That is not our particular expertise; our job is to try and understand the nuclear physics as well as possible, to find out what limits we can impose on the processes this way, and to discover whether things seem likely or unlikely to have happened this way. But you know, every little while there's some new, exciting discovery. Neither the magnesium-26 nor the sodium-22 hypotheses is particularly new anymore, but as you probably know, the beta decay of aluminum-26 was recently detected in interstellar space by a group at JPL with a satellite observation in amounts that I guess surprised a lot of people. There can't be much doubt that what was actually detected is a characteristic gamma ray from the daughter nucleus magnesium-26. This is telling us that nucleosynthesis continues on a substantial scale throughout the galaxy; none of us personally doubts that, actually, because there's lots of other evidence that nucleosynthesis continues to this day. But it's an interesting and completely independent kind of confirmation of continuing nucleosynthesis. Of course, since these results were obtained by Jacobson et al, we have been trying to understand aluminum-26 production better.

ASPATURIAN: In terms of challenges and discoveries, how does the work you've done since the late sixties relate to the studies you did in the fifties and earlier sixties?

BARNES: It's certainly a different kind of research, although I think that there really isn't that much difference between nuclear physics and nuclear astrophysics. But there is a question of motivation of why you're doing things. I think, for example, that it would be a terrible mistake for somebody to make a too narrow study of nuclear astrophysics because to me, astrophysics—and this includes nuclear physics and astrophysics—is just physics in a different context. It's a context in which most of the objects of interest are not directly accessible, so you often have to understand these objects somewhat indirectly, by astronomical observation, or via meteorites, or gamma-ray observations, or whatever. I expect that eventually neutrino observations will be added to nuclear astrophysics, as happened in the most recent supernova, 1987A. The point I'm trying to make is that a sound nuclear physics background is important, as well as a sound background in other areas of physics, just because of the indirectness involved in the process of understanding astrophysics. Those parts of physics that you can really do a good job on in the lab, one must do. There'll always be uncertainties that arise from the fact that you can't literally lay your hands on this stuff except for small amounts of matter like meteorites. There are always going to be so many uncertainties that those parts of a problem that can be done quantitatively, in the lab, must be done, and the nuclear part is one such part. It's not always easy. In fact, the particular nuclear reactions that one would choose to study if one's principal motivation was nuclear physics are often quite different from those one would choose to study for nuclear astrophysical purposes. We often find ourselves inventing new techniques that are necessary to do a kind of nuclear physics that nobody has done before. Obviously we get some reward from meeting and overcoming such challenges. I suppose that personal vanity has something to do with it; it always makes you feel good if you can find the way to overcome one of these experimental challenges that nobody else has actually figured out how to overcome. That makes one feel needed, at least.

ASPATURIAN: What was it that motivated you specifically to shift directions? Were there some philosophical considerations, or anything like that involved? Were the implications more interesting? Or was it purely the challenges at the theoretical and instrumental level?

BARNES: There are no simple answers to these kinds of questions. But as long as I've been in Kellogg, except for a brief period when we first got the EN tandem accelerator, we have been outgunned, as it were, by our colleagues around the world, and especially in the United States. Most places have had bigger accelerators, more accelerators, and so on, than we have. I would guess that our survival, going back to before I got here, has been due to the fact that we worked harder and thought harder about problems. When we were doing mostly what would only be called nuclear physics, we still had challenges to try to overcome experimental problems that other people either hadn't encountered or had not elected to try to meet. Since these challenging nuclear reactions that we studied were recognized from quite early days as being important to nuclear astrophysics, it was natural—for me, at any rate—to continue in this line. I like this mix of, on the one hand, trying to make theoretical predictions on what kind of nuclear reactions would be important astrophysically, and on the other hand, the experimental challenge of trying to carry out some measurement that most nuclear physicists would probably not try to do. They would probably consider many of the experiments we did simply unrewarding because they would be too difficult. Spending time on them wouldn't really motivate them, or at least the rewards as they perceived them wouldn't really motivate them to do these experiments. Often it was because they didn't recognize the importance of them in nuclear astrophysics. It's a lot easier to follow the course of least resistance if you have a choice between doing the hard or the easy experiment. It's often the case that people would argue, let's do the easy thing first, because we'll advance knowledge faster that way. That's not always true. It's often the case that the ones that are easy get done first, but cease to be interesting after that.

I liked this mix of experimental challenge and being able to see, with the help of my theoretical colleagues, why these results really were important in the grand scheme of the universe. Willy used to say in his talks, "We're all a bit of stardust," which may not be an original remark.

ASPATURIAN: I think Carl Sagan must have stolen it from him.

BARNES: I remember hearing it before I heard Willy say it, but I don't remember where I heard it first. We liked doing experiments that try to figure out how the universe developed. That's not something that I lie awake pondering, but it's often there in the back of my mind. I'm not so parochial as to imagine that nuclear physics is going to answer all of astrophysical questions, but nuclear physics—again I come back to this point—is one area of physics where meaningful and quantitative statements can actually be made if one puts in the effort to get the answers.

ASPATURIAN: During this period, I note that Caltech went through three different presidents, and the division chair passed through several different people. What impact did that have on what was going on in Kellogg?

BARNES: From my observation point, I think that we got really pretty good support from all of the presidents that we had, and especially from all of the division chairmen. Certainly Bob Bacher had made part of his name in doing nuclear physics. Along with Hans Bethe, in 1936 and 1937, he had published articles in the *Reviews of Modern Physics* that were a kind of Bible of nuclear physics until post-World War II days. Although his principal scientific accomplishment after he came to Caltech was the establishment of particle physics here—then called high-energy physics—he nevertheless continued to be interested in nuclear physics because he understood what we were doing, and why we were doing it.

Bacher's immediate successor as division chair was Carl Anderson, who, of course, had made his fame in discovering the positron, and later what they called the mesotron—now known as the muon—before they figured out exactly what it was. Carl was also an experimentalist—as was Bacher, although Bacher was really both experimental and theoretical. Carl was a really super experimentalist. And Carl was pretty good to us, I think. During that period, I didn't have that much to do with the administration of our project at all, but as far as I could see, we enjoyed a high level of help from the division chairman. After Carl, there was Bob Leighton, who certainly wasn't specifically interested very much in detail in nuclear physics, but Bob of course understood nuclear physics very well. In fact, there was scarcely anything in physics that Bob hadn't made a study of. He was a very versatile fellow, and his great textbook, *Principles of Modern Physics*, had, at the time it was first written, one of the best explanations of nuclear physics.

Then Bob was followed by Maarten Schmidt, who looked at nuclear astrophysics more from the astrophysical point of view, but it also meant that he was certainly somewhat interested in what we were finding and, of course, we used to talk about lots of things that had nothing particularly to do with our work here in Kellogg. There were lots of areas where nuclear physics impacted on astrophysics, but where the nuclear physics part of it was beyond the scope of any equipment that we had here. Maarten was followed by Robbie Vogt, and later Ed Stone; both of these came from the cosmic ray business, and were particularly involved with looking at elemental and isotopic distributions—the relative isotopic abundances of the elements in the cosmic rays. And that fed directly into our interest in the production of the elemental and isotopic abundances. I think they were particularly interested in more recent times in the heavy elements, where there's still discussion as to the true facts about the cosmic rays, and their elemental and isotopic abundances. But these come from our processes in nuclear astrophysics, we believe, and so there was, again, a natural common bond that depended mainly on our knowledge of neutron cross sections. We weren't specifically much engaged in measuring neutron cross sections, but it was part and parcel of our attempts to understand the nuclear physics of the astrophysical sources of neutrons, and how the neutron absorption cross sections would vary with the number of neutrons and number of protons in each nucleus.

