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Physics Oral History Series

Owen Chamberlain

PHYSICIST AT LOS ALAMOS, BERKELEY PROFESSOR, 1950-1989, AND NOBEL LAUREATE

An Interview Conducted by
Graham Hale
in 1976

Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the Nation. Oral history is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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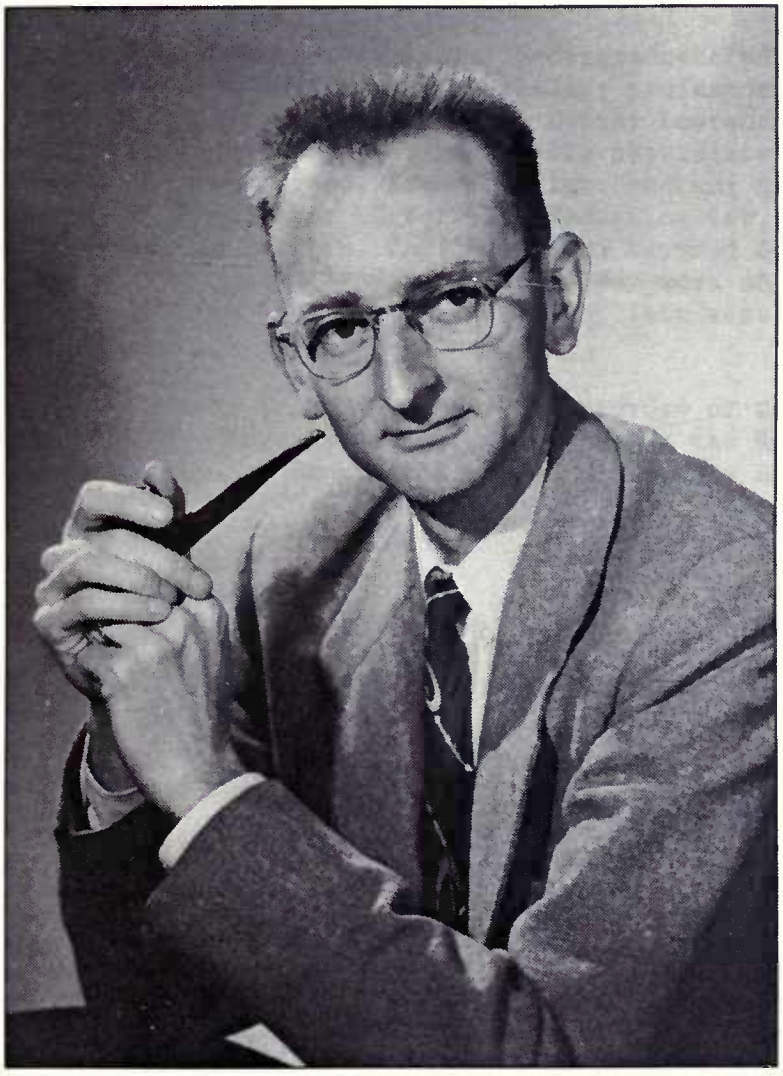

Owen Chamberlain, circa 1955.

Photo courtesy UCB Physics Department.

Cataloguing information

Chamberlain, Owen (b. 1920) Professor of physics

Physicist at Los Alamos, Berkeley Professor, 1950-1989, and Nobel Laureate, 2000, iv, 234 pp.

Childhood in San Francisco and Philadelphia; undergraduate education at Dartmouth, graduate work in physics at UC Berkeley; professors at Berkeley: J. Robert Oppenheimer, Emilio Segrè, Ernest Lawrence; work on the atomic bomb at Los Alamos during World War II; physicists at Los Alamos: Enrico Fermi, Clyde Weigand, Edward Teller; postwar graduate work at the University of Chicago; UC Berkeley Radiation Lab, and colleagues Bob Thornton, Ed Lofgren, Carl Helmholz, Raymond Birge, Luis Alvarez; Nobel Prize in physics, 1959, with Segrè for work on antiproton; involvement with anti-nuclear activism, late research in physics.

Interviewed in 1976 by Graham Hale for the History of Science and Technology Program's Physics Series. Produced by the Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 2000.

Acknowledgments

I would like to give special thanks to Willa K. Baum of the Regional Oral History Office for her contribution to this oral history. Without all her help and perseverance over the years, this document might never have been produced. Thanks go to my interviewer, Graham Hale, for the fine job he did; and I enjoyed his good humor during our hours together. I also thank Tish Solmitz, who spent many hours helping me read through the manuscript for accuracy during 1999, and my niece Bettine Birge, who began this process with several sessions in 1995 and 1996. I thank my wife, Senta Pugh-Chamberlain, for her love and encouragement, which kept this project on track even as other needs seemed to get in the way. I am grateful also to all my family, friends, colleagues, and students who have made my life so fulfilling and memorable.

--Owen Chamberlain
September 2000

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INTRODUCTION by Sally Smith Hughes

This oral history with Professor Emeritus Owen Chamberlain of the University of California, Berkeley, describes his research in particle physics, including experiments on the antiproton for which he and colleague Emilio Segrè were awarded the Nobel Prize in Physics in 1959. Chamberlain was cited for his "ingenious method for the detection and analysis of the new particle." His account of the antiproton work is of obvious interest, but so too is his description of earlier research at Los Alamos during World War II on the atomic bomb and his thesis research at the University of Chicago under Enrico Fermi. He also provides important information about E. O. Lawrence's Radiation Laboratory--the progenitor of the present Lawrence Berkeley Laboratories--and the prominent scientists associated with it, particularly Emilio Segrè, with whom he had close to a career-long association. But science is not the only topic of discussion. Chamberlain's political activities concerning disarmament, the Berkeley Loyalty Oath, and nuclear politics are also a prominent theme.

This volume is part of a series conducted in the 1970s on the history of the Radiation Laboratory at Berkeley and includes oral histories with William Brobeck, Carl Helmholz, Malcolm Henderson, Wallace Reynolds, and Robert Thornton. This series complements others in the Bancroft collection, such as the one on medical physics at Berkeley and interviews with Melvin Calvin and his laboratory, Andrew Sessler, Glenn Seaborg, Herbert York, and others. Used in combination with the E. O. Lawrence correspondence and other primary source materials at the Bancroft and elsewhere, the oral histories provide a unique view of accelerator science and the early use of artificial radioisotopes and heavy particle beams in basic science and practical application.

The interviews with Professor Chamberlain lay unfinished for many years. In 1999, the Regional Oral History Office was asked to orchestrate completion of an oral history volume. Professor Chamberlain was contacted and graciously agreed to review and approve the transcripts. He--and we--are indebted to Tish Solmitz, his niece Bettine Birge, and his wife Senta Pugh-Chamberlain for helping him see the project to completion.

The Regional Oral History Office was established in 1954 to augment through tape-recorded memoirs the Library's materials on the history of California and the West. Copies of all interviews are available for research use in The Bancroft Library and in the UCLA Department of Special Collections. The office is under the direction of

Willa K. Baum, Division Head, and the administrative direction of Charles B. Faulhaber, James D. Hart Director of The Bancroft Library, University of California, Berkeley.

Sally Smith Hughes, Ph.D.
Research Historian

October 2000
Regional Oral History Office
The Bancroft Library
University of California, Berkeley

Owen Chamberlain

Born: July 10, 1920; San Francisco, California

Educational Background:

B.A., Dartmouth College, 1941
Ph.D., University of Chicago, 1949

Brief Professional History:

University of California at Berkeley:
Instructor, 1948-50
Assistant Professor, 1950-54
Associate Professor, 1954-58
Professor, 1958-present

Professional Society Affiliations:

Fellow, American Physical Society
Member, National Academy of Sciences, 1960
Fellow, American Academy of Arts and Sciences, 1975

Awards and Honors:

Cramer Fellowship, Dartmouth College, 1941
Thayer Prize in Mathematics, Dartmouth, 1941
Guggenheim Fellowship, 1957
Nobel Prize for Physics (shared with Emilio G. Segrè), 1959
Loeb Lecturer, Harvard University, 1959
The Berkeley Citation, 1989
Hon. Doctor of Science, Dartmouth College, 1991

Regional Oral History Office
Room 486 The Bancroft Library

University of California
Berkeley, California 94720

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name OWEN CHAMBERLAIN

Date of birth 07-10-20 Birthplace S.F. CALIF.

Father's full name W. EDWARD CHAMBERLAIN

Occupation RADIOLOGIST Birthplace ANN ARBOR MICH

Mother's full name GENEVIEVE LUCINDA CHAMBERLAIN (NEE OWEN)

Occupation HOUSEWIFE Birthplace WENACHEE*, MICH.

Your spouse SENTA M PUGH - CHAMBERLAIN

Occupation WIFE Birthplace PRAGUE, Czech.

Your children 1) KAREN CHAMBERLAIN 2) DAROL CHAMBERLAI
3) LYNNE GUENTHER-CHAMBERLAIN 4) PA CHAMBERLA

Where did you grow up? SF & PHILADELPHIA

Present community BERKELEY CA

Education DARTMOUTH COLL., UCB,
U OF CHICAGO FOR PH.D.

Occupation(s) PROFESSOR OF PHYSICS, EMERIT.

Areas of expertise PARTICLE PHYSICS

Other interests or activities DISARMAMENT ISSUES

Organizations in which you are active PLOUGHSHARES; UNION OF CONCERNED SCIENTISTS, PARKINSON'S DISEASE FOUNDATION

SIGNATURE Owen Chamberlain DATE: 3/7/00

* PLEASE, CHECK SPELLING.

INTERVIEW WITH OWEN CHAMBERLAIN

I FAMILY BACKGROUND AND EDUCATION

[Interview 1: June 16, 1976]

Family Background

Hale: This is our first interview with Professor Owen Chamberlain, Wednesday, June 16th. We're in his office on the hill, in the lab. Professor Chamberlain, you were born in San Francisco?

Chamberlain: Born in San Francisco, July 10, 1920.

Hale: Could you tell me something about your family background, your parents?

Chamberlain: Yes. My father was a physician and a radiologist, and during my early years, up to age ten, he was on the staff of Stanford University Hospital, which was then in San Francisco rather than in Palo Alto. It was called Stanford-Lane, and I think the hospital has now been taken down.

Hale: Where was it, roughly?

Chamberlain: I think it was at Clay and Webster. I went by the site some time in the last year. We lived in St. Francis Wood in western San Francisco in a house, 11 San Leandro Way, that now looks small but, of course, seemed bigger at the time.

I'd say my parents both came from a middle-class background. Not too well-to-do, but not too poor, either. My grandfather was Nelson Hoyt Chamberlain, a physician in Oakland, California. I never met him because he died before I was born. He was a rather popular physician in fairly

wealthy circles in Oakland for a while. He had this situation that every time he doubled his fees, his practice doubled [laughter], so he didn't know what to do to keep up with everything.

Hale: Sounds the same thing as asking for twice as much from the AEC.

Chamberlain: Yes, it's a little different social dynamic, I'm sure.

Hale: It was a general practice that he had?

Chamberlain: Well, there weren't so many specialties at that time. He tried a few blood transfusions, and I think he had two successful and then ran into trouble. Blood types weren't known. When he ran into trouble on the third one, I believe, he had to give it up.

Hale: You don't remember anything of him because he died before you were born?

Chamberlain: That family was a very energetic family. Father's father was Nelson Hoyt Chamberlain. My father's mother was a great doer. She got into photography very, very early. In fact, my father did some color plates by a process called Autochrome. I'm not sure I know where to find any examples at the present time, but I'm sure there's some hidden away that he must have done; something near 1905 or 1910. They're quite nice.

Hale: Your father told you a lot about that, about your grandfather and your grandmother?

Chamberlain: I think I learned more about that family from my mother. My mother and father both went to Oakland High School, and had both attended UC Berkeley, which was then *the* University of California, I suppose.

My father was in the Class of 1913 with Robert Gordon Sproul and must have taken his medical degree at UC San Francisco while it was still the medical school located in San Francisco. I guess in 1916. He got into radiology, I think in large part because the only radiologist in San Francisco was killed in the Preparedness Day Parade bombing that Mooney was supposed to have been responsible for.

Hale: Radiology, then, was a very new subject?

Chamberlain: Very new subject. In fact, I think my father got his first start in radiology on the suspicion that the X-ray machines in shoe stores might be dangerous to people because of the radiation hazard.

Hale: Oh, really?

Chamberlain: And he started to look up something about this and warned the local shoe store that the shoe store shouldn't allow kids to come home from school and rush into the store and X-ray their feet and go out again.

Hale: I see, so that was in the early teens?

Chamberlain: I'm not sure, but I would have thought even before 1910, possibly. My father was born in 1892.

Hale: I remember your saying he graduated in '16 from medical school, so I'd assumed it was somewhere around then that he would be interested in--

Chamberlain: Oh, yes. Maybe so.

Hale: That sounds so far back for that sort of thing because I remember that sort of thing when I was a kid. We were still doing that when I was a kid!

Chamberlain: Yes, yes, that's right. I've had the X-ray fluoroscope used in a shoe store, too.

My mother's family had come out from Illinois when my mother was two years old, I guess. She was born in 1891 and must have come about '93 to the Oakland area. There was an uncle that had made a start, and I think the family came out in part to help him with a health problem. I don't really remember those details, but both my mother and my father had lived in Oakland from about age two. So neither was born in California, but they were Californians from way back. My mother's father was a real estate broker. He lived to almost 103, about 102.7, and he was one of those fine people that was very enjoyable through decade after decade. He was a lot of fun.

Hale: You had a lot of association with him?

Chamberlain: Yes. Well, a reasonable amount. I felt I knew him very well. My mother's mother I hardly remember because she died about the time I was five years old.

Early Childhood in San Francisco

Chamberlain: The situation in San Francisco in my youth was probably fairly typical of a physician's family. Most of the family friends were physicians; most of my friends were in the neighborhood there.

I wasn't at all well-adjusted socially at that time. I certainly was regarded as a sissy who didn't fight well, and got into various difficulties. Although, when my family went to Europe for six months and I went with them, in 1928, when I was eight years old, I discovered that for some reason, unknown to me, I had become a little bit of a hero, having made this trip to Europe. I found myself in a much better social environment when I got back. I was more one of the fellows then.

Peer Group

Hale: You'd impressed your peer group.

Chamberlain: Somehow it impressed the peer group.

Hale: Were the boys or the children that you were associated with prior to that from all social strata?

Chamberlain: I think they were from all social strata. I'd say these were families in that neighborhood who usually had the wherewithal to have some small summer vacation out of town, but just barely. I suppose they were in the top 20 percent in economic earnings at that time, maybe top 10 percent. As we look at them now, we'd say they weren't particularly well off in that, I think. A larger fraction can go away for two weeks in the summer now than used to then.

Hale: But they weren't all from professional families necessarily?

Chamberlain: No, not at all. One store manager, I think I remember. Many times I wasn't really sure what the parents of my friends did for a living. Then in 1930 my father left Stanford and moved to Temple University in Philadelphia. The whole family moved to Philadelphia at that time.

Father's Radiology Practice

Hale: Did your father essentially start the department of radiology at Stanford?

Chamberlain: Probably, something like that. I think he was the first head of a radiology department at Stanford, and I guess he was the first radiologist at Stanford.

Hale: How about in respect to the rest of the country?

Chamberlain: I don't really know how many radiologists there were. Probably not many, but I don't know. He's been a leader of moderate note among radiologists in that he's invented and originated a number of radiological procedures. He was particularly early at using these iodine-containing dyes to get good X-ray pictures of parts of the circulatory system. Blood supply in the brain, and so forth.

Hale: The beginning of tracers, isn't it?

Chamberlain: And, in fact, to this day I don't understand why more people don't use his full technique, because it can be so powerful. He does it in stereo vision; that is, he does it with two X-ray tubes that are flashed alternately so that he gets a picture that can be viewed with binocular vision. You see a beautiful three-dimensional picture of the circulatory system, for instance, when this iodine dye is used before X-ray pictures are taken. They've used this technique a great deal, but they don't use it with binocular vision.

Something like nine-tenths of people have a lot of trouble developing real binocular vision. They're used to binocular vision around the room, but given an artificial situation, they don't take the time to learn to put together the three-dimensional image. He used binocular vision on chest X-rays for years. If you're willing to study a chest X-ray for half an hour, it's incredible how a pair of chest X-rays used with binocular vision give you detail that you simply miss completely in a one-dimensional view. I would have expected that this would have taken over all of radiology, and it hasn't. The use of binocular vision seems to be unappreciated, and I'm as sure as anything that's a mistake.

Hale: Did he develop that before he left Stanford?

Chamberlain: I think so, because I can remember in 1930 or '31, when he had the shop at Temple University, him building these binocular viewers. You need a special viewer in order to use the two eyes to advantage, and he had to develop a format that had never been used there before. I'm pretty sure he brought it from Stanford.

Hale: Did you ever visit your father in his laboratory at Stanford? Do you remember?

Chamberlain: Yes. I don't think that I made as much out of it, or got as much benefit as I might. These visits were more common at Temple University because as part of his conditions to go to Temple he had something more of a research capacity within his X-ray department. He had, for instance, a shop where new X-ray machines could be made and various gadgets that went with them. I learned a little bit about shop practice in that shop. Not very much because they were busy with all sorts of other things, but I picked up a little bit here and there and learned how to keep up a shop and keep some of the tools oiled.

In retrospect, I could have done much more of that to great advantage, but somehow in these visits I was having trouble making contact with those areas that I was most interested in. There weren't very many physicists around. There was one physicist, and I learned a few things from him, but still I feel that there was an opportunity there that I didn't explore as much as I might have.

Early Schooling

Hale: Well, we might find out why that was. Let's talk about your early schooling in San Francisco. You went to Commodore Sloat School?

Chamberlain: I went to Commodore Sloat School.

Hale: Private?

Chamberlain: The only private school I went to was a privately operated kindergarten that I felt was very bad. The teacher was an old friend of my mother, whom my mother thought well of, but was actually one of those teachers that somehow bore a grudge against the children. She was terrible.

Hale: And that was until what age?

Chamberlain: I was just six when I entered first grade. I remember well that when I entered first grade, I was the only pupil in the room who could not write his name. By the fourth or the fifth grade I was doing very well in arithmetic, and I was being asked to teach the other pupils arithmetic, cooperating with the teacher. But it consisted of holding up pieces of cardboard that had the problem on the side that I was showing to somebody else, and only the answer in the back. My arithmetic actually went downhill sometime around the fifth grade, but it's recovered. About that time we moved to Philadelphia.

Hale: Did you have much contact with any other forms of science, or did you have hobbies of any sort? Did you and your friends make radios or something?

Chamberlain: No, our hobbies weren't very much at constructing things. Mostly we constructed forts and made cigarettes out of eucalyptus root. We had plenty of eucalyptus root in that part of San Francisco. If you dry eucalyptus root, it's a slightly acrid but not a bad smoke.

Hale: So your interests weren't directed towards science?

Chamberlain: Nothing scientific that I could spot.

Hale: You can't remember anybody even encouraging you to sort of fiddle with scientific things?

Chamberlain: No. My father helped me make a model airplane on one occasion; that was somewhat rare. He was pretty busy. We didn't do a lot of hobby things. He did set up a trapeze bar in the back yard, hoping I'd get a little more athletic. With him urging me on, I managed to fall on my head from the trapeze bar and get a tremendous headache.

Hale: So what did you do with your spare time? Did you read?

Chamberlain: I was really a very poor reader. My favorite readings were Dr. Doolittle stories. Nothing with redeeming social value, as far as I know.

Hale: At least they're not pornographic.

Chamberlain: I had a lot of fun with electric trains. We used to play great games with electric trains. The most enjoyable game was to set up all the tracks that one could find, starting

with a curved track and then going to the straight track, and then run the locomotive at about twice the rated voltage and have it roaring down the track, up a ramp, and off into a pillow [laughter]. Do a ski jump with the train.

Hale: So you'd say you were pretty romantic, then, in your attitude towards things at that time, rather than, you know, classic or practical?

Chamberlain: I don't know. That description doesn't ring a bell particularly.

Hale: You weren't interested in finding out how the train worked, for example?

Chamberlain: There was a lot of bike riding and various games in which we tried to lasso each other on bikes, which ended in a few crashes and collisions. We used to have a great time taking long bike rides down to the ocean. That part of San Francisco was sand dunes in large part, so it was quite a long bike ride through regions that had very few houses, to get down to the ocean. The fun-houses were there near the Cliff House, and we sometimes went down there, but we liked more to go down and watch the ocean pound against the rocks. There was a place where, I guess, a sewer exit had been cut out. A sort of square notch had been cut in the rock. It was probably twenty feet on the side or something like that, and we'd wait for just the right wave to send the water straight up in the air on this flat surface. About once every half hour the thing would make a beautiful fountain.

Hale: Was the trip you made to Europe for the culture, or was it just the relaxation?

Chamberlain: Well, it was sabbatical year for my father. There was the radiological conference in Stockholm at that time that he attended. But a lot of it was more like a vacation trip. I think it was at least two-thirds vacation and maybe one-third medical contacts. He made medical contacts practically every place we went, and saw a number of facilities and hospitals, and I think gave some talks and so forth. But it was more vacation than radiology.

Hale: What was your opinion of yourself at that time? Of your abilities and ambitions?

Chamberlain: I don't think I can really recall ambitions. I think I wanted to be maybe a streetcar motorman.

The family had a cabin up near Tahoe; still has. It's a little bit spoiled now because the road's been so much widened and it's become high-speed traffic. It's on the Truckee River, halfway between Lake Tahoe and Truckee. But it was a great place to go in my young years. I learned to fire a gun and had a .22 of my own, with which I hunted birds and that kind of thing. You can get a bird with a .22 if you're a good enough shot. I didn't kill very many birds. We played around with shooting a lot of cans, mostly, I suppose. This mountain experience of maybe a couple or three weeks, mostly summers, was good, I'm sure. It added something which is a little hard to describe in detail, but I kind of got to know the mountains and like the mountains.

Hale: Did it instill an appreciation of nature?

Chamberlain: I think a little bit, but everybody has some of that, and I can remember as a youngster waking up at four in the morning for no particular reason and seeing the bright moon coming through broken clouds and getting dressed and going outside and just watching them for a while. That wasn't a common thing. That was pretty rare.

Early Interest in Physics

Hale: Do you feel that that somehow ties in with your fascination with physics or later fascination with physics?

Chamberlain: Not really, as far as I'm concerned. I don't think there's anything clearly visible in that period that ties in well with physics. I mean, a little aptitude in arithmetic was about all I can recognize that I can think of.

In about 1930, almost as soon as we moved to Philadelphia, my father did give me a lot of encouragement to build a radio. This was a one-vacuum-tube radio, and one more vacuum tube was involved in the power supply. As soon as this radio was working, and my father gave me an awful lot of help with it, why, the family got interested in some of the programs. They immediately bought the family's first radio. We hadn't had a radio. It was like the families that resist TV in the later era. We had sort of resisted the radio. But the family got to enjoying Amos and Andy and Lowell Thomas. They came in close succession at that time, sometime around seven p.m. I guess that got started with

this little radio that I had built. Then I got a little bit into amateur radio, ham radio.

Hale: That would be what age?

Chamberlain: That's by age maybe thirteen to sixteen, in the high school years. Mostly I wanted to build the circuits and not so much use it. I never did learn to listen well to Morse code. It never came very easily. I'd work on it from time to time. But I enjoyed working on the circuitry and trying to understand what was happening. Had the most terrible time understanding inductance. Somehow my father thought he understood inductance, but I think maybe he didn't. My father, though a physician, sort of wanted to be a physicist. I think somehow, if he had a second chance, he probably would have become a physicist instead of a physician. This is very much instrumental in affecting the ways that he gave me things to work on. Well, this one-tube radio is vaguely in that direction.

Hale: So you think in subtle ways or in not so subtle ways he might have decided to push you in that direction?

Chamberlain: Oh, yes. Well, mostly subtle enough, I'd say. I didn't feel a very strong or determined push in that direction.

I should tell you that my grandfather, the physician, my father's father, did have a very large income at that certain period of his life, and he spent a lot of it on automobiles. All during my father's teens he had, I believe, four automobiles, in an era when automobiles were pretty uncommon. It was my father's job to make the things work. In fact, if he could keep them all in working order, then he had a car to motor around town in. I believe there were two Stanley Steamers and a great big White Steamer; they were practically all steam cars.

The family's full of amusing stories about how one of the cars caught fire out in Hayward and they had to go out the next day and tow back the iron frame, which then was rebuilt. In a matter of months it was again a working automobile. I believe [laughter].

The Doble Brothers and Steam-Run Cars

Chamberlain: I remember that my father and mother took their honeymoon in a 1906 Stanley Steamer; the honeymoon, I think, was in 1915. So if you think a nine-year-old car looks old nowadays, you should have seen this sort of hand-rebuilt nine-year-old car at that time. My father had a lot of new automobiles in and out, and there were certain little things he was particularly proud of. When he had blown the boiler on the Stanley Steamer, he was able to turn the gas headlamp into a torch which was sufficient to braze the boiler tube closed again and carry on [laughter]. Little items of that kind.

Hale: The Doble Brothers were in Oakland around that time, were they?

Chamberlain: Yes. In fact, I remember my father--he is, I guess, still convinced that the thing that killed the steam car was the Doble financial scandal. The vice president or something like that went off to Mexico with a large amount of the company proceeds. The Doble was so spectacular that they could sell stock just on the basis of the demonstrator.

Hale: They sold more than they were allowed to, apparently, didn't they, by the authorities?

Chamberlain: They certainly sold more than they should have, on the excuse that only stockholders would be able to get these Dobles for some years. You know, if you wanted to buy a Doble next year, you better buy some stock this year. To this day I have a little trouble understanding why the steam car hasn't re-emerged a little bit, because in certain respects it's got possibilities.

Hale: But isn't it that in a mobile body you just can't get the spread between the upper temperature and the lower temperature? You're basically limited by thermodynamics, aren't you?

Chamberlain: Yes, that keeps the efficiency a little lower than you'd like, but it doesn't make any nitrogen oxide to speak of. You don't burn at high pressure, and it might have something to offer. It's not good on fuel economy, I guess. But, you know, among these family stories was one about my uncle being chased by a policeman out in the country. My uncle's steam car would go much faster than the policeman's car, but he had to stop for water every once in a while. You know, these steamers went about one mile to the gallon of water.

So he'd go roaring down the road, gaining distance from the policeman; then he'd have to stop and put this hose over into a horse trough and he could suck up all the horse trough's water, you know, in one big gulp. Then he headed off again [laughter].

Hale: Sounds like the early Hollywood movies or something like that.

Chamberlain: It had a lot of that spirit.

Childhood Move to Philadelphia

Hale: Well, why did your father move to Philadelphia?

Chamberlain: I think that they were offering him some salary and a lot of independence. My father had a belief that radiologists should be more independent physicians, just as surgeons were, and should not be hired hands of the hospital. He had more of an opportunity to get that kind of independence when he went to Temple because he was prominent enough. They were willing to make certain concessions to attract him there, so he had a lot more independence.

I don't know whether it turned out exactly as he had expected it to. In Philadelphia all the patients in the radiology department were technically his patients. He in turn had a staff of maybe six younger doctors helping him, yet they were all patients in his name rather than their names. So in effect I think he felt, later, that he should have sought something in which there was not just independence for the head of the department but more independence for these young men. He ended up, I think, in a situation with which he wasn't quite as comfortable as he had expected to be. That was nobody's fault; it's just that he really hadn't foreseen what life would be like in a big X-ray department.

Hale: Was the function of the department at that time mainly diagnostic, or was it therapeutic as well?

Chamberlain: Well, there was both from as long as I can remember. In fact, I got some radium therapy when I was eight years old, while my father was still at Stanford; but that was done over my father's objections. It was one of those funny situations where misunderstandings crop up.

I had some kind of a mole growing on my face, and my father sent me to a surgeon in the hospital there to have this thing removed surgically. The surgeon looked it over and said, "Ed doesn't want this removed surgically. He'd surely want this removed with radiation." So he sent me back to my father's own X-ray department. By chance, my father was away for a few hours, and by the time my father got back, they'd given me a treatment with radon needles or something like that. My father was up in arms because he said you never give X-ray treatments on the face if you could possibly avoid it, because they leave scars. There was a big fuss about how this had been handled.

Hale: So he was very sensitive to safety aspects?

Chamberlain: Actually, yes. He was more sensitive than most of the radiologists--though I suppose some of the younger men now may be more sensitive even than he. But he was, I think, among the earliest to worry about the radiation damage and dose and scars, and to get better track of the dose of people in his X-ray department. He had a lot of friends about his age that had had radiation accidents of one kind or another. Well, a few at least.

Public School Education in Germantown, Pennsylvania

Hale: Was it rather an upheaval for you to move at that age to a completely different part of the country?

Chamberlain: It was very much an upheaval for my mother. I don't think I felt too strongly about it. In Philadelphia I entered a public school to finish the fifth grade and for the sixth grade. We lived in Germantown, and my public school was in Mt. Airy. It was a good enough public school. We had a few problems with two or three youngsters who were much older than they should have been. Most of us were something like eleven years old; then there were two or three sixteen-year-olds in the class who were doing badly in their academic studies, so they were just waiting until they became age sixteen so they could leave school. By law, you had to remain in school until you were sixteen in Pennsylvania, and they were four or five grades behind. So they were a little bit bullies, and we had some problems with them, but nothing really very serious.

I went to a public junior high school, to Roosevelt Junior High School in Germantown in Philadelphia, and first I was president of the freshman class at this junior high school, and I didn't handle that too well. It was the sissy image coming through a little bit too much. I got a lot of razzing and some of it fairly malevolent. I wasn't somehow appreciated by a number of the students.

But I then got in with a very tough bunch of students. I don't know exactly how this happened, but I got into a fight outside in the school yard, right after school had finished. It wasn't that I won the fight particularly, but I fought hard and took some bruises and bruised the other guy a little bit and came off well enough so that this added to the respect in school. Now, I fell in with a very tough bunch of kids that was part white and part black. I became somehow something like their mascot. They played terribly tough games. Their object was to come up behind one of their fellows unseen, deliver a smack on the back with an open hand that was enough to leave the shape of the hand as a mark. I was very much on guard that that didn't happen to me. Then my parents, I think, got alarmed that I was with such a little bit lower class--definitely a tough gang, partly black--and they lifted me out of the public school and put me in Germantown Friends School.

Germantown Friends School

Hale: Friends School?

Chamberlain: Germantown Friends School, one of the Quaker schools there in Germantown.

Hale: Oh, I see, yes. What age was that?

Chamberlain: I was just sort of finishing the ninth grade. It was as I would have gone to the tenth grade. In fact, I moved up half a year in the process. So I must have done part of the ninth grade in the public school and started the tenth grade in Germantown Friends School.

Hale: Why do you think they were worried about that sort of thing?

Chamberlain: That's a silly question. They were brought up bigots and--I mean, everybody was at that time. There wasn't anybody I knew that wasn't a bigot in terms of present-day standards.

My father was probably considered among the most liberal in this respect. I remember him saying that you must be very careful not to assume that all Jews are bad. You have to treat each person as an individual, even if he's a Jew; and he lived this out fairly well. I mean, he had friends among the physicians who were Jewish physicians, and they were reasonably close friends. But still, the image in the family was that there were certain bad things you might more likely expect from a Jewish physician than a gentile physician. The prejudice was clear. My mother's prejudice was less clearly expressed, but it was even deeper, I'm absolutely sure.

I can remember my father saying, "Now and then we have to ask ourselves, do we want the whole medical profession 100 percent to be Jewish? If we took the best candidate for medical school, they'd all be Jewish." So he said, "Our department accepts quite a number of students from a certain college in the South where Jews are prohibited." One of the mechanisms for keeping down the number of Jewish entrants was to take a number of candidates from this school that pre-screened the Jewish medical students. Already there was political pressure on Temple University not to go too far in this respect, from politicians in Pennsylvania who were supportive of the Jewish rights to enter medical school.

Hale: What about blacks? Were there any blacks in that era, at that time?

Chamberlain: It was not yet an issue. It was no question. I can't remember any black physician or any black medical student. It just hadn't yet become an issue.

Hale: What sort of subjects did you study in school?

Chamberlain: Well, in Germantown Friends School I had a situation where I spent, oh, it seemed like two or three hours a day working on my homework in things like history and English, working terribly hard. But I could do my physics or chemistry homework in ten minutes, so I was in this position where I spent all my time doing things I didn't like.

I generally had a distaste for school. I wasn't getting along too well with the fellow students in Germantown Friends School. I was accusing them of being rather snobbish and, in the process, being really a little bit snobbish myself, looking down on them for their snobbishness. But it wasn't a bad situation. I was not one

of the best adjusted in the class. I tried to hang around with some of the people who were, which was kind of helpful, and it was a fairly good situation.

Hale: Who did you consider the well-adjusted people, then, the people you hung around with?

Chamberlain: Well, one of them was Rhodes Murphy, who's a professor. He was voted in by the class at graduation to be the most likely to succeed. I remember overhearing one of the girls in the class saying, "You know, we voted Rhodes Murphy the most likely to succeed, and probably we're all wrong. Probably someone like Owen Chamberlain will be the one." I was the opposite side of the coin.

Hale: So that made you feel really good?

Chamberlain: Well, no. It indicated where they placed me.

Hale: What generally was the opinion of you at the time?

Chamberlain: Actually, I was rather surprised that I had fairly good respect from some of the teachers at Germantown Friends--even teachers who'd given me fairly bad grades in English and history courses, but they probably gave me decent recommendations for entering college. I got some hints that they thought I was at least good college material. I chose Dartmouth for college partly because I thought I'd enjoy the skiing. I was with a crowd that had different interests in college.

Science and Math Program at Germantown Friends School

Hale: When did you first have physics?

Chamberlain: There at Germantown Friends School.

Hale: Do you remember your physics masters or other science masters?

Chamberlain: Oh, yes, Mr. Bennett. It said in the yearbook that they thought I would be willing to give an A grade to my teacher, Mr. Bennett, but they weren't sure. There were some problems in which Mr. Bennett was getting the wrong answer and I was having trouble selling him on the right answer. In retrospect, I was right at the time, but I was having a

terrible difficulty getting any support from any of my fellow students. They believed the teacher, not me.

Hale: How soon was it you knew that you had the right answer?

Chamberlain: Well, by the time I was in college it was clear to me. This was one of those problems about how hard a man pulls on the oars of a rowboat. The question is with what force is the boat urged ahead when you're pulling the oars forward. Your feet are pushing back on the rear of the boat, on the floor of the boat. Mr. Bennett was ignoring this backward force. He was getting the wrong answer.

Hale: Would you say that he was very competent, then?

Chamberlain: Oh, yes, he was all right. I mean, he had the right spirit. He wasn't wrong very often, and I didn't feel he was grossly misleading or anything like that. By and large, he taught us well.

Hale: Was he teaching conventional physics?

Chamberlain: Oh, he was pretty conventional, I think. I, at least, was unaware of it being at all unconventional.

I had this experience in my senior year in high school: We were supposed to take one semester called trigonometry and one semester called solid geometry. I believe it was our twelfth-grade program. In the solid geometry course I thought, why do we have to study this? About three-quarters of the way through the first lesson, the teacher, Mr. Bryninger, turned away from the blackboard and said, "Chamberlain, you look awfully bored. Will you please leave the room?" I went out and then I came to see him after class, and he said, "Isn't there something you can do besides this solid geometry class?" And I said, "Yes, I'd like to learn the calculus." I'm not sure I said it so directly. I think we arrived at it a little more slowly than that.

But I got out a book that my father had given me within the year called *Calculus Made Easy*, and I studied that in the library during those solid geometry hours and reported back once in a while to the teacher what I was doing. I learned a little bit. I wasn't really getting the right slant on calculus, but I was getting something about what it was. I hadn't quite understood the value of memorizing some of the formulas for taking the derivative instead of working it out each time. I knew how to work it out each time,

which was good because it made it very firm what that derivative really was for me.

Hale: You did it by increments each time?

Chamberlain: Yes. What we'd now call, I think, the Delta process.

Hale: Delta process, right. Yes, I can remember doing that at great length.

Chamberlain: I did all the problems that way and thought I was doing calculus. But what you should do, of course, is memorize a formula or two; then you can jump over that process. Then I really learned it as a freshman in college, but it helped to have made this pre-trial in calculus, even though I didn't come up with too much. It was a very nice running head start on calculus, and it led, I think, to a deeper understanding of it, and making it more easily understood later on.

Hale: Well, what did you have in the way of practical training? Did you have anything to do with laboratories at school?

Chamberlain: Both our physics and chemistry classes in high school had a reasonable laboratory. I remember some experiments in heat where we heated some lead shot up to steam temperature and put them in a beaker of water, starting at room temperature, I believe. I don't remember in too much detail just which experiments we did, but there was a laboratory part to it which was quite decent.

Hale: Any indication of modern physics experiments or modern physics in principle?

Chamberlain: Nothing that touched modern physics, not a hint.

Hale: Not even radiology or anything like that?

Chamberlain: I don't believe anything approaching even that. No, there were many classical mechanical experiments done by hanging weights on meter sticks. I may be confusing some of the experiments that I did in college. I don't remember a particularly elaborate lab. I'm sure, though, there was a lab and we had lab tables in the room where we listened to lectures, and we had part lecture and part lab in the course of doing this.

Hale: Did you have any conception of what was going on in physics of the day?

Chamberlain: None whatever. I remember reading in the newspaper that someone had a theory that the amount of nuclear energy locked up in a teaspoon of salt would be enough to take an ocean liner across the ocean once.

Hale: Yes, that's a statistic that I read at one point.

Chamberlain: I thought these people must have been out of their minds. I had no inkling that there was any truth to this. I had no basis for connecting--

Hale: Ever heard of Einstein?