ASPATURIAN: Brown and Goldberger were both physicists by training. Did they show any particular interest in what was going on here?

BARNES: I don't actually think that Harold Brown really had very much direct relationship with our program in Kellogg. This might be my ignorance, because it might be that he had interaction with Willy, for example, that I didn't know about. Before him, of course, Lee DuBridge was another of these physicists like Bob Leighton, who really knew a lot about all branches of physics. Lee went to bat for us with the federal government a number of times to help keep our program funding strong, and I always appreciated that. I can remember one occasion when we were considering building a much larger, higher-energy accelerator which, in fact, never got funded. But there was considerable competition in various places in the country, including right here in southern California. Lee went with us to Washington and supported our case very strongly. It turned out that this accelerator wasn't built at Caltech or anywhere else at that time. But Lee certainly deserves a great deal of credit for the forceful and thoughtful way that he undertook these somewhat delicate negotiations, because they did involve other universities. But it just happened that we didn't have the same particular relations, as far as I was aware, with Harold Brown. Now, Murph Goldberger was a theoretical physicist, as you well know, who had done a lot of very good theoretical nuclear and particle physics. So he understood nuclear physics, and understood the relevance of nuclear astrophysics, although I don't know of any occasion in which he was particularly and specifically active on our behalf. I believe that he supported us in the physics division along with other parts of physics, and he supported all of us strongly. As you know, all of Caltech's chiefs, or presidents, have been physicists—except for our new president.

ASPATURIAN: Are the graduate students you're getting now as good as the ones you had ten, fifteen years ago?

BARNES: I think the graduate students we're getting now in Kellogg are still very good. I think there aren't as many of them as there were through the sixties and the first half of the seventies. The thing that's not generally realized is that each year the physics part of the division typically admits between 20 and 25 new students. Sometimes the number fluctuates down as low as 14 or 15, and sometimes it goes up to 26 or 27, or even higher. A lot of these are students who come here planning to do theory, and so the number of new graduate students in experimental physics in a typical year is probably 10 on the average, sometimes 15, and sometimes fewer than 10. In this division, there must be some 20 faculty members in experimental physics who are looking for graduate students. With that kind of admission rate, there aren't going to be enough to go around. In fact, it's a countrywide phenomenon, not just a phenomenon at Caltech. There's a national shortage of experimental graduate students.

ASPATURIAN: In terms of the administration and research emphasis of Kellogg, have there been major changes in the last 10 or 15 years?

BARNES: No, I wouldn't say that there've been enormous changes. There have certainly been some changes—it would be wrong to say there've been none.

ASPATURIAN: I get the sense, listening to you, there's been a familial atmosphere around here.

BARNES: There has been to a great extent quite a familial atmosphere. It doesn't mean there are no disagreements—sometimes there are disagreements in families, and that also can happen here. But there was a growing tendency for part of our program, particularly through the seventies, to expand strongly in the direction of applications of nuclear physics and the techniques of nuclear physics to classical physics problems.

ASPATURIAN: How did that happen?

BARNES: It was partly because people began to realize that the same techniques that you use in nuclear physics with accelerators, radioactive materials, and nuclear reactions could be used to learn interesting things about the structure of solids and the structure of materials.

ASPATURIAN: This was the work, I guess, Dr. Thomas A. Tombrello was involved in?

BARNES: Yes, Tom Tombrello and his associates have particularly worked in the applied area recently, although all of us have done some applied experiments occasionally. None of us really has any scruples of any kind against doing interesting things, whether they're applied or pure physics. But at some point, it became clear that the expansion of the effort in the direction of applications, or applied physics, was inevitably going to lead to a division of the grant and a division of the group. This did happen, and I'm glad to say that Dr. Tombrello and his associates actually have been successful in obtaining adequate funding to do their applied work, and that the remainder of the group who chose to work primarily on nuclear physics and nuclear astrophysics has also successfully managed to maintain its position vis-a-vis funding, and position in the science world.

ASPATURIAN: At the other end of the spectrum, you have Steve Koonin, a theoretical physicist attached to Kellogg who was, I understand, an undergraduate here.

BARNES: Yes, Steve was an undergrad here. In fact, he did two really significant research problems while he was an undergraduate. As a senior, he did an experimental problem in Felix Boehm's group with Borje Persson, who was a research fellow at that time, on gamma-ray radiation accompanying K-electron capture in Be-7. Steve also did a theoretical calculation in Kellogg with Tom Tombrello as advisor that was among the first efforts to develop a good theoretical framework for the carbon-12 (alpha, gamma) experimental work that Peggy Dyer and I did. He certainly was a very capable student and I expect that he won several of the undergraduate prizes for his undergraduate research.

ASPATURIAN: Were you involved in the decision to bring him back here as a faculty member?

BARNES: Of course we were all involved in that. We followed his progress with great interest as a graduate student at MIT. Of course, we already knew him well, and we were there with an offer in hand, along with our competitors, when he got his doctor's degree. [Laughter]

ASPATURIAN: Does that happen often, that you tag someone as an undergraduate and say, "Circumstances being what they are, if the trend continues, we'd like to have this kid back." Or is he a fairly unique case?

BARNES: I'd say it's fairly uncommon, because clearly we could only hire some absolutely minuscule fraction of our output. But, in the case of Steve, we already knew well that as an undergraduate he was an extraordinarily promising individual. It doesn't always happen that people continue to exhibit the same high level of promise. But we also personally knew the people at MIT with whom he was doing his graduate work, so we were able to monitor his progress pretty directly, and we were well aware that he was fulfilling the kind of promise that we knew from his undergraduate career.

ASPATURIAN: When did the yellow submarine accelerator come in?

BARNES: The history of the yellow submarine actually started back in 1976, believe it or not. [Laughter] We had the old homemade accelerators upstairs that were becoming more and more difficult to keep in operation.

ASPATURIAN: Homemade?

BARNES: Home-built, built in Kellogg. The first three accelerators we had in Kellogg were built right here in this lab with the hands of our own people—not including me; they were actually built before I came. The first one was built in 1937-38, and then two others were built immediately after World War II. The pressure tank to hold the insulating gas for one of them—the so-called "three-million-volt machine"—was purchased before the war. During World War II, it was shipped up to the Morris Dam, where it became the compressed air storage tank for the submarine testing facilities that were developed there. After the war, the tank came back to Kellogg, and our students and staff started to build an accelerator to be housed in that tank. At the same time, they started building a very low-energy Van de Graaff accelerator here, too. I guess the three-million-volt was in operation by about 1947; the smallest one perhaps by ’49; this was all before I came here in 1953. Somebody joked that we had to stop requiring graduate students to build their own accelerators because we ran out of space. [Laughter] We couldn’t have any more accelerators. By the 1970s, the old accelerators were getting harder to keep in repair and were becoming less competitive in performance.

ASPATURIAN: What makes an accelerator break down irrevocably?

BARNES: Things just wear out. There are a lot of moving parts in them, and those wear out. If you have high voltages around, you occasionally get huge sparks flying around, and these sparks can cause all sorts of damage to insulators, drill holes in them or crack them, and just cause a lot of general damage. But, in the mid-seventies, I recognized that if we were going to remain competitive in nuclear astrophysics, we needed an accelerator to complement, not compete with, the EN tandem, one that would produce much bigger beam currents with very well-defined energy, even though at lower energies than the EN tandem.

ASPATURIAN: Who were Caltech’s chief competitors at this time, and I suppose still are, in the nuclear astrophysics field?

BARNES: There was hardly anybody with a low-energy accelerator who wasn’t competing with us. There were people at the University of Washington in Seattle; there’s a big group of people in Argonne National Lab; the people at Yale competed with us. Oh, they were just everywhere—people at Texas competing with us; people from Florida State competing with us. Oak Ridge was competing with us. From the middle sixties on, we had perhaps some of our toughest competition in Europe, especially in Germany, where nuclear physics was well supported financially, and there was a strong scientific background and superb training in the universities. Nuclear physics was one of the areas that the German science community really believed they could excel in, and they funded it very strongly. They also funded particle physics strongly, but they put a bigger ratio of funding into nuclear physics, compared to particle physics, than we did here.

Anyhow, I just wrote up a proposal—it took many months to write—to get what I could truthfully call a low-energy but high-current, high-stability accelerator. It was a proposal to the

NSF. Funding wasn't forthcoming immediately.

ASPATURIAN: Had the NSF by this time taken over a lot of the program funding in Kellogg?

BARNES: They had taken over all of the Kellogg funding. They took over from the Navy, the ONR, about 1962 or 1963. When the money finally became available for the new accelerator, if I remember rightly, we were awarded two back-to-back $400,000 grants; one was granted in the late part of fiscal '79, and the other in the early part of fiscal '80. So it was the NSF's way of giving us an $800,000 grant, in two pieces, out of the modest equipment budgets at the time. It's still a very modest amount of money compared to what big particle accelerators cost. But for the NSF it was major equipment funding, because they simply didn't have the kind of money for building big accelerators, which were all built by DOE.

In my mind, I viewed this one as something that would replace all of the old homemade accelerators, and do a very much better job, and put us back into competition with some of our competitors elsewhere, who for years had much bigger beam currents available than we had. You can overcome a certain amount of equipment advantage on the part of your competitors if you're lucky and if you work hard, but it gets harder the bigger the discrepancy in equipment becomes.

ASPATURIAN: So, a central piece of the new tandem accelerator was built elsewhere and brought in?

BARNES: Formally, it was funded by the NSF, and entirely built commercially. When I got this money, instead of replying to the original proposal directly, NSF basically said, "We're effectively awarding you this $800,000. See what you can get with it." [Laughter] So, we in Kellogg sat down with the engineers from the various companies that made accelerators to see what they could do.

ASPATURIAN: Who else was involved in bringing this in?

BARNES: All of my colleagues participated in the discussions. To be sure, I probably put in the most time myself. But clearly I wanted this to be an accelerator for everybody, and it was natural that I would solicit ideas from anywhere as to what we could get. But in the end, it came down to arguments between us and various manufacturers. The limited amount of money available ruled out some attractive options; for example, for four times the amount of money we could have gotten a different and possibly better accelerator. But I think, in the end, we got what could only be described as an exceptional offer from National Electrostatics Corporation in Wisconsin, to build an accelerator for $800,000. The new accelerator was in all respects better than one proposed to us by the same company several years earlier, for which they wanted $1,200,000. So it was an incredible turnaround in pricing. But I think it was also a happy meeting of minds between us and the National Electrostatics Corporation on what they could do. And they did supply a pretty good machine. Now, their way of producing things was to build the machine in their plant in Wisconsin, and then to ship it here after they had actually got the accelerator working in their plant. For a number of reasons, they didn't get everything working in the plant, but you know, things don't always go exactly according to plans, even in the best of companies. Eventually, I think that the tank was shipped here in the beginning of November '81. At that point, we already had available a new laboratory that had been constructed mainly with help from Caltech—there was also some help from the Kellogg Foundation, but the lab money came mainly from Caltech. We anticipated it would still take some months before the accelerator would be able to perform up to specifications. I don't think we anticipated that it was going to take as long as it did, but in about January 1983, the company essentially handed the machine over to us and said, "We can't quite meet the specs we promised. But, at this point, we're not making progress very fast, and we know that your people would be at least as good at working on the machine as our people are, and we've just got to settle one way or the other." So there was some minor compromising with the company, and they handed the machine to us. It still took us several months, I would say, to bring it into full operation. But we began to do really serious research with it perhaps about summer of 1983. That was seven years after I had first written the proposal; and, of course, things change with time. A lot of the things that I had wanted to do in 1976 had been done by our competitors by this time. But fortunately there were still lots of really exciting and interesting things to do. We had a very versatile group of people on board by this time, including some new faculty, so we decided to concentrate first on a rather major experiment, instead of doing a large number of smaller experiments, although these were just scheduled a little bit farther back in the program of things we wanted to do. We had a new assistant professor, Bob McKeown; and not too long after that, a new research fellow, Brad Filippone, from Argonne, who's now an assistant professor here. Among the ideas that were proposed at this time was to make a search for free quarks in matter. That experiment was largely spearheaded by Bob McKeown. Bob did a very fine job doing that experiment, and it's highly regarded for the quality of the result it produced. In some ways it would have been infinitely more exciting if we'd found that matter was just bulging with free quarks. Of course, in that case a lot of quantum chromodynamics would have had to be rewritten! **[Tape ends]**

CHARLES A. BARNES

SESSION 6

August 7, 1987

Begin Tape 8, Side 1

ASPATURIAN: I'd like to talk a little about Willy Fowler's 1983 Nobel Prize. And I will preface it by saying that I've heard from several people on campus that you played a role in nominating him.

BARNES: We had previously nominated Willy—when I say, "we," I mean the entire faculty in the Kellogg lab—about ten years earlier.

ASPATURIAN: Was he nominated at that time with Fred Hoyle for his work?

BARNES: I don't think so, although I really don't recall. Do you remember what year Willy was awarded the prize?

ASPATURIAN: 1983.

BARNES: I think it must have been a couple of years earlier that I sent in another nomination, this time by myself, because I just felt that this was the most appropriate time. People everywhere were beginning to more fully recognize the importance of Willy's contributions. Furthermore, my approach to the nomination was that the unique thing Willy had done were the calculations based on what is known about nuclear physics. They weren't particularly revolutionary from a strictly nuclear physics point of view, but they utilized whatever was known about nuclear physics for astrophysics environments. On the basis of these calculations, Willy had identified the places where new experimental work was absolutely crucial to determining the course of nucleosynthesis and stellar energy generation. In a way, Willy had defined a new area of physics—nuclear astrophysics—largely singled handed. I felt that his work was now being recognized more fully, and that the time was ripe to recommend him for a prize. I suppose many others had also nominated Willy over the years. Besides, it seemed to me that there was a kind of symmetry there, with Hans Bethe winning the prize for purely theoretical studies.

ASPATURIAN: Do the various Nobel committees routinely ask senior faculty at various institutions to nominate people in their fields?

BARNES: They do invite nominations on a limited scale, I guess, and from time to time, they will explicitly send out nomination forms to faculty. It's never been clear to me how they choose whom they're going to send these to, or how often. I have received these forms a number of times in my career, and it just happened that I had the nomination forms sitting in my desk, which I'd received a few years earlier at a time when I really didn't have anybody particularly in mind to nominate. So it was natural that I would use these forms when I nominated Willy. But it's not required, of course, that nominations have been invited by the Nobel committee. Nominations for the Nobel Prize presumably often come in completely unsolicited. [CB subsequently added: "I have nominated a few other physicists over the years, and most of those physicists have actually been awarded the prize, although my nominations would have to have coincided with those of many others for that to happen, I'm sure." —Ed.]

ASPATURIAN: Did Fowler know you were nominating him?

BARNES: No. I never told him. In fact, up to this day, I've never told him. I suspect he may have heard somehow, but I don't recall that I ever told him.

ASPATURIAN: Did you know in advance that they were in fact going to select him?