Chamberlain: I can't even remember that I heard of Einstein except a little bit in the newspaper. Einstein did appear in the newspaper once in a while, and sometimes it was just that Einstein refused to comment or Einstein remarked something or other, but they were small tidbits. Maybe I heard of Einstein, but not in the scientific context very much.

Hale: But you probably, then, would have never heard of Rutherford, for example?

Chamberlain: I don't think I'd heard of Rutherford.

Hale: Or Lawrence?

Education at Dartmouth

Physics and Chemistry

Chamberlain: No. At Dartmouth my education was all too classical. In fact, I feel in retrospect that Dartmouth sent me off to a rather poor start as a physicist. It was all classical education. Now, of course, I realize that a lot of the fault was mine, for not digging deeper and looking for other views and so forth. But the impression I got from my teachers at Dartmouth was that quantum theory was very dubious in that it maybe predicted a few things correctly, but it was very unlikely to be an ultimately true theory. It was kind of speculative and doubtful. Some people believed in it, but they weren't particularly the people that these teachers trusted.

Hale: That's from physics teachers, or philosophy?

Chamberlain: Physics teachers. Modern physics hadn't really started. Most of my teachers were of an older generation, and it happened that many of them were about to retire. There were some younger men who must have known somewhat better, but they didn't happen to be close to areas where I would learn modern physics from them.

Hale: Had you declared a major pretty soon when you entered college?

Chamberlain: Well, I sort of fell into a physics major. It was always the easiest thing to do. In fact, at one point I'd taken most of the physics courses that Dartmouth offered. I was toying with the idea of doing a chemistry major because having finished the physics I'd almost had enough time to go ahead and do all the chemistry. But then it turned out not to be so workable. I stuck with my physics major. The toying with a chemistry major--I don't think it was that fully serious, you know. I thought about it seriously enough to start adding up the courses and see whether it was doable, but I didn't stick with it very long. I took a number of chemistry courses.

Hale: Did you enjoy chemistry? Often physicists don't like chemistry.

Chamberlain: Well, the part that I did enjoy was when I got to beginning organic chemistry, and the professor began to teach me that you could predict what the substances were going to be like. That's, I think, where it began to catch on for me. Unfortunately, I've lost the basics. I can't at the moment remember whether a halogen is tightly bound to a benzene ring or tightly bound to an ethyl or propyl molecule. But once you learned that there was a difference in the tightness of binding of halogens to, on the one hand, a benzene ring, on the other hand a straight chain hydrocarbon, you could begin to predict what reactions were going to occur and what not.

Then he started showing me how ortho-para molecules had one chemical characteristic and meta molecules had another chemical characteristic, so that you could begin to tell now what was going to be solid and what reactions were going to be allowed to change this molecule. I kind of began to enjoy it because it was a little bit of a challenge to see how well you could take these factors into account and guess what some new molecule would be like.

I did a little special project on hippuric acid, which, if I remember correctly, is one of the simplest of the two-ring molecules. It got its name because it was found, I guess, first in horse's urine. I first synthesized it from other chemicals, and then he wanted me to find it in nature in various places. He wanted me to find it in my own urine, which I eventually did. But he wanted me to eat benzoic acid, and I said, "I'm not going to eat benzoic acid." He said, "Well, okay, will you eat prunes?" So I ate prunes and then got hippuric acid from my urine. That was kind of a special project that I did, just to indicate that I was going slightly beyond, perhaps, what most of the students were doing--but very slightly. I got good grades in chemistry because I washed my glass very well [laughter].

Hale: Your litmus paper always turned the right color?

Chamberlain: Oh, I had various difficulties. Once I got several percent more of the product than I should have from the ingredients that I had put in. I must have subconsciously or consciously put in a little extra to get a good yield.

Hale: Did you like any equivalent type of project in physics that were a little more than just rote learning or a laboratory?

Chamberlain: Well, not too much, not too much. I remember a few examples. I was getting a little bit bored in freshman physics because I really knew the material pretty well from high school. I came into class one day with a close friend, and we looked at the demonstration things that were set up on the lecture table and decided that we knew what was going to happen exactly, so we went out again before the lecture started.

The professor noticed the fact that I was absent, and at the beginning of the next lecture he said, "What happened to you last lecture? I saw you in the room for a while and then you disappeared." I said, "Well, I thought that I'd seen some of the demonstrations before and maybe I could skip that one." He acted a little bit hurt or something.

Had a very good optics laboratory from a different professor. Among other things, I remember he showed me how a Michelson interferometer worked. He took it all to pieces in front of me; just listed all the pieces of glass and mirrors and everything and put them on the table and said, "Now reconstruct this and make it work and find me the white light fringes." That was a good experience because, oh, it must have taken me four or five hours to get the thing back

together and working and find the white light fringes. But I knew exactly how that thing was constructed, and I began to understand how one piece of glass compensated for another piece of glass in the instrument.

I think this optics laboratory was good from beginning to end. We had what I supposed were old-fashioned but good optical instruments. We had at least two Fabry-Perot etalons, and we had one of those stair-step etalons. There's a proper name. We had some not too bad spectroscopes.

Hale: So would you be doing atomic physics, for example, in that sort of thing?

Chamberlain: Well, it might have been, but there wasn't any hint that we were doing any atomic physics. Nothing.

Hale: You were just looking for spectral lines?

Chamberlain: A few spectral lines from the sun, but no atomic theory behind them at all.

Gripes with the Dartmouth Chemistry Department

Hale: I see. You weren't even taught the Bohr theory? When you say you learned quantum mechanics wasn't accepted, wasn't the early quantum theory relatively well accepted?

Chamberlain: Well, let's say it wasn't emphasized. I can't remember. There must have been pieces of it, because I knew something about Planck's constant in college, but it sure wasn't carried very far. I realize part of the weakness is mine, a large part. But it was kind of curious to have such a good classical education in physics and such erroneous ideas about quantum mechanics.

Hale: I wonder why you keep on saying that the problem was yours.

Chamberlain: If I'd read the *Physical Review*, I would have realized that quantum theory was central to everything that was going on in the *Physical Review*.

Hale: But I think very few undergraduates would be doing that of their own volition.

Chamberlain: True. No, the text that we were using had a great emphasis on classical physics. There was indeed one course that was called Modern Physics, but we sure didn't get into it very far.

Hale: It was probably a mishmash of atomic and a bit of nuclear.

Chamberlain: Like many of those courses, it was very poorly put together intellectually. Get a lot of observations, but no theory to hang the information on so that it tended to be--

Hale: Phenomenological?

Chamberlain: Very phenomenological. It certainly wasn't useful to me; I hadn't mastered it. We knew a lot about propagation of radio waves and light and heat, mechanics, classical electricity were all very good. As soon as you got close to the quantum theory, it was either lacking completely or it was handled in a dubious way.

Hale: Do you remember some of the names of your professors that I might recognize?

Chamberlain: The one that I think you'd be more likely to recognize, though I wouldn't expect you to, was named Hull. He was the man who taught this freshman course. He had done some physics research work that I can't remember the nature of in World War I. He was perhaps moderately prominent as a result of that, I think the best known of the people in the department. Professor Proctor may have been the one who did the optics lab. I don't remember the names of those professors as well as I'd expect to.

Hale: So, anyway, you wouldn't characterize it as at the forefront of physics.

Chamberlain: Oh, no. There was always a little research activity going on in the department; but there was little research activity of any kind, and what there was tended to be very classical in nature.

Other Activities at Dartmouth

Hale: Outside of your courses and things you were interested in, what other things did you do? Did you take part in the extra-mural activities, sports, drama?

Chamberlain: Not too much. I didn't do a lot of outside activities. I was having great trouble learning German. I had trouble with all languages. I thought that to learn German I should join the German Club, which I did. It was fun, though it was a peculiar activity in a way. We sang various German songs, which I sort of learned by rote, but I just didn't learn any German that way. It was not helpful at all in learning the language. We dressed up a bit; we had white caps with a little black brim, and we had some kind of ribbon that we wore to dress occasions. We could pronounce the name of the organization with the right German accent. But somehow just the amount of German I learned was a disappointment.

Some of us fooled around with ham radio during the college years. We rented the use of a barn for five dollars a month, or something of that sort. I think the farmer was worried that we were using too much electricity. Finally, in lieu of paying rent for the barn, we paid his whole electric bill, which was even less than five dollars. We kind of had a good time with this. It was sort of fun.

There was a big hurricane in 1938, just about the time we got back to school. All the roads were closed in our part of the world that led out of town, so our radio station was the only contact with the outside world. There were no problems to be solved because there was no shortage of food. There was no disease, there was no reason to worry about outside contact, but it was kind of fun. We sent a few messages home from some of the students--things like that. Mostly we listened and talked to people on Long Island, where the storm was much worse. They heard what had happened to them.

Hale: What about things like music?

Chamberlain: I took a lot of piano lessons. In fact, I took piano lessons because my parents insisted on this from age six to sixteen. But I haven't got any real aptitude for the piano, or for that matter for any other music, as far as I know. I think it was kind of a waste of my time and their money.

Hale: You didn't appreciate music?

Chamberlain: Oh, not particularly. A little bit. I could play things like Rachmaninoff's *Prelude in C-sharp Minor*. I never did learn to read music well. I think it would be more fun if I could read music easily, but even when I put some effort

into trying to learn to read music, somehow it never really worked.

Hale: Is there any one thing, one activity that you can pick out that you felt that you did, put some energy into?

Chamberlain: Not really. I can't really pick out anything. There were a few other things here and there. In high school I was the so-called manager of the baseball team. It meant I tried to keep track of some of the equipment, and I got to report the scores and whatnot to the local newspapers, for which I got paid a small amount. I had some vague realization that science was poorly recorded in the newspapers, and I used to dream about trying to learn enough about what was going on in something scientific to make a report in a college newspaper. But this name came to any fruition whatever.

Hale: Did you do much reading in general? Philosophy or literature?

Chamberlain: I read slowly; much preferred to learn by talking to people rather than reading. As a result, a lot of things I didn't learn so well. Somebody pointed out that I had managed to go to a liberal arts college, namely Dartmouth, and to avoid getting a liberal arts education. I'd taken quite a lot of physics, quite a lot of chemistry. Kept a pretty heavy emphasis there, and what I did in the social sciences was to limit my work to rather general courses. I remember taking a couple of courses that were called Social Science rather than Economics or Political Science or something that would have been one of the more specific fields.

II GRADUATE SCHOOL CAREER

Chamberlain's Politics, World War II, and the Depression

Chamberlain: I had what looked more like rather erroneous ideas about world politics at the time. The background was that my father insisted that the Nazis were awful, but he also insisted that during World War I, which he reminded me he lived through, the Germans had--I don't know--tortured babies; there were various war atrocities which he believed in from World War I. The information I was getting at school was that, in fact, this was all part of the war propaganda, that the atrocities were not particularly lopsided in World War I. So when my father insisted that the Nazis were a horrible lot, I assumed he was falling into the same war propaganda again.

My view of the situation was that somehow these stories about what the Nazis were doing to Jews, and that sort of thing, were pure propaganda. They were all part of a drumbeat for a coming war with which I had very little sympathy. I graduated from college in '41 still believing at that time that this was a war between a bunch of big powers for which they were probably all somewhat responsible. But I remember when the Pearl Harbor attack came, my view was immediately that the United States was forced into this, that the United States was attacked and we were certainly going to respond with whatever we could.

Hale: You were not aware of things like the invasion of Poland and that sort of thing?

Chamberlain: I was aware of the invasion of Poland in '39.

Hale: I was brought up in England, and the war started there, of course, in '39. I'm very surprised to find that for most

Americans it started in '41. Was the general amount of information in the newspapers rather scarce?

Chamberlain: No, it was all over the newspapers. Let me see if I can get it straight. I think we understood that Germany was on the march into Poland all right; that was an invasion for which Germans offered some excuses, but they were extremely lame excuses. I think we were aware of the militarism but not of the atrocities. The U.S. sympathy with Britain and France was clear-cut and was solid. I think there was at least a certain amount of doubt in many people's minds about how much U.S. responsibility there was to respond directly to that situation.

Now, the information about the atrocities, in retrospect, was certainly there. It was reliably enough reported, but the fact that these were reliable reports was not coming through well in the newspapers. If you are a critical reader of the *New York Times*, you can dig out facts that would make it very persuasive, but it wasn't coming through in the ordinary press. It was coming through sounding more like propaganda and not like well-established facts. At least, that was the way I was reading it.

I know that, in retrospect, I was way off the mark in my attitudes at that time. I see this as a reaction, partly against my father. My father had proved himself wrong, I thought, about World War I and was following the same track over again. So when my father insisted there were documented atrocities going on, he didn't come up with as good a documentation as he might have. I'm sure he could have persuaded me with some of the reports that I know were available then. I think we only got the Sunday *New York Times*--that should have been enough if we wanted to scan it carefully. But I wasn't doing that at the time.

Hale: Did you have much political awareness of what was going on at home? In the States?

Chamberlain: No, awfully little. We couldn't help but be aware of Depression problems with people coming to the door selling apples and things in '32 and '33, the first few years we were in Philadelphia.

Hale: Did that directly affect you in any way?

Chamberlain: Not really. There were some economies that had to be made at home, and I knew my father had a severe financial crisis on his hands. People had stopped paying their bills for

radiology. I think there was one year that the X-ray department was $100,000 in the red. The way these things were set up, it was my father that was $100,000 in the red. But I think he handled it just beautifully. He made deals with everybody. I mean, he was something like eight months behind in paying for his X-ray film, and Eastman or somebody understood this and kept it coming. He kept reassuring them that he had a plan--you know, going to get it all straightened out. I think he bought some X-ray equipment that he wasn't going to pay anything on for two years or some darn thing. It was becoming almost a barter arrangement as much as they could arrange.

The hospital loaned the department some money to bail them out, and he borrowed all over on his life insurance and all sorts of things, whatever personal finances that he could come up with. Then, finally, when this got ridiculous, why, the end of that year came, and he renegotiated his contracts with his young physicians. They all got their salary cut in half, and within another year he had the thing on an even keel. I think he handled it very well. Nobody was fired, and nobody failed to get their X-rays taken, and nobody went hungry. Eventually everybody got paid off and everybody was happy.

Hale: Other than that, you probably would count yourself somewhat insulated from political questions?

Chamberlain: Yes, I think rather insulated. I think there was a reasonable attempt to get close to political questions in my high school education, but there was also a reticence to get too close to things that were going to be controversial. I remember in our biology class we could study sexual reproduction up to the frog, and then we quit [laughter]. We just said, we don't go beyond that point. But we could live with that. The same sort of thing was happening in political life.

More about Chamberlain's Family and Father

Chamberlain: Now, let's see, the summer before my senior year at college, the family came West, as it did many, many times in the summer; that must have been in 1940. My father knew Ernest Lawrence at the Bohemian Grove. My father had never belonged to the Bohemian Club when he lived in San Francisco. He said he couldn't possibly afford it. But

when he moved to Philadelphia, he discovered that out-of-town memberships were not so bad. I don't know how much the membership cost, but he became a Bohemian Club member about the time that he went to Philadelphia.

Hale: It gave out certain status to be a member of the Bohemian Club?

Chamberlain: I don't think that was really his motivation. I think his motivation was that somehow he had visited there a couple of times, and he liked that as a place to come back to in some way. In retrospect, I'm not sure that he didn't find it a great help to get away from my mother for a period of a few days at a time, a place where my mother was forbidden to come. I think that may have had something to do with it.

Hale: Did he figure your mother was a burden on him?

Chamberlain: Well, by overmanaging the situation, she could be a psychological burden of sorts; anyway, I'm not sure what his reasons were. He certainly liked the Bohemian Club, and he tried to go to the Grove for at least a couple of weekends--sometimes for a solid week if he could at the time of the summer encampment at the Bohemian Grove.

Hale: So he'd be doing this previous to 1940, right?

Chamberlain: Yes. We had gone to Philadelphia several summers. We had driven west for a vacation period. I suppose at least every third year we drove to the West Coast during the summer and enjoyed this cabin in the mountains, and he'd get some days at the Grove. We'd see a lot of the U.S. as we'd cross the continent. It was a lot of fun.

I learned to drive on those occasions. When my sixteenth birthday came, I went down to get my driver's license and passed the driving test just fine, and the instructor said, "You're sixteen today?" "Yes." "Now, where did you learn to drive?" I'd been driving around Philadelphia without a license for a couple of years. When you drive without a license when you're two years younger than you should be, you learn to drive so perfectly that a policeman never questions. I always looked young. I was driving a decent-size car, some cases a La Salle or a Packard, maybe a Franklin at that time. I guess it was a big enough car and looked opulent enough that the policemen were hesitant to stop me. But I was driving all around without a license. It's a marvelous way to learn to drive well, when you don't want to be caught for any reason.

Hale: So your father had gotten to know Lawrence?

Chamberlain: Well, I don't know how well. Not very well, I suspect. He was certainly not a close family friend or anything. They knew each other's names on sight. That summer, just before my senior year at college, we dropped in at the physics department here, and my father said hello to Professor Lawrence and got one of the young graduate students to show me around the Radiation Lab. It was mainly the thing that was called Old Radiation Lab in the wooden building down on campus. Well, the following year, I started graduate work in physics just because that was the easiest thing to do; it was like, you know, walking down stairs, sort of the next thing that was much easier than going out and looking for a job. I didn't have any great purpose.

There was one contact with a physicist which in retrospect was very important. Let's say when I was in about eleventh grade in high school, we'd have sometimes something like a Thanksgiving dinner with a fellow radiologist, who was Dr. Edith Quimby from Memorial Hospital in New York. But her husband, whose name is Shirley Quimby, was a physicist. I only learned later that he was a physicist. He used to give me puzzles and problems which were sort of physics games.

I can remember one problem he gave me. He said, "Now, you know there's no such thing as a perpetual motion machine." I said, "Yes." And he said, "Then explain to me why this perpetual motion machine won't work?" The machine consisted of an electron and a positron that gained energy as they fell down the evacuated pipe under gravity. When they got to the bottom of the pipe, they annihilated and gave energy. They gave up their mass and some additional gravitational kinetic energy they picked up from gravity, to make a photon which goes back up the evacuated pipe and has enough energy to make a new electron and positron pair.

I explained why this kind of machine can't work. I got the right answer: there's a gravitational red shift. That's the way I learned about the gravitational red shift, from this puzzle. That's one of the most sophisticated of the problems he gave me.

He gave me lots of mathematical puzzles that were easy to solve. He was an amateur magician; in fact, he was almost a professional magician. He brought magic tricks along, and I tried to dope out how his magic tricks worked and how his card tricks worked. I was able to redo some of

his card tricks just having observed him doing them, and he was pleased with this. But it was a constant intellectual challenge with him.

I think that's the first time that I was exposed to a physicist under circumstances where he could toss me problems that made me think. Some of these problems he'd give me and say, "Now, I'll be back here in a few months and you tell me the answer." I think that's what really got me into physics, even though I didn't understand he was a physicist. I sort of got started with a fascination with some of these problems and puzzles. That's what physics is all about: problems and puzzles.

Graduate School at UC Berkeley

Chamberlain: After I graduated from college, I entered graduate school at Berkeley.

Hale: When you were shown around here, what did you see? Essentially, how did it appear to you?

Chamberlain: Oh, it was a muddle. There was a cyclotron with all its insulators, and I seem to remember sort of a few aluminum electrodes and some sparks. I can remember hearing these big zaps occur when something would arc over. The cyclotron itself I could hardly remember from that visit.

Hale: You would have seen the 60-inch?

Chamberlain: It was the 60-inch, I believe, yes. The 37-inch was there, but it wasn't working when I visited, as far as I can remember.

Hale: It was about to be converted at that time, I think, to a mass spectrograph. Maybe that was a little later.

Chamberlain: Well, I had no contact with Lawrence that I can remember in my first semester of graduate work. I either took or audited a course by Segrè in my first semester, even though other students were more advanced. Oh, this was amusing. Professor Birge, who was the chairman of the department and also acting as my advisor for my graduate work, somehow had been told that I was a very promising student. I don't know what the word was that he used. I had won a mathematics prize at Dartmouth and, in fact, they gave me the Kramer

fellowship, also at Dartmouth. This supported me--most of the money I needed in my first year of graduate work--at whatever school I chose to apply. It was a very nice thing from Dartmouth. It was a one-year tuition grant for graduate school.

Hale: What did you have to do to win that?

J. Robert Oppenheimer's Quantum Mechanics Course

Chamberlain: That came as a surprise to me, and I don't think there was any open competition that I was aware of. They awarded this on the basis of what I had done as a regular student, not through any special exam. The mathematics prize came through an exam. Thayer Prize in Mathematics. Anyway, Professor Birge said, "You can go right into the graduate quantum mechanics; you don't need any undergraduate mechanics." So I went into Oppenheimer's quantum mechanics course. I was completely lost. I didn't know what a wave function was. This wave function he was talking about was absolutely mysterious. It had no relation to reality as far as I was concerned, and I started asking questions in the first class or two. I remember Oppenheimer saying, "Well, physicists are still discussing what the wave function is." Then he went into some abstruse discussion, and it wasn't helping me a bit. Then I learned you're not supposed to ask questions in that class, and so we all sort of clammed up.

Hale: But he did seem willing to start off and admit that it wasn't all set?

Chamberlain: He was. That was all right. But he wasn't helping me understand the parts which were well set. I was trying to understand what the wave function was, and I wasn't learning a thing. Oppie had the idea that you lecture at a higher level and lecture fast and the students that are good students will catch up somehow. You just make them run, make it work.

Hale: Was there any previous course available?

Chamberlain: Yes, there was an undergraduate course in quantum mechanics, but I had had nothing like it at Dartmouth, and I would have been much better off if I had taken this undergraduate course. There's nothing wrong with starting graduate school with some undergraduate courses in your first year.

Hale: Sure.

Chamberlain: I think Birge thought I wouldn't need it. I was having such a time. I got an A in Oppie's course by copying everybody's homework I could get my hands on and by learning to do every problem by one particular method that was the WKB method. That wasn't the method that I was supposed to be doing the problems, by, but it was progress of a sort. I got an A in Oppie's course, which meant I had permission to become a theoretician. I knew perfectly well where everybody stood in the class. I was the last A and the other people that got B's knew who they were and where they stood with respect to me, and I felt very, very lucky. I was just doing the best I could to get by, and that was all. I absolutely hung on by my fingernails as far as some understanding went. In fact, the understanding was very marginal.

Hale: Well, I venture to say that's the experience of any physicist I know that's been educated in the last twenty or thirty years.

Chamberlain: Well, no. This was much more extreme than the usual experience because we can put it on a more logical basis if we'd keep things in a more logical order. If I had had an undergraduate course, I think I would have understood this much better.

Hale: Can you tell me about the students, the other students that you were working that were in that class? Was Boehm in your class at that time?

Chamberlain: He was around. Let's see, there was a fellow named Ray Murray who was very good; he was good enough to let me copy his homework now and then. I learned a lot of things from him. I believe George Farwell was in that same class.

I made one wonderful move which made all the difference at that time, and it was an important difference. I went to Professor Birge about the second week of classes and said, "Is there any place I could have a desk within the physics department area at which to study?" And he said, "Well, you're not a teaching assistant, and I don't know whether we'd find a desk for you, but we'll try." About a day later, word came that he'd found a desk for me in the office of the 4A teaching assistants. That made all the difference because then I was in contact with students that were in the same courses as I. I think George Farwell was one of them; I remember there was another.

We got a lot of help from Stanley Frankel, who was an older student that was willing to take some time with us. When we couldn't understand the quantum mechanics, the three of us would go find an older student in the building. Stanley Frankel helped us, and Bernard Peters helped us. At that time, we couldn't tell the two of them apart. We just knew one was one and one was the other, and didn't know which was which. And they looked a little bit similar.

First Work with Emilio Segrè

Chamberlain: Well, in the middle of this school year came Pearl Harbor. Within a day or two, word came around that Lawrence was looking for people to volunteer for some national defense project. Well, I just wanted to think over the situation, make up my mind, so I kind of lay low during the exam periods. But by, I think, the 30th of December, I went and saw Lawrence and said I was ready to sign up for some national defense project. He assigned me to help Professor Segrè.

I never have known whether having known my father, he gave me a particularly good spot to work as assistant to Segrè. I don't think so. As a matter of fact, I don't think he remembered any connection between me and my father at that moment. At least there was no recognition when I saw him. I think he knew that Segrè was somebody; I think he knew that Segrè was a good physicist, but he didn't have a profound respect for Segrè because Segrè's style was so different from his. I think it was just an accident that I was assigned as a helper to Segrè because a lot of the other people that signed up the same week were helpers to other people who did much less physics and much more plumbing. In fact, I worked for about nine months on that project before I really understood what the project's purpose was: to make an atomic bomb.

Clyde Wiegand was already working for Segrè, and I found Clyde building a power supply where he was connecting up chokes and transformers and vacuum tube rectifiers; he was making the power supply. But a few months later, he and I doped out how you make a regulated power supply. We never heard of a regulated power supply after that. This was a fortunate assignment because with Segrè we learned physics all the way along through this war. It started with a part-time job rather than full-time, and my graduate study kind

of dribbled off for about a year--less and less graduate study and more and more research.

Other Courses and Professors at UC Berkeley

Hale: A couple of questions. What other courses did you take, say, in your first semester?

Chamberlain: Somewhere early on I took a thermodynamics course from Williams; it may have been that first year. I know I was taking it somewhat prematurely because all the other students were second-year students.

Hale: He was a theoretician?

Chamberlain: Yes. I think I took an electricity and magnetism course.

Hale: Lawrence? That was his area?

Chamberlain: It may even have been taught by Lawrence. I remember seeing Lawrence do some electricity and magnetism, but I can't remember whether it was the whole course. The important point is that my memories are all centered around that Oppenheimer course, and trying to pass it. That was terrible.

I remember Williams' thermodynamics course. I was having trouble because I wasn't too well prepared and somehow I wasn't quite as sophisticated as these older students. I remember there also I got an A; again, it was the last A in the class. There was only one person that was worse than I in that thermodynamics class, and he got a B. Again, I knew sort of where everybody stood.

Hale: Outside of the classes like that, were you doing any practical work at that time, like work on the cyclotron?

Chamberlain: Well, only when I started helping Segrè.

Hale: I see. It seems that most graduate students that arrived at that time, especially if they were oriented towards the Radiation Lab, were involved with the cyclotron.

Chamberlain: Oh, I think I would have been in another year or so. But there was still this idea that you better get through your course work first.

Hale: Oh, I see. That was the emphasis.

Chamberlain: The older students were involved more in that way. But I would have been.

Hale: Had you intended before you came to be a student of Lawrence's?

Chamberlain: Not particularly. I was just doing what seemed like the most natural thing.

Hale: Oh, I see. So you had no particular idea in mind.

Chamberlain: I liked physics and that was about it. Now Segrè started to teach me things. First of all, I learned that Oppenheimer's quantum mechanics course was about the best there was, and that was a great surprise to me because it seemed all like nonsense. It didn't fit together well, and so I learned that I'd better take seriously what's in that course.

Hale: Well, he essentially could be credited with introducing quantum mechanics to the States, couldn't he?

Chamberlain: Could be. I don't know enough about what was going on at other places to be happy saying yes or no to that from my own experience. But probably so. From Segrè I learned a lot of things. Segrè has an active mind, always had an active mind, and if nothing else he came up with puzzles. This is more typical of the Los Alamos era. The people in charge of this little sub-project were Segrè and Joe Kennedy, who was a chemist. He died much younger than you'd expect a man to die. I had some contact, I believe, with Gerhart Friedlander at that time.

Hale: Gofman and Lawrence--

Chamberlain: Gofman was in there somewhere, too. And Art Wall, that's right. In fact, the first time I published a paper, Gofman was a co-author.

Hale: Did the research take up all your time?

Chamberlain: First year, about half-time. Within about a year after Pearl Harbor, they'd become full-time, and I dropped all graduate stuff.

Hale: Yes, I see. Did you have a sense of urgency about the work during that period? You said it took you nine months to realize what it was for.

Creating Spontaneous Fission with Oppenheimer

Chamberlain: I don't think we had a sense of urgency during that period. Segrè was pressing us pretty hard. Oh, I thought these guys were out of their minds because we were trying to measure spontaneous fission. Well, the data that we had clearly indicated to me that the apparatus wasn't working. But here they were, reporting the data to Oppenheimer and people that were in the higher responsibility positions.

A fission made an electrical pulse which we amplified, and then we had to count those strong pulses in a mechanical device. To drive the mechanical [counter], we had a cold cathode thyrotron-type of thing that had a gas discharge in it. The gas-discharge tube that we used to actuate a mechanical register worked in daylight and didn't work at night. That's what the trouble was. I went down to service these things and record whether there had been a count, or one or two counts in the night, and usually there were no counts and sometimes one, typically.

I serviced these things twice a day, but I didn't always do it at sunrise and sunset, so the dichotomy between night and day showed up a little bit less sharply than would have if I happened to do the thing about ten in the morning and ten at night. So it was obvious to me that the nighttime intervals were counting less than the daytime intervals by a factor of almost two. It was a perfectly clear difference, and I kept saying, "Segrè, the thing doesn't work at night" or "the counts are all spurious in the daytime."

The building we had this housed in, part of the time, was a building that was mostly full of practice rooms for the music students. They practiced cellos, you know, so I could hear in other parts of the--I was afraid the cello would every now and then resonate with my ionization chamber and introduce enough noise to get through and count, make it look as thought it had been a spontaneous fission. So I was of the opinion that it was equally likely that the daytime counts were false and the nighttime counts were more correct.

Finally I showed that by leaving a flashlight on all night inside this copper box which was the apparatus, that I could get the thing to count also at night. But in the meantime, Segrè and Joe Kennedy were reporting these results to Oppenheimer, and I was thinking, "You're out of your mind. Those aren't results. Nothing but noise."

Hale: And you turned out to be right in the end?

Chamberlain: Well, I turned out to be part right. We were a little off the beat, but they were also right in the sense that there wasn't complete garbage. The samples that we were counting were indeed the ones that we were counting, but we were about a factor of two off the rates because actually it was dead at night and correct in the daytime, approximately.

Hale: The overall end of that part of the project was to find the spontaneous fission, right? Plutonium 240?

Chamberlain: Well, the important ones were the spontaneous fission uranium 235 and uranium 238.

Hale: Oh, I see.

Chamberlain: And on that we were getting the wrong answer at the time. By some luck we moved to Los Alamos in mid-1943 and because the altitude in Los Alamos is so much higher, the cosmic rays are more intense. We immediately found that the counting rates at Los Alamos were maybe two and a half times what they were in Berkeley. That was the tip-off that all that time we'd been measuring cosmic-ray-induced fissions instead of spontaneous fissions. So we were saved by chance; we were saved getting all the wrong answers. We didn't have sense enough to put cosmic-ray shields over our neutron shields. Finally, at Los Alamos we used boxes about three inches thick filled with borax or something that would get rid of the slowest neutrons.

Hale: I see. That would be very critical in the process.

Chamberlain: Oh, yes. Segrè had asked Hans Bethe to predict whether the cosmic-ray neutrons were of any importance. Bethe had said no; he didn't think there'd be any problem calculating cross-sections or something. He was wrong. We didn't have sense enough to check him. I mean, it was such a simple thing to do. I could have gotten some stuff at the grocery store to pile up around those counters if I'd thought of it, and tried to show whether they made a difference or didn't. Anyway, the correct answers in due time came out; the real spontaneous fission that was there was from uranium 238.

Hale: Did you think about the neutron-absorbing power of different materials?

Chamberlain: Well, that's a good question. I'm not sure at what point I learned that. Let's see, Segrè had done some experiments

with slow neutrons, and we learned something now about the slowing-down time for neutrons and how long a slow neutron would last in water. We knew some of those things. I must have known them sometime in 1942, at least some of those pieces of information. I don't suppose I had a well enough rounded view so that I could have doped out what to do for that experiment. Though as soon as we understood we needed a shield from cosmic-ray neutrons, I had at least some idea what to do. I would probably have started with paraffin, but I think borax is better. I don't remember when we learned this. Of course, the first nuclear reactor was December 2nd of '42. Word of the reactor having worked came quickly. I got it from somebody that never should have known.

Hale: Who was that?

Chamberlain: Forgotten who it was. I knew something was up because I overheard Segrè saying something about how satisfying it was at least something was working. I knew some bridge had been crossed somewhere, but I didn't understand that it was Fermi, and I didn't understand that it was the reactor right away.

Hale: Did you feel that you were in a very junior position during that time?

Chamberlain: Oh, my, yes. I was just a helper. I had one semester of graduate work plus dribbles of a little bit more, at most one year of graduate work. And I was just acting as a lab helper to Segrè, that's all. Wasn't close to him at all.

Let's see, there's one or two things that I think would be fun to indicate here. After I'd worked on the project about nine months, I learned what the project was all about to a large extent from somebody that should not have known. That was Miss Wu, Chen Chun Wu.

Hale: She was having problems at the time, wasn't she, about--

Chamberlain: Citizenship. Yes, that's right. She wasn't supposed to be part of the project at all. She told me in the basement of Le Conte, "Well, they've got a bunch of stuff over there that they call aluminum magnesium, but they're obviously uranium isotopes." [laughs] I think that was the first time I understood that these things that I was working were isotopes of uranium.

Hale: Really?

Chamberlain: Yes. I was just sticking them in, doing what I was told. No need for me to know. One was called 49. I didn't know what 49 stood for. That was the element 94-239. That was the first plutonium sample, but I didn't know that.

Hale: That sample, then, was the one that came from the 60-inch cyclotron?

Chamberlain: Yes, from those boxes of uranium oxide that were piled around the 60-inch. They just replaced part of the shield of the 60-inch by boxes of uranium oxide and then turned on the machine and got as many milliamps out of it as they could. After a few weeks of bombardment--could have been anything from two weeks to two months--they extracted this microscopic sample. But at the time, I didn't understand what was going on.

Hale: What contact did you have with the other projects in the laboratory? Did you know much about the electromagnetic separation process?

Chamberlain: No. George Farwell was working on the electromagnetic separation, so I heard little bits about the difference between gunk and crud. It's a little hard to remember. Let's see, I think it was about near the end of '42 that I understood they were separating uranium isotopes. I heard something about the electromagnetic separation problems and went to some seminars where this was discussed.

Hale: That was going on all very heavily through '42, of course, and in the end of '41, even before Pearl Harbor.

Chamberlain: Yes. I didn't know about it before Pearl Harbor.

Hale: In fact, the 37-inch was being used at that time as a mass separator--even before.

Chamberlain: Well, during the time I worked for Segrè, the 37-inch was definitely being used. We used to have trouble with the arcs and sparks that would affect our apparatus. We tried to shield it from the discharges of the 37-inch, and just nothing we would do would shield those discharges out of our apparatus. We just had to put distance between us and that cyclotron in order to do anything.

Push to Finish Ph.D. Before Embarking on War Work

Chamberlain: In mid-1943 we left for Los Alamos. But there's another story I want to tell you. Knowing that I was going to go to Los Alamos, I tried in the spring of '43 to see whether it was possible to get a Ph.D. all in a hurry. Well, this was lucky I didn't succeed, actually. I took the mechanics exam, the oral. These exams were, at the time, in several pieces: there was a mechanics exam, there was an optics exam, there was a modern physics exam, and an electricity exam--something like that. I took the mechanics exam and passed it with flying colors. I must have done one of the best jobs that anybody had done on that because I considered myself just top-notch in mechanics and I kind of knew it forward and backward.

Then, at a date two weeks later, I signed up for the optics exam. At the beginning of the exam, Professor Birge said, "Didn't I understand that people were supposed to study a whole semester for these exams?" Did I think I could take an exam every two weeks? Something inspired me, and I told this story: "Well, let me tell you this happening and then you'll understand my viewpoint. We had a sample that had to be measured for its fission rate with neutrons over at the cyclotron." I probably didn't say it was fission. I probably just said, "It was a sample that should count over closer to the cyclotron." This sample was particularly interesting to Professor Lawrence, and he had rushed in and said, "Have you counted this special sample yet?" I said, "Well, I just got it in the apparatus and I'm about to turn on the count switch, so let's see what happens."

I turned on the switch and about two seconds later there was a count, and Lawrence said, "Oh, bully!" and rushed out of the room. Didn't wait for anything more. I said, "I wanted to realize that in my opinion, one time interval really didn't define a rate very well." This was much appreciated. I didn't realize that there were some anti-Lawrence types on my examining committee [laughs].

Campus Feeling about Lawrence

Hale: Who were the anti-Lawrence types?

Chamberlain: Well, let's see. Who was there? Papa Birge. My story may have gone around the department because I heard a few ramifications of this little story come back to me [laughs] a few weeks later, from various people that enjoyed it. I flunked the optics exam, which was all right, and they suggested that I learn some geometrical optics besides the wave optics. That sort of terminated my attempt to get a quick leg up on the Ph.D. It's just as well that I didn't because I got a chance to stay a graduate student longer, and it was very important.

Hale: Was Loeb on that committee?

Chamberlain: Yes, I think Loeb was on that committee. Loeb was anti-Radiation Lab, and Brode was anti-Radiation Lab, for sure. Whether either of them were on that committee, I'm having a little trouble remembering. I think Loeb was, and probably Brode wasn't. But either of them would have been good fertile ground for this remark [laughs].

Hale: Do you have any idea why they were anti-Rad Lab? Anti-Lawrence?

Chamberlain: Oh, well, not in detail, but here was this cozy little physics department, and the tail was starting to wag the dog, you know. Lawrence was coming along and all the money was going to Lawrence, and all the ballyhoo was going to Lawrence. Lawrence was being consulted like a backup department chairman on a lot of decisions, and they resented his extreme success and his power and his domination of other things in the department. I mean, it was just as natural as could be. You can imagine this growing up.