BARNES: It was a complete surprise when he was awarded the prize. I can tell you how the news came to us. About six o'clock one morning the phone rang—and that's a little unusual. We're used to people calling us late at night but not that early in the morning. [Laughter] My wife leaped out of bed to get to the phone and called me immediately. I was initially very puzzled by this. It was Ardie Fowler, Willy's wife. And she said, "You'll never guess what's happened!" I said, "Willy's won the Nobel Prize." She said, "How did you know? Did they call you also?" I said, "No. But why would anyone call at six o'clock otherwise? It was just a wild guess." So, Ardie continued, “I’ve just heard from the reporters, and also from the conference that Willy was attending at that time in Michigan at the Yerkes Observatory, that Willy’s just been awarded the Nobel Prize.” She was really completely excited. I realized right away that this was not any usual type of occasion, and realized also immediately that, of course, this news must have been known some hours ago in the East, with people being up that much earlier. So I said, “Have you called anybody else?” And she said, “Well, I was just about to call Ward Whaling.” And I said, “Well, that’s great. You should tell him that we’d better get over to the lab.” So both Ward and I came over here as quickly as we could get dressed, really, and the phones were already ringing like crazy all over the building, from people who didn’t know that he was not here on campus at that time calling to congratulate Willy. Ward and I just sat on two different phones in the offices upstairs and manned the phones until the first secretaries came in at eight o’clock. And all we could really do was to write down and note the names of all the people that called. Most of them were also going to send written congratulations anyway, but they just wanted to be on record verbally as soon as possible, too. So about eight o’clock, I guess I went and got some breakfast, because I was beginning to get a little hungry at this point, and the secretaries very capably took over the job of manning the phones. I guess our first job was to notify other people at Caltech. I think the first people we notified may have been the Public Relations Office. I don’t remember for sure, but I suppose we called the president and the department chair even before Public Relations, but the wheels had to get into motion pretty fast.

Later that morning, a large roomful of the media assembled in the Athenaeum lobby, where we presented interviews involving Bob Christy, Robbie Vogt—then our division chairman—and me, as associates of Willy’s for many years. This televised interview was fun. One of the highlights that had been arranged, and it even worked, although it looked for a while as though it wasn’t going to, was a telephone hook-up with Willy at Yerkes. So we were able to bring live to the audience a conversation with Willy on how he felt at that moment. Of course, this was several hours after Willy had first heard about it at Yerkes, and I’m sure that Willy must have recorded somewhere how he came to hear about it in the first place. It was clearly a great surprise to him. He’s often told me how it happened. He simply didn’t believe it when somebody appeared at his bedroom, at this conference, before breakfast, while he was still shaving, standing there in half a pair of pajamas. He thought they were pulling his leg. But of course, it electrified the conference, at which Willy was already scheduled to give the keynote address that day on nuclear astrophysics. So it couldn't have been a more opportune occasion for this to happen to him.

ASPATURIAN: You had co-edited a book about a year or so earlier, *Essays in Nuclear Astrophysics*, for the occasion of Dr. Fowler's 70th birthday. Do you think that had something to do with preparing a favorable atmosphere?

BARNES: I wouldn't say that something like that really affects the Nobel selection committee. I'm sure that they're human beings like everybody else, but I'm sure they don't pay much attention to things like this. It was true that this book, *Essays in Nuclear Astrophysics*, turned out to have a wide circulation, on an international scale. I felt rather pleased, because I had put a lot of effort into the book, and I think it turned out very well. Of course, some of the credit for how well it turned out belongs to the publishers—Cambridge University Press. In particular, they brought out a paperback copy intended for students and postdocs almost simultaneously with the hardbound copy, and unknown to me, they had put Willy's picture on the front cover of it. Everywhere I went in the U.S. and Europe for a couple of years after that, I'd see Willy's face looking out at me from the bookstands. It certainly helped to make Willy even more widely known than he would have been otherwise, I'm sure. But no, I don't think the selection committee for the Nobel Prize would be influenced particularly by something like that. I think that there are a lot of things that, no doubt, went through their minds. One probably was that, though there had been some recognition of the role of astronomy and astrophysics, the committees may have felt that on an integrated basis over a long period of time, there had been insufficient recognition of this side of physics, at least partly because there is no Nobel Prize for astronomy. I vaguely remember hearing some story to the effect that Nobel had some trouble with his wife—or one of his wives—eloping with an astronomer, and that he decided on that basis that the whole subject of astronomy was too flakey to merit a Nobel Prize. [Laughter] I believe it was explicitly expressed in his will that the Nobel Prize would not be given for astronomy. I suppose that one way out of that is to call it astrophysics, and that's clearly a legitimate and highly valuable branch of physics. However, the story that I heard was probably complete nonsense, anyway, just part of the lore accompanying the prize.

ASPATURIAN: Did the selection committee invoke anything that you had said in the nomination when they put together the citation for Dr. Fowler?

BARNES: The wording wasn't exactly the same as in my nomination, but my nomination particularly included the words "for his work in experimental and theoretical nuclear astrophysics." I felt when I nominated him that this was a particularly unique contribution Fowler had made, and I was rather pleased to see that they had reproduced that part of the wording, just as my nomination had said. Of course, I don't doubt that lots of other people had nominated Willy from time to time, but I was not aware of anybody else nominating him just at this time. But I think the Nobel committee did a fine thing. They chose to honor Willy and [Subrahmanyan] Chandrasekhar simultaneously, and I think that was a fine thing for the selection committee to do. It indicated that, Mr. Nobel's will notwithstanding, they recognized the importance of astronomy and astrophysics, and they were going to acknowledge the subject area under the guise of astrophysics. That was an exciting and really very rewarding interval!

We were all pretty excited on the campus, of course. It was a Wednesday when we heard about this, and Willy was stuck at this conference until Friday morning. So the Institute scheduled a general party in Dabney Gardens for Friday afternoon to celebrate the prize. Of course, this all had to be arranged ahead of time because the press would all be here, television cameras, and so on. A bunch of us decided that we should try to do something a little bit more unusual. One of the prime movers who deserves a lot of credit for her enterprise in this was Julianna Sackmann-Christy. She came to me and said, "We must have something for Willy." And I said, "Yes, I think we really ought to. But, you know, I am really very busy with a lot of other things in connection with this at the moment. How about getting the undergraduates involved in it?" She said, "What do you mean?" And I said, "Well, why don't we get a great big banner on the wall of the Millikan Library so people can see it from a long way off, saying congratulations or something like that. We'll get the undergraduates to put it up on Millikan." She said, "Well, is there anything else we can do?" I said, "Well, I've been informed that he's going to walk along such-and-such a walk at a certain time heading for this party. Why don't we let the undergraduates do their thing?" What I meant was this great custom that they play at exam times, in the student houses, *The Ride of the Valkyries*, by Wagner. They play it to wake themselves up during exam time. It just happened that we had the RA of one of the student houses in Kellogg at that time. So Julianna and this young man, Rick Kremer, got together with the undergraduates, and they got in touch with some former Caltech students who ran an electronics, computer, and hi-fi business up on Colorado Boulevard. These people loaned for the occasion one of the highest-powered sound systems I've ever heard. From the top of the Millikan Library at the appropriate moment boomed out *The Ride of Valkyries*. I'm sure it could have been heard all the way to JPL. The undergraduates lowered a huge banner that Julianna had helped them make, which said, "Whoopee Willy!" and I think that added to the occasion, because I noticed that most of the TV cameras actually took a picture of it. People asked Willy questions as he walked by; and a lot of these were quoted that evening on television; and then he went to the reception that the Institute had scheduled in Dabney Gardens.

ASPATURIAN: Was there any fallout from this award that affected Kellogg as a whole?

BARNES: It's very hard to quantify such things. Some of our friends around the country, even some of our competitors, used to make remarks to be funny that were perhaps close to being on the snide side, by saying, "Well, it doesn't hurt to have a Nobel Prize winner in your group when it comes to funding." It really doesn't hurt, but the fact is that it may not help that much either. The funding agencies are used to Nobel Prize winners, and they base their funding on merit and promise as they perceive it. But, you know, it undoubtedly didn't hurt. In a sense, it didn't really change Willy's position in Kellogg, because Willy, by virtue of pure merit, was already the undisputed leader of the nuclear astrophysics effort in Kellogg. He didn't need a Nobel Prize to confirm that—it was a fact. Still, all of us basked a little bit in the reflected glory. But Willy was really generous about it. On every occasion he had, he made it clear to people who asked him about the award that he himself considered it an award to the entire lab.

ASPATURIAN: Yes, he seems to have a great generosity of spirit in that respect.

BARNES: Yes, indeed. That was very nice of him, and obviously much appreciated by the people in the lab.

We got letters for quite a long time from people abroad who would start out with puns like: "Dr. So-and-So, of the recently enNobeled Kellogg Lab." That went on for quite a while and it felt good. But I would say if it was good for Kellogg, it was actually good for the whole area of nuclear physics. There's always competition among different areas of physics for the attention of the public. There's also competition for the attention of the people working within physics.

ASPATURIAN: Where do you see nuclear astrophysics heading in the next ten to twenty years?

BARNES: I think that it's never been a really large branch of physics from the point of view of the number of people working in it. It really can't be a very large branch. In the first place, the number of things that can be done at any one time is not all that big, and is dependent on the developing technology. The experiments require a great deal of thought and ingenuity. Certainly people are always writing to us and saying, "We think this is a fine field to work in. Could you send us a list of things that we should do? We'd like to start doing nuclear astrophysics." To some extent we try to make suggestions to people, but it's not all that easy to do. It's a very personal, small kind of research.

ASPATURIAN: Personal?

BARNES: Personal may be the wrong adjective. It's a field in which the individuals are involved in an intimate way in very small groups; it's not a big team effort kind of thing. This is not to say that it will always be that way; it's hard to predict the future. But there are, of course, still a great many areas in which we can see that there's a crying need for more astrophysical information.

In a generalized way, the largest area of work that needs to be done involves nuclei that are radioactive, made in events that may occur quickly in stars, or even occurred in the Big Bang, for that matter. The times in which processes evolve may be short compared to the beta-decay lifetimes of these nuclei, and so one very often gets, in an astrophysical context, situations involving nuclear reactions in which the target nucleus—for example, nitrogen-13—is radioactive. Nitrogen-13 has a half-life of ten minutes, and ordinarily, in hydrogen-burning in main sequence stars, the rate at which the burning occurs is sufficiently slow that nitrogen-13 essentially always has the opportunity to beta-decay to a stable isotope, namely carbon-13. We studied radioactive proton capture on carbon-13 long ago in this lab. But so far nobody has been able to directly study proton capture on nitrogen-13. In a few cases in nuclear astrophysics, we've actually been able to get reaction rates involving radioactive target nuclei, but only by a trick. If the result of the reaction happens to be a stable nucleus, then we've learned from the earliest days of nuclear physics how to turn the reaction backwards. If the resulting product of the reaction of interest is a stable nucleus, we can then do the inverse reaction—a backwards reaction—and from some theorems that depend on time-reversal invariance of fundamental interactions, we can infer the astrophysical information that we need with essentially no uncertainty. However, most reactions that involve radioactive target nuclei also involve radioactive product nuclei. In fact, the product nuclei are often even more radioactive than the target. So the inversion trick will not work for such cases, and quite a few of us have been involved in trying to persuade funding authorities in the United States, Canada, and Europe to build an accelerator facility that will make it possible to study the reaction rates for these radioactive nuclei. I think a strong case can be made for studying the rate of nuclear reactions on these unstable, sometimes highly unstable, nuclei, because they involve a region of nuclear physics in which we can only guess, or extrapolate from stable nuclei what will happen. From a general nuclear physics point of view, it's more than likely that new and perhaps even quite startling new information can be obtained there. But completely aside from the basic science of it, there can be no doubt that these radioactive nuclei do play an important role in many astrophysical contexts. If we are going to meet the goal that we set for ourselves—and which I think others expect of us—namely the goal of being able to say quantitatively what the results of nucleosynthesis or nuclear reactions in the astrophysical context are going to be, we're going to have to measure these things. It's even more true for these unstable nuclei than it is for the nuclei nearer to what's called the line of stability: nuclear theory is simply not capable of predicting with any high degree of accuracy what's going to happen with nuclei far from stability. In the absence of adequate theory, we must have recourse to the only other alternative, which is to measure these things.

ASPATURIAN: There's a lot of concern among particle physicists these days that the U.S. is being left behind by the Europeans. Do you see the same thing happening in nuclear physics?

BARNES: I think it might even be more true in nuclear physics. Certainly there are strong efforts in nuclear physics, and also in the smaller area of nuclear astrophysics in Europe—particularly in France, England, and Germany. I think I mentioned earlier that the support per capita for nuclear physicists in Germany is about three times what it is in the United States. It's a little difficult to get an exact number, but it's really greatly in excess of what it is in this country. There are many extremely competent physicists and other scientists in Europe, and they are mighty severe competition. It's not that we shrink from competition; but they certainly keep us on our toes, and there are moments, I would have to admit frankly, when I envy the kind of support they have, because it does sometimes enable them to do things that we can't do. It looks to me, at this moment, as though the first viable efforts to build an accelerator facility for studying radioactive nuclei may be in Europe. Although there's quite a bit of interest in the U.S. and Canada, which would probably work jointly on it, it does look as though the first steps are actually being funded in Europe right now, at a university in Belgium, as a joint Belgian-French-German effort.

ASPATURIAN: Have you ever testified or talked to various federal agencies that are funding fundamental research in physics? Has this been part of your professional career?

BARNES: It hasn't really been a major part of my professional career. I've been involved on a smaller scale, talking with nuclear physics funding people and discussing the philosophy of it on a number of occasions—what kinds of things I think should be supported, completely aside from our own work. But I haven't really participated in the major kind of thing that I think you're asking about.

ASPATURIAN: Do you think that the special intimate quality of nuclear physics research you mentioned, and the special techniques needed to study some of its problems, are going to hurt it in future competition for funding and for gifted students? Will the money and the people go elsewhere?