See, the Radiation Lab had a style that was so different from the physics department's more scholarly academic ivory tower style. Lawrence was out trying to convince the army to give support and the Research Corporation to give support. He was very expressive about his hopes, which weren't very well founded scientifically in many cases. I think Lawrence was more of a promoter than a physicist in a way. What Lawrence brought was enthusiasm, not unusually acute judgment.

The Cyclotron

Chamberlain: I wish I could say I knew this first-hand, but from the stories I understood that the cyclotron only worked when Lawrence went out of town to Lake Tahoe for a couple of weeks. Then Stanley Livingston, who was his second graduate student on the problem, tore the cyclotron all to pieces and put in the electrodes the way he thought they should be and the cyclotron worked in Lawrence's absence. But that didn't mean that the cyclotron wasn't due to Lawrence.

The thing was Lawrence had already burned up one graduate student on that cyclotron and couldn't make it go. Then Stanley Livingston was the second, and if Livingston hadn't made it, then the next graduate student would have because Lawrence was determined he was going to make the cyclotron go. He didn't understand why it should go in full detail, but he understood enough to convince himself that something ought to be done there. And he was right.

Hale: On what are you basing that opinion now? Because you're talking obviously a much earlier period.

Chamberlain: Well, I suppose I picked it up partly from Segrè and--I don't really remember where I've gotten some of these ideas.

Hale: He came late, though. See, he was here in '38, wasn't he?

Chamberlain: That's right, Segrè wasn't here during that period either. As I have to say, I'm not sure that my ideas about this were correct. They certainly seemed in keeping with my own direct information on Lawrence. For instance, Lawrence would come around the lab in the evening to find out how things were going.

Hale: This is during that period that we've been talking about?

Chamberlain: I'm thinking of a period after the war, after mid-1948, when I was back here doing work on the 184-inch cyclotron. He'd come around in the evening, but I learned that what I had to give him was not anything very sophisticated about what we were doing. I was telling Lawrence about the mesons or the glue that holds the nucleus together, and things like this--very simple-minded notions--and Lawrence wasn't bucking at all because he wasn't very sophisticated in the nuclear physics. He was an enthusiast who liked to see the machines go. We all knew back in the time before I went to Los Alamos that when Lawrence came to the control desk for the

60-inch cyclotron that he'd turn all the knobs to the right and pretty soon there'd be lots of sparks, and somebody had to go replace something. Lawrence would go off and do something else, and things would go back to normal.

Hale: Sure. I'm afraid I have heard that sort of story many times now. Helmholz mentioned that he often did manage to get out twice as much current. But then he certainly did have a--stand a great chance of blowing fuses and filaments.

Chamberlain: Well, I'm sure Carl Helmholz does know more about that than I, but my impressions were that Lawrence was pretty clumsy at the controls [laughs].

Hale: Well, I say, I get the sense that he might have become more remote from the day-to-day, hands-on approach. He didn't quite know what each knob did.

Chamberlain: I don't know about that. I was around enough to hear the stories all right. We'd even hear, "They're repairing something or other. Lawrence has been around."

Hale: That was a general joke.

Chamberlain: Oh, yes, yes.

Hale: So how many of the other people, the old hands in the Rad Lab did you meet during that period?

Chamberlain: Well, I met Martin Kamen, but it was pretty peripheral. I met Joe Weinberg, also David Fox.

Hale: They weren't particularly old hands, though.

Chamberlain: No, actually, they weren't such old hands, that's right. They were more like fellow students.

Hale: Now, I'm thinking of the people like William Brobeck, for example, or Don Cooksey.

Chamberlain: Don Cooksey I had certainly met. Brobeck, I don't think I was conscious of at the time. I mean, I don't think I really had met him. Later I did, when I got back after '48. I can't remember Thornton from that period, honestly; but I probably met him to some extent.

Hale: He was only back that year. He came back the very beginning of '42, and then he was gone in '43 to Oak Ridge. So you would have only overlapped for a year at that time.

Chamberlain: That's right. I had forgotten that.

Hale: Frank Oppenheimer?

Chamberlain: I certainly didn't know Frank very well during that period.

Hale: Helmholz?

Chamberlain: I can't remember really knowing Helmholz until later.

Hale: Lofgren?

Chamberlain: I was completely unaware of Lofgren.

Hale: Because he was relatively--you know, a newcomer, too. It does seem that there was a very sharp split between what you were doing and the electromagnetic separation process.

Chamberlain: Yes, I think there was. I think it was a sharp--I mean, Segrè was working on this spontaneous fission problem, and we really weren't directly connected with the electromagnetic separation process.

Hale: I assume that there was a definite reason to keep people apart; sort of the right hand doesn't know what the left hand was doing. What was security like at that time? Come much into contact with the security people?

Chamberlain: There weren't any security people that I was aware of. There must have been some, but I didn't know who they were. It was handled awfully informally. We were told what was secret and what wasn't, but it was clear that people sort of weren't used to it because violations of these rules were fairly common. At least people knew things that you wouldn't have expected them to know, such as Miss Wu about the uranium isotopes, and somebody or other told me about the nuclear reactor having succeeded. I believe it was also somebody that shouldn't have known. But as people got used to the security and the rules became more formalized--in later years--the rules were adhered to as a matter of course. In recent times it's been a long time since somebody told me something that they weren't supposed to tell me, according to the rules, a very long time. Decades, I think. Because people now obey the rules as a matter of course.

Early Funding and Hiring Issues at the Radiation Lab

[Interview 2: June 21, 1976]

Hale: I've noticed that quite a lot of people at the Rad Lab seem to come from what Lawrence thought of as fine family background. Lawrence seemed to me a little hung up with that. I wondered why that was. If you came to the laboratory, if you were independent, had fellowships or had your own money you were given opportunities. Other people who were perfectly well qualified didn't do so well; either weren't accepted by the laboratory or somehow had to be diverted to less interesting things to support themselves when they got here. Do you have any ideas on that?

Chamberlain: I don't see that there was any tangible sign of anything that I would call bigotry on Lawrence's part. In fact, he was quite a champion on international cooperation, and he supported people like Sagani from Japan very, very strongly at the lab. If there was a tendency for the physicists to come out of fairly well-to-do families or middle-class families, I think that's quite possible. It's probably based on the fact that money was indeed short. It's hard for us to realize that the conditions under which the Radiation Lab got started involved no public support for scientific research as such. Now, as I understand it, Lawrence had a small grant or something from the Research Corporation. Other people know much more about this than I. But it was minuscule by present-day standards.

I think it's true that Lawrence paid very low salaries where he was responsible for setting the salaries; had to kind of love physics and be determined to be a physicist in order to stay in the business. I don't think people made much money as physicists. Beyond that, I suppose it's just the usual business that most professors come from middle-class families, don't they? At least I always supposed so, because those are the families that tend to have more intellectual traditions. But I think it was a natural development at that time. There were no affirmative action programs that I was aware of.

Hale: All right. At the time when you first came, obviously, you were very quickly into war work, but did the Rad Lab seem a smug sort of place in any way? They didn't really rely on anywhere else, that it was self-sufficient?

Chamberlain: I don't know. I had awfully little experience with the other institutions. I remember one time I made some remark to the effect that one had to keep up somehow with reading the literature, and Lawrence replied, "Well, isn't it really more important to make history than to read history?" [laughter]

Hale: Which implied that he was making history or Rad Lab was making history?

Chamberlain: Yes, I think the Radiation Lab was in his view making history.

Hale: The attitude was that, no, we were it.

Chamberlain: I think there was a period when the Radiation Lab and people from the Radiation Lab were rather preeminent in accelerators. Certainly McMillan came up with the phase stability not while he was in the Radiation Lab here in Berkeley, but he's a person that we all associate with the Radiation Lab before and after that time. I'm not sure when Luis Alvarez came up with his form of linear accelerator structure, but I associate it with something close to the war years.

Hale: It was immediately after the war, really.

Chamberlain: That's when I was familiar with it, yes.

Hale: I haven't thought this subject through very carefully, but I suppose the one accelerator principle that's very, very important, that was discovered elsewhere, was the alternating gradient focusing. It originated for the real world at Brookhaven National Laboratory, although it was apparently suggested by--

Chamberlain: Chrystofoulos.

Hale: Who was ignored for a long time.

Chamberlain: Yes. I don't think Chrystofoulos wrote to me, and I don't know whether I could have unearthed anything from what he wrote. Probably I would have missed it just as the other people did.

Hale: Oh, but he eventually did get the credit for that, but that of course is a good bit later, after the war.

Chamberlain: Well, let's see, strong focusing was certainly used in our proton experiment in 1955. We must have known about it a few years before, I suppose. I would guess that it was originated somewhere around 1950 to 1952. But you could look it up, of course.

Hale: But offhand that's the only thing that you can think of that really did come from outside the lab, until the later period, when, of course, other laboratories became more important. Do you think that they weren't looking outside very much, and possibly could have done even better than they did? Because there was the famous examples of the missed discoveries of the earlier years.

Chamberlain: Yes, missing the discovering of artificial radioactivity, quite so.

Hale: Right. I think that that might have been to do with not really keeping up with what was going on outside.

Chamberlain: Well, I don't know of anything along the accelerator line that would be illustrative of that, up until the strong focusing.

Hale: All right, I wanted to see whether your memory was jogged during our last session about your fellow students.

More about Chamberlain's Courses, Professors, and War Work

Hale: In general, during that time you were there at the lab, who most impressed you?

Chamberlain: Of course, I guess Segrè really stood out in my experience then. Took a course from Victor Lenzen, and most people were complaining that the course was a little too much like the course he had taught the year before and was a little too repetitive. But I found that it helped me enormously to have him be repetitive because I think I finally learned the material.

Hale: What was it he was teaching?

Chamberlain: Well, it was calculus of variations, which I never found very easy, and as applied to mechanics, Legendre's equations and Hamilton's principle, the highbrow mechanics. I think it helped me to have a teacher that didn't really advance too fast, but went over the material several times from slightly different but rather similar points of view. I was grateful for the course, even though it wasn't one of the courses that created a splash. Oppenheimer's course in quantum mechanics was recognized as, ah, more original and new and considered very good.

Hale: Of course, Lenzen, now, has got a reputation for his interest in philosophy in relationship to physics. Did you get any of that off him at the time?

Chamberlain: I had no contact with that at all, really. Nor was I particularly interested in it, really.

Hale: All right.

Around that time, in fact, just before the war, there was a lot of trouble to do with unions. Obviously, Oppenheimer was involved. The graduate teaching assistants were sort of sought after to join the AFT, and things like that. There was the FAECT. I still don't know what that completely stands for.

Chamberlain: I can remember some of those, but vaguely.

Hale: Were you ever approached to join any of those?

Chamberlain: Oh, I'm sure I must have been. I don't believe I joined any unions at that time. My attitude was that I was an individual who was going to work out his relationship with the institution on an individual basis and the union didn't seem appropriate to me.

Hale: Did you know much about what other people were doing? For example, Lofgren was in the AFT at one point.

Chamberlain: I don't think I was aware of very much. I think I was approached by individuals who wanted me to join the union. I don't think I went to any union meetings.

Hale: Was it much of a subject around the lab, apart from Lawrence telling everybody, "Don't join the unions because you're going to be working on war work"?

Chamberlain: Had awfully little to do with it. I'm aware that there was unionization activity going on at that time, but I couldn't tell you whether I was really aware of it at that time or whether I just heard about it later.

Hale: I see, because there is a good bit of documentation on that stuff, and obviously it came out a lot in the Oppenheimer case, people being dropped from the Rad Lab.

Ed McMillan had left for MIT, I think, probably by the time you arrived. However, he and Abelson had already discovered neptunium, and sometime during 1941 Seaborg was continuing on that work.

Chamberlain: With Art Wahl and Joe Kennedy. Certainly work on that plutonium formation experiment.

Hale: So they continued essentially Ed McMillan's work, and I guess there was communication back and forth between Seaborg and McMillan. They eventually sort of established what you would be doing. What was the story behind that communication, as far as you know? Was it between Seaborg and McMillan, and how did Seaborg get hold of the idea originally?

Chamberlain: Oh, you're way beyond what I have any contact with. I was a helper in the sense that I was doing some of the counting--measuring the alpha radioactivity of samples that somebody else electroplated, maybe on a piece of platinum. I also measured the fission rates in a neutron flux around the cyclotron and at various times, spontaneous fission rates with the samples removed from any neutron source. But what sort of communications they were having was completely unknown to me. I was really too young, scientifically, to be in on any of those decisions. In fact, you're talking about a period when I didn't even know what the project was all about.

Hale: So you wouldn't have got to know Seaborg, then? That he was working in the same general area, you know?

Chamberlain: No.

Hale: How about Wahl? Did you get to know Wahl, who was a student?

Chamberlain: Yes, I got to know Art Wahl better--Art Wahl and Emilio Segrè and Joe Kennedy. Actually, people I knew best were Joe Kennedy and Emilio Segrè. They were really in charge of

the work. Then Art Wahl and Jack Gofman a little bit less well, but I also had a fair amount of contact with them. I think those were the people. Cornelius Tobias was around but not working on our project. I have a vague suspicion that he probably didn't have a clearance at that time to work on our project, but I don't know.

Hale: I think you've mentioned Segrè's opinion about Lawrence, briefly, before. Did you know what his opinion was of Seaborg, for example?

Chamberlain: You're asking questions about relationships between people that still have to have continuing relationships, and I really don't feel like getting into that area.

More about Segrè

Hale: You mentioned that Segrè probably was the person that you remember most from that period.

Chamberlain: Yes.

Hale: Could you tell me something about how your relationship with him developed, because obviously it was going to be very important for you later.

Chamberlain: Yes. The first contact that I had, I audited or signed up for a course that Segrè gave in spectroscopy. That's probably in the fall of 1941 because I know that I had at least sat in his classes to some extent before I was assigned to be his helper by Lawrence about the next to the last day of 1941. So it must have been in the fall semester. It was a course that was by no means required for graduate students. Most of the other students were older than I, at least scientifically, in that they were more advanced graduate students in their training.

I rather liked listening to Segrè, although most of the other students thought that he was a very poor lecturer. This was based on the fact that he didn't have his lecture material prepared letter perfectly and well organized. But I liked it because I could find out how he thought when he got stuck, as he very frequently did. I listened as he figured. He sort of wiggled out of the box he created for himself by forgetting the crucial steps. I felt that I was learning from Segrè extremely well, and I still believe this

is true to some extent, that Segrè is an excellent teacher for the better students who were interested in how he thinks about physics. He thinks about physics in very significant ways and important ways and a little bit differently from many other physicists.

Well, there was a situation that arose in class when he was talking about luminescence excited by X-rays or fluorescence excited by X-rays, or something of that sort. I put my hand up and said, "Well, maybe that explains why it is that a beam of X-rays, if it's shown on a patient's retina, causes a visual sensation and in fact you can recognize the shape of things if you cast a proper shadow on the retina." Segrè thought about this a moment and he said, "Have you seen that with your own eyes?" And I said, "Yes." He said, "I don't believe you." And he turned around and went back to the blackboard. I didn't know what he was doing. I felt quite upset, you know. I felt affronted that he would dismiss my testimony, and so quickly.

Hale: You had seen this in connection with your father, right?

Chamberlain: Yes, that's right, exactly.

Hale: And what did he say after that? I mean, did you go up to him?

Chamberlain: A few months later, when I was working with Segrè and Joe Kennedy on this project, I discussed this again and said I had been upset about it. Joe said, "Well, that would be just like Emilio. He doesn't want to spend any more time on it. He just wants to cut off the discussion abruptly, so he just says, 'I don't believe you' and turns and changes the subject."

Hale: How about day-to-day in the laboratory, for example?

Chamberlain: Well, he was quite a scientist, and he responded more to me as he grew to respect me as a scientist or potential scientist. I think we were all afraid of Emilio. I certainly was, and I don't think I was the only one. I think most everybody was. I don't quite know why, but he tends to be abrupt, a little gruff, and stated things very directly and not always diplomatically.

Hale: Would that include Lawrence, for example, you think, who would be afraid of him?

Chamberlain: No, I think Lawrence felt very much on top of the situation as sort of running the Radiation lab. I don't think that includes Lawrence. Although Lawrence probably had a healthy respect for Segrè. I think Segrè somehow served notice on Lawrence, without doing so quite directly, that there were circumstances under which Lawrence could take some advantage of Segrè, but they weren't permanent.

Hale: Because after all, Segrè owed his position to Lawrence, really.

Chamberlain: Yes, I think so. I think it was Lawrence that invited Segrè to the University of California and so forth, I would imagine.

Hale: That makes for a very difficult situation, doesn't it?

Chamberlain: Later on at Los Alamos, Segrè was very instrumental in teaching me physics, and other people in the group also. We'd go for walks and hikes and fishing trips and whatnot. Many times we would be discussing physics in one form or another. I can remember learning about the Einstein A and B coefficients while walking through the Valle Grande at Los Alamos. It was sort of an enormous meadow with a nice creek wandering through it, where we'd fish.

I wonder in retrospect how we learned without a blackboard in that case, but I learned. I got it straight. It was so straight in my mind that as soon as I got to a piece of paper, I could write it all down and work out all the details, just based on what we had talked about along the trail. That was an excellent period because Segrè somehow took pleasure in teaching us physics, and we certainly took pleasure in learning it. We were getting things that were almost indispensable to our later development when we returned to graduate school. There's nothing like having a one-to-one relationship with a good teacher, especially under circumstances where he's kind of enjoying teaching.

Hale: Sounds like the way education should be.

Chamberlain: We used to compete as fishermen, and Segrè's really a little better fisherman than I, but he'd always bring numerically more fish than I, always. I don't think there was any exception to that. I got a few of the bigger ones, so there were days when I could weigh in a little heavier than he did. He usually fished with bait. I'd usually fish with a fly. But we'd vary.

Hale: Did he talk to you about his view of physics, the way it was done in the States, especially in the Rad Lab, you know, and talk about his European background?

Chamberlain: No, not really. There was a spirit in Segrè's physics; he was more of an individual than I suppose most of the people at the Radiation Lab. Segrè's original research tool was an ionization chamber with an electrometer amplifier, which he had built or even had built for him by some expert from Caltech. It could have been Strong that helped him build this. The vacuum tube, I remember, was called FP54, and it was a vacuum tube with, I guess, an unusually high vacuum in it. It was operated under circumstances where you tried to keep any residual gas in the vacuum tube from ionizing it, so it was run at very low voltages, four volts or something like that. It was intended to have a very high-impedance input and a fairly high-impedance output that would actuate a galvanometer hung on the wall.

Segrè did a great many experiments with just this FP54, a good quantitative ionization chamber. He measured lifetimes of radioactivities, and he measured absorption coefficients of radioactivities. The ionization chamber itself was filled with compressed gas, some heavy gas. It could have been argon or even methyl-bromine. I never filled that ionization chamber; it never leaked during my experience, so we just left it alone, and I never saw the inside of it, to my knowledge. It was always closed, never gave trouble, so there's no reason to open it.

I think he got help from the man who originated that circuit, the FP54. That FP54 was sort of a holy instrument. If that instrument got broken or in any way destroyed, it would have set the work back a long way. Everything was based on this simple instrument, and that was sort of characteristic of Segrè's style. He looked for something that could be done by one person or with very few helpers, sometimes none.

Hale: Do you think that's maybe necessitated by the nature of the work?

Chamberlain: No, no, that's his personal style. I think it was clear at every stage that Segrè preferred smaller experiments that could be done by a smaller group of people, rather than big collaborative enterprises. Well, he too needed helpers; he needed me and maybe the help of a chemist at times, so it was cooperative, too.

There was an experiment--I suppose it was in the spring of '43. We were trying to measure fission cross-sections for intermediate velocity neutrons. It was in Berkeley, and we were doing the experiment in the building where the 60-inch was housed, in Donner Laboratory. This consisted of making a large amount of radio-sodium in the 60-inch cyclotron, in the order of two curies, I think. We dissolved the radio-sodium in heavy water. This heavy water had come to the Radiation Lab from all over the United States. I think Urey contributed a few cc; some of it came by diplomatic messenger from the East Coast. We had assembled something on the order of, I would guess, 100 cc. It had come in small vials from here and there. Somebody had scoured the nation for these heavy water samples.

I really used the whole U.S. supply of heavy water. I was sort of in charge of this part of the experiment, and I had done most of the essential practice runs. I don't remember why we had so little supervision from Segrè or why I had so little supervision from Segrè at that point. He may have been away on a trip or something. But we got this thing filled with heavy water with sodium dissolved in it. It glowed like a moon if you turned out the lights. I guess it was the radiation from beta rays in the water. Because it was highly radioactive, I had it on the end of a nine-foot pole. I think we even made it out of one of those poles you open high windows with.

We had done part of the experiment, it was evening, and I thought I was supporting the weight of this thing on this long pole. But when I tried to move it from one place to another, it slipped down. Apparently I wasn't supporting enough weight. Unknown to me, it was hooked on something, so I thought I was holding the weight, but I wasn't. Down it went and broke. The U.S. supply of heavy water and two curies of radio-sodium spilled on the floor of the 60-inch cyclotron. I hoped it had decayed to one curie by that time. It was a fairly short life, like twelve hours. I cleared out of there in a hurry and phoned Segrè.

He came down and I thought he was going to go all to pieces in anger over this, but he was really very nice about it. A few attempts were made to clean up, to see if we could save any of the heavy water, but it was hopeless. It exchanges with the atmosphere so quickly. It evaporates and at the same time condenses a little of the atmospheric water. When it's exposed to an open surface, it loses its heavy-water characteristics.

Then there was a problem of whether I had been exposed to much radioactivity as a result of spilling this. So I got a week's vacation out of it, in which I was supposed to recover from it. That's kind of silly, but it was handy at the time. I didn't mind a week's vacation. Segrè was very nice about it, but I thought I'd be in the doghouse forever more. But, you know, that same week the Stewart Oxygen Company came into production with a new heavy-water plant that produced in one week about what I spilled on the floor. I was saved from disgrace by the technology of the day [laughter].

Hale: Well, I think this is probably a bit better than the use that Latimer put the first sample he got from Urey.

Chamberlain: What did he do with it?

Hale: I think he fed it to a mouse.

Chamberlain: I'm a little in the dark as to how poisonous heavy water is to animals and plants. I've heard evidence that it is a little bit poisonous, but I've never known whether this was good evidence. It's just sort of at the borderline. Obviously, the stuff isn't a terrible poison because the water we drink has one part in ten thousand or so of heavy water in it. I don't know whether something that was 10 percent heavy water would be poisonous to drink; I wouldn't drink it myself because I'd just not be quite sure.

Usually all that's important in most cases, we think, is that the mass of the atom is a little different. In most reactions that doesn't make a terrible lot of difference. You could feed me something with carbon 13 in it and I wouldn't worry too much about the consequences. But with hydrogen, it's a factor of two change in weight, and it could have a rather significant effect on what enzymes do and don't do. It's one of the biggest changes which could be associated with an isotopic substitution. As it turned out, Segrè decided that we had done enough of the counting before that accident, and the results looked consistent enough that we should just not repeat the experiment. I've never been clear in my mind whether those results were particularly significant. I think they were so overshadowed by later results within a few years, nobody ever went back to look [laughs]. I think those results were considered rough, tentative answers and were soon improved upon at Los Alamos, among other places.

The most important thing about the story, to my mind, had to do with Segrè's personality. When the chips were down, he was very forgiving.

Hale: Is that a measure of his character in general? You know, would he be bossy or anything like that?

Chamberlain: Oh, he tends to be very bossy and he tends to be very critical. If he's fully aware that somebody may be feeling very sensitive about something, I think he lets up. Segrè is actually quite sensitive and quite intuitive about how other people feel, but he doesn't ordinarily limit himself too much, and he tends to be gruff and direct. If he thinks you've done something stupid, he'll say so. People tend to be at least a little guarded in his presence. But on this occasion, when the chips were down, he was not giving me a rough time.

Samuel Ruben, Martin Kamen, and Other Researchers

Hale: I didn't ask about Ruben. Did you know Ruben?

Chamberlain: I certain knew Martin Kamen a little bit. I must have known Ruben, but I can't really remember him right now.

Hale: Well, he was working with Latimer. Kamen's idea is that Latimer sort of wore him out, had him working so hard that he just wore him out practically.

Chamberlain: Who was the chemist that was killed in the laboratory about that time?

Hale: That was Ruben.

Chamberlain: By phosgene or something.

Hale: That's right, which Kamen thinks was probably a result of his being so overwrought, tired, overworked. Did you come into contact with Latimer at all?

Chamberlain: No, not really. I knew him when I saw him, but I had no real contact with him. I didn't have any very close relationship with Kamen, either. I knew he played a violin or a viola or something like that, and I saw him at the 60-inch. He was a quiet fellow who seemed friendly, but I just didn't have a lot of contact with him.

Hale: You didn't know anything about his security troubles at the time?

Chamberlain: No.

Meeting and Marrying Babette

Hale: It must have been about this time that you met your wife?

Chamberlain: Yes. I started living at International House when I first came to Berkeley. I lived in International House for three semesters, and then moved into an apartment with three other students in Berkeley. I probably met my wife somewhere near January '42 on a bicycling outing from I-House.

Hale: You just went out as a group and she was in the group?

Chamberlain: Yes, exactly.

Hale: Could you tell me something about her background?

Chamberlain: She had been raised in southern California. Her father was in and out of the family life. Her mother mostly supported the family, sometimes retail selling and often manufacturing artistic objects for the home--anything from shell-decorated cases for jewelry to shell-decorated mirrors. There were a lot of shells in the whole thing. She used the firm name of Oceana.

My wife, Babette, had dropped out of college for one year to help her mother in the family business. Her mother had done a number of things to support herself and her two daughters. She had taught elocution--she had an elocution school--and she arranged programs for some of the women's clubs in the Los Angeles area. They had speakers and maybe a mime troupe or something like that. There was something that might have been called the Saturday Morning Breakfast Club or something of this sort that was considered very prominent in a social sense. I guess Babette's mother did a program for that group at one time, which she was very proud of.

Hale: Where was it your wife went to school? Because she dropped out of college?

Chamberlain: She had done some of her undergraduate work at UCLA. She had been out for a year along the way somewhere, but she finished with a bachelor's degree at Cal. Then she took up work as an expediter at the Richmond Shipyards. We got married just before going to Los Alamos in the middle of '43.

Hale: Did that seem like a very exciting prospect to her, to go to Los Alamos?

Chamberlain: Not really. I think the place was too isolated for her taste, but any just-married situation is bound to have exciting prospects [laughs]. We've now been separated for about five years. Some of that's worn off.

Hale: Oh, I didn't know that.

Chamberlain: We're technically still married. The divorce is in process, but it's not final yet, at the moment.

Hale: I also didn't ask you about your sister. I know that she got a Ph.D. in physics.

Chamberlain's Sister Ann Marries Bob Birge

Chamberlain: That's right. I remember Elfrieda Segrè, Emilio's wife, said that my sister was going to marry Bob Birge before they had ever met. She knew that they were both going to Harvard Graduate School [laughs].

Hale: You've made that as a joke.

Chamberlain: But that's the way it turned out. Bob Birge was the son of the physics department chairman that had taken the responsibility for bringing me into the department in '48. I'm sure Segrè was my local champion; but, I mean, he was referred to as department chairman.

Hale: Did your sister go to Harvard after the war?

Chamberlain: I was in the class of '41 at Dartmouth, and she must have been in approximately the class of '45 at Vassar. Bob Birge was at Los Alamos for a part of the war and in army uniform. I had seen him there but wasn't all that close to him. It must have been right after the war that they both went to Harvard Graduate School.

Hale: He met her just by chance?

Chamberlain: Oh, I think just by chance. But it's not such a big group of graduate students that go to Harvard in a given year. As for my sister being a physicist, I guess it's some combination of her wanting to keep up with me. She and I had a very competitive relationship. I guess I was teaching her, but I was really not doing it in the best possible spirit. It was a funny relationship in which she tried to show she was a good physicist, too, and I sort of put her down.

Hale: Was it just you two?

Chamberlain: There were just two, that's right. It's my belief that it was a sort of a combination of her following me in this physics direction and her following my father's ideas about physics as a good thing to do. She took a lot of physics at Vassar, and after she had taken about all the physics there was at Vassar, she turned to a music major. She finally got her degree in music at Vassar. That was quite something to get a music major into Harvard as a physics graduate student. But she managed.

Hale: Did she become a practicing physicist?

Chamberlain: She found that some doors were closed to her in Berkeley by the anti-nepotism rules. I was really in doubt whether they ought to settle down in Berkeley after they got their degrees because I think I was too close with the family relationship with Bob, and Bob's father was too close to make a normal physics department position here a real possibility. Since then they've amended the anti-nepotism rules, but at the time they were very strong for the ordinary people. They weren't very strong if people were sufficiently powerful. I think John Lawrence was brought here by Ernest Lawrence, and as far as I'm concerned, the anti-nepotism rules should have applied and didn't.

My sister went into medical physics, where the department was a little different, so she didn't have a conflict of that kind. She worked with or for Cornelius Tobias for a while and then with other people in medical physics. She did some moderately interesting experiments, or took part in some experiments, on the effects of radiation on yeast organisms. I don't know that much about medical physics, but they're probably fairly classical-type experiments now.

III LOS ALAMOS AND WORK ON THE ATOMIC BOMB

Move to Los Alamos

Hale: Let's move on to talk a bit more about your time at Los Alamos. When did you go there?

Chamberlain: It was very close to the first of July of 1943.

Hale: It had been really established in about April, so you missed those very beginning conferences?

Chamberlain: Yes, I was told when I got there it was good that I had arrived. I was really the last person to complete the technical staff, but it was only about three weeks before another bus-load of technical staff arrived. They kept finding reasons they needed more help and more help and more help at Los Alamos. There was an original idea that the technical staff would be about a hundred people, and I was about the hundredth person to arrive.

Hale: Yes, I remember that figure of one hundred.

Chamberlain: It was thought to be very small, but it kept growing.

Hale: Had Segrè gone there before?

Chamberlain: A few weeks before I had. We weren't actually the last to arrive. I mean, heavens, we lived for a while in the Brodes' apartment before the Brodes got there, and before our house was ready for us. I think we lived in the same building with Cyril Smith and Edward Teller. Who else? Also possibly the Bachers.

Hale: What were these houses?

Chamberlain: This first building in Los Alamos was a four-plex. It was two apartments upstairs and two apartments downstairs, sort of that typical collaborate wartime construction. We still have one of those buildings up here at the Rad Lab: Building 29. Same construction.

Hale: Segrè had said to you that he wanted you along. Was that the reason why you went?

Chamberlain: Yes. I must have been asked at some point, but it was sort of taken for granted that the whole bunch of us that had been helping Segrè would go along. George Farwell had joined us a few months before. George had been assigned originally to work with some of the more plumbing-like aspects of the Calutrons, and he had requested a transfer to work with Segrè's group. I think he realized that he would learn more physics in that capacity. He maybe joined us three or four months before we went to Los Alamos. But I still thought of him as a new member in the group when we were in Los Alamos. So there was Lindenberg, Wiegand, Farwell, myself.

Hale: And that was called the Radioactivity Group?

Chamberlain: I don't know what it was called [laughs]. If it had such a name, it was probably given such a name later on. Might have gotten the name when we first got to Los Alamos maybe, but I wasn't really conscious of the name. We worked with Segrè. Bob Wilson had a lot of supporting staff working with a Van de Graaff. John Manley probably was working with a lower-energy Cockroft-Walton or something like that. And Segrè. I thought of these as the experimental groups at Los Alamos in the early days.

Early Lack of Confidentiality

Hale: Were they well separated, or did you communicate, in groups?

Chamberlain: That's a hard question to answer because it seemed it was part and part. There was no lack of communication between the group leaders. I mean, Segrè and Williams and Manley and Wilson met frequently, with and without Oppenheimer. Yet I didn't feel very close to any of the other physicists in the other groups, particularly. So I think we could have had a lot more contact with other people, and it would have been a good idea.

I was very doubtful of my own position and my own ability and so forth at the time, and I was very tentative about exploring the situation. I was mostly responding to what Segrè wanted to get done, and I wasn't really spending much time conversing with other people. Fortunately, we did have these once-a-week meetings at Los Alamos. One of the best things that Oppenheimer did was to insist that this was a small enough group at Los Alamos that we should have full discussions of our technical problems. So the barriers between one group and another in terms of secrecy were, I believe, absent completely, at least I thought. I can't remember anything that was in the way, and we had very interesting discussions at these once-a-week meetings.

Chamberlain's Almost-Contribution

Chamberlain: I almost succeeded in helping the effort along a little because at these once-a-week meetings--I can't remember which night of the week it was, but it was a standard night, Monday or Tuesday--we had two lectures about the elements of explosives, TNT and the like. I had heard that if you take a bar made of explosive and ignite it at one end, it has a speed of propagation which depends on the diameter of the bar; faster if it's a big diameter and slower if you make it small. Finally, it will quit propagating, just fail to explode. So I understood from this that the speed of propagation of the wave in an explosive depends on the curvature of the wave-front. In a large piece there was not much curvature of the wave-front and it propagated fast. In the small piece the curvature of the wave-front got considerable, and it propagated more slowly.

They were trying to make explosive lenses, and they were having trouble getting the lenses to focus the way they wanted. It occurred to me that maybe they were forgetting that the velocity of propagation depended on the curvature of the front. But I thought this can't be the trouble because, I mean, these men are more intelligent than that and they've just given these lectures to us on this subject, so it can hardly be that that's not understood.

But it turned out that that was the case. I didn't carry my idea because I was turned off about talking. You know, it can't be that--what they need can't be that simple; it must be something else. But I learned a few--maybe four, five, six weeks later--that indeed that was the trouble.

Hale: You never spoke up?

Chamberlain: I never spoke up. It's the origin of the theory that I have that at least some people have to miss making an invention or a discovery before they can make a discovery. You have to somehow get the feel of what's interesting when you first run across something like that. In retrospect, I should have carried the idea to enough people to find out whether it was dumb. Instead, I was thinking, "Oh, it's too unlikely that it's that simple, what they're hung up about." You have to be on guard against putting your own ideas down. It's better to speak up. At least discuss it with a few people and find out whether it makes sense to them.

Hale: The risk of feeling foolish.

Chamberlain: Sure. Well, Segrè tries very hard, I think, to teach that to people, at least by example. He doesn't mind making a fool of himself, and sometimes people's mouths drop open at my ignorance. But it's better to ask the question in physics and be thought a fool, in the hope that now and then you'll come up with a question that has a valuable answer.

Hale: Yes. I have a sense that there are quite a few people around the Rad Lab that don't operate in that mode, though.

Chamberlain: Most people have a background, I think, of being very worried about their image. It's natural that that would show up among physicists, even if it would pay them to be less worried about their image. But it's nice to have someone like Segrè who plays it out so well, and worries so little about his image.

Hale: He doesn't need to worry about his image, does he?

Chamberlain: That's right.

Hale: The explosives problem you were talking about, was that in connection with the detonator?

Chamberlain: Yes. You assemble the parts of an atomic bomb by using high explosives as a rule. That had to do with assembling the parts.

Hale: Alvarez was working on that, right?

Chamberlain: Probably, but I'm not sure what Alvarez was working on.

About Oppenheimer at Los Alamos

Hale: How did Oppenheimer strike you as an organizer for that--you know, for a project like that? Lawrence was the one who proposed him and backed him for that position.

Chamberlain: Well, he was certainly from Berkeley, so probably that's true, though I don't know. I think Oppenheimer did a very good job. There's no doubt about it at all. It was somewhat to my surprise because we generally thought of Oppenheimer as a rather impractical theorist. There are theorists who speak well to experimentalists, but Oppenheimer didn't seem to be one of them.

But when he came to managing that project, he really did awfully well. He communicated well with Segrè and all the other group leaders, and he managed to communicate well with the generals, General Groves and so forth. I'd say he did very well. I certainly couldn't find any fault that I know with Oppenheimer's management.

I felt that Oppenheimer did make one mistake of establishing a kind of a social elite at Los Alamos. It was a rather sharp division between those who were invited to Oppenheimer's parties and those who weren't invited to Oppenheimer's parties. I thought that it was a shame because it tended to introduce a kind of class feeling at Los Alamos which was unnecessary and rather undesirable. I'm sure as anything that it was inadvertent, but it had an unfortunate effect.

Hale: Did you yourself have much contact with him personally?

Chamberlain: Not very much. Sometime maybe in late '44 I was asked to go to one of the chemical companies--I think it was Monsanto in Ohio--and assess how they were doing. Their job was to produce a fairly massive amount of polonium, an alpha emitter. They were obviously lagging behind the original timing, and I was to go see how they were doing. I brought back what they thought was a very pessimistic report and then--oh, dear--when Oppenheimer heard my report he was just terribly disappointed. I seemed to be sort of caught in a crossfire because the people at the chemical company insisted they were doing much better than my report indicated. They almost did do better, but somehow they just about fulfilled my prediction. It turned out that my report was surprisingly accurate--by chance, I'm sure. But they

weren't used to dealing with radioactive samples, and it took them longer than they had expected.

In that case there was interaction with Oppenheimer directly. Oppenheimer sort of personally sent me off on a trip and personally got my report when I got back. Then there was this flurry of telephone calls back and forth after my trip, and the chemical company insisted that there was more that I hadn't seen or something. I don't know what it was.

Hale: Well, did he seem to trust what you said?

Chamberlain: Well, he trusted it enough to know that there was a problem. In fact, I think he sent somebody else a few months later, on the same kind of a trip to see how they were doing. I'm not sure that he trusted my report at the time, but I'm not sure I knew enough that he should trust me. It just happened that I came out with a good prediction. But it's awfully hard to go into some lab and find out how far along they are on something or other. You can ask the obvious questions to find out whether they at least know how to handle small samples of the same material.

Segrè and I could talk back and forth on the telephone. We had a personal code arranged so we could discuss what instruments they were using and so forth, over the telephone, which worked out very well. We understood each other quite well.

Segrè at Los Alamos

Hale: How was Segrè as a leader of such a group?

Chamberlain: Well, Segrè had a character fault which showed up at Los Alamos. He tended to get down on one person and would start blaming all sorts of things on that one person. There'd be somebody that would be in the doghouse for the best part of six months. I don't know why he did that. It wasn't a help because we'd get sort of burned up at him. There was a lot of tension at Los Alamos. We didn't have an easy way to shift jobs around. We wanted to contribute to this war effort, and there was a lot of tenseness, pressure to make progress quickly on this bomb program. As a matter of fact, at Los Alamos I had a repeated stomach upset about once every three weeks. I'd get a kind of diarrhea and nausea

and whatnot; it disappeared the week I left Los Alamos. Obviously, too much pressure for me to accept.

Hale: Could it have been something as simple as the water?

Chamberlain: Oh, I don't think so. You don't have trouble with the same organisms in the water for month upon month. This was a problem that lasted for more than a year, I'm sure. They were X-raying me and--I don't know--trying to find out what the hell was the matter with me. But the problem disappeared the week I left Los Alamos and never recurred, really.

Hale: So you really felt the pressure?

Chamberlain: Oh, I think so. Partly the pressure was brought by Segrè, and it was very hard to know how to react to it. He was down on Linnenberger for a while, and he was down on Farwell for a while, he was down on Clyde Wiegand for a while, he was down on me for a while--each for something like a six-month period.

I had worked extraordinarily hard for three days running, and Segrè walked in and said, "What have you been doing? Haven't you made any more progress than this? Don't you understand this is an important project? You've got to get on with it!" I didn't know what to tell him. I was repulsed by this. It wasn't helpful in getting me further involved; rather, I rigidly went back to reporting hour by hour what I was doing and working eight hours a day only. I was just sort of angry with the situation at one point. But these attitudes of Segrè's sort of wore off with time. I don't think he does the same thing in recent years at all, but it was a problem at Los Alamos.

Hale: Do you think it was just that he was so unused to being under pressure too?

Chamberlain: Well, I suppose it could be. I don't know.

Hale: Has it left any sort of permanent attitude on your mind?

Chamberlain: It's not been a problem since 1948, when we started working together again. It's not been a problem at all.

The Elite at Los Alamos

Hale: You mentioned about the elite were invited to Oppenheimer's parties. Who were those elite? Were those generally your group leaders or Spanish people and all?

Chamberlain: Well, they were the group leaders, that's true. But there were also some of the younger people. I believe the Frankels were invited to those parties. Stanley Frankel was probably the youngest of the physicists that I can remember going to those parties. I'd say very few people in my position as a kind of laboratory helper went. I had the feeling that if I had a couple of more years of graduate work or, certainly, if I had a Ph.D., I would have been invited to those parties.

Hale: Was he sort of a snob in some ways?

Chamberlain: Oh, I suppose. I don't know.

Hale: Were these his intellectual friends?

Chamberlain: Well, it was hard to pin it down. To some extent they were intellectual friends. I think the young theoretical physicists were there in larger numbers than the young experimental physicists. But, you know, they probably wanted to restrict the numbers of a reasonable size party. But they didn't vary the crowd enough. There was a big party at the Oppenheimers'--I don't know how often--every three or four months or something; but it was too much the same crowd that went each time. That caused a kind of social stratification, I thought.

Hale: But outside of that, amongst the plebeians, what was the social life like?

Chamberlain: We made most of our friends in the process of seeking rides to ski slopes or rides to places on a Sunday, just to get out. We didn't have a car at the time. We were able to get along without it; it made sense to me, mostly as an economic measure, but also there was the gas rationing. It was perfecting fitting to do without a car. We got to know the Lavatelli family quite well, and through them the Jorgensens. Jorgensen was from Nebraska and went back to Nebraska afterward, and Leo Lavatelli probably came along with Bob Wilson from the Harvard group, but went to Illinois later and went into mathematical and computer-oriented things. We got to know Joe Hershfelder a little bit. He

used to rent an airplane and keep up his pilot's license. A few times we took a flight with him, which was fun.

Security at Los Alamos

Hale: Wasn't that a bit scary for the security people?

Chamberlain: No, no problem. Well, this security thing was done in a kind of a spotty fashion. There was, on the one hand, the pretense that nobody in Santa Fe knew who was at Los Alamos. This was kind of silly because by the time I arrived, the people at the clothes cleaning establishment in Santa Fe knew that Oppenheimer was in charge and knew pretty well who else was there. I've forgotten how it would come up, but I think they enjoyed dropping these names to sort of befuddle us. Here we were, under instructions not to talk about other people that were there. Fermi was supposed to be under a code name. He was Farmer. That was not when we first got there, because Fermi came a bit later.

Enrico Fermi

Chamberlain: Maybe we'd been at Los Alamos four to six months, and I went to see Segrè, who was in our little shop room. We had a small room about a third the size of this room that had a drill press in it and maybe a hammer and a vise or something. I went to ask Segrè a question about did he want this assembled with sealing wax or something else. There was somebody else in the rom, but I didn't really notice who it was. He was kind of sitting in the corner, and I asked my question of Segrè, and as I turned to leave, Segrè said, "Oh, Chamberlain, I want you to meet Fermi." My mouth must have just dropped open because here I'd heard about the great Fermi and here was this person sort of sitting unobtrusively in the corner of the room. I was really not even conscious of his presence particularly because he was so unobtrusive. My surprise was extreme, and I managed to pull myself together enough to shake hands, and I was still shaken by this experience because I'd expected Fermi to be more an image of Lawrence.

Lawrence was a big-chested man who kind of led with his chest everywhere that he went. You sort of expected

everybody to notice him. Fermi was so much the opposite. I heard stories to the effect, later on, that Fermi had been a professor for a year or two at the University of Rome before the janitor understood that he was the professor because he wasn't at all a showoff. He didn't make a big thing out of himself. I learned later on, and in a much more subtle fashion, that Fermi had a lot of pride in his students, but it didn't show.

Hale: Would you say, then, that he really was a modest person? Or was he sort of like a reverse snob?

Chamberlain: Well, I don't know how to put it. I think Fermi understood that he was one of the most intelligent of people, but he studiously avoided showing that. I think if he was to be recognized, he'd prefer to be recognized for his intellectual accomplishments rather than for his personal attitude. For a long time, I didn't realize that Fermi had any personal pride in himself as a teacher. When he was a good teacher, he taught well. But you couldn't tell there was any personal pride involved. As I got to know him better, I remember he remarked one time about how there was a degree to which Oppenheimer students were very prominent. At the time, I think he thought Fermi students were less prominent. I think he was kind of puzzled by that. As a matter of fact, over the long haul, Fermi students were much more prominent than Oppenheimer's, but he probably couldn't tell that at the time. That was later on at the University of Chicago, say, in 1947, that Fermi was making these remarks about Oppenheimer students.

Hale: What in fact did Fermi have to do at the Los Alamos laboratory while you were there? Obviously, he was brought in for a special reason.

Chamberlain: No, he was kind of a consultant to all the groups. What he had first done was to get the power going in December of '42. I suppose it must have been December '43 or later that Fermi came to Los Alamos to stay. He'd had a few short visits, I'm sure. But one thing I learned from Segrè was that Fermi was it. Fermi was *the* best or among the very best of the then-living physicists, and it was clear implicitly that when Fermi spoke you had good reason to listen. So when I left Los Alamos finally, it wasn't a very hard decision to know that I wanted to go to the University of Chicago to study with Fermi, if possible. It was pure and simple that you went to Chicago because Fermi was there, and you hoped maybe you'd work with Fermi.

Hale: Yes. He had a lot of consulting input to different groups.

Chamberlain: Oh, yes. Segrè relied on him very much, and I think he was very valuable to the other groups. I didn't have a whole lot of contact with him. But there was one interesting thing. When I came back from this trip to Ohio, I came back by train from Chicago. There were airplanes in those days, of course, but I'd forgotten that the laboratory had a compartment signed up, one every day, on the Santa Fe Chief. I'm sure they canceled it some days, but there was enough laboratory traffic so it made it worthwhile.

Well, who should I find in this compartment with me but Fermi. Well, I just kept Fermi busy the whole trip back with my questions about various things in physics. I'd just stirred up a lot of things. One thing tended to lead to another, for about a day and a half. Fermi taught me physics. Great--it was a great trip!

Hale: Just you and he?

Chamberlain: That's the first time, just the two of us, I believe. I think he was traveling alone, as I remember, and I was certainly traveling alone.

Hale: That was after he already moved to Los Alamos full time?

Chamberlain: I'm not sure. It could have been because I didn't have to be introduced to him. I knew right away who he was, and I think he knew who I was. He must have already been at Los Alamos long enough so I had gotten to know him--a little bit, anyway. Met him at Segrè's house a few times.

Hale: And so you must have had a good chance to form an opinion of him on that journey.

Chamberlain: Oh, well, there was no need to form an opinion. I knew in advance. Fermi was Fermi.

Hale: And how did he treat you?

Chamberlain: Oh, he was very helpful. He answered my questions beautifully and led on to other things that went beyond my direct questions. I think it was one night and two days, the trip from Chicago to Lamey, where the train stopped. Lamey was nothing, a little house in the desert. Trains stopped if somebody wanted to get off.

Hale: Sounds to me as though you've had some of the plums fall into your lap.

Chamberlain: Yes, that's right. I probably had the obvious advantage that after that Fermi knew for sure exactly who I was, and it certainly couldn't hurt a bit.

Physics Education at Los Alamos

Hale: Do you think that the atmosphere at Los Alamos was more like a university, say, than it was like other places?

Chamberlain: Oh, yes, indeed so. I wasn't at the other places, such as Oak Ridge or Hanford or something like that, but I'm sure as anything that it was much more like a university. We learned much more physics at Los Alamos than we would have at the other alternatives where we might have contributed to the same project.

Hale: Yes. Was that just because of the closeness of everybody?

Chamberlain: Firstly, see, there were a lot of very acute minds there. Segrè, Teller, Bethe, Oppenheimer, Bob Wilson, Fermi, Feynman--I have a feeling that I'm not remembering all of the ones that I much respected. Well, Leonard Schiff was there.

Hale: Weisskopf was there sometimes.

Chamberlain: Yes, Weisskopf was there. That's a very good person to mention. Some of these people were sort of irrepressibly intellectual in their approach, and I think this included Bethe and Teller and Segrè and Fermi. At the end of the war period, you know, we had something we called Los Alamos University. During some of this period, there were a series of lectures in which Teller tried to show that you could make a hydrogen bomb and Fermi tried to show that you couldn't. Must have been very important. When I left Los Alamos, the situation looked more like you couldn't than you could build a hydrogen bomb. They called it the "Super" at that time.

Hale: Yes, that was always Teller's baby, wasn't it?

Chamberlain: Yes, that was always Teller's baby, and I think the way it became Teller's baby was that Teller must have gotten into some kind of fight. Anyway, it was obvious to everybody that Teller didn't get along with Bethe well, and Bethe was the head of the theoretical group. Bethe was doing the main line stuff. Teller, I think, was emotionally looking for some way to do a thing that he could feel involved with, but somehow allowed him to develop a separate project from what Bethe was working on. I think somehow he got an emotional attachment to the "Super" as a result of the friction developed between his personality and Bethe's and I guess also Oppenheimer's. It was a friction which I blamed completely on Teller. I didn't think that Bethe or Oppenheimer contributed to it.

Hale: I see. What was it about Teller that produced that friction?

Chamberlain: I don't even know. Teller thinks about physics problems in an unusual way, so he's very good to listen to and learn from. But I've no idea what the origins of the friction were. I think it was clear to everybody that the friction had developed very early on, and Teller got separated from the main theoretical group within a matter of two months or less; that's my impression.

It seemed absurd at the time, you know, because here we were, all working on the fission bomb and once it was obvious that a fission bomb was going to be made before a "Super," then spending effort at Los Alamos on the "Super" seemed like foolishness. And it still does. It just seemed as though it had better be forgotten until after the war, or until such time as the first model had worked, and then you started to worry about the "Super."

I guess I looked on it as a form of Teller's irresponsibility because he probably was making contributions, suggestions to the main project, but he wasn't apparent most of the time. I certainly had the opinion that if he wanted to help, he should get on with the main project and not fuss with this "Super."

Hale: Do you think that he might even have slowed down the development of the fission bomb?

Chamberlain: Well, I don't know. He didn't contribute as much to it as he could have if he'd been working on it. I don't think he slowed down what other people were doing. He didn't contribute to it himself, I imagine, the way he could have.

Hale: That's strange. You lived in the same house as him?

Chamberlain: Well, I didn't know him all that well in the scientific sense. It was probably just a matter of maybe the first eight weeks I was at Los Alamos until our regular house was ready. The house that we were temporarily in was a bit too big. It had three bedrooms instead of one or something, and it was used only temporarily. We didn't get to know the Tellers all that well in the process.

Hale: So you wouldn't say, in a personal way, that you learned anything to base your opinion on?

Chamberlain: No, not through living arrangements. They were friendly enough, but we were enough younger than the other people in that building that we weren't the most natural social friends for the Smiths or the Barnes or the Tellers.

Hale: Do you think that for the scientists who had to deal with the organized military, organized security, that sort of thing, that it rather cramped their style? Or did some scientists embrace that sort of thing wholeheartedly? Maybe it affected their attitude towards science.

Present Strict Code of Confidentiality among Physicists

Chamberlain: Well, I'm not quite sure what you mean. Certainly, I found it an impediment not to be able to talk more freely about what we were working on, and I found it also an impediment to have to stop and think, now and then, "Is this person supposed to hear what I know about this topic? Am I talking about something classified? Is this something that's not classified? Has it been published somewhere? Have I got up-to-date information on what's not classified because it's been published?" I don't think it helped to have these security arrangements.

I think in the early years it was not absolutely clear to the physicists whether the security arrangements were to be taken seriously or whether they were the sort of things that the military liked to set up and that could be somewhat

safely ignored. There were times in the early war years when I think I heard some things from people that shouldn't have known about them, where somehow a friend had passed on information. After the war the security was understood in a much more serious sense by the physicists. Breaches of the security rules are practically unknown to me at the present time. I haven't had anybody tell me something they shouldn't for ages and ages.

Hale: Do you think generally that has a serious effect or a good effect on the whole tenor of science and its relationship to government and the military? Taking science out of the private scientist's realm?

Chamberlain: Well, I think that these secrecy things don't help in terms of development of science. Generally speaking, I think we should somehow have rules which allow things to become unclassified; that is, non-secret after some reasonable time, which I imagine would be five years. You know, it's very hard to keep track of what's secret and what's not. I occasionally have to ask people whether something's been published because it's hard for me to remember. I think that it would be easier for me if I could talk freely about anything that was at least five years old.

To pretend that other nations that need the information and want the information can't have it within five years, I think, would suffice. There can be something that's secret in the United States that's also secret in the Soviet Union, and it's hard for either side to get absolutely incontrovertible evidence that the other side knows this information. I think it would be better to presume that within five years they either have it or have somehow lost the pressing need for it.

That would at least make it a little easier to get people involved. Physicists tend to stay away from the secret areas to some extent. You don't say that all physicists do, but enough do so that it impedes scientific progress in areas where some stuff is secret. That being the case, the government ought to have more interest in declassifying things and making them public.

Hale: You don't think it's affected the structure of science since the war?

Chamberlain: Oh, I don't think it's affected it all that much.

Hale: Science since the war has become like America's answer to established religion. Scientists, especially if they win Nobel Prizes, become like the fountainhead of knowledge on science policy, whether they've ever studied anything to do with politics or political theory or that sort of thing before. I wondered whether you saw this as a good thing or a bad thing.

Chamberlain: Oh, I think it's a good thing, and I think this is to some extent borne out by the actual performance of some of these committees that have advised the government. Now, many things are clear: There's conflict of interest because, you know, I'll find myself in the position of advising the government on whether they should or shouldn't build a certain accelerator which I may expect to do work on. So the conflict of interest is clear, and I think Congress understands that.

I used to believe that Congress didn't accept our statements that particle physics has no practical applications that we're aware of. I sort of felt Congress thought we were being too humble, somehow retiring or modest in our presentation. The part of the story that I don't quite know was whether Congress felt that a new atomic bomb was likely to emerge, but we didn't admit it. We didn't admit it because we couldn't make advance claims on something that we hadn't really pinned down yet. But I think in the last decade it's become fairly clear that Congress understood the sense in which we'd call for support of these things, and I think they understand the competition between the United States and Western Europe and the Soviet Union in these areas. I don't think there's anything particularly unreal about them.

I noticed in the newspaper in the last few days that scientists are, at least according to one poll, more trusted than many other segments of our society. I hope there's some justification for that. In most cases, scientists have gone into science knowing they could make a good living but knowing they could probably make a better living in something else. They're at least after some kind of intellectual reward or status reward, but they're not out after the maximum direct financial reward.

This doesn't mean that people can't be awfully interested in their own little empires and their groups and so forth. But I think that, by and large, scientists have been a pretty responsible group in their public utterances. When scientists say that we should go ahead with nuclear

power, I think they really believe it. Some scientists said we shouldn't, and I think they really believe that. I think there's a degree of sincerity in what the scientists have done in terms of their public expression. That's good and should be trusted.

Hale: Do you have in your own mind any example of scientists that you would not consider responsible in that sense?

Chamberlain: No. I disagree with Edward Teller on a lot of his conclusions, but I think he's completely frank in stating his views and stating the basis for his views. You may not trust his conclusions as much as you trust someone else's, but I think he makes the basis for them available for scrutiny, sufficiently so that you have a good basis for knowing whether or not to accept his advice. Some of Edward Teller's arguments leave me a little bit cold. He's told me a few too many times, "If you knew what I know, then you'd reach the same conclusion as I." I don't trust that mode of argument one bit. Too many of these things have gone by, and it's never turned out that his statement about what I would believe was right. It's never been right [laughs].

These things that he says he knows that I don't know, they can't stay secret forever. Eventually they'll come out in the *New York Times* or somewhere. I'm assuming something that maybe I don't know absolutely positively, but I know to my satisfaction that these things, if they're important, don't remain secret forever. The attitudes come out at some point or other.

Hale: Yes, I was struck that that was the attitude than Hans Bethe took in a lecture that he gave here a year or so ago. It was the pro-nuclear power propaganda.

Chamberlain: What attitude did Bethe take that you were commenting about?

Hale: He took the attitude to the audience that if they only knew what he knew, they'd believe that nuclear power was safe. Essentially, he said that he knows that it's perfectly possible to dispose safely of radioactive wastes. Obviously, that must have been in the area of something classified, or if it wasn't, he just presumed his audience was dumber than it was.

Chamberlain: Well, that's not the best way of arguing. I'd forgotten that Bethe relies strongly on that viewpoint.

Hale: Is that not the way it seemed to you?

Chamberlain: Well, it's awfully hard in something like that lecture of Bethe's to cover the field as well as you'd like to. He had an hour or so. I heard it. You have to discuss the storage of the radioactive wastes, you have to discuss the matter of sabotage, you have to discuss accidents in normal operation, the release of radioactivity when the fuel rods are processed. Well, this means that at most you have ten minutes to talk about any one of these subjects, and to convince people that there is a solution to the storage of the radioactive wastes in ten minutes is awfully difficult. So he might have gone into a little less detail than I thought he might, but he has to make some judgment about how much his audience can absorb on the technical side of something. I didn't feel particularly critical inasmuch as I thought most of what he left out should be blamed on the fact that he didn't have forever to discuss it on that occasion. I would have welcomed talking to him more about the radioactive waste storage problem because they're the ones that bother me. But he covered a lot of ground on that trip.

More about Los Alamos

Hale: Let's get back to Los Alamos. I've seen just two papers or two reports on stuff that you worked on at Los Alamos. One was the spontaneous fission rate of plutonium 240.

Chamberlain: And a number of other isotopes.

Hale: Spontaneous fission of those, too?

Chamberlain: Spontaneous fission of all the uranium isotopes and plutonium 239 and plutonium 240.

Hale: Also I have a report that I saw on the half-life of uranium 234.

Chamberlain: I guess it was a spinoff of some kind. I've kind of forgotten about that. But the other things that we were working on were our main contribution to the project: trying to find out how spontaneous fission would affect the bomb.

Hale: At one point wasn't it so critical that it was almost considered that the bomb wouldn't be possible?

Chamberlain: Gee, I don't remember that being a likely conclusion. It was thought to be important enough so that we should check it carefully. And I think, as far as I know, there was wisdom in this.

Hale: Because there you depend upon precisely how pure you could make the plutonium 239. If the rate of the spontaneous fission rate were too high, you might not even be able to get the thing pure enough so that you could *stop* the bomb igniting spontaneously.

Chamberlain: That's right. Well, I think you're expressing good understanding of what this situation is, but the parts don't ignite spontaneously until they're assembled. What you're trying to do is get the fissionable material into close physical proximity, like a small, highly compressed sphere, before it starts to make a nuclear reaction. So you try to compress it fully and then introduce the first neutron. If there's spontaneous fission occurring too rapidly in the material, then you can expect that the first neutron is likely to be introduced earlier than you'd like, before you get this things fully compressed and fully assembled. So the supposed danger was that if you had a high spontaneous fission rate, you would be in danger of fizzling because the reaction would start too soon and blow itself apart before it had got a large amount of total energy release.

Hale: And you understood why you were doing the work?

Clyde Wiegand

Chamberlain: Oh, I think so, by that time, at Los Alamos. I think then we all understood. The other things that we did were less major, perhaps. Clyde Wiegand had to take the parts that were to be used in the nuclear weapons and measure their neutron output--just the pieces before they were assembled together, the separate pieces. I remember walking into Clyde's laboratory one day, just coming into his room, and he said, "Here, catch. Here comes your uranium 235," and he rolled down some of the rings about so big around down the bench at me, and I caught it. It was a mockup of the thing made out of ordinary uranium [laughs].

Hale: Wiegand, again, turns out to be somebody important in your future. Could you detail something about the relationship you had with him at that time and how it evolved?

Chamberlain: Well, the relationship wasn't terribly close at Los Alamos, though we worked well in the same group. We relied very heavily on Clyde for all of the electronic knowledge. I mean, it was always Clyde that understood which vacuum tube to use and which circuit to use, and he knew which circuit other people had done well with in various things. If there was anything electronic, I can't remember learning it from anybody else.

There's a significant thing that occurs to me about the time we were still here in Berkeley. Clyde had a room in the upstairs of Old Radiation Lab, and at the time we were getting these arcs and sparks because the 37-inch was being used for Calutron development, I guess. I remember the day that Clyde put nitrogen gas into the ionization chamber that had always had air in it before and showed me the oscilloscope. I couldn't really quite realize what was happening at first. We were used to seeing pulses come out of the air-ionization chambers, and they looked like you'd expect pulses to. They were sort of surges of voltage, and then they went away again.

When he put nitrogen in the ionization chamber, the line of the oscilloscope pattern looked discontinuous, and at first I didn't realize what it was. The pulse was rising so rapidly that the oscilloscope trace was too faint to see, and then when it leveled off, there appeared to be pulses starting in mid-air on the scope screen. What he was learning was that in nitrogen you can have electrons that aren't attached to atoms. Instead of being ions, they're free electrons; they travel much faster in the gas, so we could collect the charts in a microsecond instead of in a millisecond. I don't know where Clyde got the information, but it was a real surprise, and it was kind of astounding. It was the basis for a lot of our later work with argon as the gas for the ionization chamber. But this first trial was with nitrogen.

Hale: So in your mind, then, he's always been associated with electronics.

Chamberlain: Clyde was particularly good at electronics. Well, after all, he worked in a radio station for a while as an engineer, I guess, even an announcer in a radio station. When Clyde did something, he invariably did it carefully. When he made something, it was well built, and there were no loose solder connections. The rest of us were kind of poor in comparison at constructing things. So he brought reliability in an area where the circuitry might not have

been reliable, done the way most of us would do it. Excellence in the electronics--and some good physics, too. But I think his particular contribution has been more in the electronics and his systematic way of doing things reliably. Some of the later work, his ability to test out something new and get it working reliably, was just terribly important.

Hale: Do you think that because of his abilities in that area that somehow his physics was being neglected?

Chamberlain: Well, Clyde has had a tendency to skip over or omit some of the higher-brow aspects of physics. For instance, Clyde is not a great one at quantum mechanics. He knows some quantum mechanics--I don't mean he's totally ignorant--but he doesn't consider himself able to keep up with the most modern things about quantum mechanics. There's a sense, then, in which people expect others than Clyde to carry some of the responsibility for these higher-brow aspects. Clyde has a lot of originality, but there's a sense in which the higher-brow lectures get listened to more acutely by me than by Clyde. I think this leads people to overlook Clyde's contributions a bit.

Hale: Does he underestimate himself?

Chamberlain: Well, maybe he underestimates himself a little bit. The main thing that comes to mind is the anti-proton work later on, when he was just as directly responsible as I for the experiment. I think of it as basically an experiment that was sort of originated by Clyde and by me, but in the later stage of the experiment there was involvement of Segrè and Ypsilantis, and there was help from Herb Steiner and probably other people, too. Segrè was interested in some of the methods of looking for anti-protons involving emulsions and so forth, but the experiment that was successful I think of as really originating with me and with Clyde.

More about Segrè

Chamberlain: Segrè has done so many other things that almost got him a Nobel Prize--he has such an extraordinary physics career, in retrospect, I can kind of turn it around and say that I might not have gotten the Nobel Prize at all if it hadn't been that people were in fact looking for a way to recognize what Segrè had done. See, the finding of an anti-proton had

a lot of significance. Segrè had done so many different things, including the chemical separation of isomers of the same radioactive nucleus, discovering two elements--acetin and technicium--and done some of their early experiments on changing the half-life of "k" capture by the influence of chemical state.

What are some of the other things that Emilio's done? He's found a number of radioactivities that nobody had found before him, and he helped McMillan on the neptunium work. He's probably a co-discoverer of element 94, I guess--I've forgotten. He certainly took great part in those activities. I'm leaving out a lot of things that Segrè is very well known for, but I think this anti-proton experiment gave a good excuse to give Segrè a Nobel Prize. Maybe I got it at the same time by my good fortune.

I think it's too bad that Clyde Wiegand wasn't honored at the same time because he had such an important part in that experiment in such a central way and was responsible from beginning to end for the major part of it. It really would have been more appropriate, in my view, to recognize Clyde Wiegand along with me and Segrè on that development.

I don't even think that the discovery of the anti-proton was a particularly suitable thing for a Nobel Prize, in a way. It was important in the field, but it fell to people that were at the right place at the right time, to some extent. In other words, we were one of three or four or five groups who were destined to find anti-protons because we had the first accelerator that was making anti--protons, and under those circumstances, it's not such a great achievement to find them.

Now, that's not to say that it wasn't important to physics. I think it cleared the air in physics in the sense that it led to the recognition that all of the charged particles have anti-particles, and that's important to know. I think people quit worrying about that question as soon as the anti-proton and the anti-neutron had been found.

But it's hardly an example of masterful experimental technique in that it could have been found by conventional techniques. We did some unconventional things which probably made it look harder than it might have looked. Still, it could have been found with conventional techniques.

Calutrons, Photographic Emulsions, Explosive Power of the Bomb, and Experimentation

[Interview 3: August 4, 1976]

Hale: Did you know much about how the Calutrons were performing? Obviously, this was somewhere else, in Oak Ridge. Did you have much contact with that, since you had to be in Berkeley?

Chamberlain: Well, what contact I had, had probably very little to do with the fact that I'd been at Berkeley. But I think that information was fairly readily available in discussions at Los Alamos. I think they came from private discussions more than from those discussions that I'll call Monday evening discussions, though I'm not sure Monday was the day. I think we knew. It wasn't one of my greatest concerns, so I wasn't following it anxiously or anything like that, but I think we knew something about what promises Lawrence and his colleagues had made about the Calutrons and something about how well those promises were being fulfilled.

Hale: What about the other processes, like the diffusion plants?

Chamberlain: Yes, we knew something about how the diffusion plants, late in this period, were used to supply material to the Calutrons. I've never studied the wisdom of this decision as a separate question, but I think the general idea is that hold-up time in the diffusion process may be long and the hold-up time in the Calutron is short. If the Calutron can be given a little bit less material to work with, it makes things work out, so there was certainly some kind of hybrid between the diffusion process and the Calutron before the war was over.

Hale: Well, they were feeding it directly to the beta stage. Instead of having to go to the alpha stage--that became obsolete--they put it directly through the beta stage. They did have a problem, though. Thornton told me of having to change the Calutrons according to the enrichment of the material they were getting. A different enrichment meant they had to change slits and everything more completely all the time. It was like they were balancing one against the other.

Chamberlain: I see.

Hale: Now, you mentioned a couple of other projects you were involved in at Los Alamos. The first one was the use of photographic emulsions with gamma rays.

Chamberlain: Oh, it's a small matter. In the last few months before the test at Alamogordo, it was suggested that some X-ray films ought to be placed behind various thicknesses of lead to see what the gamma radiation was, and I volunteered to do this. I remember spending a lot of time working; a large part of it was probably done with dental X-ray films. I'm having a little trouble remembering details. But we punched holes in a coded pattern to tell which was which so that many of these films could be developed in the same developing solution at the same time, without losing track.

I remember going through a lot of effort to get all these things punched and coded. We put out containers that I remember as being four-inch cubes made of sheets of lead with inside compartments made by cutting holes in some of the sheets of lead so that you ended up with a solid cube with some chambers in it for the films to be located in. So there were X-ray films exposed behind various thicknesses of lead, I suppose, running from a sixteenth of an inch of lead up to a half inch of lead, or something in that vicinity--maybe a little more.

Well, these--let's call them gamma-ray detectors--were placed in various locations before the shot, and then, on something like the fourth day after the test at Alamogordo, I went back to recover these lead containers. I had the experience of driving a jeep up close enough to the location of the explosion so that my dose meters went up to full scale, and I had to stop going further because once your dose meter's at full scale, you can't tell what you're getting. It can be 10 percent over full scale or 100 times full scale and you'd be none the wiser. So I had to back off. I was convinced at the time that some of the containers I could have reached if I'd had a better dose meter that had less sensitive scales. It couldn't be reached within the logical limits of the dose meter available.

In retrospect, a few weeks later, it was realized that I wouldn't have learned anything if I'd picked up the ones closer because the neutron radiation got through the lead in sufficient quantity that the X-ray films were in fact overexposed from neutrons. Now, I don't think much came out of this effort. It was one of those small efforts which--oh, it seemed big to me at the time because it took every

moment of my time for weeks, and I had to get all the volunteer help I could to get the codes put in all these X-ray films. I don't remember how many there were, but there must have been quite a number because I remember going back to the lab night after night to get these things fixed up.

Hale: Why was it necessary to have codes?

Chamberlain: Well, for standardization. I wanted the films that had been exposed at Alamogordo to be developed along with quite a variety of films that were in the same developer solution at the same time, in the hope of getting a better standardization that way. There were probably at least three kinds of film in these holders, maybe four. Each of those had to have test exposures of several different exposures made in the lab rather than at Alamogordo. So I was probably trying to develop thirty dental films at the same time, and I didn't want to rely on keeping track of which position these things had been clipped into the holder. I wanted to have a more positive identification. I wanted to mark right on the film.

There were lots of little efforts of people trying to implement ideas about how to get measurements on the detonation, and many of them probably ran into trouble just as this did. These data turned out to be of little importance because the neutron exposure tended to be greater than the gamma-ray exposures for all but a few of the films. We hadn't anticipated that. Maybe we should have, but it was done fairly hastily. On these things you tended to feel, "Now, let's make the test. We've got our chance--one chance, really. Let's make the test and we'll see later whether it's any good."

Hale: Well, obviously, in that situation, you know, it sort of does give you information because it tells you that--well, you go about it in a different way than the next guy. It's as simple as that.

Chamberlain: Many of these tests were more designed to pick up information if the nuclear weapon fizzled than if it succeeded. If it released a great deal of energy, no one felt it was all that important to find out the details of how it operated quickly. That could be done at more leisure. If the thing fizzled, one would dearly like to know on what basis it fizzled so that one could try again. So many of the things were known in advance to be things which would only be of use if the detonation was very poor.

Hale: I see. Now, another aspect--related aspect, of course--is the assessment of explosive power of the bomb. Is that the same thing as the gamma rays, or were you on another project?

Chamberlain: No, another project. This was more a central project during the last six months before the test. We had ionization chambers--I'm having a little trouble remembering now why the same neutron problem was not thought to bother those--with self-recording instruments located fairly close to the source, to the side of the detonation. Could have been as close as one or two hundred yards. The local recording instruments were located in steel drums that were hung on rubber suspensions so that we hoped that no earthquake induced by the detonation would jog the instruments too much and cause them to fail.

We also had a remote connection through a series of twisted-pair telephone lines to a remote galvanometer, which we hoped would give a reading within a few hours of the detonation as to how strong the explosive force had been. I believe it was measured with an ionization chamber. Well, that failed completely. This instrument had worked well during all of the tests--we had dry runs and dress rehearsals and whatnot, including one twenty-four hours before, in which it worked very well. But of course, still, there were last-minute changes being made in the last twenty-four hours, and one of them affected our instrument.

I think it was minus twenty seconds, twenty seconds before the test weapon was to be detonated that the galvanometer spot that Clyde Wiegand and I were watching--it was a little spot of light--just disappeared from the screen. We thought it had bene overloaded, but we just knew that the thing quit at minus twenty seconds. There was no way that we could correct it in the last twenty seconds, of course.

So at ten seconds we walked outside the house so we could get a chance to look at this explosion. Later we found the galvanometer had been just torn apart by a big surge of voltage. You had a sensitive instrument which was suddenly turned into a dynamo or a motor, rather, by a huge surge of voltage, and it had just been broken by the tremendous overload.

Hale: The voltage was something to do with other instruments?

Chamberlain: It had something to do with the other instruments. Somebody had hooked onto our line in that last twenty-four hours, somebody that shouldn't have. There had been a mixup. Although it had been okay during the tests, it wasn't on the final day. So we lost that recording. That was kind of amusing.

Hale: That was the only remote one?

Chamberlain: That was only on the remote ones. Within a few weeks--probably a few days--we could go to this underground bunker where the primary instruments were--the local recording apparatus--get out the proper film and develop it and get an answer. But by that time, the answer had been known by other means also, so we didn't contribute an immediate answer as we thought we would. It gives you some respect for the people that go out and explore Mars because, you know, something going wrong at the last minute is very hard to correct when you're millions of miles from the thing that needs correcting. It makes a tremendous challenge.

Hale: That's been very dramatic, hasn't it? The way they've sort of come up against the various problems and solved them as they've done, like this oxygen leak that they sort of sorted out.

Chamberlain: What's the latest word on the oxygen?

Hale: They say that it's something to do with reaction of sunlight on the earth, when they put it in the apparatus.

Chamberlain: Well, I'll find out more about that later. We don't have to talk about that now.

Hale: Well, the principle of the ionization chamber, the idea was what? Just to measure the flux of radiation?

Chamberlain: I wish now that I remembered with certainty whether it was gamma rays or neutrons that we thought that instruments were sensitive to.

Hale: Did it matter much, actually?

Chamberlain: Well, it didn't matter much in the sense that that answer didn't get used all that much because the various air pressure recording devices, I think, gave the most accurate answer and the most consistent answer. That's, I think, not unexpected. There were lots of crude gauges which in retrospect seem a little too crude. For instance, there

were things similar to aluminum foil held in various-size holes exposed to the explosion, and they were to go afterward and find out whether the explosion was enough to break certain sizes of aluminum foil and not others. It seems a little crude. The devices that measured the atmosphere pressure were very consistent. I think we called them milli-barographs or something like that, implying that they could measure a thousandth of an atmosphere, a millibar.

Hale: Yes, I see. Of course, one of the crudest of all was Fermi's.

Chamberlain: Yes. Fermi didn't tell anything about that in advance, you know. He just did it on the occasion.

Hale: We talked about it a little after the tape had ended last time, and you said that it gave a pretty accurate estimate?

Chamberlain: I think so. I don't remember how accurate Fermi thought he could make it, but I think it mostly depended on the accuracy with which he could estimate with his eye how much a piece of paper floating through the air was displaced by the explosion. It would be suddenly moved outward from the explosion, and you'd have to estimate as it moved outward a foot or eight inches. I don't think Fermi thought it could be anything but something rather crude. But it had the advantage that he knew it immediately.

Hale: I see. It was just an instantaneous displacement of some sort.

Chamberlain: Yes, yes, that's right.

Hale: Now, there was a dummy test, right? On May the 7th, as far as I can tell.

Chamberlain: There was a dummy test about that time. I didn't get to the dummy test because I had been bitten by a cat and I had a very swollen hand, and I was in the hospital with a high fever. They would come to the hospital to find out how to carry on my projects. They had to keep consulting me at the hospital, but I guess I was getting penicillin, which was newly available at the time.

The End of the War in Europe

Hale: It was about the time Germany surrendered. I think it was while that test was taking place. I wondered how did that affect your enthusiasm for the whole project? How did it affect morale and the application of people at that point?

Chamberlain: Well, it certainly caused a fair amount of discussion as to where we were and where we were going. But I think there was a feeling the war isn't over in the Pacific yet. I do remember a talk in which Oppenheimer said that he felt it was very important that the nuclear weapon be demonstrated to the world during this wartime period, that if peace came and the weapon were still under wraps, it would remain secret forever, or an attempt would be made to keep it secret forever, that it would put civilization in a somewhat perilous position.