BARNES: Where the money will go is not so easy to predict. It depends on how productive we are. To some extent, the money goes where advances are seen, or perceived to be ready to be made. I would say, to a large extent, the onus is on us to prove that nuclear astrophysics is a productive area and that we can continue to make discoveries that will be recognized as important. There is, certainly, some considerable worry about where the students are going to come from. We are in an era where the students are strongly attracted to applied physics. This is, no doubt, partly caused by their perception that there are important things to be learned in this area, and indeed there are. It may also partly be caused by their perception that this may be the most direct way to a good job. The latter, I think, is a mistake on their part, because there simply are essentially no unemployed physicists in the country in any subject area. [Laughter] If you look at the figures, you find that there's some small fraction of one percent of physicists unemployed. And when you start looking at this list to see why they're unemployed, it's abundantly clear that they're mostly unemployed for reasons of choice, of one sort or another. They may be maternity cases if they're women, or people who have had a kind of burnout and are taking a couple of years off, or something like this. Still, I would be the first to admit that at the time I got my doctorate in 1950, there was a frantic effort being made to hire people. And it isn't quite the same anymore. Students still get usually a number of offers, but they don't get an offer every time they talk to somebody, which really was the situation when I got my doctorate. But there are many openings for any physicist in any area who really is even modestly competent. Much has been said over the years about jobs running out, but it simply hasn't happened, and I don't think it's going to happen; because we seem to be tapping about all of the potential talent in this area that we're going to get with the techniques we've evolved so far. That's not to say that there might not be other people out there who would be just as good, but with whatever approaches we have used to interest students in physics, we seem not to be increasing our fraction of the pool. In fact, what's happening on a nationwide basis is that a bigger and bigger fraction of the graduate students in the country are foreign-born. You might say, well, that's fine; a lot of them end up staying in the U.S.; and that's our country's gain and other countries' loss. But looking at a more worldwide picture, that's not so good. Many foreign countries have tried to ensure—when they give students visas to go abroad to some other country—that the other country will guarantee that when the students finish their education, they'll be sent back to their country of origin. To a large extent, the U.S. actually subscribes to this; they really do make an effort to try to persuade or require students to return to their country of origin. But it's very hard to make it stick. I've talked to these students from other countries. These are very smart people and they're also plenty smart about understanding ways around this, and they either come back to the United States within a very short time, or sometimes they don't even leave the U.S. These students enrich the pool of scientific talent in this country, but it disappoints me a bit that we aren't able—or seem unable—to meet the demand for such highly qualified people from within our own country. Also, a lot of people who get their PhDs in other countries come to the U.S. as postdocs, and manage to stay here somehow.

ASPATURIAN: Do you attribute this fall-off in this country to anything specific?

BARNES: That's a difficult question to answer, really, because, as for most such questions, there are probably many causes that would take time to recognize and examine. But, you know, it may be advancing age on my part. [Laughter] One always has to be wary of these sorts of biases that develop with time. But I do think that a high school and undergraduate preparation in mathematics, physics, chemistry, and biology is somehow more demanding than the effort most students are willing to expend. They really can't trifle with these fields and expect to succeed at them. There are many short-term, apparent rewards to people for doing other things that may seem more enjoyable as well. Also, there is such a long lead time in the building of a scientist, starting right from earliest mathematics and the mathematics and science programs in high school. The place where, I think, so many are being lost is at the earliest stages. Looking at it on a general statistical basis, it is a disappointment to us—to me at any rate—that we have not succeeded in increasing the percentage of students who are going into physics. In fact, the percentage has fallen substantially over the last twenty years. The total number has also dropped, but that's actually now beginning to come up a little bit. The 1986 figures on physics graduate students are actually up a few percent. But this is a kind of misleading statistic, because they're up mainly by virtue of an increase in the number of foreign-born students who are coming here to do graduate work. The number of native-born physics grad students is actually still falling, I think. It will rise again when the second wave of the baby boom comes along, but it's always still a percentage problem. I guess all of us would feel that we were doing our job better if more students were persuaded to take advanced study in physics, not to mention other scientific subjects.

ASPATURIAN: Do you see that affecting the caliber of the undergraduates who have come to Caltech in the past ten or fifteen years?

BARNES: I think that we still have extremely good undergraduates. I've often heard the statement made that they're better now than they have ever been. I can't really say what they were like before I came here; obviously there were some pretty bright people here, because their lifetime careers and records prove it. But I have seen a fair sample of the student body, since I've been teaching here already about thirty years. I personally couldn't verify that the students now are better than they were when I first came here; I don't think they're less able, but I really don't think they're any better.

ASPATURIAN: Do you think students going into physics now face a tougher time than you did when you started out in the field thirty years ago?

BARNES: They face a tougher time in the job market. There's also more information to absorb, but the students now don't take any more coursework than they did thirty years ago. There are a lot of things that we used to concentrate on with the students that we don't teach much now. There's been a continuation of a process that's always gone on in that as new information is added to the pool, something clearly leaves the pool. To some extent, perhaps, we make up for it by getting more efficient generalizations. But there are other things that we simply drop as being of lesser interest at this particular time, and try to concentrate on the things that we think are going to be most important. **[Tape ends]**

Begin Tape 8, Side 2

ASPATURIAN: Describe your work on parity violation.

BARNES: I will interpret your question as referring to parity violation in nuclei, or parity violation in the nucleon-nucleon force. Parity violation in beta-decay was well studied by us and others years ago. We continued our work through the 1960s and on into the seventies and eighties doing what I thought was important nuclear physics. We were doing this along with the nuclear astrophysics. One of the areas of study that we got into was the question of whether parity-violating effects could be found in the strong or nuclear force. It isn't directly involved in nuclear astrophysics, but might have some unexpected significance in the future, in the sense that anything that's important to nuclear physics may be important to nuclear astrophysics somewhere. Our problem was, first of all, why would you expect to find parity-violating effects in a nuclear force? The only real guide to why this might occur was that the work by Feynman, Gell-Mann, and others on the theory of the weak interaction showed that it was clearly a universal interaction—that is, universal to all fermions. Since the neutron and proton are clearly fermions, there should be a weak force—and I use the word weak not only in the sense of being a very non-strong force, but weak in the sense of being different from the obvious hypothesized strong nuclear force that we believe is parity-conserving. If you simply looked at the strength of the nucleon plus nucleon force in a direct head-on assault, there would be no possibility of seeing the effect of the postulated universal weak interaction between nucleons, because it would be just too small compared with the dominant strong interaction effects.

One way that you might have a hope of finding the nucleon-nucleon weak force is to make the assumption that the strong interaction per se, or maybe even by definition, is an interaction that is parity-conserving. Then, if you can find a parity-non-conserving effect in nuclei, you could say, "All right, I've seen evidence for the weak nucleon-nucleon force. And indeed, experiments of this sort have been done at a few other places over many years. Some of the best ones were actually done here at Caltech by Felix Boehm and his colleagues. But these experiments are very hard; they're tricky because the results are predicted to be very small. The theory was usually quantitatively uncertain. In Kellogg, we have been looking for parity-violating experiments involving what we call the "light nuclei," the point being that we imagine that the light nuclei are somewhat simpler than heavy nuclei—which is probably correct—and that it might be easier to unfold from the experiments those things that followed simply from the properties of individual nuclei, and to separate these effects from the effects that follow directly from the expected weak nucleon-nucleon force. The first experiment of this sort in Kellogg was a study that I started with graduate student Alan Moline in 1965 to look for parity-violating effects in the gamma-ray decay of the first excited state of fluorine-19.

We thought this was a good place to look, because the ground state and the first excited state of fluorine-19 are very close together—just 109 kilo-electron volts apart. They both have the same angular momentum, one-half. The ground state has even parity and the excited state has odd parity; and so the weak nucleon-nucleon force would be expected to be capable of producing some small mixing of these two states. If that were the case, then there were certain experimentally observable effects that should follow, such as the fact that, even if the excited state of fluorine-19 nucleus was completely un-oriented in space, it would still follow that the gamma decays from the excited state to the ground state would have a small circular polarization—that is, the gamma rays would have a characteristic handedness. In other words, the gamma rays wouldn't be equally likely to be left-handed photons and right-handed photons. Now, the theorists had already been trying to predict the results in this case, and in other cases; and generally speaking, the theorists had predicted parity-violating effects that were much bigger than anybody had found. The thing that was kind of nice about the fluorine-19 case was that it was in some ways a rather simple case theoretically, since the two states that were involved as a parent and daughter state in the gamma-decay were also the two states that should be admixed, one with the other. In any case, there were predictions that we should get circular polarizations of the order of 10^{-4}. It sounds pretty small; that's 1/100th of a percent, but it was something that we knew we could easily measure. In particular, we had the good luck to think of a novel method of doing the experiment that made use of a fact that very few people realized. Most people didn't realize that nuclei could be polarized to a calculable extent by the process called Coulomb excitation. That became an intrinsic part of our experiment.