Hale: That was at one of the Monday evening meetings, or Monday meetings?

Chamberlain: I hope I'm not wrong on my timing. There's a shadow of doubt in my mind that Oppie may have said afterward that it was important to demonstrate to the world this thing. I know it was an argument that I hadn't considered at the time. I rather respected that argument. But I'm not sure that I remember when Oppie made that point. I don't think there was any difference in the minds of the people at Los Alamos which enemy it was to be used against. This has been asked of me very frequently in recent years. Did I think that the weapon would have been used against the Germans, who were, after all, sort of a white race? I'm absolutely convinced that there was no question of skin color involved in this; it was to be used against any enemy where it would be most effective, as far as anything that I have ever heard.

Hale: In other words, the surrender of Germany didn't come as a shock to most of the scientists who were there? Was it really expected? Were people following it very closely?

Chamberlain: We were following very closely. There was a map in every morning's newspaper of how things were going. There had been the Battle of the Bulge. I think I was a little shocked by the Battle of the Bulge. I had trouble believing that the newspapers were reporting correctly the plight of some of the Allied forces. But in retrospect, the newspapers were indeed right, at least when I talked to

people that had been captured in the Battle of the Bulge. Later, it seemed to me that the reporting had been rather correct. I think the final surrender came a bit quickly, more quickly than we had guessed. I think it seemed inevitable, but I thought a few months more would be required. I thought it was inevitable, though with a lot of doubt in my mind. You know, I had no real way of estimating how strong the adversary was. There was a lot of hope that surrender was inevitable, but still I think people heaved enormous sighs of relief when the victory in Europe was announced.

Hale: Did you have a political stance at this time? Had your political ideas matured or what?

Chamberlain: Well, I don't feel that they had matured because I've changed them a great deal since. In retrospect, my political views were somewhat naive. The background for this was that my father had lived through World War I and was convinced that the Germans committed great atrocities in World War I. So when World War II started, my father and I were just locked in disagreement over this because he said, "I know what happened. I have lived through it. You can't convince me that I'm wrong."

And it was true that I couldn't convince him. But when there started to be stories about Jews being annihilated in great numbers, I attributed this to wartime propaganda. I didn't believe it for a moment. I was quite shocked at the end of the war when the story came out in a fashion that I couldn't ignore, that witnesses had been to the camp and they found mountains of shoes and mountains of teeth and the whole business. I was really dismayed.

There was documented evidence available during the war that this sort of thing was going on, but somehow I wasn't aware of it in the satisfactorily documented form. I was aware that the newspapers were saying this kind of thing, but I thought it was just part of the propaganda mill and that this was to fire us up so we'd put a lot of effort into the war.

It wasn't that I was unenthusiastic about the war. I thought the United States had been attacked and we should respond with everything we had, and if that meant making a nuclear weapon, that meant making a nuclear weapon. I didn't really have great doubts about whether a nuclear weapon should be made. I thought that we were in a wartime situation, one where it was my belief that the United States

had a very just cause. That's probably a little naive because I knew that the United States had the lend-lease program to help Britain and had no such program to help Germany. There was no doubt about it being a one-sided situation. But the attack at Pearl Harbor gave me quite adequate excuse to believe that the United States was really pulled into the war in an irrevocable fashion by the action of others.

Chamberlain's Feelings about the Creation and Use of the Bomb in World War II

Chamberlain: I certainly supported the atomic bomb program wholeheartedly. I think I also recognized that if we didn't make a nuclear weapon, somebody else would. I didn't have any feeling that I was doing something unique to change the state of the world. My feeling was that the state of the world was going to change; this was rather inevitable. I was indeed taking part in it, but dropping out was not what I wanted to do, nor did it look like a useful alternative in any way.

Hale: Let's divide it into two parts. First, after the test what were your feelings at that time, you know, and did you have any philosophical considerations, ethical questions coming in your mind?

Chamberlain: Well, I thought that the weapon then ought to be somehow demonstrated for the Japanese rather than used on a city. I can't remember whether I signed any of the petitions along those lines.

Hale: There were petitions in that period?

Chamberlain: Well, yes, I'm sure there was something sent around by Szilard. I was exposed to it and knew about it. A group of the project members, including specifically Szilard, sent a letter somehow to the powers-that-be--I don't know just where it went--requesting that this bomb now be demonstrated rather than used militarily. In retrospect, I think that it wouldn't have been very satisfactory because one of the things that struck me when I went back to the site to pick up these lead containers was that the plant life in the desert had been very little affected by that nuclear weapon. It was not very impressive at all.

Hale: Really?

Chamberlain: The only thing that gave me cause for pondering the situation and left me with some doubts was that the fence posts were charred on one side. Some of them were burned; the closer-in ones rather completely. It was interesting. The plants, cactus and sagebrush and stuff, would look completely unaffected, and right next to these unaffected plants would be a fence post that had been charred on one side. The dry wood caught fire temporarily or something like that; it got hot enough so that it charred. And the living plants had enough moisture so that it just didn't affect them in the same way. So it was my feeling, in retrospect, that something like a demonstration of the nuclear weapon over some lightly populated area or unpopulated area would have failed in its purpose. It wasn't that impressive until you gave it the real city to work on.

Hale: Even though one would let the Japanese know in advance that, "Well, we're not trying to destroy much."

Chamberlain: Now, there was a thing that I was very much surprised at: that this nuclear weapon really ended the war so effectively. It had a much greater effect on the military situation than I had any idea that it would. You see, I knew that we were sending thousand-plane raids against Tokyo and other Japanese cities, and at that time we were doing it every week or ten days, I believe. I know that, typically, these planes carried something of the order of twenty tons of TNT so that the explosive force of one of these big raids was about 20,000 tons of TNT.

We had also been told, and I think on good authority, that many small explosions would actually do more damage than one large explosion, although they'd be comparable in destructive power. The one large explosion has an overkill in the central area and dies out pretty rapidly. I expected the military effect of one nuclear weapon to be about like one thousand-plane raid, of which there had been many. As we all know now, that simply wasn't the case.

First of all, the element of surprise was very effective in actually making the thing more effective. Secondly, the completely different character stunned people. The fact that it was a new kind of weapon on the scene magnified its effect and then gave it a mark of mystery, I'm sure, to the people that were on the receiving end. It gave a new aspect on the situation, actually gave the Japanese a better excuse

to surrender than they had had up to that point, so that it served a number of purposes that I hadn't expected.

But I think mainly that the thing had a much greater psychological effect than I had any idea. The psychological effect was really greater than the military effect.

Hale: After the bomb was dropped, it was obvious what the immediate effect was, anyway, in physical terms, whatever strategic effect might have been. What were your feelings then? Did you feel any qualms of conscience?

Chamberlain: No, I thought it was a very good thing that the war had ended abruptly on this note, and I felt the war would have dragged on for many months. I didn't know how many, but just as an invasion of Germany was needed before the final German collapse, some kind of invasion of Japan could have been anticipated before the final Japanese collapse. Given the frame of mind of the Japanese, with some of their leaders so dedicated to a militaristic view and feeling that they were serving a religious as well as a military purpose, I think the nuclear weapon was barely enough to cause the complete surrender in Japan. There was a real danger that there would be significant hold-out pockets in Japan. That didn't occur, but was very helpful to have a nuclear weapon to give them an excuse for finally ending.

Hale: Allow them to save face?

Chamberlain: Yes, though I don't think that we save face any less than the Japanese do [laughs].

Hale: In the period following that, did you have any serious thoughts about what this might mean for the future of the world in general?

Political Activity in the Scientific Community

Chamberlain: Yes. One thing I was concerned about was that people who read the newspapers and realized that the atomic bomb had been instrumental in stopping the war would still not realize what a revolution had occurred in the military. So it wasn't too many months before I was taking part with the then-young organization called the Federation of Atomic Scientists.

One of the things that we did was to go back to Alamogordo and pick up some of this radioactive melted sand. The sandy soil at the Alamogordo site had been close to the detonation tower, had been petrified and been made into a kind of glass. We picked up some of that glass, broke it in pieces, sealed it in little plastic cylinders, sort of as paperweights, and we sent a hundred of these things to the mayors of the hundred largest cities--with a lot of press releases at the time, saying that they must realize that this is the kind of material that their cities could be turned into by the use of nuclear weapons, that warfare had really changed dramatically and the need for peace was greater than ever. We didn't at all visualize these limited wars in which nuclear weapons are not used. We've been through a number now. We thought that every war would escalate to a nuclear war.

Hale: Really?

Chamberlain: That was a misjudgment, but we felt that peace had become imperative.

Hale: The idea that in the future all wars would turn out to be nuclear wars, was that because of the scientists not being that sophisticated in historical or political or military knowledge?

Chamberlain: I think. I think the division of wars into nuclear and non-nuclear hadn't really taken place in people's minds. My knowledge of history may be at fault, but I have the impression that there are very few examples in history in which new varieties of weapons were made and stockpiled and not used--except possibly some of the poison gas weapons may have been stockpiled and not used under certain circumstances.

That might have been a key to the situation that grew up with nuclear weapons. People didn't use nuclear weapons because they were afraid what would happen when nuclear weapons were used in return. The number of times that that had happened with poison gas weapons was probably not too many. Most of these weapons had been used if they were developed. I think the warnings weren't misplaced in the sense that we still have nuclear war hanging over us. We thought the imperative was that you have to preserve the peace, instead of avoiding using nuclear weapons, because it would be too awful what happens when they're used in return.

Hale: Could you say in essence that there was sort of an overreaction on the part of scientists?

Chamberlain: I don't think so at all; at least that's not my impression. I think it was good that we tried to make people aware that this problem had changed things. We had the feeling--and I think it's justified--that most men in the street had little basis for comparing big weapons with little weapons. The scientists had a better way of quantifying what the differences were between conventional weapons and nuclear weapons and then appreciating the answers that they got.

For instance, with the advent of nuclear weapons as used in World War II, one airplane can be as destructive as a thousand airplanes were before. Now, I've got some feeling for the effort that went into those thousand-plane raids: the tremendous lines of supply ships, the dollars, the airplanes, and the training of the pilots. It's just been a fantastic effort. Suddenly, you've got a thousand-fold increase in the destructiveness of one plane. Now one plane can do what a thousand used to.

Furthermore, that was the primitive atomic bomb. With the advent of some refinement due to getting the things more efficient, with the addition of the hydrogen bomb principle to the fission bomb, you get altogether another factor of a thousand. So the truth is now one airplane can release a million times the explosive force of one airplane of World War II, pre-atomic bomb. One airplane now can release a thousand times that huge effort in the whole Pacific Theater.

Now, I think scientists are better able to understand these ratios of a thousand and a million that are involved than most men on the street. And I think it's very important for scientists who think they realize, who think they understand something that the man on the street has trouble understanding, to make his findings known and his mode of reasoning accessible for people to learn about it and maybe criticize it, too.

Federation of American Scientists

Chamberlain: I think it was quite important what the Federation of Atomic Scientists did at that time. I believed in it then, and I believe in it still. In fact, I'll probably join on this

coming Friday morning in San Francisco with a group that wants to remind the world that nuclear weapons are still in our stockpiles of weapons, that they're still a problem, that we have to cope with it. There's still a danger that these weapons will be used in large numbers and that huge numbers of human beings will be annihilated.

Hale: Yes, that's my own predilection, too. I'm just thinking about the genesis of the scientists' movement. Obviously, it's been well catalogued by people like Alice Campbell Smith, for a certain period. Now, that movement germinated before that, at Chicago, I guess because the Chicago scientists seemed to have more time on their hands. Was the scientists' movement in general very active at Los Alamos before the end of the war, or was it not very evident?

Chamberlain: I think it was an awfully small group before the end of the war, as far as I could realize. I think a part of that is simply the enthusiasm for getting on with the job. The pressure to get on with the job was very strong, and most of us didn't put a lot of effort into the philosophical considerations behind the then-existing situation, while the war was still on.

Hale: You mentioned Szilard, particularly. Of course, his involvement is very well known. What other scientists were most active at that time? You yourself, you said, were active.

Chamberlain: Well, I was active only after the war was over. I'm really having trouble remembering. I think Willy Higgenbohm was active very early, and I would probably ask him for more information on what went on at that time.

Hale: You weren't involved in the formal organization at that point?

Chamberlain: Oh, I don't think so.

Hale: Was yours actually more of an ad-hoc attitude?

Chamberlain: Well, I had very much a feeling of being young and inexperienced, and I had some doubts about whether my own ideas were fully formulated. I think that there was a kind of expectation that the older physicists would probably lead the way, as far as I was concerned. I had a lot of this attitude, that it wasn't so much my responsibility, at least not yet. I felt a responsibility to pay attention to what

was going on, to do a lot of listening, but I didn't feel the responsibility for leading any movements at the time.

Hale: I see. Was that a common feeling, do you think, amongst the younger scientists? Or were some of them literally the Young Turks?

Chamberlain: The ones I was in closest contact with on an everyday basis were not so inclined to develop their own philosophy. I was at least interested in developing my own philosophy, and I think many of them were even more inert than I was on this line. I think that of the other people that I worked with fairly directly, very few took an active hand, with the obvious exception of Segrè, who paid a lot of attention to these things. I'm sure I've got a lot of my ideas from Segrè.

Hale: But then he was, of course, one of the older physicists.

Chamberlain: He was one of the older physicists, and I very much looked to him for leadership. I don't think Segrè was active with the Federation of American Scientists at all, but he had considerable personal influence with Oppenheimer and other people in the project and with Fermi. And I'm sure he made his ideas heard and felt.

Hale: What was to have a great effect was the development of legislation on the control or the use, manufacture, of nuclear weapons. Obviously, the first thing that was up for contention was the May-Johnson bill. Did the FAS have any specific effect on what was happening at that level, or was it individual scientists?

Chamberlain: I think the FAS had a tremendous effect at Los Alamos, anyway, on this question. Now, I remember that Oppenheimer supported the May-Johnson bill, and though he admitted it wasn't what he had hoped it would be, he thought it was the best that one could get at the time, politically and so forth.

Hale: So did Fermi and Lawrence, though, right?

Chamberlain: I don't have any memory of Fermi's view about that or Lawrence's view. But in fact, I think that's one of the issues around which the FAS group kind of coalesced. I think the FAS group was practically the only group at Los Alamos that put forward, in meetings, a counter-argument. I think I can remember Willy Higgenbohm taking a part in that very effectively, saying that this should be fought, and it

was successfully fought, and we're very grateful now that it was.

Hale: Did it seem at that time that there was a great chance that the whole thing would remain in military hands?

Chamberlain: You know, I had no basis for knowing what the political realities were. I didn't know the mind of Congress. I think I wasn't doing enough of the right kind of reading to have any real feeling for what was doable in that respect. I certainly didn't feel that I had personally entered the May-Johnson bill controversy in any very effective way. I was mostly a listener at that point. I tried to understand the issue.

In fact, I can understand much better now than I did at that time why there was fear of the military having so much responsibility in the nuclear weapons area and the atomic energy area of nuclear science. But I think it was one of the first successes that you could perhaps attribute to the FAS activity.

Hale: In some senses you were probably supporting the FAS position without completely understanding what the effects were politically?

Chamberlain: Yes, the FAS activity that I supported most directly was getting out the word to these hundred mayors and ballyhoo about the importance of peace, to try to make people aware of the fact that warfare had changed dramatically. We were afraid that people hadn't realized the drastic extent of the change in warfare.

Hale: And so would you say that this was being dealt with well by the older scientists?

Chamberlain: Well, you know, the Federation of American Scientists' group was not so old. They were mostly young people. I think in that area, I was leaving it up to people that I hoped understood the problem better than I, because my basis for judgment was not particularly good. I think as the years have gone by, I feel it's at least a little bit better. For one thing, I've been exposed to the *New York Times* a great deal more.

Hale: Now, you stayed in Los Alamos, then, quite a fair time after the war ended.

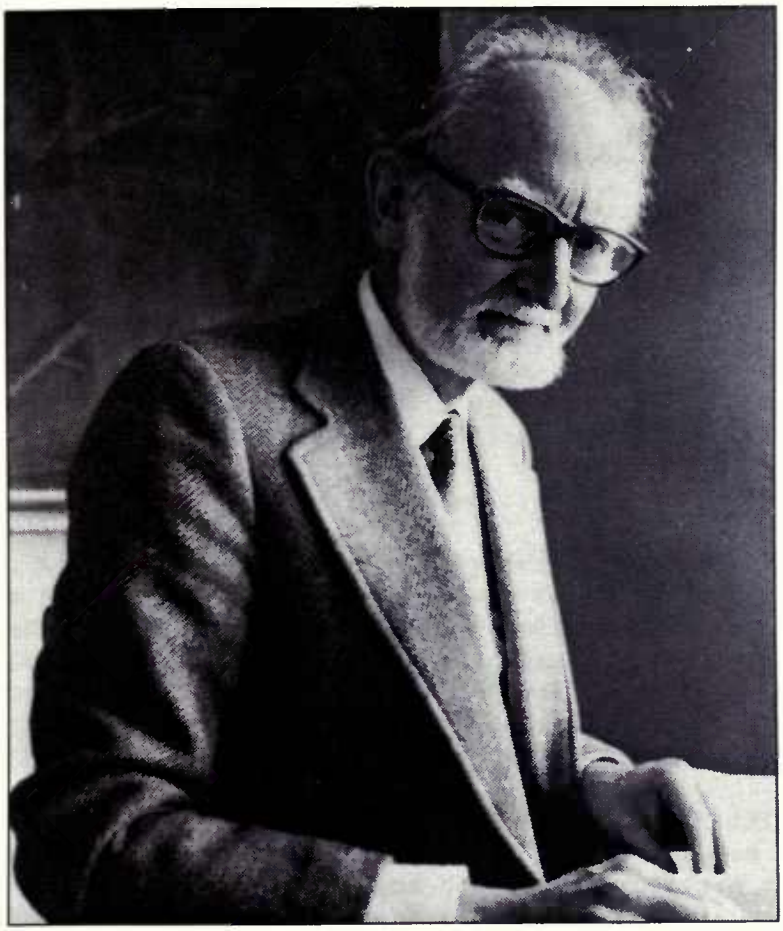
Chamberlain: I stayed exactly as long as I had to, to satisfy my draft board. Once a month I wrote a letter to my draft board asking, "May I leave Los Alamos without being drafted now?"

Hale: Oh, really?

Chamberlain: They wrote back saying, "No, you may not." When they finally said, "Yes, you may," then I went to Chicago to school, within a very short time.

Hale: I see. So what were you doing in that period?

Chamberlain: The draft laws were on the books, and there was sort of a general understanding that nobody was really likely to be drafted. My deferment rested firmly on the fact that I was working on the war effort at Los Alamos, and until my draft board said I could return to civilian life with impunity, I wasn't going to do so. People understood that. I wasn't sacrificing my future by waiting till the draft board said okay. If the draft board hadn't come around, I think eventually people at the University of Chicago would have said, "Well, you better consider risking it."



Owen Chamberlain, circa 1975.

Photo courtesy UCB Physics Department.

IV EDUCATION AND CAREER AFTER LOS ALAMOS

Return to Graduate School at the University of Chicago, March 1946

Hale: So what went on during that period? That was about a year, was it?

Chamberlain: Yes, I think I left in March '46 from Los Alamos. Why, the main thing that occurred then in my life was Los Alamos University. You heard about Los Alamos University?

Hale: You mentioned it briefly last time.

Chamberlain: Well, we had some of the great physicists teaching some of their great courses. Hans Bethe was teaching electricity and magnetism. Edward Teller was teaching quantum mechanics. Leonard Schiff was teaching statistical mechanics. And we were trying to enroll in those courses. I enrolled in Teller's quantum mechanics class and found it very helpful and revealing and valuable--very worthwhile.

I also enrolled in Leonard Schiff's statistical mechanics course, and Leonard Schiff kindly allowed me to drop the course toward the end because he realized that somehow I just didn't have enough background in quantum mechanics to appreciate his statistical mechanics course. I was in deep trouble, and it's just as well. I don't know whether anybody's really looked up those grades, but I would have flunked Leonard Schiff's course. I did all right in Teller's quantum mechanics.

Hale: Was that set up pretty formally, then?

Chamberlain: Well, rather. We had courses, and they went from prescribed times, which must have been the order of three months, I guess. We had two quarters, I think, at Los Alamos

University before the reorientation. See, there was practically a changing of the guard at Los Alamos to a large extent. The physicists that had come to Los Alamos during the wartime period mostly went back to university positions, and a new group of recruits came in to make a more permanent staff at Los Alamos. It was really a switching of personnel.

Hale: You were still working, though, on aspects of the bomb, is that correct?

Chamberlain: I turned to a project to try to get the spectrum of neutrons from fission, to try to find out how many fission neutrons there were of various energies. It was not to be an easy project, and I don't know whether anybody's finished off where I left it. I had to measure the pulse heights in a proportional counter of proton recoils from neutrons striking protons.

I never got the apparatus in working order. What I wanted the people at Los Alamos to do was to let me take the whole thing to the University of Chicago, and I intended to use it as my Ph.D. thesis. I think it would have been a very smart thing if they had sent me off with it. At least at the time, it was thought to be one of the better methods. Nowadays, I guess, this would be done by neutron time-of-flight experiments, much more effective; but at that time, I think it was thought to be an acceptable method. But they couldn't see sending me off with this ionization chamber that had been built at Los Alamos at some expense, and the amplifiers which nowadays look trivial but in those days looked like a big investment. So the project, I think, must have died when I left Los Alamos.

Among other things, I think I had over four thousand volts of batteries. I wanted a high-voltage supply that was more stable than any of the electronic high-voltage supplies at that time. Rather than make a more highly stabilized supply, I chose to make a supply with batteries. A four-thousand-volt battery is a very nerve-wracking thing to work with because you can't turn the damned thing off. So you have to connect it hot, you see. I didn't electrocute myself, but I was worried that I might.

Hale: Yes. There would also be, of course, a question of secrecy if they'd have let you take such an apparatus, is that not correct?

Chamberlain: Well, I think they were also concerned about the secrecy, though we argued--and I think rather persuasively--that this kind of measurement just had to be in the public domain. I don't really remember how much the secrecy was a problem. See, there were other secret projects at Chicago, so the Metallurgical Laboratory in Chicago would have been a perfectly natural home for this. The idea was that the experiments would be declassifiable. We were confident, but the experiments indeed were to be done originally in secrecy.

More about Teller and the Hydrogen Bomb

Hale: So there was, as you mentioned, great change of personnel at that time. What reasons were there, other than your personal reason, for other people staying at Los Alamos? Why did Teller stay?

Chamberlain: I'm not really sure. I remember when I was at the stage of getting my Ph.D. at Chicago, Edward Teller made a plea to me to return to Los Alamos and measure some of these neutron spectra that needed measuring. He was essentially saying, "Look, during the war years we kind of rushed through this without a real scientific understanding of the details of what's going on. Now what we ought to do is go back to Los Alamos and finish the job up and get the neutron spectra correct and find out exactly how these neutron reactions work."

I felt that Teller was quite wrong. He was, I think, saying that it was very important to the United States that something like this happen, and I just didn't feel that way. I simply disagreed with him and didn't find his arguments persuasive at all. We all felt that the existence of nuclear weapons was important, but the notion that rapid further development of nuclear weapons was important to the United States seemed to be an idea that Teller had and other people didn't have, as far as I know.

Hale: Really? Did he stand out very much on his own at that time, as he still does in some ways?

Chamberlain: I think so, yes. I think so.

Hale: I noticed a little contradiction in one answer that you gave in the last session regarding Teller. In one breath you

mentioned that he would often use an argument with you that if only you knew what he did, you'd agree with him.

Chamberlain: That was later. Teller had tended to be someone who thinks in a somewhat unconventional fashion and thinks a little bit differently from most physicists. That makes him a valuable physicist because physicists who think differently are good to have around. But many times I'm left in disagreement with Teller on his political conclusions. Now, it was in later years when I was no longer associated with any secret project, and probably didn't have a clearance at the time, that Teller would say, "If you knew what I know, then you'd agree with me that we should have more weapons or we should have shelters or something."

I've always felt that that's a lousy method of argument. It is dangerous to listen to it, and in particular Teller has proved himself wrong over and over again, as far as I'm concerned. I'm absolutely convinced that if his arguments had been right, the reasoning behind his arguments would have come out within a few years.

Hale: But at some point, then, he did indeed make a basis for his conclusions open to scrutiny--I mean, scientific scrutiny.

Chamberlain: You're saying something that I can't remember now. This was in connection with what kinds of situations?

Hale: I guess scientific questions rather than political questions or anything like that.

Chamberlain: Well, in a scientific area, I think Teller's been perfectly open. He's sometimes hard to understand, simply because he thinks in an unconventional fashion. I think it's when he gets into political areas where Teller may think that he knows something about what intelligence says the Russians are doing or something like this, where he says, "If you knew what I know then you would know that I'm right." I've been glad that I haven't listened to those arguments.

Hale: You did mention a bit about the open debate--I guess open within Los Alamos at that time--about the possible future of the "Super" and the arguments going backwards and forwards between Teller and Fermi and other people?

Chamberlain: Yes, those I believe were in the period after the war. Teller tried to show that the "Super" could be made, and then Fermi tried to show that it would fail.

Hale: That was in public discussions?

Chamberlain: It wasn't in this Monday night thing. It was in the afternoon. It was probably a couple of afternoons a week, maybe. I can't remember for sure its frequency. The group that listened to this was fairly small. I don't think it was as many as twenty.

Hale: I see. Why were you listening to it?

Chamberlain: Well, I had been made aware of it by Segrè. I don't think it was a highly advertised thing at all. I think it was a little more like an invitational seminar, and I must have gotten an invitation through Segrè. I listened to those with great interest.

Hale: Did you understand the arguments that were put up by either side? Was it basically Teller and Fermi, or did a lot of other people have input?

Chamberlain: Well, it was really very much Teller and Fermi doing it, and they set it up this way. I think Fermi was the originator of this format. I think Teller wanted to give some talks to a good scientific audience about the "Super" because Teller had worked a lot of the time during the war on the "Super," and I attributed that to his inability to get along with Bethe.

Anyhow, I think Teller wanted to talk about the "Super" once the war was over, and I think Fermi said, "Well, then, what we ought to do is to--if you're a believer and I'm a non-believer--we should have a debate of sorts." I think Teller, if I remember correctly, would present a long presentation in which he tried to make the case that the thing might work, and Fermi a shorter presentation of why he thought there was trouble.

Hale: Was everybody able to ask questions?

Chamberlain: Oh, I think so, yes.

Hale: Raise objections?

Chamberlain: I think so.

Hale: On either side? Did you have a feeling that a general consensus of any sort came out of that?

Chamberlain: Oh, sure. At that time Fermi won the argument hands down, and what it essentially means is that Teller hadn't yet figured out how to design a hydrogen bomb. His ideas at that time were not going to succeed, and those ideas didn't succeed. But later ideas must have.

Hale: I see. But still, he was propagandizing for his project?

Chamberlain: I guess, yes. But you can't call that propagandizing, really. If you had a seminar for twenty people, that's not propagandizing, that's a serious discussion! But it's interesting, his interest in the "Super" was great even at that early time.

Segrè Recommends that Chamberlain Work with Fermi

Hale: Right. Now, I gather--Segrè mentions in a book that he encouraged you and Fowler to go to Chicago to do a Ph.D. with Fermi.

Chamberlain: Well, now, it was because of Segrè's recommendation, I think, that I had no doubt. I mean, one didn't have to be with Segrè very long to learn that Fermi was *it*. So sure, it was obvious that we'd study with Fermi if possible to do so. Segrè's recommendation was probably explicit later, but it was implicit long before.

Hale: I see.

Chamberlain: I certainly didn't mean to contradict Segrè's statement.

Hale: Had you talked to Fermi on that point?

Chamberlain: No, not really. I hadn't. But once it was known that Fermi was going to return to the University of Chicago and that Sam Allison--he was also at Los Alamos--was going to return to the University of Chicago, and that Sam Allison had several fellowships in his hip pocket that he could hand out, then we all went to Sam Allison.

Hale: And what did he say?

Chamberlain: Oh, well, at first we filled out applications. I don't know whether we had letters of recommendation or whether he just talked to people, but at first he told me, well, he was sorry that they felt there were even more deserving people

that were going to get these fellowships and he didn't have one for me. So I did something which was more forward than I usually was at that time. I think I pounded on his desk and said, "Well, I certainly hope to prove that you're wrong in your present judgment, and I hope that you'll change your judgment." Then, later on, Geoff Chew was awarded a National Research Council fellowship, and that freed one of the Chicago fellowships, and I got to have Chew's.

Hale: That reminds me the way Rutherford got to come to England originally.

Chamberlain: I don't know about that.

Hale: He got a fellowship by default.

Chamberlain: I somehow would have gone anyway. I told Allison that I was somehow going to come to Chicago with or without fellowship, but that the fellowship would make it an awful lot easier for me. I wasn't making any claim that I wouldn't be able to get to Chicago; if it wasn't for the fellowship, I was planning on leaning more heavily on my father. It turned out that I was leaning very heavily on my father even with the fellowship. What with housing being in short supply in Chicago and one thing and another--having a young child at that time--we relied on my father for extra income for quite a long time.

Chamberlain's Greatest Influences from Los Alamos

Hale: What would you consider from Los Alamos the most fruitful contacts among the important scientists, or the most important factors that affected your subsequent career?

Chamberlain: Well, of course, Segrè and Fermi were the big contacts in my scientific life. Segrè was the contact made before Los Alamos; Fermi was a contact made starting in Los Alamos; but I wasn't close to Fermi at Los Alamos.

Another contact that was very important to me was Hans Staub. Now, Staub is a Swiss who had been at Stanford, and I found Staub to be a delightful person. He answered my physics questions in the way that I could understand them. Sometimes he didn't give me all the answers that I wanted, but he never left me without one more of argument on some of these things. He was very helpful. He seemed to me very

informal and a delightful person, and he often went along on some of these walks with Segrè, or maybe I went along on some of these walks with Segrè himself.

Well, since that time Staub has gone back to Switzerland, and he's become the unapproachable Swiss professor who's somehow on high. He's a completely different person from all I've heard. I feel that I got some of the best days of Staub because his informal phase was so valuable to me and I got such a pleasure. It wasn't that I was working with him, but he answered my questions so well and I found him somebody that I could bring my questions to. He taught me how an ionization chamber worked, and some of the things that were basic at that time and are still valuable but were new to me.

I think those are probably the most important contacts. There was some contact toward the end of the war with Martin Deutsch, and that was valuable, but Martin was also reminding me that I hadn't shown that I was a good physicist yet. Somehow this introduced a competitiveness in some way into the relationship with Martin Deutsch, so I wasn't able to learn from Martin Deutsch in the same way that I was from Staub or Segrè, who of course felt no competition with me.

You have to realize that I was very young and, you know, I had had maybe one year of graduate work when I went to Los Alamos. I had learned some physics at Los Alamos, but it was very patchy, and I really returned to my graduate education at Chicago after the war.

Hale: In your overall makeup, can you think of anything major that you learned at Los Alamos?

Chamberlain: Segrè was very influential on me. He took a great deal of pride in not publishing things that were wrong, so he would work over very carefully to make sure the result was correct before it was published. Now it turns out that even so, you could still publish results that are wrong. But he guarded his scientific reputation in a way that few other physicists do. That was good training. I think Segrè also trained me not to be wasteful of the government's money--spend the government's money the way you would spend your own.

Hale: So in fact those general things about your makeup, you think you mostly got from Segrè?

Chamberlain: I think so, yes. I think Segrè was by far the greatest influence.

Hale: Well, now you moved to Chicago. You left in March, you said. Did you go directly to Chicago?

Chamberlain: Well, I think we went directly to Chicago. I sent my wife and very young daughter to Philadelphia for a short while, to live with my family while I tried to find a place in Chicago. That didn't work very well because finding acceptable housing in Chicago seemed very difficult. Within a couple of weeks my wife phoned me to find something where she could live in Chicago, and I found a funny little basement apartment that we lived in for a few weeks. That was next to intolerable. So within a few months, I brought the problem to my father, and he dug up a little capital, and we, along with a young professor at Chicago, bought a two-flat building a little south of the Midway. That worked out very well. I think that the total cost of that building was nine thousand dollars. I think we each paid four thousand five hundred dollars. At that time, it seemed like a good deal of money, but looking back at the cost of housing in big cities, it looks cheap.

Early Coursework at the University of Chicago

Fermi and Segrè

Hale: Probably just turned out to be an investment.

Chamberlain: And it turned out to be, I think, a reasonable investment. I've forgotten the details about that.

The big thing in Chicago was that Fermi organized an evening seminar which was a little bit like a course with no credit. It wasn't a formal course, but he just invited certain people to go to this evening seminar. Geoff Chew, George Farwell, Frank Young I believe were there part of the time, maybe not all. Joan Hinton was there. Joan Hinton was somebody that Fermi liked and had been a helper to Fermi a lot during the Los Alamos period. Leona Woods Marshall and myself. I don't remember anybody else. That's probably about all. I might have forgotten one. In this evening seminar, Fermi just sort of taught us physics. We did quantum mechanics from the ground up. We did a little bit of general relativity, but that hasn't stuck with me very well.

This evening Fermi seminar was probably two nights a week--Tuesday and Thursday, I think. It was very helpful. I think the study of quantum mechanics was really most important to me. The other things were helpful but not as essential. Quantum mechanics is a subject sufficiently different from the other areas of classical physics that it's a great thing to learn from a good teacher.

Hale: This seminar started about as soon as you went there?

Chamberlain: It was probably after I'd been there about six months. A little hard to remember. Maybe the first fall. It went on for quite some time. It was fairly hard work; we didn't do all the homework that we should have usually, but it was very valuable. The nice thing about studying with Fermi--and this was very important all during my thesis work--he always used methods which I knew I should have learned and mastered in the courses. Somehow Fermi knew exactly when to pull out the standard results and how to use them. We used to say that Fermi had about seven basic arguments and we managed to force every problem into one of these seven basic situations [laughs].

Hale: Were those analytical situations?

Chamberlain: Well, to mention one that everyone knows about and yet few physicists know how to handle it as well as they should: We all know that the index of refraction that exists in a piece of glass is different from 1, due to the scattering by the molecules of glass. Yet very few physicists can go directly from the scattering amplitude of light on those molecules to the index of refraction of glass. One of the standard methods that Fermi used was to make that argument. It wasn't that he got the index of refraction exactly, but if the scattering is fairly small or the glass molecules are fairly dilute, then you get the exact answer.

It always seems as though Fermi cast each problem into one of these, we said, seven basic methods. We never did enumerate the seven precisely, but it seems as though there were about seven. It was very rarely that Fermi used a method that was beyond our standards--what should be our standard value of tricks as physicists.

Fermi also had a way of dodging the difficulties. Fermi would manage to go around difficulties in such a way that they would appear to be no difficulty at all. Then, when you tried to reproduce his steps, you'd fall into various traps which he knew how to dodge. You know how it is that

maybe an integral which turns out to be infinite, but if you approach in just the right way, it gives the sensible answer and you don't realize that Fermi has dodged the trap [laughs].

Hale: Yes. Obviously, that would make more efficient use of his time, too, in the seminar.

Chamberlain: Well, I often wondered if Fermi wouldn't have been a still more effective teacher if he had then pointed out these traps which he knew perfectly well. He'd always prefer to go around them without comment--present the part that seemed to make a smooth presentation.

Hale: Was that to leave plenty of room for you to think later on?

Chamberlain: I don't know why he did that. I really don't.

Segrè, on the other hand, was rather the opposite. Segrè always fell in the trap himself. Watching Segrè work himself out of the trap was extremely instructive. Segrè had the reputation of being someone who didn't prepare his lectures particularly well, and some of the students didn't like Segrè as a lecturer. But I found him marvelous because I liked to watch him fall in these traps and then work his way out. I learned more physics that way--my kind of physics--than I could have if he'd had a smooth-sailing presentation.

Hale: Yes, I think you mentioned that before, too. Did this seminar then include all the students or people who'd been working with Fermi at the time? I get the impression there were huge numbers of students that went to work under Fermi at that time.

Chamberlain: Well, you have to realize that at Chicago, besides Fermi, there was Gregor Wentzel and Edward Teller. So there were three star "theoretikers" at Chicago, not to mention Maria Mayer, who turned out to be more of a star than we knew at that time. Now, for instance, when I started going to the seminar, I wasn't doing my thesis work with Fermi yet. I was still taking courses, and I didn't know whether I could do my thesis with Fermi. I kind of hoped so, but now and then I would have to tell him that if he'd have me, I'd like to be his student. But I didn't press that very hard because I didn't feel that I had all that much to offer. I wasn't sure whether I deserved to be a student of Fermi's or not.

Hale: Really?

Chamberlain: If he'd have me, I wasn't going to turn him down!

Thesis: Neutron Diffraction

Hale: Did you have any idea at this stage of the sort of area that you wanted to do your thesis in? Or would that very much depend upon who you were doing it with?

Chamberlain: Oh, no. I didn't know what area I should do my thesis in. In fact, if I had any ideas about what to do as a thesis, it was this thing at Los Alamos. I don't remember whether Fermi was given a chance to respond to that or not, but Fermi suggested the thesis topic. He said, "Look, neutrons in gases have been pretty well studied and understood. Neutrons in solids have been pretty well studied, and a lot has been understood. Why don't you try neutrons in liquids, which really hasn't been touched yet?"

Hale: Neutron diffraction?

Chamberlain: Yes, that's right. Originally Fermi had in mind less extensive experiments than I finally did, I believe. We didn't know how to approach the scattering liquids initially. I think Fermi was quite pleased when I dreamed up a kind of spectrograph that would allow us to study the neutron diffraction in liquids. There were some instrumentation problems that had to be solved.

Hale: He and Leona Marshall had already been working on neutron diffraction, is that right?

Chamberlain: Oh, yes. I'd forgotten how much of their work you would call neutron diffraction. They certainly knew a number of things. They knew about filtering neutrons to get out the slowest. They knew about Bragg scattering of neutrons in crystals in great detail. Well, in fact, my work was built on that.

When I wanted to do my neutron diffraction, I tried various crystals that were available, and I couldn't get very good intensity. Fermi let me try a piece of fluorite that he had gotten somehow--I think in the Alps or something--a great big hunk of fluorite, as big as your fist. It was so much better than any of the other crystals

that after I had shown there was nothing that could compare with it, Fermi let me slice up this fluorite crystal that he had in his personal possession.

We sliced it into layers which could be arranged in a kind of six by six inch or seven by seven inch square pattern of these pieces of crystal. But it turns out that in making mono-energetic neutrons with diffraction on a crystal, it pays to have the crystal with lots of impurities in it; a perfect crystal doesn't work as well as a waved crystal that gets a little bit of waviness inside it because of the impurities.

Hale: And you needed that in order to produce a source of mono-energetic neutrons.

Chamberlain: Yes. Essentially I was producing a source of mono-energetic neutrons.

Hale: Yes, I see.

Other Coursework

Hale: What other courses did you take while you were at Chicago?

Chamberlain: I took electricity and magnetism from Professor Zachariasen. I took quantum mechanics again with Teller; that was valuable, too. That was the second time I'd taken Teller's quantum mechanics: once at Los Alamos and once in Chicago. I may have audited it the second time. And I took quantum mechanics courses from Stanley Frankel and Eldred Nelson. They were both young men. Both had been at Los Alamos--certainly Stanley Frankel. The two-flat house was owned with Stanley Frankel, so he was a neighbor.

I took statistical mechanics from Maria Mayer. And there was something sort of amusing: the cluster theory that Maria Mayer tried to teach us is really very important physics, but I didn't learn it at the time because I thought it was a favorite hobby horse that she invented. But years later I found out that it's quite important. I should have paid much more attention to it than I did. But she taught an excellent course in statistical mechanics.

She took about three days of her time trying to teach me group theory, and after three days we agreed I wasn't a very

good student of group theory. It's funny because group theory's the kind of argument that I usually like to make. It's just slightly more involved than I can keep in my head. I mean, I can keep simple arguments in my head, but when things get too involved, I kind of lose them. I've never been good at group theory because you have to keep a greater structure of knowledge in mind than I'm using to keeping.

My physics can all be done with fairly simple arguments, and I have kind of mechanical pictures in my mind which help me remember. Somehow with the group theory, it was too symbolic for me. I could understand one theorem after another, but I could never get them together in a unified body of knowledge through which I felt I knew my way around.

Hale: I see. You're rather addicted to the linear method of thinking rather than this systematic way of thinking?

Chamberlain: Well, I don't know. I think sometimes I'm okay with the systematic way of thinking, providing I have a lot of pictures in my mind that I refer to so that I could pick and choose my arguments wisely.

Hale: Do you mean physical pictures or just mental pictures of models? Mathematical pictures?

Chamberlain: Well, to some extent they're physical pictures in that they're idealizations--you know, scattering of a wave off a crystal. As a physicist I usually feel that I know which kind of arguments go to work in which situation, the most easily and the most correctly; but the corresponding knowledge in group theory I never was able to develop within my patience. I suppose I could eventually do it, but somehow I never got to the point where I could get the impetus needed to discipline myself to stay with it long enough and hard enough to master it.

By the way, I took one electricity course from Fermi--that's right--as well, as one from Zachariasen. Heat, I guess, was in the statistical mechanics course with Maria Mayer.

Hale: There was a great availability at the Argonne Lab--

Chamberlain: Of neutrons.

Hale: --of neutrons, of high fluxes of neutrons in the reactors. Did this technological aspect affect Fermi's ideas as to what would be a good area of study?

Chamberlain: Oh, of course. It was definitely with that in mind that one thought this was an opportunity. One has the neutrons; let's see what we could do with them in interaction with liquids. He was at the same time working on some other problems of the interactions of solids with neutrons, I believe. At least he and Leona Marshall were out at the Argonne Lab about once a week for a while, while I was working on my thesis topic. By the way, I tried to become a theoretical physicist before taking up experiment. When I first worked with Fermi, I wanted to be a theoretician.

Hale: Really?

Work as Fermi's Graduate Student

Chamberlain: And Fermi said, "Very well, let's see if we can make you a theoretician." And we tried several problems.

Hale: This is with a view to doing a thesis?

Chamberlain: With a view toward doing a theoretical thesis. Now, these were starting problems. I don't think they were--I'm not sure they were thesis problems. Maybe one of them would have been a thesis problem.

Hale: But this was after he'd accepted you as a student?

Chamberlain: Well, yes, it was after he had accepted me as a student, the acceptance always being implicitly qualified as long as we were getting along all right and looked as though we were making progress all right.

One of the problems that we were working on was the scattering of slow neutrons in deuterium. Now, the scattering of slow neutrons in hydrogen had already been taken care of, and there was something published about it, or I had written a paper, at least. I think it was published. So it shouldn't have been terribly difficult to extend it to scattering of deuterium. Well, I worked on this and was kind of mired down in the details, and I was having terrible trouble keeping my orientation on the whole problem. I'd get lost on a section, and then I'd forget how that section fit with the others. I was very poorly oriented in the problem.

And then out came a paper by Schwinger and somebody, giving all the answers to the thing I was working on. Well, the thing that bothered me was that I couldn't even read Schwinger's paper. I don't think I understood at the time that Schwinger writes in a very abstruse fashion; it's not so easy to read Schwinger's papers anyhow. But I was very much impressed with the fact that even though I'd been working on the problem for maybe six weeks, I still couldn't read it and understand this paper.

So I went to Fermi and I said, "Maybe I should give up trying to be a 'theoretiker' and turn to experiment." He said, "Yes, I think you should." [laughs] That's when we started on the neutrons in liquids.

Hale: Did he have any prejudice before that to imply that he didn't think you were going to be a theoretician?

Chamberlain: Oh, I think yes. I think it implied that he didn't think I was going to be a theoretician. But he wasn't yet pressing the point. Probably he wanted to give me a little more chance to show whether I could do the theoretician's line of work. I think he was quite pleased with the idea that I might come to that conclusion myself, rather than feel that the decision was forced on me by him. So it worked out very well, and I think it's been great.

Oh, there was one very important course that I took at Chicago which I shouldn't forget, and that was a course from Leonard Zener, the inventor of the Zener Diode. It was called "Introduction to Solid State Physics." Among the courses that I've had, leaving out Fermi's seminar, it was about the best. Sitting in the front row most days in this course were Geoff Chew and Murph Goldberger and myself. And we took turns getting Zener to justify statements that he made. You know, Zener would say, "Well, it can be shown..." that something or other--and one of us would put up our hands and say, "But Professor Zener, how do you show that?" And then the whole day's lecture would go off in a different tack. He hardly ever finished a lecture the way he intended when he started into it because we'd ask him questions and he'd divert and try to prove this. Sometimes it would turn out that the theorem wasn't true at all, and sometimes it was true but he couldn't prove it.

It was absolutely a marvelous course. We learned so much. He gave problem sets that we thought had no conceivable answers, and we could go to complain to Zener

that there weren't enough data given with the problem to solve the problem, and Zener would say, "Of course there aren't enough data! Say, do you expect that when you get a problem in the lab that it comes with a list of the data you need to solve it? You have to make the obvious assumptions. You have to look up the data that you need." [laughs]

He said, "You've got to work with what you've got." And I think that spirit that you have to work with what you have is the kind of thing that we got out of that course that was very valuable. We learned a lot about solid state physics that didn't seem to be in agreement with experiment. But in the years immediately following that course, they started to get purer and purer solids; with purer and purer samples of germanium they began to look more and more like Zener's theory. So I had an excellent introduction to solid state physics before solid state physics had really gotten a good start. And that was great.

Hale: I gather that Fermi was famous for spending his lunch hours with graduate students as well. That's spending a lot of time with students. Is that correct?

Chamberlain: Well, we always ate lunch with Fermi. Just about every day we could, we ate lunch with Fermi, and that was most days, I guess. I don't think we realized at the time how exceptional that was, but we certainly enjoyed it.

Hale: You mean amongst professors?

Chamberlain: Amongst professors, yes. Most of them ate over at the Quadrangle Club by themselves, I suspect; but Fermi ate with us at the student cafeteria.

Hale: Do you think that that was just an essential part of his character that you mentioned before? He seemed to be rather retiring and inconspicuous and didn't force himself, didn't lead with his chest like Lawrence, I believe you mentioned. Was he, then, very unassuming in that way, do you think?

Chamberlain: Yes, I think so. I don't know what exactly that had to do with the lunch conversations.

Hale: I mean, did he have a genuine interest in communicating with you and the other students?

Chamberlain: Well, I think so. I certainly say yes, and yet somehow I don't think that puts one's finger on just what it was that motivated him, for instance, to have lunch with us.

Whatever it was, he liked it and I'm not quite sure what it was that he liked.

I remember that Leona Marshall and Joan Hinton were almost invariably at lunch with us when Fermi was there. The rest of the crowd was a little bit more variable. I don't think I ate every lunch with Fermi by a long shot, and I think sometimes we'd join his table and sometimes we didn't.

We discussed a lot of interesting physics problems in the course of this. I remember one of the rare times that Fermi drew on something that wasn't in his standard bag of tricks. We were talking about what happens when you throw a fifty-cent piece in the air and how it does a combination of tumbling and rotating and whatnot. Then he brought out a theorem which I'd seen in mechanics texts which refers to something like the Polholt rolling on the Horpolholt. It's in the advanced physics texts, but we very rarely used it. That's one of the very few times when Fermi drew on something that I didn't consider to be in my standard course material. But the great fun with Fermi was that he could use the standard methods to solve such an array of problems. Tremendously masterful intellect.

Hale: Would you say that he almost had an answer for everything? Any question you could ask?

Chamberlain: Well, he recognized various limitations. For instance, we had a rule laying about physics called the conservation of parity, which we thought was a law of physics. Parity is always conserved. When you asked Fermi about the law of conservation of parity, Fermi would say, "It isn't proved." And he then made certain mistakes from that point on. He apparently knew that it was different from the other rules, but he himself didn't fall upon all the answers.

He said, "You can't prove the conservation of parity because you can't turn the world inside out." It indicated that he knew there was something different about the conservation of parity, but he didn't have it quite right, as a matter of fact. It was interesting that he would at least recognize that the conservation of parity wasn't proved. I never did understand the conservation of parity until Lee and Yang explained the conservation of parity. I would have been a big step ahead if I had realized what non-conservation of parity looks like in the real world.

Hale: They showed you hypothetical examples of non-conservation?

Chamberlain: Well, it turns out they are in fact real examples. Things are so simple in retrospect and so difficult before you understand.

Hale: Well, now, they were there at that time? You worked with both Lee and Yang?

Chamberlain: No. My office was next door to Yang's office in Eckhart Hall, and I learned a lot from discussions with him and Murph Goldberger. Yang was the same age student as I. He started his graduate work at Chicago about when I did. Yang and I were sort of contemporaries. T. D. Lee was several years younger, maybe three years younger.

Originally I didn't think Lee was very smart because he asked me some questions around the lab that I thought were stupid questions. What that meant, in fact, was not that they were stupid questions; they were very good questions. He'd come to me with this thing in his hand and say, "Is this a resistor?" "Yes, of course that's a resistor." I had seen and worked with a lot of resistors, so I knew what a resistor looked like and recognized kind of the standard color coding for a resistor, and I was in no doubt.

But, of course, to him as a theoretician who'd never seen a resistor before, it was a perfectly good question. There was nothing stupid about it, and I shouldn't have classed it as a stupid question. I guess Jack Steinberger was the one who first recognized that T. D. Lee was this real smart fellow.

Hale: When you say "around the lab," what do you mean by the lab?

Chamberlain: Well, I had been around in places like Los Alamos and had worked with experimental equipment in Berkeley; any lab, I just meant in this case; I'd been around a lab.

Sam Allison and the Institute of Nuclear Studies

Hale: I see. What were the physical facilities like at that time? Did the Institute of Nuclear Studies exist at that time on paper, or didn't it exist until buildings were--

Chamberlain: I think the Institute of Nuclear Studies existed from just about the time I got to Chicago. I think it was formed even before I got to Chicago because when Sam Allison was handing out these fellowships, I'm sure it was connected either with the Institute of Nuclear Studies or something that was definitely turned into that. The Institute of Nuclear Studies was an attempt to give Fermi a good opportunity to do more or less what he wanted to do academically without too many administrative chores. So Sam Allison agreed to do the administration, and Fermi was so respected by everybody that Fermi's decisions were taken in everything, without anybody really raising much question.

I wasn't really present, but they said that in the staff meetings in Chicago, some question would come up and they'd just--everybody would discuss it for an hour, then finally Fermi would speak up and say, "Well, I wonder if we couldn't do something or other." He would make some suggestion, and then there'd be no more comment. The meeting would break up because everybody would assume that *that* was to be done.

Hale: You mean university staff meetings?

Chamberlain: I mean physics department staff meetings, probably. Possibly Institute of Nuclear Studies staff meetings. I'm not absolutely clear on that. But in either one, Fermi was awfully influential. If he had an opinion, people tended to go along with him because he had a lot of wisdom. Many of these things were questions about what kinds of facilities to build up and how to use them, so forth, where Fermi's input was terribly important.

Hale: What facilities did he have? A couple of offices, or what? You went there before the new buildings were built, whenever they were--sometime later, wasn't it?

Chamberlain: Yes, yes.

Hale: Forty-seven or something like that.

Chamberlain: Oh, I'm pretty sure Allison's office was in Eckhart Hall; and Fermi's office, if it wasn't in Eckhart Hall, it was in the next building.

Hale: Did he have laboratories at his disposal and things like that?

Chamberlain: Well, Sam Allison had a low-energy laboratory in the basement. Yang started to be an experimentalist, and he

broke so much glass, Sam Allison threw him out of the laboratory, and he had to become a theorist. He wanted to be both, I guess. He thought he was going to return to China and would have to be able to do everything.

Hale: What facilities did you have during that time? Did you have a desk or office?

Chamberlain: Yes, I had a desk in a room with Geoff Chew and Yang. I can't remember whether Goldberger had a desk in the same room or not. Joan Hinton was down the hall. This was the top floor of Eckhart.

Hale: Was that the physics department?

Chamberlain: Eckhart is more the mathematics building, I believe. Physics was supposed to be in the next building, but under this pressure, physics had overflowed a little bit into Eckhart. I think most of physics was supposed to be in Ryerson, and we kind of overflowed into Eckhart Hall, with an extra crowd of people at that time. It was a lot of fun being in the same room with Geoff Chew and Frank Yang. He pronounced it "Yun," I believe. People would get Geoff Chew mixed up with Frank Yang because it sounded as thought Geoff Chew was going to be the one of Oriental extraction.

Hale: Yes. So you must have formed close relationships with those people.

Chamberlain: Well, the closest relationship was, I guess, with the Goldbergers. There were a lot of outings on Sundays in which the Goldbergers' car was used, as I didn't have a car. We spent a lot of time with them. Quite a lot with the Frankels also.

Hale: Did your social life pretty much revolve around physics and the university?

Chamberlain: Pretty much. The Goldbergers were very important, the Chews somewhat, the Marshalls. Among the professors, Fermis and the Mayers--Maria Mayer and her husband, Joe. He's been a chemist but he was, even so, made president of the American Physical Society, unusual for a chemist. In the latter part of that period, the Wattenbergs were very close with us because the Frankels left the university, and the Wattenbergs moved in above us in this two-flat, and we were very close with them. Al Wattenberg and Shirley. I still see them once in a while.

Hale: Did you have, yourself, any specific outside interests or, you know, hobbies or things--

Chamberlain: Very few. I've been a little peculiar in that I haven't really had a whole lot of hobbies. I guess I find that physics, with its various aspects, fills an awful lot of those needs, you know. The gadgetry of physics relieves me of much need to be a gadgeteer on the home front, and the intellectual challenge of various aspects of physics covers other needs. I don't really pursue other hobbies very much.

Hale: What about political activity at that time? Did you have any affiliations or anything? You still associate with the FAS?

Chamberlain: Well, more loosely at that time, and I didn't really get deeply into the Chicago organization that you think I might have. I guess I was kind of concentrating on graduate studies. There wasn't a heck of a lot of time available. It was marvelous to be a purely physics student. It was just a great period. I was eating it up, enjoying it tremendously, the best possible situation for me. I had few responsibilities and best chance to learn.

The period at Los Alamos had given me a certain amount of know-how that would allow me to determine what I wanted to know, what I wanted to study, what I considered important, and how to fit things together. It was very helpful. So when I returned to graduate work, it was with a lot of enthusiasm, and by that time I had been pretty well primed for it. It was a great period, but it didn't leave much room for political activity just because we were awfully busy.

Problems with Chamberlain's Thesis

Hale: I see. How long did your work on your thesis take once you started it?

Chamberlain: Well, I went there in March of '46, and I suppose I was preoccupied with the courses about a year, and by September of '48 I supposedly finished the experimental part. Fermi thought it would be okay for me to go to California as a young instructor. But I didn't really finish the thing. Something like December of '48 I went back to defend my work before a thesis committee, and they didn't like so much what

I had done, so they sent me away to do more. I didn't really get the degree awarded until December of '49. I'd forgotten how long it took me to pass that exam. Probably it was September of '49 before I passed that exam.

Hale: That's the defense of the thesis?

Chamberlain: Yes. The main impediment seemed to be Herb Anderson. He was right, however. The work was a good deal better once Herb Anderson had criticized it and I had responded to that. I remember I was doing 32-point Fourier analysis sort of numerically by hand, which was considered unusual. We didn't have computers at that time, and doing these by hand was quite a step. Actually, I just used methods that were published, but they were published in obscure places like the *Journal of the Franklin Institute*, so they weren't very well known.

I've forgotten who put me on to that. Must have been Fermi, but I'm not sure. Nowadays we do Fourier analysis with the help of a computer, and in the last six years there's been a new fast Fourier analysis method found, which goes lickety-split, and we can do thousand-point Fourier analyses with no difficulty. At that time, thirty-two points was doing very well.

Hale: Yes. And so his criticism was on that?

Chamberlain: Well, his criticism was that I had left all my data together from different runs and therefore you couldn't tell which aspects of my data were reliable and which aspects might be the results of imperfections in the data or noise or something. The main thing I did as a result of his criticism was to break down my data into three periods of running and to analyze separately each of these three periods to find out which features of the results could be relied upon, because they showed consistently in all these analyses, and which features were a little chancy. You know, when you see a wobbly curve, you can start to see peaks in it and you want to know which of the peaks are real and which are fortuitous, and this was a step toward doing that.

The good theory of my work didn't come out for a few years, when Van Hauge made the theory, I think, correctly. I think he made a better fit. At that time I could only copy what was done for X-rays. I didn't realize the essential differences between neutrons and X-rays. So my

analysis was a little bit simpleminded, but it served some purpose.

More about Fermi

[Interview 4: August 13, 1976] ##

Hale: Were you in any sense privy to Fermi's deep involvement at that time in national policy on nuclear questions on his position on the general advisory committee?

Chamberlain: I don't think we really were. I can't remember Fermi ever discussing, say at the lunch table, anything that I could attribute directly to his work as a national advisor. Oh, I'm sure some questions came up, but we talked mostly in a kind of common sense vein. Fermi tended to be, in many cases, moderate in his views, and I think tended much less in that field to stick doggedly to a certain point of view. My impression was he seemed to be more of a compromiser in these national problems.

Now, I know I've been told by Segrè that Fermi, in his last year of life, felt rather regretful about the way he had handled the whole business of the Oppenheimer hearings. I think he felt that he'd kind of known that something was wrong with the way that had been handled, but he regarded it as rather political and sort of kept hands off and didn't really try to influence the situation so much. I think he regretted that later, as he saw it play out.

Hale: He felt, he thought he should have taken a stronger stand?

Chamberlain: He felt he should have taken a stronger stand. Segrè might comment on that sometime. I'm sure Segrè knows and remembers the story much better than I do.

Hale: I do have the impression, from comments of various people like Oppenheimer, that Fermi was in some sense rather prim on those questions and rather conservative in many ways. "Cold and clear," as I think John Manley described him--and in many senses possibly like Lawrence in that way. Detached.

Chamberlain: It's funny. In the realm of physics Fermi was quite incisive in his logic. It was very difficult to dispute his physics in most cases because he had a very incisive mind.

In the realm of political things, he seemed more to see some validity to both sides of a situation. In part, I think he was very uncomfortable with political problems and tended to back away from them, stay away from them. One way of avoiding involvement was to take neither side particularly strongly, I suppose. I certainly have the impression that he could have had much more political influence if he had chosen to wield it a little bit.

Hale: Of course, Lawrence had a lot of influence; only in many senses he was apolitical, or he supposed that he was apolitical.

Chamberlain: I think Fermi felt he was apolitical. I'm sure I questioned that at times because he seemed to be coming out so much on the side of conservatism; routinely, I thought.

But one thing I might say about Fermi's personality, if I didn't say it before: my first impression was that Fermi had such a measure of self-confidence in physics that he really didn't care too much what his students did in physics. He certainly had made his own reputation, as we all know. But after working with him a couple of years, it became apparent that he did take quite a bit of pride in what his students did. He remarked, for instance, that the students of Robert Oppenheimer had quite an influence on physics of the day. Actually, Fermi students really had more influence later on, I think, mostly after Fermi's death, and if he could have seen how his students did, I think he would have been quite pleased.

Hale: In other words, he wasn't quite as retiring as he had first seemed to be, in that way?

Chamberlain: Yes.

Hale: What would you say were the major points of comparison or distinction between him and Lawrence, for example?

Chamberlain: Well, their spheres of work were so completely different. Lawrence I think of as the great promoter and enthusiast and as a rather demanding leader of the laboratory effort. I think Alvarez is right when he says that in science Lawrence to some extent invented the team effort.

Fermi, on the other hand, was simply a fantastic intellect. His intellectual power was something to observe. I felt as though I had never seen, never met such an intelligent person anyplace else.

Fermi was certainly of the school of expecting physics to be an individual effort or the effort of a very small group of physicists. The large team wasn't anything that Fermi thought of as natural. He might take on a big project that needed lots of helpers, but in fact it wouldn't be so many helpers as a general rule--more of an individualist, a great intellect.

I had the distinct impression that the team of Segrè and Fermi was particularly good because Segrè is very imaginative in posing questions, and Fermi is very imaginative in answering them. Those two together amount to much more than the sum of those two working individually because Fermi is being able to answer questions that Segrè proposed. Still, Fermi needed to have those questions laid in front of him.

I saw some examples, I think, of a good productivity when the two got together, but their styles reinforced each other tremendously. Of course, I don't know the nature of their interaction during the early work. There's a famous Fermi-Segrè formula in atomic physics, and I don't know how that occurred. But seeing them interacting in later years, I can sort of imagine that Segrè posed the question, and I'm almost positive that Segrè would not have been able to manipulate the theory so well as to do most of the work of finding the answer.

Hale: So synergy came into play.

Chamberlain: Yes.

Hale: I see. Did Fermi let on very much his opinion of other physicists, or of politicians, or people in the news on nuclear questions?

Chamberlain: You know, I'm having a little trouble remembering the answer to that question. I think we knew his opinion of many other people, but by listening for relatively subtle comments. I can remember, I think, Fermi very occasionally making a comment such as, "Well, he isn't the greatest physicist in the world after all," or something like that, meaning he had a very low opinion of somebody. But that was rather rare. I think most of the time we knew it by very subtle things. I don't feel so secure in my memory of that point that I certainly wouldn't contradict somebody else that gave a different impression.

Postwar Comprehensive Examination at Chicago, 1946

Hale: I gather that there was something rather special about the comprehensive examination at Chicago.

Chamberlain: Well, we called it the "basic exam." I'm sure that's the one you're referring to. I think that the first time that the basic exam was given after the war, which might have been something like summer of '46, that of the eighteen students who took it, nine passed and nine failed.

I think most of the students who passed it, and maybe some of the others, had been at a place such as Los Alamos or the MIT Radiation Laboratory, where they'd learned a lot of physics during the wartime period. Both were very active places for physics of one kind or another. We came almost as a young instructor might take the exam, instead of graduate students. I don't know what it was. We had at least gotten enough sophistication so we understood, most of us, what constituted an appropriate answer or a full answer to some of the questions that were proposed. In some cases we understood that a problem was basically a simple one. It could be made complicated. I think we had a kind of savvy about how to go about these things that younger and less experienced students might not have had.

The next time that the basic exam was given, a number of people passed. But I remember that it was widely rumored--and I thought correctly--that Teller had made the remark that if it had been graded on as difficult a basis the second time the exam was given, only one person would have passed. We knew who the one person was. It was Jack Steinberger, who actually flunked the first exam. I was quite proud to have passed an exam that Jack flunked because he's a mighty intelligent person. I had lots more experience. He didn't have the same benefit of that rich Los Alamos atmosphere when he took it. Now Steinberger stands out as a remarkable physicist.

Hale: Wasn't Fermi very zealous in input to examinations like that? Was he very interested in posing questions?

Chamberlain: Let's see, did I know which questions? I may not have been sure which questions Fermi had proposed, though I remember one question that seemed to have Fermi's stamp, and I must have known that Fermi proposed it. The question started, "A hole is drilled to the center of the earth by a method which

should not concern the student." So then his question was, "Describe the behavior of the atmosphere as it goes in the hole." The point of the question was that air, as we know it, behaves like a gas. But, of course, it would become more like a compressed fluid once it gets to a density comparable with the density of water or liquid air.

He wanted to see whether people would blindly follow the gas formula to an absurd condition at the center of the earth, or whether they would recognize that once it got condensed as water, things were going to be different. I think that was a typical Fermi question. But I didn't know whether Fermi had proposed other questions or not.

I certainly remember I had a completely upset metabolic system that week of that exam. I mean, I was just sick: couldn't sleep, couldn't digest my food. It was a four-day exam, given Monday and Tuesday. We had Wednesday off, which did not do a bit of good because I couldn't study and couldn't sleep. And then the rest of it was on Thursday and Friday.

Each day we had eight hours, and the questions were adequate to keep us fairly busy, but by eight hours we'd completely done everything we were going to do, and we weren't pressed for time. I remember the last day, maybe the last two days, we had only four questions for the whole day, and as we made two hours per question, that was as much as we could use. I think that's the way that a good exam should be given. I think it showed what we could and could not do.

Hale: Fermi probably would think that you might have a need for five minutes, anyway, to do the question, if you really knew the answer.

Chamberlain: Yes, but Fermi would be glad to give me two hours to do five minutes of work, to see whether I could or couldn't do it. I think he, too, recognized the value of an exam in which people aren't very much rushed. The fact that Fermi could do it in five minutes didn't necessarily mean that I could do it in five hours [laughs].

Hale: Did you maintain a lot of strong contact with people at Berkeley--for example, Segrè--and were you aware of what was going on here from that distance?

Chamberlain: Only fairly loosely aware. I'm a very poor correspondent, and I hadn't maintained very close contact with Segrè.

Segrè did come through--I don't remember in detail--but it was typical at that time. Segrè went to the East Coast once or twice a year, and it was a good bet that he always stopped in Chicago for a day. I'm sure we renewed contact on some of those occasions, and I believe at least once I sat in on a discussion between Emilio and Enrico.

Hale: Apart from your personal association with Segrè, did you learn what was happening here at Berkeley in terms of machines?

Cyclotron, Synchrotron, and Linear Accelerator

Chamberlain: Oh, I think we kept up on that pretty well, must have, because it was common knowledge and common gossip and we were all talking about it, so I think we felt moderately up-to-date. There were certainly some discrepancies between my mental vision of these machines and the machines that I actually found when I got here. For some reason I hadn't realized there was such a concrete shield around the machines. At the 60-inch cyclotron, there was a shield all right, but most of the time we worked within the shield, as I remember.

Hale: The cans of water?

Chamberlain: Not within the cans of water.

Hale: Wasn't it that it was operating at such low power?

Chamberlain: Well, the 60-inch cyclotron made a lot of radioactivity. It was a relatively high-power machine in the sense that its beam went continuously rather than being pulsed.

Well, this is actually unimportant, the discrepancy between the way I visualized the 184-inch cyclotron and the way I found it. I think we were up-to-date on the performance of the machines fairly well, though I guess there were some things that I hadn't kept up with. For instance, there was a time at which I don't think I realized that the 184-inch accelerated principally deuterons, but that's not so important.

Hale: Did you know about the other machines? McMillan's synchrotron? And the linear accelerator?

Chamberlain: Well, McMillan's synchrotron, I must say, I felt a little bit badly that I hadn't invented it myself, because I realized, first of all, that I had all the ingredient information so I could have invented it myself if I had been careful enough in following the things that I knew and piecing together the right information.

Hale: You don't mean by that the principle of phase stability?

Chamberlain: Yes, I do. I was interested in the principle of the synchrotron, and I felt that I should have been able to invent the thing.

Hale: Why do you say that?

Chamberlain: Well, while working around the 60-inch cyclotron, I had tried to discuss with whatever physics friends that I had--I've forgotten who they were at the time--what would be the consequences of frequency modulating the cyclotron. What could you do if you frequency modulated the thing? I was getting close to the concept of adjusting the frequency as the particles moved out, but the phase stability was really not coming clear to me at all, so I was really unable to make a satisfactory prediction of what would happen if I were to frequency modulate.

Hale: You never sat down and thought it out?

Chamberlain: I understood the vertical focusing in the cyclotron, I believe. And I was coping with the radial focusing on a perhaps primitive basis, but on some kind of basis. But the phase focusing was unclear, and I don't know what the missing concept was. Probably the missing concept was the recognition that if it didn't focus, it wouldn't accelerate a reasonable number of particles at all. It would in effect never accelerate anything if it didn't focus. I think that was the note to concentrate on the focusing. I saw the focusing as an advantage, but I didn't see it as essential to the operation of the machine.

Hale: In other words, you didn't see that you didn't have focusing in all those various ways?

Chamberlain: All the particles would disappear. That's it.

Hale: It's a very simple concept.

Chamberlain: Yes, it is.

Hale: And you might have thought about it more or somehow tried to work it out on paper if your attention had been directed entirely towards that?

Chamberlain: As for the electron synchrotron, I wasn't as much in touch with what that machine was doing as I guess I should have been. For some reason, I was more interested in the proton or deuteron accelerator.

Hale: Does that mean to say that you had in mind very much the field of high-energy physics itself, though you were working with neutrons?

Move to UC Berkeley, 1948

Chamberlain: Just the fact that you're getting into an unknown area was very exciting. Well, you know that Wolfgang Panofsky is a much respected physicist that we all admire a great deal. When I came to Berkeley in the early fall of 1948, Panofsky was working on proton-proton scattering at 30 MeV, and here I had the opportunity to investigate proton-proton scattering at ten times the energy.

Well, there wasn't any doubt in my mind that we're going to find out a few things that nobody knew, although there were predictions of what the scattering would look like. We correctly suspected that those predictions were not going to stand up very long because they were based on much too crude a knowledge of what might be going on. And, indeed, we went through a period of a year or two in which every three months we'd measure one more point on the curve. It would confound the theorists once again because they couldn't believe that this cross-section, which was supposed to be large in the forward and backward directions and looked small at 90 degrees, could actually be so level.

In fact, the differential cross-section was almost constant over a wide range of angles, and each time we extended the region over which this constancy appeared, we sort of had to go back to the drawing board to try to find some way to make that fit with their other pieces of information.

Hale: Was your coming here fixed in your mind well in advance, or did you think about it suddenly?

Chamberlain: Mainly, I had opportunities to go to Harvard and Berkeley, and I was leaning toward Berkeley because of some friends here and the accelerator facilities here. But I think it was Fermi that made up my mind. I asked Fermi where he thought I ought to go, pretty much with intent to follow Fermi's advice, and he thought I'd be better off at Berkeley. And I think this worked out very well.

Hale: What would you imagine that you would have done if you had gone to Harvard?

Chamberlain: I have no idea.

Hale: What did they have in the way of machines? Did they have a cyclotron?

Chamberlain: I can't remember the timing on the Harvard cyclotron. I believe it was built very shortly after the war. It was a fairly low-energy machine, maybe 120 MeV or something.

Hale: Would it have been very much like the 60-inch?

Chamberlain: Bigger than the 60-inch, appreciably, but quite a bit smaller than the 184-inch.

Hale: And it was a cyclotron, not a synchrocyclotron?

Chamberlain: It was a synchrocyclotron. Now, it was too low in energy to be outstanding. Most of the interesting Harvard work in particle physics was done at the Brookhaven Laboratory. And the cosmotron there certainly was operating in the summer of '53.

Hale: That's when you went there?

Chamberlain: That's when I went there and worked with George Snow and Oreste Piccione and Leon Monensky.

Hale: So you decided to come to Berkeley. And what did you find essentially when you got here?

Chamberlain: Well, I must say, the atmosphere was completely dominated by Segrè. They had done quite a bit of work on neutron-proton scattering, and here was an opportunity to follow this with proton-proton scattering. It was sort of lying there in front of me. Clyde Wiegand was about to start on it, and Segrè thought it would be a good thing for me to do. It was sort of left in my lap. Fortunately, I think we started a good collaboration right from the start.

The runs were eight hours long. Nowadays, runs go more for a week or ten weeks. Every Thursday, from eight in the morning until four in the afternoon, we seemed to have the cyclotron for our proton-proton scattering studies. The first half hour at least, or a little more, went into lowering our apparatus by crane into the little experimental area there, and maybe shifting it a little until it got lined up properly.

Then we did our work, which consisted usually of testing one new counter, or more likely measuring one new scattering angle, always returning to our standard scattering angle at 90 degrees for comparison. We were quite afraid we might get different answers different weeks, so we always included at least one angle where we could think of this as a comparison of the scattering at one angle compared to another.

Hale: I don't see any papers before 1950. Did you have a period of building the apparatus and getting the gadgetry together?

Chamberlain: Well, I guess so. For one thing, I did have to put in quite a lot of effort on my own thesis in the first year that I was actually at Berkeley. So while I wasn't idle in the lab here, it did put limitations on my rate of progress. I think the main point was that, as far as I can remember, Clyde Wiegand and I started doing the proton-proton scattering, reasonably carefully looked after by Segrè, sometime during that first fall.

Initially, we had to make sure our counters worked, and we had very few angles measured. I don't think we published until we had several angles measured and had tried rather hard to get absolute values to work cross-sections. We had a different cross-section measured on the East Coast and the West Coast for similar experiments. Looking back on that, we were foolishly concerned about the disagreement. We could have ignored the rest of the world to advantage, just gone on doing our own work. Instead, we put a lot of effort into finding out why we disagreed with the results done elsewhere.

Hale: Who was wrong? You were right?

Chamberlain: Well, looking back on it, it seems, yes, we were right. It was hard to see why we spent so much effort on it because the calibrations of the cross-sections done in the East were, first of all, rather indirect and, secondly, were

based on an earlier Berkeley experiment which itself turned out to be not as accurate as was hoped.

It was a little bit off--I've forgotten what it was--so that in effect we were checking the wrong place. If we'd wanted to follow the discrepancy to its real roots, we would have repeated that Berkeley experiment. Anyway, we went to some effort to get an absolute cross-section before we published too much.

Hale: You started to publish in about 1950.

Chamberlain: Some of that may have been submitted in '49, and it takes a while beforehand. Of course, our burst of activity really started with the polarization experiments coming into the picture.

First Teaching Responsibilities in Physics Department

Hale: What were your teaching responsibilities?

Chamberlain: Fortunately, the first teaching assignments that I had were rather easily manageable. They were fairly easy assignments. I think the first semester I acted more as a teaching assistant than a young instructor in the elementary physics class that's given for the pre-med people and to architects. The intermediate level of the elementary physics. It would now be called Physics 6, I guess.

Actually, I didn't do quite what I should have in that case. I foolishly tried to help my students with a more advanced way of looking at things than they were being given in the lectures of that course. See, this was a question of meeting with a recitation section of about twenty students, whereas the lecture was being given to, let's say, 120 students. That might have been typical.

In the lecture with 120 students together, they were getting a more elementary approach than I was trying to give in my recitation section. In fact, my recitation students weren't helped very much. I was doing them a little bit of harm not realizing it because I should have gone more to listen to the lectures to see just how things were being handled. I didn't realize how important that was. I thought if you knew physics you could do this job. One

learns sometimes by making these mistake and hopes to do better in the future.

Hale: That was your first teaching experience?

Chamberlain: I think that was my first teaching experience, really. I had never been a teaching assistant as a graduate student. I had a fellowship from Dartmouth when I came to Cal as a beginning graduate student, and I really had a fellowship at Chicago. While I sort of liked the idea of teaching, I really hadn't done it.

Then one of my other area assignments was in what we called at that time the 110 laboratory, now called the 111 laboratory. That was a funny little mixup because I think Professor Birge, who gave me that assignment, felt that it was a rather easy assignment, and I felt it was rather hard and rather time consuming. That was an unfortunate disparity between the opinions.

Hale: This was the electrical laboratory?

Chamberlain: Yes. It had two parts at that time. One was called the electricity and magnetism, and the other was called modern physics. Actually, I've always enjoyed the laboratory, as it always helped me to keep a firm connection between the textbook material that I was reading in the books and its real meaning in the world of physics. So the lab always gave me a benefit.