The result of this experiment, not to make too long a story of it, was that while we found that the circular polarization was at least a factor of ten smaller than predicted, we were not able to show that the effect was really zero. So we were left in limbo; we'd neither proven nor disproven the effect we were looking for. But we'd given it our best shot. And Alan Moline submitted a thesis on this—what I thought had been a very pretty experiment—and for which a large part of the credit goes to him. He was certainly one of the many very smart students I had over the years. He went from here to Bell Labs, and I saw him later on television a few times; they would occasionally feature him in their advertising as one of their bright young men, which he really was—an extremely ingenious and bright student.

But we'd shot our bolt on that one. It wasn't for another twelve or thirteen years that somebody else came back to try that same experiment. This was another former student of ours, Eric Adelberger, who, by this time, was a professor at the University of Washington, in Seattle. He actually did find a parity-violating effect in fluorine-19 by a totally different technique. It was one that we could not have done here anyway, because it required a higher energy particle beam than we had. But he found the effect to be a factor of ten still smaller than our upper limit. So the result was completely compatible with ours, but altogether about a hundred times smaller than the original theoretical prediction. Of course, these things try the patience of experimentalists; and it isn't the first time in history by a long shot that experimentalists have undertaken to do something on the basis of a theoretical prediction that turned out to be grossly optimistic in the sense of being too large. But on the other hand, one can't blame the theorists; they obviously also do their best, too, to make the best prediction that can be made with the knowledge at hand.

Although we hadn't really succeeded in finding evidence for parity violation in the nucleon-nucleon force with fluorine-19, in 1975 an even better example—the nucleus fluorine-18—was suggested to us by Ernest Henley from University of Washington. As we began to focus on this nucleus, the interest in doing this case was considerably enhanced, because the electroweak theory was then being developed. It became clear that sizable effects were to be expected in the parity mixing, or the parity-violation, caused by the so-called neutral weak currents, which were not part of the previous theory of beta-decay or the nuclear weak interaction. So the new wrinkle was the neutral weak currents, and the recognition that these could actually dominate the parity-mixing effects caused by the weak nucleon-nucleon force. We had the good fortune to have a former graduate student, Eric Adelberger, visiting us that year—and to be quite candid, I've forgotten which year that was; I think it was about 1976. I also had a very good student by the name of Mike Lowry, and the help of Ross Mars and John Davidson, who were postdocs in the lab. Also, we elected to actually do this experiment at Cal State LA, for reasons of the particular accelerator they had there; so we enlisted colleagues from Cal State LA, also.

In the experiment, we embarked on an attempt to find a gamma-circular polarization—a parity-violating effect—in fluorine-18. From a theoretical point of view, I think this was by far the "cleanest" case that people have looked at up to this time. In this case, we were looking for the mixing of two excited states of fluorine-18, both of which had angular momentum zero. One had even parity, and the other odd parity. The even parity state had isospin one, and the odd parity state had isospin zero. Because of that rather restrictive situation, it was supposed theoretically to be a very attractive case. In addition, one could make other measurements that had to do with a forbidden beta-decay in neon-18, which should have enabled a largely model-independent prediction to be made.

Once we started our experiment, we again found only an upper limit, which we published in 1978. We could say that the parity-violating effects were less than a particular number, which was, again, about ten times smaller than the predictions. The theorists, however, rallied quickly to that, and immediately said they now understood why the effect should be smaller, but that their revised prediction was about equal to the error of our experiment. Other people in Germany and a group in Italy immediately undertook experiments on the same nucleus, which were supposed to be improvements on our experiment, but were very closely similar to ours, with only rather minor improvements. The group in Italy, I think, didn't get as good results as we got; the group in Germany got results about the same as ours.

Finally, to bring the story of the search for nuclear parity violation in fluorine-18 up to date, in collaboration with some former Canadian colleagues, I participated in another fluorine-18 experiment at Queen's University in Kingston, Canada. It was yet further upgrading of our original Caltech experiment of 1975 to 1978, and we managed to improve the precision from the original Caltech experiment by a factor of nearly five. And as the experiment has turned out, what we have found is still just an upper limit. It's embarrassing to the theorists still. In fact, now they really say we don't know our way out; we don't know how to reconcile the fundamental weak interaction theory and the effect of neutral weak currents with this result because we now think we can do the calculation well enough that we don't have anywhere to turn.

It is too early to say how this contradiction will be removed eventually; maybe the theorists will find there really is something that they overlooked. Maybe it will turn out that the experiments for some reason have hidden faults. But it's also possible that this now very severe contradiction between the upper limit on the magnitude of the parity-violating effect in F-18, and the best predictions, may actually tell us that there's some new physics involved. In other words, there may be some new mechanism which, in fact, quenches or limits the magnitude of parity-violating effects in truly hadronic weak interactions. The other cases where parity-violating effects actually seem to have been observed are almost entirely ones that would be called leptonic or semi-leptonic. They involve electrons or muons and neutrinos. Parity violation in nuclei is a purely hadronic effect, involving only the weak nucleon-nucleon force.

Now, it would be a pretty bold statement to say that we have proven that there is some new effect in purely hadronic weak interactions by our failure to observe nuclear parity violation in what are considered by theorists to be their best cases for quantitative predictions. We can't make that strong a statement. But as of 1987, the theorists who work in this area are very puzzled about this apparent contradiction. And in a sense, people say, "Well, aren't you going to improve the experiment further and try and beat the upper limits down until you do see these effects." We'd like to, but it gets harder and harder, simply because the limiting statistical error only goes like the square root of the number of data points. To improve the fractional statistical error by a factor of ten would require one hundred times the data. What starts as a three-year experiment would suddenly become a three hundred-year experiment! Clearly we're not going to do that. It's just impractical to keep beating it with brute force. Any significant improvement in the experiment will have to come from a more clever idea on how to do the experiment.

ASPATURIAN: Is that something you're going to be working on in the near future?

BARNES: I'm certainly considering looking at a different case, a different nucleus—in fact, within the next year—where again, the theorists assure us that they can do a good calculation. But it's too early to predict whether we'll find a result that is still far below predictions or is in fact compatible with the theory.

ASPATURIAN: Are there things about theorists that you think sometimes particularly cause friction between experimentalists and theorists?

BARNES: Oh, I'm sure that there are some theorists that could be very annoying to everybody. [Laughter] I know what you mean, but I don't think it would be fair to characterize theorists on this basis as being generally at fault. It's intrinsic to theoretical physics that out of the infinity of possible things one might try to calculate, they have to make a rather narrow selection of things they're going to invest their time in. They tend to choose problems that they perceive as simple, or else have some relatively simple approximation that they can make, which should make the calculation tractable. It does sometimes happen, that when they make the problem tractable, even with the best of intentions, the calculation ceases to be as reliable as one expects. They can usually say this approximation is certainly good to such-and-such an accuracy; but it's not always true. As soon as one makes an approximation, especially if you have to make one approximation after another in the calculation, you could certainly be very far from what nature actually does by the time you get through.

ASPATURIAN: How about the other way around. Did you ever get the feeling, working with some theorists, that they felt many experimentalists were more interested in their machines than they were in physics?