I remember when I first worked in that lab as a student, I got a great benefit. I was having a little trouble understanding quantum mechanics, and at least some of the quantum mechanics came into sharp focus when I saw the results in that lab compared with what was in the textbook. I began to understand the chain of events between observing something and starting to get an explanation that fits with the theory. I liked working in the lab very much, and I learned things, in fact, there in the laboratory that helped me a great deal.

I remember we studied the electromagnetic modes of oscillation in a cavity. That was important later on in the polarized target work. It came right out of that laboratory, and that's the kind of thing you run into when you combine teaching and research. The teaching ends up reflecting well on the research because you get exposed to a lot of problems you wouldn't think to discuss otherwise at all. Keeps your mind roving into new areas.

Hale: Do you think that you've developed a healthy respect for teaching, whereas many professors demean it, rather?

Chamberlain: Well, I had a particular attitude which I doubt is shared by very many of my colleagues currently. My attitude was that I earned my living by teaching, and one of the ways that I was paid for my teaching was to be allowed to spend time in the laboratory. This led me to take my teaching career seriously. I didn't always teach well, but I always tried hard, and with experience and comments from students and so forth, you can gradually improve.

Hale: Birge, in his *History of the Physics Department*, records that you were supposed to have done a very good job. I don't know whether he means that *he* thought you did a very good job or if he heard of students that you did a very good job.

Chamberlain: Well, I think he based his opinion in part in talking to students. He was, especially at that time, in touch with a great many students. I think in retrospect Birge was extremely helpful to me. I think he gave me an excellent opportunity, and I think he wanted to see me succeed in both teaching and research. He tried to avoid teaching assignments that I was going to have any particular initial trouble with. I think he recognized that I had some work to do to finish up my thesis work at Chicago.

It would have been wiser for me to stay in Chicago for another six months at least, but I didn't realize until later that that was the case. I'm afraid that I have to admit that we still make that mistake. We'll send a student off a little too soon or let him pressure us by accepting a job that he's promised to show up in the fall, and we know he's not quite ready to go. Sometimes we live to regret it.

The Radiation Laboratory

Ernest O. Lawrence

Hale: I wanted to ask what impression you got of the relationship between the physics department at that time and the Rad Lab. Were they recognizably separate, or what? Was Lawrence sort of overwhelming?

Chamberlain: Oh, yes, they were. They were recognizably separate. It was clear that Lawrence had a kind of sub-department going, and a lot of people were certainly worried about the tail wagging the dog; that is, the Radiation Lab part having almost an undue influence on the physics department. I believe there was a feeling that Ernest Lawrence had almost a cadre of physicists kind of working closely with him. It was sort of assumed that a certain group of people--including Luis Alvarez, Bob Thornton, Ed McMillan--were inclined to vote in physics department matters pretty parallel to Ernest Lawrence.

I'm sure they were in very close contact with Ernest Lawrence and kind of shared his views. The first Radiation Lab people that we felt had a kind of a more independent attitude--and one certainly couldn't assume that they were going to take action very parallel to Lawrence--were Segrè and later myself. Segrè, of course, was awfully influential on me. I probably used Segrè as somewhat my measure of what I could get away with.

Now, Lawrence never questioned my attitudes on any physics department matter. My attitudes weren't all that important. I was certainly non-voting for a long time. In fact, at that time most of the decisions that amounted to anything were really taken by the full professors in the department, without reference to the younger professors very much.

I was, in those early years, an instructor, not even an assistant professor yet. In recent years, the assistant professors have much more influence on who might be invited to come to the department as a new staff member or something like that. But that's rather recent years. So Lawrence had no very strong reason to concern himself with my view or my vote because I was busy.

Hale: Did he ever press you in any sense as to what your opinion, what your attitude toward the Rad Lab would be? I mean, research-wise. Obviously, you'd kind of depend upon him and his machines for your research.

Chamberlain: Sure.

Hale: And so he had a trump card there.

Chamberlain: Yes. Well, I might say Segrè's salary had come completely from the physics department during at least the school year, with the exception of his summer salary, whereas many of the

other people working at the Radiation Lab had half and half or one-third, two-thirds salary arrangements during the academic year. I requested to follow the same pattern that Segrè had had: have my nine-months salary come completely though the physics department and not through the Radiation Lab at all. I think the reason was that I felt that it gave me a little greater independence from potential requests by Lawrence to follow some particular applied pattern.

Now, my mind wasn't closed to doing applied physics, but I had a hunch that some of the projects that Lawrence might want me to work on I would find unsuitable either to my talents or my preferences. So this was a kind of a hedge against being asked.

Hale: So you thought about that yourself? It was all very early.

Chamberlain: I don't feel it was an indication of my thoughtfulness about those qualities in my environment. I think it was more a reaction to what Segrè had done. Now, I had one example in slightly later years of Wilson Powell spending a great deal of time measuring the magnetic field within the Bevatron. I thought this was somewhat forced on him by Lawrence asking him to do that. However, other people have told me that he was kind of fascinated by those pulse-magnetic measurements, that he kind of wanted to do it, and I don't know firmly which of these points of view is right.

I certainly had no direct evidence that Wilson Powell was in any way coerced into making those measurements. At the time, I thought it was a shame that he was spending so much effort on that engineering-type problem. I thought he could have been doing better paying attention to what came out of this cloud chamber.

In later years, when my position became tenured, it was the university that guaranteed the tenure, regardless of whether half my salary had come through the Radiation Lab. That's one of the problems the universities had: if something like the Radiation Lab folds up or greatly diminishes in size, then the universities find themselves saddled with tenure salary responsibilities beyond what they actually had been paying. Now, it rarely has happened that a research institute or the Radiation Lab kind of institution collapsed that much. But universities have been worried by that at times, and they hesitate to take on this position where they pay half the salary but are responsible for 100 percent of the tenure [laughs].

Hale: That's rather the same sort of problem as limiting the responsibility for outside projects, such as the MTA--limiting the university's responsibility only to consultancy and advice.

Chamberlain: I think it was particularly appropriate for me to have my salary come completely through the physics department because of my view of the job. I regarded myself as primarily earning my living by teaching, that the research was partly to upgrade my teaching and partly was an activity that was part of my payment. I remember believing that we certainly weren't overpaid inasmuch as I was having a hard time keeping a small family going on the salary. I felt that, in fact, if there weren't this research activity that I would feel very much underpaid and probably would seek out a different kind of work that paid more.

Hale: Brobeck, in my interview with him, mentioned that he didn't consider the Rad Lab as very much an academic place. He remembers he came under fire by people like Loeb, for example, who I gather considered it not to be the most appropriate place for graduate students to do their training. They sort of got into applied aspects at crucial points in their education. Could you comment on how academic you think the lab was?

Chamberlain: Well, certainly this question was raised by people like Loeb and Brode. I think they both felt some opposition and some animosity with the Radiation Lab idea. The notion that one of their physics department, Ernest Lawrence, could set up this sort of sub-department where he had an independent budget and had somewhat dictatorial powers with respect to who was hired and how the account was spent, was something that they were certainly a little bit unfamiliar with and rather suspicious of.

We're all the time criticizing the methods of training graduate students in nearby departments. It's almost endemic to the universities. Certainly, I can remember criticizing Loeb's students' approach to some of these problems. They weren't interested enough in what was going on in the molecular level to find out what ions were present, and how those ions were made and destroyed. But they were busy discussing what sounded to me like engineering aspects of point to plane discharges and crude regimes in which these arcs and sparks operated. Sounded to me as though it was very unfundamental physics in Loeb's approach. Well, the same criticisms could, I'm sure, be reversed.

I think there was a danger for young graduate students; young graduate students spent too much of their time helping to repair the cyclotron as cyclotron operators some of the time, though I don't think that was a way that most of them spent a lot of time after the war. I think everybody took a shift on the 60-inch operating crew at an earlier period. Many of the physicists a little older than I talked about working on the operating crew, and still, nowadays, a few physicists take a turn, half a year or so, on the operating crew of some of our machines, in order to learn the machines. It's not the central part of their training, to learn a sideline that can be useful and perhaps important as broadening of their training.

Hale: But it's an option?

Chamberlain: It's an option. These criticisms of the training crop up all along the way. For instance, there have been many years in which we "counter types" have been rather critical of the "bubble chamber" physicists because they practically never see the bubble chamber in operation. All they get is this film which is sometimes airmailed in from Europe or from the Brookhaven Lab on Long Island. They maybe superintend some scanners, hardly look at the film themselves and do most of their work as computer-based operations on the output. They really don't learn to use anything except the computer. You question their training as physicists, but most of this is grumbling under one's breath rather than actual confrontation with somebody else about how they're treating their graduate students. It's clear we all make mistakes in training graduate students.

Hale: Did you avoid having anything to do with operating the machines? I mean, the 184-inch?

Chamberlain: Well, I did, but there was no special problem. There were regular crews that were operating those machines, and I was under no pressure to join the operating group. In fact, in retrospect, I might have been a better user of the cyclotron if I had spent some time on the crew--or, better yet, I guess spent more time myself thinking about how that machine operated.

The beam came out of the cyclotron in very short bursts. We'd say it had a poor duty cycle. In one second of operation, while the machine was running, the beam might be coming out for maybe sixty microseconds, quite a bit less than a tenth of a percent of the time. Now, we were doing coincidence experiments where two counters were to be

treated differently if they received pulses at the same time rather than at separate times, and for those experiments it usually pays to spread out the beam in time as much as you can.

Well, if I spent more time thinking about how the cyclotron works, I could have contributed to some of those extensions of the duty cycle. I suppose it wasn't until 1960 before people learned to get nearly 100 percent duty cycle out of the cyclotron. We should have done that much earlier. Had we kept our wits about us, we could have; we had all the information to do that decades earlier.

Hale: That was, I remember, one of the things that in some sense put Lawrence off the synchrocyclotron to start with, when it first came out with phase-stability ideas. He was sort of bit down on the idea that, gee, he was getting two protons an hour or something like that. I mean, he was wanting to see huge, bludgeoning beams of protons.

Chamberlain: I think you're right. Yes, I think Lawrence had a prejudice in terms of lots of microamps of beam, and any machine which seemed to give a smaller current, he was doubtful about. But, of course, the synchrotron principle has been awfully important to us.

Hale: It has been voiced in various places that after the war he was sort of moving into the background, away from the laboratory itself because the major ideas of postwar were not necessarily his.

Chamberlain: Lawrence? You know, I'm sorry, I'm not really quite understanding your question. I don't think I know the answer to it, anyway. I don't think I knew Lawrence well enough to know where his central interests lay at that time.

Hale: He was very conservative when the announcement of the intention to build the Bevatron came about. He was rather hesitant in what he claimed could be done with the machine, you know, which was sort of very much against his character.

Chamberlain: Well, I remember he used an argument which we all respected: "If we knew what we were going to find with this machine, we'd hardly have to build it." We almost expect to find the unexpected, and certainly our reason for building it is that we hope to find something unexpected. That made sense to me. I think I read something like that in the newspaper about that time, credited to Lawrence.

Hale: When Lofgren was building high-current cyclotron for injection into that, as a possible means of injection into the Bevatron, he said that Lawrence was extremely interested in this high-current cyclotron, probably more so than the main machine.

Chamberlain: I can't verify that at all. I don't remember that phase. The problem is that the cyclotron makes lots of current, but it tends to spread that current over quite a wide solid angle and quite a wide range of energy so the particles come out a little too spread out and not concentrated enough in what we call "phase specs." So after we got through this period, I think it turned out that the linear accelerators were indeed better injectors for something like the Bevatron.

Hale: I'd like to know how you started forming relationships with other people at the lab--for example, like Bill Brobeck.

Chamberlain: Well, Bill and I didn't know each other terribly well. I think there was a reasonable amount of respect both ways. I certainly respected him.

Bob Thornton

Chamberlain: A person that I came to respect a great deal was Bob Thornton. I always felt that he was just an excellent influence at the laboratory and helped solve a great many problems. He was a genuine "smoother" in difficult times. We, for instance, were frequently running into the trouble of people who wanted to do more on the cyclotron than there was time for. The total things we wanted to do added up to more time than there was in the week.

When this would occur, Bob Thornton was the one who would call a meeting to discuss the problem. He'd pretty much get everybody there to say again what he'd like to do, and the problem got well out in the open: where the conflicts were and who was wanting to do more. Toward the end of one of these meetings, Bob would say, "Well, what would you all think if we went along this line?" And he made kind of a three-way mix between what people wanted to do, what people historically had been doing, and what good physics would dictate, in his opinion.

He pretended not to change the assignments of cyclotron time too rapidly, so he took account of recent past history and didn't change it too much. He tended to give in the direction of allowing a little more time for people that were trying to expand their activities, or maybe a new person would be given some time to get started--something like that. He recognized, where he could, something he thought was going to be good physics and give it a little extra, too.

We tended to feel, when we got through with these meetings, that at least we'd solved the problem for a four- to six-month period and that we'd done so in an amicable fashion. I thought everybody felt good about it, at these meetings. If you had to give up time, you kind of knew whom it was going to, and you felt you had a chance to explain why it was a mistake if you had been asked to give up time that you really thought you should have. I think he succeeded in skirting around any cases where an experiment might have been prevented because somebody couldn't use the cyclotron time somehow. I think many experiments were slowed down somewhat, but I think everybody felt good about this.

Now, I really respected Thornton for his ability to handle this with enough strength so he didn't get pushed around a lot, but enough pleasantness and goodwill, and obviously trying to do an honestly good job in an evenhanded fashion. He became very important. I think the main thing that Bob Thornton did was to give everybody the feeling that they were getting a fair shake so that he improved the atmosphere a great deal. What might have been kind of cutthroat competition, he made a friendly competition.

Now, as far as I know, he certainly had a good influence on the experimental results because he managed to see to it that somehow or other pretty near every experiment that was proposed could be done. I mean, the door wasn't completely closed to anybody. So I think he just made our lives a lot more pleasant, and I think in the process improved the research output.

He was very dedicated to the research output of the whole gang in that he wasn't trying himself, really, to do so much of his own research. I felt this was much more because he chose to give us all a helping hand rather than do his own research, than that he was in any way inept in his own research. I think he felt the need of somebody to act as manager and responded to it, I'm sure, at Lawrence's

request. But partly I felt Bob Thornton was working for me in a way, and in a very real way. He was handling the administrative questions and the administrative problems in order to let me concentrate on research.

Hale: Do you think the processes and the principles that he established were very important for the lab later on when questions of use of machines become more critical?

Chamberlain: Well, no, I don't think that he set the tone for the later situation. As the competition got stiffer, all of the labs migrated gradually in the direction of having some kind of program advisory committee. It's called PAC at many institutions. The director relies on it for advice at many laboratories. Certainly there's one at the National Accelerator Lab.

Hale: Yes, I realize it became more institutionalized, even here.

The Committee System

Chamberlain: Yes, well, that's right. Now, I think one of the reasons that the committee structure was a little bit delayed in Berkeley was that Bob Thornton sort of set a precedent in a different direction. I felt what Bob Thornton did was very valuable, and I felt that probably the step toward a committee in my mind was a step backward, though I would have a hard time making a strong case that would convince others of this. Many people support the committee system.

I felt that it's too difficult to do particularly risky or particularly provocative experiments if you have to persuade a whole committee or a majority of a committee that they're justified. So I thought it was better to have some parts of the accelerator time distributed to a "home team" on an almost automatic basis, for the home team to use as it saw fit, sometimes based on a hunch rather than solid theory.

Now, the closest I can come to an example of what you can do when you don't have the committee system was one experiment which I tried to do unsuccessfully at the 184-inch cyclotron. I wanted to try to study the diffraction of neutrons off nuclear matter. Well, there were lots of things that I didn't understand at the time, so that I'm afraid my experiment was destined to fail for good reason.

I didn't spend a lot of time on it--I think somewhere between eight and sixteen hours of running time.

I put on that problem of seeing whether I could get any evidence of diffraction in a nuclear fluid. What I didn't understand at the time was that I really had to look for this in the inelastic scattering of the neutrons, rather than the elastic scattering of the neutrons. I didn't do it quite right.

I still to this day have a feeling that I don't understand as fully as I ought to why that experiment failed. I probably should have studied the theory of it a little bit more and tried to understand it better. In other words, if I could observe some sign of neutron diffraction, then I should be able to say something about how close a proton resides to another proton in the nuclear fluid.

When they bump each other, how closely do they come? I could get a measure of the correlations of position, one proton to another. When they live with each other, how close neighbors are they? I certainly felt that this kind of experiment was a big enough departure from the kinds of experiments that people were trying to do that it would have been very hard to persuade a committee that it was doable, or that some time should be given.

Now, it's well known that we do lots of experiments on these accelerators nowadays that aren't really okayed by the committee in advance. We work them in along with some other experiments. You know, we have an essential purpose for the experiment; we may have some subsidiary experiments that we can do either at the same time or with very little accelerator time devoted to them.

When we're given the okay to do an experiment, after the committee's given its advice, it's understood that we'll put our main effort on that experiment, unless we come back with some inclination to the contrary. But the main effort might be 80 percent of the effort; we could still use 20 percent for various subsidiary experiments, I suppose, without coming back to the committee for a detailed okay.

But to some degree I've always supposed a committee system suppresses the outlandish or the big departure experiments a little bit. The committee system also forces us to think very critically about what the outcome might be from an experiment. You know, in the committee system, you get asked, "Suppose you get that result. How useful would

it really be?" So the committee system certainly forces you to focus on certain questions, and it's very helpful, I think, in keeping the experiments close to those that are related to current theoretical questions. It keeps the experimenter from ranging too far, too wide. But with the advantages of the committee system go disadvantages. I thought that Thornton did well to postpone the committee system in Berkeley for quite a while, and I think we probably benefitted from that.

Ed Lofgren

Hale: How about other people like Lofgren, for example, who arrived about the same time you did?

Chamberlain: Well, I've never been that close to Ed Lofgren, so that I haven't really had that much interaction with Ed. He's been more concerned with the machine construction and machine operation. I felt that I was more concerned with how the beams could be used in experimental setups. Obviously, I think we have a lot of common interests because of this overlap in those areas.

I've sometimes felt a little bit critical of Ed Lofgren and partly critical of myself in some of these things. For instance, there've been some rather important experiments that have been done in the last four years at the Rutherford Lab in England. They have an accelerator that's not too much unlike the Bevatron, called Nimrod. The results I had in mind are some rather accurate and detailed polarization studies, particularly in ion-proton elastic scattering.

I think that we should have done more of that type of experiment here in Berkeley, and I think we were deterred by a couple of things. One is my not having enough perseverance to go back and repeat those experiments with better accuracy. We did some of these experiments, but we didn't push them to as good an accuracy as we should have. I think we should have gone for a second run, another round of those experiments.

Part of the reason we didn't go back for another round of those experiments was we were a little bit discouraged by Ed Lofgren. I think Ed was afraid that we would set up a section down there on the floor of the Bevatron that would become in some sense our personal beam for a long period, or

that we would be setting up a facility that was too narrowly oriented to our own needs and not generally enough oriented to the lab's needs.

I think this was probably a mistake. I think he was too protective of the ability of the Bevatron floor to be changed every six months from one kind of setup to another. It's a mix of things. But I think we would have done better physics if we had set up a facility, somewhat narrowly conceived, to do those experiments. We would have used a meson beam for quite a period, probably a two-year period off and on, but lots of the time on.

Hale: Do you think you would have liked to put more energy into that had you felt that you had more encouragement?

Chamberlain: Yes. I regret now that I didn't put more energy into it anyway. It's my misjudgment as well as Lofgren's, so I don't really want to heave blame on Ed Lofgren. I think it was also my mistake that I didn't talk to him at the right time in enough detail about how we felt and what we thought we might do if we did go in this direction. I think I could have done better at working out my differences with Ed if I had talked to him more. Primarily it was a fault in our own program, but in a secondary way I think he was actually discouraging the point of progress we should have been making.

Hale: At one period during the anti-proton work you gave him, in your papers, a lot of credit.

Chamberlain: The point there is that the very machine itself owed so much to people like Brobeck and Lofgren, so that we recognized this was one of the first big pieces of output from the Bevatron. And it was particularly appropriate to recognize the central role that the machine itself played in that. I think it took a lot of discussion among ourselves to decide whether or not Ed Lofgren shouldn't appear as one of the authors of that paper, simply because the machine was so central, and it was a new use of a new machine.

Hale: But other than that, over the years, have you had a good relationship with him or do you have a continuing feeling that you might have been discouraged?

Chamberlain: Well, I think Ed Lofgren and I get along well with each other, but are not particularly close. I think we both instinctively keep some moderately distant relationship.

Carl Helmholz

Hale: How about Carl Helmholz?

Chamberlain: Mr. Helmholz. I felt that Carl has been another of these very helping persons. He has a kind of evenhanded judgment. He's another person like Thornton that we could take problems to if we needed to. Now, usually these problems went to Thornton rather than to Carl Helmholz, but they both have good personalities as compromisers and finders of middle ground between disparate views.

I haven't worked particularly closely with Carl but always felt a lot of warmth for him. He had a lot of responsibilities with the electron synchrotron for many years. I was very pleased with Carl Helmholz as our physics department chairman, and I think if I had had my way, he would have served as our physics department chairman for lots more years. I think the members of the physics department had a hope that if they chose a new department chairman after Carl had been chairman for maybe seven years, that it would serve to put a little more emphasis within the physics department on greater effort toward choosing new staff members.

I think there was a feeling that Carl hadn't organized a search countrywide or maybe worldwide for new staff members. My own feeling was that the way to solve that problem was to bring it to Carl's attention and once he saw the problem he'd make a committee that would do the search.

He had kind of gotten wiped out in the research fashion by his years as department chairman. Once he'd been wiped out as an active research person a little bit, I felt we should keep his advantages as department chairman and not take away from anybody else's research activities. In other words, Carl was willing to continue to serve, I think, at the time. Now other department chairmen, being chairman for periods like three years, have done awfully well at keeping up their research, so my worst fears haven't been realized.

[Interview 5: August 17, 1976] ##

Hale: You have mentioned about how Helmholz had been willing, as far as you know, to continue as department chairman in 1962, at the end of his term. Why, in fact, did he not continue?

Chamberlain: Oh, I think in our last discussion I alluded to the fact that I think there was some sentiment in the physics department that if a change were made maybe a more aggressive policy of seeking out new young staff members would occur.

Hale: You did mention that. I wondered whether that was the main thing?

Chamberlain: Oh, as far as I know, I think that was the main thing.

Hale: You told me most of what you know, care to relate about it?

Chamberlain: Yes.

Raymond Birge

Hale: Now, we also mentioned something about Raymond Birge. You mentioned how he eased you in, as it were, to your teaching duties and allowed you plenty of time for research. Now, what was your overall relationship with him, apart from the obvious one that you were related to him by marriage?

Chamberlain: Well, that was later, of course.

Hale: Okay, I didn't know when that was.

Chamberlain: Well, I'm not sure when the marriage occurred. Ray Birge--I often call him Papa Birge--Papa Birge was my advisor when I was a beginning graduate student. He made this one outstanding mistake: He put me in the advanced quantum mechanics course before I had had any elementary quantum mechanics.

Hale: That's right, I remember that.

Chamberlain: Apart from that, he was an excellent advisor and gave me much good advice. During the war I had little contact with him once I had gone to Los Alamos, say from '43 to '46. There wasn't really any contact with him, though in the latter part of that period his son, Bob Birge, showed up in the army at Los Alamos.

Hale: That's the SED [Special Engineering Detachment], is that right?

Chamberlain: Yes, he was part of the SED. I'd forgotten those initials. I really didn't have much contact with him [Raymond Birge] while I was a graduate student at Chicago, '46 to '48, but I had kept a little contact with Segrè during that period. Rather, Segrè kept a little contact with me; that's the better way to put it. Then, when I first joined the teaching staff as an instructor, Papa Birge was, as we said, easing me into my teaching assignments in a very helpful way. He realized that my Ph.D. work at Chicago wasn't quite finished yet. I looked for Birge as chairman of the department to bring me any good or bad word about my own appointment, whether it was thought I was doing okay and whether I was going to be advanced or fired.

I know that I had the feeling that this wedding didn't really affect materially my relationship to Professor Birge. At least, I didn't sense any change in him, and I would doubt he sensed any change in me. Of course, the combination in the physics department of Bob Birge's father and Bob Birge's brother-in-law--that's me--sort of precluded under the anti-nepotism rules Bob Birge's joining the department, which I think anybody would feel was unfortunate, but that was the way that worked out. But that didn't prevent him from joining the Radiation Laboratory.

Hale: So professionally you got along well? And he appreciated your research and so on?

Chamberlain: I think so. I think the best description is to say I got along moderately well.

Hale: Moderately well?

Chamberlain: Well, I had an outstanding opportunity, and I didn't muff it completely [laughs]. The thing was that the 184-inch cyclotron was at that time working at ten times the proton energy of any other machine in the world, though I'm not sure that statement is true. But it was an outstanding opportunity to look at proton-proton scattering and see what the heck it was like.

I thought Professor Birge did a very good job as chairman of the department, and I think if any leaning could be discerned, I think he sort of leaned in my favor. But he handled the situation in I think a very evenhanded way. Certainly, the physics department grew in size and in stature in a remarkable fashion while he was chairman.

Hale: Would you say that was his major achievement, rather than his research? He's often thought of as a collator of facts and details.

Chamberlain: Well, first of all, as a collator of facts and details he was rather extraordinary. He could reach into other people's experiments and find places that they had gone wrong. While it made them look a little bit silly, it certainly showed him to be a remarkable person, looking in depth into an experiment. I've tried it, and it's very difficult to look into someone else's experiment and have it come out in a productive fashion. Most people get buried in a welter of detail that keeps them away from the central issues. As I understand it, he practically told Harold Urey where to look for the heavy isotope of hydrogen. He shared a considerable responsibility for finding deuterium.

Hale: Wasn't it a mistake, though?

Chamberlain: Not known by me to be a mistake. A difference between the atomic weight of hydrogen that's revealed chemically and physically led Birge to suppose that it might be that the chemists were including a small fraction of a heavier isotope in their weighings of hydrogen.

Hale: I forget exactly what it is, but there's an interesting story about that. One is that Urey was put on that track by the wrong piece of information. But, you know, he was lucky. I can't remember exactly what the sequence was, but it was like a double negative, and it turned out to be okay. Ray Birge predicted the abundance of deuterium in normal water, based on the difference between the chemical and physical atomic weight scales. Both had errors, but the errors cancelled each other.

Chamberlain: It can happen. Incidentally, we often use erroneous theories to our advantage simply because even the erroneous theories may depend upon the right variables.

Hale: True.

Chamberlain: So they allow you to ask questions more intelligently than you could without any theory, and it sometimes happens that the erroneous theory leads to the right questions.

Hale: I think it's just one of those little, often-quoted quirks in the history of science. Okay, so you will credit Birge with that sort of ability to look into other people's experiments.

Chamberlain: I think the department is his greatest work, in a way. He displayed a remarkable combination of strong leadership and flexibility. For instance, if he had been too rigid, he might well have rejected the idea of taking on the staff a person such as Ernest Lawrence. Now, Ernest Lawrence had characteristics that were very different from most of the academic types, especially at that time. I think it was, you know, an excellent move by the department to go with Lawrence's differences along with his exceptional positive qualities.

Hale: Of course, that applies even more to somebody like Oppenheimer, I think. Noel Pharr Davis, in the book, *Lawrence and Oppenheimer*, mentioned how both Birge and Hall had a predilection to prefer Anglo-Saxons. The idea was that he was very unusual, a Jew from New York, very different from the Western type or even the old New England type, wanting to be at Berkeley. Of course, that was a master stroke.

Chamberlain: Well, yes, that's a subject that I have a great deal of difficulty dealing with because it was my feeling that Birge displayed a preference for Anglo-Saxons. And yet I could not pinpoint any example where this could be in any sense documented. I don't mean documented in writing, but I mean I can't remember an incident which would pin down anything clear in my mind. I think I ended up with the feeling that I couldn't justify it. My feeling was that he leaned toward Anglo-Saxons but not to the exclusion of the more important point of seeking out good physicists.

Hale: I see.

Luis Alvarez

Hale: Let's talk about the people that came to be very influential in the lab, like Luis Alvarez. How would you comment on your relationship with him, how that grew?

Chamberlain: Well, I was for years not at all close to Luis Alvarez, and I suppose this must have been largely of my own choice. I thought of Luis as someone who worked rather apart and maybe once a year or once every two years would sort of come blistering in with some bright idea.

Hale: When you say apart, you mean apart from what sort of people?

Chamberlain: Well, he didn't work with the rest of us; that is, I didn't see him for months, and then he would show up. He would have gone to Ernest Lawrence and banged the desk of Ernest Lawrence, saying, "Look, I've got to try this great idea." Maybe he didn't even have to bang the desk. Lawrence or Alvarez would phone to Jimmy Vale, who was running the cyclotron, indicating that Lawrence had given his blessing to high-priority effort to explore this idea of Luis'.

Then all the work on the cyclotron would stop, and Luis would take over the cyclotron for a period--I don't know--a few days, maybe a week. And somebody down the hall would say, "Oh, Alvarez is after the whistling meson again." I remember once I went around to see how Luis was doing, and he said, "Oh, all work and no play makes Jack a dull boy. Why don't you go out and have some fun this afternoon?" [laughter]

Hale: He told you that?

Chamberlain: He told me that. I wasn't very close to Luis, and I regarded him with a little suspicion because he seemed to work so differently from the way I did or the way I thought other physicists were usually expected to. He was kind of a prima donna at all stages. I didn't really appreciate that, so I didn't warm up to him particularly.

Of course, in later years, I realized I have a tremendous respect for Luis' physics, and with respect for Luis' physics comes a great deal of tolerance of his personal way of doing things. I get along with him very well, much better in recent years. But we had very little contact in the earlier years, I'd say.

Hale: Would you say that he and Lawrence had a lot in common? Obviously, in some senses he was Lawrence's protégé temperamentally.

Chamberlain: Yes, I think he and Lawrence had a lot in common. They both had a great deal of independence of spirit and seeming self-security because neither one minded at all working outside the usual norms of professors, to some extent. But I suppose Ernest Lawrence must have had a great deal of influence on Luis' style.

Hale: I've got an impression of Lawrence as being, for all his drive and enthusiasm, not all that arrogant. I don't know whether I feel the same way about Alvarez.

Chamberlain: I don't know quite what to say. I think it's true that Luis is more arrogant than Lawrence was.

Hale: A lot of people seem not to express that much friendliness towards Alvarez.

Chamberlain: Well, now, I'm not quite sure I understand you. No, I think Luis expresses a lot of respect for Lawrence and the fact that Lawrence got the lab started and established its style. Well, you can sense this remarkable difference between two people such as Luis Alvarez and Bob Thornton. You know, completely opposite types, as far as I can think. They're both early members of Lawrence's team, and Bob Thornton saw his role as helping other people, helping the lab along, and making the lab a fertile place for us to work. I had the feeling that Luis was more looking out for Luis' own personal things, which in fact most of us are doing.

Physicists tend to be their own selfish lot, all in all. I'd say that within the people that have worked closely under Luis, there's a very great degree of respect for Luis and a lot of loyalty--a little more loyalty than I can sometimes explain. I think I would have resented working under Luis perhaps; at least I thought I would. Since I didn't do it, maybe there are qualities there that I don't know about. But one has to take account of the fact that this loyalty to Luis is very great in people like Art Rosenfeld, Bob Tripp, Lynn Stevenson, and Frank Crawford. I could go on giving a long list of people that originally were part of Luis' group. I think Luis has done an excellent job pursuing ideas that were his--and there were many of those--but also giving good room for people under him to do their own thing.

Hale: I wondered whether the feeling of people who've worked under him is the same or very different from the way it is with people more closely associated with Lawrence.

Chamberlain: I'm not sure. Remember that while in some sense I worked in Lawrence's laboratory, I really worked for Segrè in that period. And Segrè worked for Lawrence. So my contacts with Lawrence weren't really as numerous as you might suppose. Now, it was my opinion that there was a lot of elementary physics that Lawrence was not good at. If you asked Lawrence questions about elementary quantum mechanics, I would expect you'd find him making rather serious mistakes at an elementary level.

Luis is much more of a well-rounded scientist. Luis is kind of a scientist's scientist. He thinks the way a scientist is respected for thinking, and he ferrets out the central facts in a physical situation very quickly and does it very well.

Ed McMillan's Directorship of the Lab

Hale: How about Ed McMillan?

Chamberlain: I was not very close to Ed. Let me try to constrast these points. Luis picked up the spirit of Lawrence's motivation, and Luis came closer to behaving in some sense the way that Lawrence would have if Lawrence had been alive. Ed McMillan, on the other hand, I thought tended to get frozen trying to preserve Lawrence's ideas unchanged. For instance, when Ed McMillan ran the lab, he was, I think, a little bit inflexible in that he preserved the attitudes that he had sensed during Lawrence's lifetime. If Lawrence had been alive, he would have changed more than Ed did. You understand what I'm saying? So Ed McMillan was very much influenced by Lawrence, but in a different way.

I haven't had awfully much contact with Ed McMillan except where we've been sort of fussing about things within the lab. The laboratory Free Speech Movement was a situation where I was quite angry with Ed McMillan. There was a little branch of the FAS, the Federation of Atomic Scientists--later called itself the Federation of American Scientists. We had a Berkeley branch, and we met first on campus and then we started meeting at the Radiation Lab. I've forgotten just how it was conveyed, but it was clear that Lawrence didn't like the idea of an FAS branch having meetings on the Radiation Laboratory premises.

He felt that scientists should be non-political, while, in our view, he was political in his own way. He had a different realm in which to be political. Our politics involved more public announcements, whereas his politics was, in our opinion, more behind-the-scene politics. You know, we considered it part of his politics when he entertained a local congressman or anything of that sort.

But when there was a breath of criticism of the Radiation Lab that Lawrence heard about, to the effect that people couldn't be free-thinking individuals around the

Radiation Lab, that they had to conform to this almost corporate-like view of the world, Lawrence immediately retorted by bragging about the existence of the FAS branch [laughter]. So in the end, I think Lawrence changed his mind, at least partially, about our FAS branch.

Well, when Ed McMillan said that we couldn't have non-scientific or maybe political discussions at lunch hour in the Radiation Lab, I think he felt he was following the policy that Lawrence would have followed if Lawrence had been alive. I think I could have more easily persuaded Lawrence to accede to those meetings than Ed, though after resisting for a good part of a year, Ed changed his mind and allowed the meetings. We had a few meetings, and then the meetings stopped because a lot of the pressure for the meeting was a feeling of resentment that the meeting itself wasn't allowed, I think. You began to feel more and more things you would like to discuss at noon hour when such discussions were not permitted.

Well, we've had a continuing low rate of meetings. I think all of the candidates from the Berkeley City Council are invited to come and give a talk at noon at the Radiation Lab, not all on the same day. They don't all come. But they're all invited, and that's part of this freedom at noon-hour talk tradition which Ed did establish in the end. He should get the credit for that; it's just that we put a lot of effort into fighting his resistance for a while.

Hale: A much more important aspect of McMillan's is the effect of his directorship on the whole future of the laboratory.

Chamberlain: Yes, you're absolutely right. Sure.

Hale: Could you tell me something about the way in which you think these attitudes could have affected the laboratory?

Chamberlain: Well, I think Ed McMillan probably was not a great leader for the laboratory; on the other hand, it takes a wealth of different kinds of people in the laboratory. I have kind of mixed feelings. For instance, if the laboratory didn't come up with a new design for an accelerator as, let's say, it might have, it's my fault as well as Ed McMillan's. In other words, I think as far as the development of the laboratory, Ed was not a great active leader, but also he didn't put any constraints that would prevent the normal development of the laboratory.

I think the lab was, in retrospect, a little bit slower to develop a higher-energy accelerator plan as it might have been. There was a plan for a 100- or 200-BeV accelerator. One of the locations for it might have been Camp Parks or Camp Park, between here and Livermore. One of the locations was not far from Sacramento, in the Sierra foothills.

When I got together as part of some national committee to look over this situation, I had the feeling that without realizing it, we had let a little too much time elapse. We should have planned that accelerator a few years earlier. Whether that accelerator might have been built in Berkeley if it had been planned earlier, I couldn't possibly tell you. There were strong reasons for building it at other places, I'm sure. I think frankly the lab has fallen out of its leadership in high-energy accelerators, but I don't think that can be laid at the doorstep of Ed McMillan particularly. After all, a trend of that sort had started during Lawrence's lifetime, with the strong focusing beam developed elsewhere--alternating-gradient focusing.

I think that in most respects Ed McMillan was a very benevolent and soft-spoken dictator, that he certainly didn't stand in the way of the natural development of the lab. You know, the lab's a couple of thousand people, and you can't put all the responsibility on the director of the lab. After all, he had some very energetic associate directors in various areas: Ed Lofgren, Bill Wenzel--

Hale: Who would obviously have a large effect on him or upon the direction of the lab?

Chamberlain: Oh, yes. I think Ed Lofgren was very influential.

Hale: I've got the impression that at the time when it seemed that Berkeley had lost its lead in high-energy accelerators, I gather that Lofgren was very enthusiastic about pushing for yet another, bigger accelerator here--whereas in fact the Young Turks at the laboratory had begun to see the writing on the wall that that's not the direction for Berkeley to go anymore. I get the feeling that it was through this sort of tension that Ed McMillan ended his directorship. The laboratory went into many more different directions.

Chamberlain: Yes, I think that's true.

Hale: Do you know any particular details about that period, to illuminate that transition?

Chamberlain: No, I don't know. There certainly have been periods where I've felt that there was an undesirable lack of trust between the accelerator types and the research-physics types. Frankly, if I try to look at it in a somewhat fair and evenhanded way, I think there is a certain amount of justification on both sides. I don't know whether this is an inevitable thing that happens in other laboratories, but it could almost have been an inevitable thing here.