BARNES: I've heard some theorists expound on this subject. [Laughter] I've heard that some theorists think that experimentalists are really plumbers. I've heard the word used, though I won't mention any names. I don't think that they're serious; I think that in most cases, if not in the great majority of cases, it's actually a kind of grudging admission that there are things that the experimentalists can do that they themselves can't do, or at any rate, never learned how to do and probably aren't ever going to learn how to do. But there are all kinds. Anybody who involves himself in doing an experiment or in doing a theoretical calculation is making a certain kind of selection, or displaying a kind of scientific taste, in making the choice. Matters of scientific taste, as for matters of taste in general, are very subjective things. It certainly happens that there are sometimes theorists saying, "My goodness, I don't know why that guy did that experiment; I can't think of any reason why he would have done that." Or we might know experimentalists who say, "My God, I can't think of why that guy would choose to do that calculation. It doesn't seem the slightest bit interesting to me." So that kind of thing happens. I've said things like that myself. I suppose it's more in wonderment than anything else. [Laughter] "Does he see something there that I don't see? No, I don't think so. I don't know why he did it." But there isn't any shortage of really important things to do. They're not always the easy things to do, however. Occasionally there are experimentalists or theorists who might put the ease of doing something ahead of its real merit as something that we need to know. It's a matter of scientific judgment, and taste, too, as to what you think is really important.

ASPATURIAN: As you moved from nuclear physics to nuclear astrophysics, did it become easier to explain to non-physicists and lay people what it is you do?

BARNES: I think it's become easier to explain to lay people why I think that what I'm doing is interesting. I think they can understand that it might be interesting to some people to understand how the stars work or what happened in the beginning of the universe. That does appeal to people. You see that in an interesting way—even in 1987, astronomy still has a very generous component of its funding coming from private sources, whereas physics can only get almost all of its funding from federal or state sources. I think that, by default, the federal agencies have not seen fit to provide the same level of funding for astronomy that they have for most other areas of science. However, I personally think that some astronomers at least have been a little bit lax in seeking federal funding. This is not intended to refer to any specific individual, but I think there's been a general tendency on the part of astronomers, to avoid the nitty-gritties of raising funding. Many choose to make observations that don't require much in the way of expenditure, because it's easier to do that than it is to go and build a new world-class, frontier-type piece of equipment that will let them do something different. But also, of course, there are fortunately astronomers who don't follow this course of action and, in some way, I consider their method of proceeding to be much closer to that of the physicists. They decide that if they're going to learn something about some aspect of astronomy, what they must do is develop some new method of measuring things. The quality, importance, and excitement of the discoveries that are going to result really does depend on doing new things. Caltech is a good example of what's true generally—many of the radio and infrared astronomers who've made the greatest contributions are people who got their education in physics to begin with. But I think you'll find that's happening more and more in other areas of astronomy, too. I think you'll find that more people are building equipment that depends more and more directly on the same kind of newest up-to-date physics that we know. A prime example of this is the use now of silicon detector arrays for detecting photons in optical astronomy. It's clearly revolutionizing optical astronomy, and the revolution has really only just begun, because the first step has been to learn how to utilize the enormous increase in efficiency of detecting photons. It makes a given size of telescope many times more effective than the older style of using photographic plates. Once one has the information coming in, in this on-line way, in the form of electrical pulses, there are many more things that can be done with the information and many ways of combining the information and checking coherence of signals from different parts of an image, and so on. So, it looks more and more like physics—at least to a physicist.

ASPATURIAN: Have you been involved in any of the research or discussion that has come from the Supernova 1987A?

BARNES: Not directly. I've certainly followed whatever's appeared publicly on the subject. Because of our interests in nuclear astrophysics, it's natural that we would be interested in anything that comes out of a supernova. That's aside from the fact that as physicists, we are also interested in what neutrinos do. So we have tried in Kellogg, to keep up-to-date with the observational astronomy part of it; and at the same time we have certainly followed with great interest the excitement over the detection of neutrinos from SN1987A. The people who have done the most work, or what I think is some of the best work, on theories of supernovae, are the same people who do the theoretical nuclear astrophysics that we've known for twenty or thirty years. Most of them, in fact, were research fellows at one time or another in Kellogg. So obviously, they always send us their papers and preprints as they develop them. They're not all, I would say, equally likely to be right, but it's an exciting development. A shorter answer to your inquiry would be that we're very interested spectators.

ASPATURIAN: Is it stretching a point to say that the nuclear astrophysics community today is dominated by people who came out of Kellogg?

BARNES: Without making a really highly detailed count, I'm sure it would be a fair statement to say that a large fraction of the people engaged in both theoretical and experimental nuclear astrophysics have at one time or another been at Kellogg, either as theorists or as experimentalists. The Fowler book, which you alluded to before, includes only a fraction of the people who have worked with Willy right here in Kellogg; but all of the twenty-three authors in that book were people who had spent time in Kellogg working with Willy and the rest of us, at one time or another.

ASPATURIAN: If you were to single out Kellogg's two or three most important contributions in the years that you've been here, what would those be?

BARNES: They are bound to be projects in which I've been involved. [Laughter] It's such an ongoing business; I sort of hesitate a little bit. We are talking, I think, about nuclear astrophysics. I also assume that you're talking about the period since I've been in Kellogg. I think the measurements that were done in Kellogg—many of them by my colleagues, more than by me—on hydrogen-burning reactions, and the measurements that my colleagues and I were able to do on the helium-burning reactions, have really been essential steps in trying to develop a quantitative theory of stellar evolution. Also, the hydrogen-burning reaction studies especially bear on cosmological nucleosynthesis—the Big Bang—but it's such an incomplete story yet.

We're still continuing on with this, and we see more interesting problems emerging all the time. I talked a moment ago about the enormous uncharted area of nuclear reactions involving radioactive nuclei; but there are lots of other things. It may turn out that perhaps the picture of nucleosynthesis in the Big Bang is not completely correct, because it essentially assumes that the early universe was isotropic and homogeneous. And everywhere we look in microscopic examples of pieces of the universe, we see examples in which it fails to be homogeneous, and often is not even remotely isotropic, even on a large scale. Although the black body radiation seems to be pretty isotropic right now. One thing that certainly follows is that density fluctuations in the very early universe would automatically lead to fluctuations in relative concentrations of neutrons and protons. As soon as you admit that there might have been regions of greater or lesser than average neutron concentration, right away the kind of nucleosynthesis that could occur in the Big Bang is changed and much enhanced. That, I would say, is a truly fundamental point on which we have practically no information at the moment. People have simply not looked at the kind of reactions that would occur in a sufficiently quantitative way, if you had neutron-rich regions in the Big Bang. As a result, the conventional picture we have now is that in a homogeneous and isotropic Big Bang, the only elements you can make are hydrogen, helium-4, and traces of deuterium, helium-3, and a little bit of lithium-7. But once you beef up the concentration of neutrons, you could run quite far up the scale of nuclei in the Big Bang. It would certainly make some people pretty happy, because a lot of astronomer are alarmed by the fact that even in the oldest objects we look at, we see evidence of material that has gone through a certain level of nucleosynthesis, which goes beyond the kind that we usually talk about in the Big Bang. Of course, there are alternative explanations of where that matter might come from, such as an early generation of rapidly evolving, very massive stars, called population III stars.

ASPATURIAN: Do you have anything else you'd like to say?

BARNES: Not really, except that I don't know how to convey adequately to people what an exciting enterprise it is to do research in physics, and particularly in nuclear physics and astrophysics. I consider that we're highly fortunate to have had this opportunity, and full of wonderment that society puts this high value on what we do and continues to support our work. But, you know, it's an exciting and never-ending quest. I'm sure that a lot of us are driven partly by the feeling that just around the corner, there is going to be a major discovery that will make us famous. To some extent, that gets tempered with age; but it is eternally exciting to do things just for their own sake, just for the knowledge that we get. It's a wonderful feeling to think, "Today I know something that I didn't know yesterday—that nobody knew yesterday." That's really what keeps us going. **[Tape ends]**