For instance, I believe Ed Lofgren was quite hesitant to put before the research types his plans for some change in the accelerator, for fear that research types would immediately say, "No, no, you shouldn't spend the money that way. You should spend the money on more research." And I think Ed's fear was well founded. The research types--and I consider myself one of them, I think I do this--pay a lot of lip service to the fact that we have to put maybe a third of our resources--money and time and effort--into machine development or detector development, some kind of instrument development.

After all, that's the lifeblood. Without the next accelerator, we can't get into the next energy region, and historically it's pretty obvious that getting into the next energy region has been what yielded the new findings. And yet, in detail, when faced with a particular decision, there's a tendency to see it as somehow not sufficiently important to divert funds away from existing research projects or something like that.

Well, the upshot of this has been that the changes in the Bevatron have occurred, for the most part, without the permission or the acquiescence of the research types. They've just occurred. They've been discovered after the fact, to some extent. This would include the acceleration in the Bevatron of particles other than protons. Now it's deuterons, alpha particles, and even argon atoms. It includes various steps taken to increase the intensity of the Bevatron.

Certainly, during this period, as the Bevatron converted from a proton machine to a heavy-ion machine, there have been a series of steps which were simply taken by the accelerator types without much announcement to the research community or at least without any study sessions involving much of the research community.

So there has tended to be a kind of a separation between the research types and the accelerator types, which is unfortunate. But I want to be very specific and not lay this necessarily, say, at Lofgren's door or anything like that. It certainly was aided and abetted by some of the policies that Ed McMillan either allowed to continue or actually instituted.

For instance, the Bevatron budget never was part of the physics division, although, I must say, I didn't realize that fact. The Bevatron budget was going sort of directly to the director's office, not through the associate director for physics, which really I was unaware of. That sort of thing has tended to emphasize the isolation between the accelerator types and the physics division. That's been formalized now, in recent years--the accelerator division being introduced as a separate entity, apart from the physics division.

I didn't approve of this formation of a separate accelerator division. I still don't think it's a great idea, but we're living with it, and without too much difficulty. We still have some joint committees, I believe.

Hale: Do you think, then, that this divergence between the physics types and the machine types brought about a sort of crisis at the time, and McMillan resigned?

Chamberlain: The laboratory's not going to fizzle out because it's got too many different things going. They can't all fizzle out at once; at least I didn't ever think so.

Hale: Now it can't, but at that time it seemed to be sort of weak.

Chamberlain: Oh, but it's been a long time that it's had so many different aspects: physics, a little bit of plasma, lots of chemistry and inorganic materials, and so forth. So it's very difficult to talk about the whole lab fizzling out. Now you might worry more about whether something would happen to the physics division.

Something could indeed happen to the physics division, and at the moment I'm unaware of any guarantee that the physics division might not have quite a catastrophe. For instance, this lab has no longer any high-energy accelerators. The high-energy research could be drastically reduced in terms of financial support to the level of some of these other outfits that are called "user groups." We

here constitute in a sense a collection of user groups so we can probably justify a somewhat higher budget than many of the other user groups that we might be compared with.

Still, the level of support as a user group could be awfully much smaller than the lab has been used to. Obviously, we haven't as yet been relegated to being purely a user group because there's lots of accelerator design and support activity that goes on at this lab. For instance, the pulsed-superconducting magnets; there's quite an advanced program going on here, I think. At least it competes moderately well, maybe very well, with the other groups around the world that are trying to do similar things.

There's an attempt to make a small accelerator really work with superconducting magnets. This is supposed to give us practice for the superconducting magnets that we think are in our future. But it could happen that our physics research decreases an awful lot in the years ahead, and it could be a much more difficult situation for many of us in that case. But we'll have to see.

Hale: What do you think, then, was the main reason for Ed McMillan stepping down as the director of the lab?

Chamberlain: Oh, I don't know. I haven't been particularly a part of that. In a way, I hesitate to comment on these most recent laboratory developments. I'd say Ed McMillan took less initiative as director than he probably should have. And I think that it's really maybe in response to that fact that made for a growing feeling that Ed should step down eventually as director.

Hale: Do you think it took him a long time to come to that conclusion, or do you think he might have given up the directorship reluctantly?

Chamberlain: I don't know. The directorship's enough of a responsibility. I would think anybody could also heave a certain sigh of relief to leave. He must have felt a lot of pressure in the position. I would think that he must have sensed a feeling that there was, I think, a growing thought within the lab that a more aggressive leadership should be tried. I think, then, that his position became more uncomfortable as he felt those pressures, and he probably was delighted to step down and let someone else shoulder that burden.

Hale: Maybe you can tell me how you were getting along with Glenn Seaborg at that time?

Chamberlain: Well, I've had very little in the way of contacts with Seaborg, and so I don't think there's really much to say there. There are so many people that know Seaborg so much better than I that I don't think it makes much sense for me to try to comment. I get the feeling that Seaborg's not the easiest man to work with, but I really don't know. I never worked with him myself.

The Loyalty Oath, and Involvement with UC and Nuclear Politics

Hale: Why don't you tell me about the problems that you had with the loyalty oath around 1950.

Chamberlain: Well, my problems with the loyalty oath were fairly easy. Maybe after I had once gone through the thing I might have taken a different attitude on some other similar issue. But my attitude was that it was really up to the tenured professors to fight these battles on things like the loyalty oath. And in fact I guess it was my view that one of the justifications of the institution called tenure is that it would allow a situation in which the tenured professors could prevent themselves from being overrun with new regulations or new oaths. I didn't sign the loyalty oath as quickly as some people must have, but when the chips were down, I signed with the feeling that it was sort of the duty and responsibility of the elder professors to fight those battles. I was obviously not very sympathetic with the loyalty oath.

Hale: Do you think in some senses that it was almost purposely designed to try and purge the university of the undesirable elements? A lot of people did leave, didn't they?

Chamberlain: Well, yes, a fair number certainly left. The number that left I would have thought would be like forty, but I don't really know. There was one point at which there were a hundred holdouts who hadn't signed the oath, but I don't think all of them left.

Hale: Did it include quite a fair number of theoretical physicists?

Chamberlain: Oh, probably. In fact, I had somewhat the feeling that the people who were eliminated by the oath were the ones that I found most desirable and most independent thinking and that I respected the most. I thought that there was clearly an erroneous kind of sifting that was occurring as a result of the oath.

Hale: Wasn't Nielens, the regent, very much of an opponent of the oath?

Chamberlain: Well, I suppose, yes. Panofsky announced he was moving to Stanford from the University of California. I inquired what had caused him to make this decision, and he said he had gone to talk to the Regents--likely a subcommittee--about the oath, and having been unable to persuade them to give up pursuing the oath, he really felt that he had to leave.

This struck me as being strange. I wouldn't have felt any more or less compelled to leave as a result of the oath just after I had or hadn't had a discussion with the Regents, and whether the Regents had or hadn't accepted my view in good faith or whatnot. It was hard for me to imagine the situation in which the reaction of the Regents to me or my reaction to the Regents would have caused me to feel that a move was very important. Maybe one of the worst aspects of the oath was that it sent Panofsky away.

Hale: At that time, were you getting more politically active?

Chamberlain: I don't know whether I've ever been what I would call politically active. The things that I have done are--okay, for a while I served in the Representative Assembly of the Academic Senate, Berkeley Division. That lasted for about one term of office because I think my physics department constituency decided I was more radical than they wished in their representation, and they elected somebody else.

I've stayed out of campus politics since the existence of a Representative Assembly. Nominally, any faculty member can go and speak to the Representative Assembly, but I discovered on one occasion or possibly two that going as a faculty member who wasn't a voting member of the Representative Assembly, I couldn't get a discussion going. I could get up and make a speech, but I couldn't get anybody to respond to it. After my speech, there was no discussion. No opposing point of view was presented. They just moved to go on with the vote.

And so, you know, to go there and say some words that you feel aren't particularly being listened to isn't much fun. You don't feel as though you're having much influence on the situation. So I've completely stayed away from campus politics, as far as I can think, once the Representative Assembly was instituted. I felt that it was a mistake on the Berkeley campus.

I thought it was better to have the Academic Senate meetings with all members of the Senate present who wished to come. The trouble was that we have 1,400 Senate members --any tenured faculty member can be a member of the Senate-- and 140 who did. Only 10 percent of the faculty showed up at the Academic Senate meetings, but I felt fine about that. I thought the people who were interested in the issue were the ones that should go there.

There were some slightly unfortunate happenings in which a particular issue would bring out a particular segment of campus thinking, and you'd get a kind of lopsided vote at one of the Senate meetings, which you didn't feel represented that full Academic Senate. But my feeling was that whoever felt strongly enough to show up at the meeting should carry the day, and whoever didn't show up shouldn't complain.

This went both ways. I mean, the campus left sometimes got something through by showing up in unusual numbers, and the campus right sometimes got something through by showing up in unusual numbers, and I don't think on balance the situation was a bad one.

Hale: You didn't take that much part in those campus politics at times?

Chamberlain: Well, I went to most of the Academic Senate meetings while it was a full Academic Senate. Since there's been a Representative Assembly, I've stayed away and stayed out.

I've done a little bit of activity with the Federation of American Scientists [formerly Federation of Atomic Scientists], but it's been very little. I was somewhat active in the early days at the Berkeley branch, and in 1959 I was the national treasurer or secretary-treasurer, or something like that, of the FAS. But that's not a tremendous involvement either.

I've made occasional appearances in some of the general issues of peace, nuclear arms, nuclear disarmament sort of

questions. I generally took the point of view that the United States should still do more in the way of obtaining negotiated agreements with the Soviet Union in the direction of decreasing the armaments and putting limitations on the development of future armaments and limitations on tests and so forth.

I remember an amusing incident in one of the Adlai Stevenson campaigns. Adlai Stevenson came to Oakland and appeared maybe someplace like the Oakland Auditorium, and a sizable group from our FAS Berkeley branch wanted to talk to Stevenson because he had just come out in favor of a test ban in some form. I think it was a fairly comprehensive test ban, and our group wanted him to qualify his stand to make sure that what he wanted to do was doable within some reasonable measure of conservatism. Mainly, we wanted to have a ban on all the tests that were big enough that they could surely be detected by seismographs rather than have a total ban on all nuclear tests.

We didn't see Stevenson, but we did meet for a good part of an hour with William Wirtz, who was a campaign advisor for Adlai Stevenson. I suppose this must have been in '56 rather than '52. Actually, what we were advising was very close to what the United States in later years adopted: namely, tests over a certain size are forbidden because we are sure that tests above that size can be detected with seismographs.

We felt that a comprehensive test ban was difficult to support without some concern that the treaty might backfire. The advantages of the limited test ban was that it could be supported without any worry that we would be tricked by the Soviet Union or somehow that the agreement would be used to put us at some material disadvantage.

You can argue that if the Soviets put us at some disadvantage in testing, it might not really have much effect on the world interactions, but you can't argue that to most American voters. So it's a political reality that you have to be pretty conservative on a test ban type of question. It was our judgment, and that's still mine, I think. But I'm not sure what you mean by politically active otherwise.

Quite often I used to see my congressman when I went to Washington, if I could. Sometimes I'd duck out of the Physical Society meting for an afternoon and go see my congressman. But, well, in some case, I came to have great

respect for the congressman. In the end, had quite a great respect for Senator Kuchel. I never talked to him very much, but I talked to his staff, and I discovered that they were very much on top of a lot of the things that I wanted them to know. They were going to disarmament seminars, and seminars on new kinds of weapons, and they were very well informed.

I was grateful. You can't always tell when a senator makes a speech how much he's listening to a variety of opinion other than the central opinion that he's expressing in his speech. I feel much more confident if I find that a man has done his homework and has considered lots of possibilities before he has made his choice, and he hasn't been railroaded by some narrow set of views.

Hale: Have you been active in any given political party at the grassroots level, or is that something that doesn't interest you?

Chamberlain: Well, on rare occasion there have been parties to meet Democratic political candidates at my house. I can only really think of one big such occasion where maybe between fifty and a hundred people showed up, or something like that, at a cocktail party to meet candidate so-and-so. I can't even remember who the candidate was, to tell you the truth, but I remember he was from southern California. He wasn't so well known in northern California at that moment.

I think in '56, either directly or indirectly, I contributed something both in the way of work and money to Stevenson's campaign. My contributions were indirect because it was really my wife who was doing most of the activity, and my support came largely in the form of cooking dinner at home [laughter]. On some occasions I took part. We were both very enthusiastic.

About Being a Nobel Laureate

Hale: You of course have a ready-made reason for people to listen to you, being a Nobel Prize winner. Do you find that very useful?

Chamberlain: Well, I think that no doubt I'm listened to much more as a Nobel Prize winner. I often think that people in the public domain pay too much attention to the Nobel Prize winners and

not enough attention to other scientists who are also very well qualified to speak up on similar issues, or just as well qualified anyway. So I feel some additional responsibility with a Nobel Prize. Responsibility not to go off half-cocked, responsibility that goes with feeling that I am at least listened to in a particular way.

It's also a bit of a nuisance because, although most people are pretty respecting of my time, there's a large minority who somehow feel that I owe it to them to read and comment on their latest paper or to support some political something or other. I am much more bombarded both with literature and people wanting to talk to me about some group or other, and I find it difficult to avoid doing a lot of things that are wasted time as far as scientific work is concerned. Some of them I wanted to do. Many of them are kind of a nuisance, and it's very difficult to get someone in and out of my office in less than fifteen minutes.

Hale: Would you say that most of the people that are importunate in this way are from the left or the liberal side of questions?

Chamberlain: Yes. I think it's mostly from the left or the liberal side, that's true. That probably has to do with some feeling that among the professors I'm a little on the left or on the more liberal side. But you'd be surprised how often I agree with Segrè. I joke a lot because I'm supposed to be the flaming liberal, and he's supposed to be the black conservative, and we often agree on issues.

Hale: Do you think that, say, amongst Nobel Prize winners that you find yourself in a sort of a minority that way?

Chamberlain: Well, to some degree. I think I can listen to a lot of people that are more conservative than I, right among the Nobel Prize winners right here. They would include Seaborg, McMillan, Alvarez, and Segrè. I have been a little bit more prone to urge my fellow Nobel laureates to take a stand either as a group or as individuals on a few issues--things like Cambodia or some of the test ban things.

I have forgotten which things have actually boiled up. I think when Nixon ordered our troops into Cambodia, I think there was an attempt, maybe a successful one, to get Nobel laureates to sign some kind of a fairly widely-placed newspaper ad--well, the *New York Times* [laughs]. It's hard to remember those details.

But Nobel laureates don't like to act as a group of Nobel laureates too much. Partly I think it's because they all realize that there is much more to science than the limited number of Nobel laureates. They, I think, feel that they're not being suitably humble when they join on as a group to some kind of a statement. It leaves the impression that they're saying to many people, "Ah, we Nobel laureates, we're the wise ones of the world, and we know how to run the world better than anybody else, better than politicians, better than kings and presidents."

That, of course, seems a little unsuitable because scientists can be narrow as well as broad. In fact, many of the Nobel laureates have tended to get quite isolated from public life. Many of them just won't talk to a newspaperman and for the most part won't see visitors, I guess, unless they have some science reason for wanting an interview, conference.

Chamberlain's First Important Research, Proton-Proton Scattering, 1951-1952

Hale: Let's go through the various stages in your major research areas. We've touched on them a little already.

Chamberlain: As we get to the most recent things, the need for the oral history is at least a little bit reduced.

Hale: Your first big slew of papers in '50, '52, was basically to do with proton-proton scattering on the 184-inch.

Chamberlain: Yes.

Hale: Now, Bill Brobeck has mentioned to me how the opening up of a new region of energy like that means that the best physics is very easily skimmed off the top. You had that chance to do that. Could you sort of fill me in with some more detail about the main questions you were interested in in those experiments?

Chamberlain: Yes. I often ask myself what did I do that I'm particularly proud of. As a young man, I invented a very simpleminded device that we called the demultiplier. We wanted to measure the alpha activity of samples that were too highly active for our counters to stand. We had difficult choices. Could we cut up the sample with scissors and measure the alpha activity of small pieces? That we didn't want to do.

We were afraid of losing track of the total mass of material once we started using scissors on it. Little flakes would stick to the scissors.

My first small invention in physics was to put the sample at a distance from the detector, using a set of plates with holes in such a way that I could get a very good sampling of the activity but have reduced activity reaching the counter. We called it a demultiplier, as it cut down the counts to a lower number. I don't even remember just how we built the device, but I know that we put some thought into how we could build it in such a way that it would give a fair, unbiased sampling of the activity that was on the radioactive sample.

That was during the period when Segrè was in charge and I was a helper to assay the uranium isotopes by alpha activity, fission activity (that is, fissionability) and mass. Of course, the measurement of fission is done in a flex of slow neutrons. [Note: Alpha disintegration is mostly from uranium 234, fission activity is mostly from U235, and mass is from U238 (the long-lived form). Thus, a measurement of these three reveals how much of the sample is U234, how much is U235, and how much is U238.]

Then, when it came to my thesis work while I was working with Fermi, I was rather proud of the fact that I had made a neutron spectrometer which had many times the counting rate of comparable spectrometers, by zeroing in on just the quantity that we were interested in.

The traditional method had been you make monoenergetic neutrons and you scatter all the neutrons that count by the same angle in the sample. But what you really wanted to do was to get a standard momentum transfer when the neutron changed from the beam of neutrons to the counter direction. My spectrometer sought out a wide range of neutrons that gave the right momentum transfer, even thought not all those neutrons had the same energy at the time they collided with the sample.

So my smaller nuclear reaction at the Argonne Lab was able to compete fairly satisfactorily with people at Oak Ridge, who had a higher flux of neutrons. My neutron spectrometer had this advantage built into it: that it focused just in terms of the momentum transfer, which was the very quantity one wanted. I must say, to this day I have some doubts that the principle shown in my spectrometer is used as much as it could be.

Nowadays the neutron sources are so much more intense that people don't necessarily have to work quite so hard, but it still seems to me that I would prefer to use a spectrometer with this feature because as yet I've still had the feeling that to use this feature always makes the spectrometer one step better, wherever you are in the process.

Now, when it came to the proton-proton scattering, I frankly fell into this problem just by arriving on the scene at about the right time. Neutron-neutron scattering was already being studied, and proton-proton scattering was of equal or probably even slightly greater interest.

Hale: It was the comparison between the two that was of--wasn't it one of the major points of interest?

Chamberlain: Actually, there is a sense in which it was one of the major points of interest, that's right.

Hale: Since two protons together are more like two electrons together than the neutron and proton.

Chamberlain: You're quite right. Certain aspects of the problem were best considered by comparing the neutron-proton or proton-proton scattering, and in fact the two looked more different than we had guessed they would. There was a lot of discussion as to why, and I think that's why the theoreticians tended to be rather confounded at the time.

Every time we extended the angular range over which we looked at the proton-proton scattering, the theoreticians would have to run back to their blackboards because we were getting a practically constant cross-section at each angle. And every time we extended the region over which this cross-section was constant, the theoreticians would have to run back to their drawing board because their predictions always allowed them to fit our points with a level cross-section, but as soon as we got out of the measuring region, then their cross-section rose. And every time we extended the region in which it was constant, they'd have to go back to work.

There was a short time when it was even thought that we must be wrong because they couldn't find a theory that would agree with these results. Later it was recognized that, in fact, there shouldn't have been a problem because there was a way of finding a theory that worked.

Hale: That sounds to me like the tendency for theoreticians to jump at a very tiny area of the experimental results, rather than sort of sitting back and waiting to see what happens in a broader sense.

Chamberlain: I don't have the same feeling, really. I'm trying to think whether there are any examples that I would cite where I had that feeling.

Hale: Not even the psi-J particles? [Note: Psi particle has two names: "psi" and "J."]

Chamberlain: Oh, heavens, no. The theoreticians have jumped on those with miraculous success. No, in fact, the theoreticians again startled me with the accuracy of their interpretations of these psi-J particles. It's been a fabulous success for the theoreticians. I think it's amazing that they've gotten on some correct tracks, in view of the paucity of the evidence at certain periods when they were still nursing along the correct theory even though nobody could seem to get any agreement with it.

Well, you see, think of it this way: We were measuring these cross-sections at about ten times the laboratory energy that they had ever been measured at before. Therefore, we were exploring in a new region, and the theoreticians were trying to find out, did they have any outline that would give them a rough picture of what was going on. So they constructed their pictures as best they could, and in that case their rough pictures were in error at that period of time.

They foresaw a phenomenon at higher energy, which is seen all the time, that there's a lot of scattering near the forward direction and the scattering at wider angles is negligible. The cross-section is big near forward angles and goes down drastically, by factors of millions, when you look for wide-angle scattering. That's what they were expecting in our results, and that wasn't what happened in that energy region. I think it was true that the theoreticians were trying to get some feeling for what this new energy region was showing, and they were right to grasp at each new point and see what it said about new directions.

Polarization Experiments

Chamberlain: Now, believe it or not, we were some of the first people to do polarization experiments. We actually got into the polarization experiments a bit slower than we should have. We should have probably responded to the first rumors that they were seeing polarization at the University of Rochester, and we should have jumped in immediately. Instead, I kind of waited a little to see what they came up with. I think we responded as soon as they'd published their first paper. Now, the polarization experiments had a good input from Segrè, by Wiegand, myself, and, as we went along, Ypsilantis and Tripp.

It must have been the summer of '52 when Wolfenstein was invited to visit the lab by Segrè. Wolfenstein explained to us what the polarization phenomenon was all about--how to think about it in quantum mechanics, which we hadn't known. And he asked a very crucial question, "Couldn't you do triple scattering experiments?" You see, the experiments we had first done we couldn't really understand until Wolfenstein got us and taught the basic theory.

The experiments we had done, to begin with, were double scattering experiments. In the first scattering, the particles were polarized--that is, just as you have a collision between billiard balls on a billiard table that leaves the balls spinning afterwards. At least the geometry was similar. The protons that came out of the first scattering were left predominantly spinning in one direction rather than the other.

Then, in the first collision, the protons were left predominantly spinning in one way and a few spinning the other. In the second scattering process, this spinning motion revealed itself by having a different number of scatterings to the left and to the right. So maybe at the first scattering we chose only protons that scattered to the left and then, in the second scattering, we looked to see whether the left-scattering predominated over the right and, if so, to what extent. We could judge from that how much the protons were polarized, and we did some good experiments.

But Wolfenstein fortunately asked us the question, "Could you possibly do triple scattering experiments?" Now, I have to say that my immediate reaction was no, that it's hopeless to do triple scattering experiments; there's not

anywhere near enough intensity; we hardly have enough intensity to do double scattering experiments. However, fortunately, Tripp and Ypsilantis--and they certainly deserve the credit--went into a room by themselves for about three weeks. The two of them worked together it seemed like forever. They finally emerged, saying, "We think we know how to do triple scattering experiments." They came up with a design which we had to alter very little for the next couple of years.

It happened that the cyclotron was about to be shut down because it was to change from a 300-MeV machine to a still higher 700-MeV machine. Tom Ypsilantis and I wisely, somehow, decided we would prepare our experiment--even though it looked as though there'd be no cyclotron time to run it--in the hope that something might happen. Well, we had our luck. The very day, or the day before the cyclotron was to be shut down for its changeover to a higher energy, word came down that the parts weren't ready, so they'd have to postpone the shutdown. We would run the cyclotron some more at 300 MeV.

Our experiment was the only one ready to run [laughter]. Well, always before, we had eight-hour runs. What we needed was a couple of weeks at one time, and nobody was ready for a couple of weeks, so we had two weeks of the cyclotron to ourselves. It was absolutely marvelous, and we did the first successful triple scattering experiment.

Then, as a group, we did a series of triple scattering experiments which were so outstanding that they actually set the field back, if you can imagine; namely, they were so outstanding that nobody wanted to compete with us in the business of triple scattering. Now, this must have been around '53 or '54.

Ypsilantis and Tripp and Triple Scattering

Chamberlain: We succeeded in making I guess it was the first complete analysis of proton-proton scattering. Well, we had to inch toward it. Essentially, we did the bulk of the first complete analysis of proton-proton scattering that had ever been done, unless maybe something similar had been done at the lowest energies. But we did something that was quite new and which I was very proud of: We put a number of triple scattering experiments together.

In the first collision, the protons are polarized so the result of the first collision is polarized protons. The second collision is, in our case, the proton-proton scattering which is under study. In other words, we send in polarized protons into a collision and then we ask the question, "What comes out? Is the thing coming out spinning?"

To find out whether what comes out is spinning, we need a third collision process in which a left-right symmetry is looked for or maybe even a different symmetry, an up-down symmetry or asymmetry. We looked for a difference between left-right scattering. So the first collision produces polarized protons; the second scattering is the scattering that you're trying to learn about; the third one is the analyzing one, where we try to find out whether the products from the second scattering were indeed polarized and, if so, how much.

Hale: So it's analogous to an optical experiment.

Chamberlain: Yes, it's very analogous to an optical experiment in which you polarize the light, then you put the polarized light on something, and afterwards you find out how polarized does the light remain in them. That's a good comparison. Now, I think that these triple scattering experiments were among our great successes. The contributions came from Segrè in many overall ways and in management ways and in getting Wolfenstein there; they came from Wolfenstein in particular; they came from Ypsilantis and Tripp, who figured out that you could do triple scattering when we thought you couldn't. They had some inputs from me that I thought were important about how to avoid pitfalls in triple scattering experiments.

Hale: What was their major breakthrough?

Chamberlain: Well, it allowed a complete spin analysis of what was going on. We could make, for the first time, a complete description of a high-energy scattering process.

Hale: I meant the main point that Tripp and Ypsilantis discovered.

Chamberlain: Oh, their main point was--first of all, they realized that we could still do work, even though it might be only one or two counts an hour. If you add up enough hours, it's possible to be patient. But they then optimized and reoptimized the apparatus parameters: how thick should the targets be, how closely spaced, how do you manage them, how

do you avoid being swamped by all your counters being overloaded with particles.

They managed to find a workable apparatus. Since they were closeted by themselves, I don't know all the things they did. What I imagine they did was to do a kind of cut and try; calculate for various geometries how much counting rate they would get. They probably had to work up in slow stages to get the counting rate up to two an hour. As I remember, we got on the order of one or two an hour, depending on just what the setup was; but seldom more in those triple scattering experiments.

Hale: So they did those sort of operations research?

Chamberlain: Yes, they had to optimize the equipment, and they also had to use their brains to realize that a lower counting rate experiment could work with patience. It's such a change of style to go from maybe counting a hundred counts a minute to something in which you maybe get a hundred counts per forty-hour week. Even though on paper it seems straightforward, finally it took some imagination to put oneself in this new realm.

Nowadays we do long experiments all the time, but at that time it was rather unusual, I think. At least it was unusual for us. Those triple scattering experiments weren't redone for about, I think, ten years, which is unusual in physics. They were so outstanding that nobody wanted to compete [laughter]. It's terribly funny because it's an unusual situation. Ten years later those experiments were being repeated at several institutions: Carnegie Tech, University of Chicago, Harvard, and, I believe, Rochester.

Hale: Were your results improved upon or just extended?

Chamberlain: We came out decently well. I think somebody found one of our results was a little bit in error, but they stood up decently well.

Hale: If I understand it, the main point in that area of research is to consider the contributions of other than S-wave scattering?

Chamberlain: Yes.

Hale: And to determine the phase shifts?

Chamberlain: Yes. Phase shifts were very much in the picture, that's right.

Hale: In other words, you're getting much more information about the scattering process.

Chamberlain: Yes, and it's awfully hard to feel that you've studied a subject thoroughly unless you've gotten the spin dependence that's there, you know. In quantum mechanical terms, we need the amplitude for scattering in each of these separate spin states before we have a complete description.

The kind of knowledge you get depends a little on the circumstances. In our case, we got kind of a mix of the different angular momentum states. We didn't get any very clear picture. But as other people moved to other energies, resonances showed up that couldn't have been detected without the polarization measurements. In fact, they've learned more from partial sets of experiments than I thought they would.

And the complete analyses have been done relatively rarely. Ours, at about 300 MeV, was the first, and then it was extended to 400-plus MeV. I don't believe we ever succeeded in doing a complete analysis at 700 MeV, where it's more complicated. It's more complicated to make a complete analysis because the inelasticity of the scattering allows for more possibilities. At lower energies, the scattering can be thought of as always elastic. And then with elastic scattering you get some theorems that help you to limit the possibilities. You lose those theorems when it becomes highly inelastic, and so at 700 MeV I don't believe the complete analysis has ever been done.

Hale: Really?

Chamberlain: I suspect that I was the first one to figure out a few years ago that we could do a complete analysis at 12 GeV using the polarized beam at the Argonne Lab. And I think that at the Argonne Lab they're going to do complete analyses both at 6 GeV and 12 GeV, and for the first time in that energy region. We're just now trying to put enough pressure on the powers that be so that we can keep the accelerator at the Argonne going long enough to complete those experiments before the Argonne accelerator gets shut down. It's supposed to shut down two and a half years from now, at the end of calendar '78. We're trying to keep it going just a little bit longer.

Hale: What is it that makes it possible to do studies at those energies?

Chamberlain: Well, what's particular and unusual is that nowadays they can accelerate polarized protons in the machine itself. They can inject and polarize them and maintain their polarization through the whole acceleration process. They got up to 6 GeV polarized protons more than a year ago, and they got up to 12 GeV polarized protons this last February or March.

Hale: So you're starting off with greater intensities. Is that the main point?

Chamberlain: Well, nowadays we can put polarized beams on polarized targets. I think I was the first to figure out that if you have polarized beam on polarized target, you also can measure the polarization of one of the particles in the final state after scattering. You don't need both, just one; then you can get a complete analysis of the scattering --at least in one energy and angle, any one energy and angle where you're willing to do this complete set of experiments. So I think that will materialize in the next few years at the Argonne Lab, and I think it might tell us something rather interesting about the high-energy scattering processes: what kind of thing jumps across from one particle to another in energy collisions.

Hale: How is it, though, that you avoid the problems that you ran into at 700 MeV? The problems of the inelasticity.

Chamberlain: I'm cheating you slightly--I hope not significantly. The complete analysis at 300 MeV involved analyzing at *all* angles at that energy, so at that energy of scattering, we could say that it was complete, in the most complete sense. What I meant about the Argonne scattering was a little different; it was to take this scattering and look at it in a little bit more limited way. You look at the scattering in one energy and one angle and do twelve to fifteen different experiments on that same energy and angle.

And in that case, you make the scattering at that angle completely describable up to one unknown phase, which in quantum mechanics often remains. There's often one overall phase in quantum mechanics that you have no means for determining. So up to one overall phase, which is, you might say, of no physical importance, we could make a complete description of proton-proton scattering, but in a little different sense. You see, at the low energy I made a

complete description of all angles, and at the high energy I made a complete description of one angle.

The thing that makes a lot of the difference is the fact of having a polarized beam accelerated right at the machine. We didn't have that in Berkeley. We do not at the 88-inch cyclotron, although in the cyclotron there's a polarized beam accelerated down here all the time, combined with highly polarized targets. When you measure three polarization things in the same scattering--such as if you put a polarized beam onto a polarized proton target and still measure the polarization state by rescattering--you do all those three things, you tend to get lousy statistical results unless you can use very highly polarized beams and targets. That's what we have nowadays: beams that are, let's say, 70 percent and 80 percent polarized. We have polarized targets that are 80 percent and 90 percent polarized, and that's a very important part of doing these three-polarization processes.

Anti-proton Experiment

Chamberlain: You need these more elaborate experiments involving three polarizations in order to solve the problem. We also know that with those you can solve the problem with six and twelve GeVs. Now, you know, in my professional life the anti-proton experiment has an unusual character, almost that of an artifact.

Hale: I was visualizing this a couple of days ago. It's sort of out of place.

Chamberlain: Yes, it is sort of out of place. I got the idea that I was interested in finding anti-protons when I came back from a summer at Brookhaven in '53. Clyde Wiegand and I were working together on this anti-proton for a long time rather secretly. We sort of worked in the daytime on our triple scattering experiments, and we kind of worked at night on the anti-proton thing.

When we finally came to take it to the Bevatron, there was the involvement not only of Clyde Wiegand and me but also Segrè and Ypsilantis. I think Tripp had moved on by that time to the Alvarez group. But Herb Steiner helped us a little on the anti-proton experiment. When it came to authorship on the anti-proton experiment, one had to make

some difficult judgments as to who should be included in and who should be included out. But Clyde Wiegand was the one who designed the quadrapole magnets, and I was the one who figured out how to focus them, set them up for an actual beam.

Oh, there was this funny business about the Cherenkov counter. I have to tell you about that. We may have been the first ones to use a band-pass Cherenkov counter. All the Cherenkov counters that we knew of up to that time were what we called threshold Cherenkov counters. If a particle was too slow to make Cherenkov light, it didn't count; if it was faster than a certain critical speed, it always counted; whereas our counter counted only particles in a band of velocities. If a particle either was too slow or too fast, it would be excluded by our counter. Now, I thought that I had invented this velocity-selecting Cherenkov counter, as I called it.

It happened in this way, that Segrè had gone to the East to a conference, and he had heard a description of a Cherenkov counter from Sam Lindenbaum. Well, Segrè came home and tried to describe this counter, and he botched it. He described it as a threshold counter, but he had about the right picture of it. When he drew it on the blackboard, he got somewhere near right and somehow this drawing on the blackboard suggested to me the way of making a velocity-selecting counter that would count only in a band of speeds. So I made it, and I kept the test model, which is the little round thing up there. I first satisfied myself the thing would work, and we tested it in the cyclotron. Then, on the basis of that test being okay, we built the larger size that we actually used at the Bevatron.

Well, now, this turned out to be exactly the same thing that Lindenbaum was doing; the principle was the same, the geometry was the same. A minor difference was that we used a piece of glass for the radiator, and he had used a sample of gas under high pressure. But the principle of the counter was the same. And I learned, I think before we did the anti-proton experiment, that this was really Lindenbaum's counter. I seem to remember I talked to Lindenbaum about it, and he said, "Well, very good. Good luck." But when we set up the anti-proton experiment, it helped us to have two measures of the momentum of these particles. We went through a pair of magnetic spectrographs, and we had two measures of the speed: one from this velocity-selecting Cherenkov counter and one from time-of-flight between two of the counters.

In the setup where we were trying to detect anti-protons, the time-of-flight measurement should detect one pulse from each of the counters, the pulses being separated by forty nanoseconds. Any particle that got through our magnet system was deemed to have the desired momentum. When we got through with the particle, we knew whether its momentum and its velocity were right for an anti-proton. In the anti-proton experiment, we measured the speed by the two methods: the Lindenbaum counter (our version) and the other, the time-of-flight measurement of the time of travel.

Hale: The time of flight that got you into trouble with a certain person?

Oreste Piccione

Chamberlain: Oh, well, I don't know what to do with Oreste Piccione. Piccione had talked to us in December and January, a period of a week or so, or a little more.

Hale: Fifty-four?

Chamberlain: End of '54 and January '55, he stayed over. He was out for a meeting between Christmas and New Year's, and he stayed over a few days into January. He was interested in finding anti-protons all right. In fact, our original arrangement with him was that he should be part of the experiment, but he went back East promising to figure out how to do some part of the experiment. I think it was the electronics.

Hale: So he was aware of the experiment?

Chamberlain: Oh, yes.

Hale: About his possibly being part of it?

Chamberlain: Yes, yes. But then he sort of dropped out. He got interested in other things when he was back East, and he just didn't follow through. He wrote one letter at the end of January, but most of that was sort of unusable because he was off on a sidetrack. He wanted to build a new kind of quadrapole, which we didn't want to fuss with because I had already built quadrapoles and there was no need to try to design new kinds. And so Piccione sort of went dead and didn't do what he said he was going to do.

I remember we got close enough to seeing anti-protons on the first of October that we thought to put up a blackboard so physicists passing by could know how we were doing. But we must have started trying to find the thing sometime in September. And Piccione actually showed up in the lab about the same time, and I think he too realized that after having sort of dropped out of the experiment he couldn't just jump in the moment we had the apparatus all set up and were starting to do something.

In fact, when he came to town he didn't come see us; he was working with a different group. I remember Clyde saying at one point, "You know, Piccione is in town nowadays," and I said, "Yeah, but he too must realize that he can't very well come suddenly and say now he's a part of the experiment after he's done nothing in this long period."

You aren't immediately part of the list of authors just because you discuss an experiment with somebody. And then, when we declared that we had seen anti-protons--it might have been, like, the 23rd of October--Piccione wrote a letter to Lawrence the same week, saying he was disappointed at not being included. But there was never any thought in my mind that it would have been sensible to include him because he neither had that much influence on what we were doing, nor did he actually do it.

Hale: I thought time of flight was something that was public domain.

Chamberlain: Oh, it was.

Hale: Was it his suggestion to use time of flight as one of the methods of velocity selection?

Chamberlain: I think his claim was more about following one magnetic spectrograph after another, using a double magnetic spectrograph.

Hale: Oh, yes.

Chamberlain: He thought he had contributed that. But, again, the whole business of magnetic spectrograph was pretty much in the public domain. It was typical of Piccione's thinking that when he thought of something, he assumed no one else would be bright enough to think of the same thing--which is kind of silly in science because, of course, it happens all the time.

Hale: Well, after all, also, the time-of-flight selection, which you sort of are at pains to point out in one of the accounts, is a lot cruder than the band-pass Cherenkov counter selection. Is that correct?

Chamberlain: You know, I've kind of forgotten. I think you're right. I think that the time of flight, the way we were using it, was a bit cruder than the Cherenkov selection, although once we had discovered anti-protons, it wasn't so difficult to see anti-protons in a beam, using time of flight only, without the help of a Cherenkov. I know there was some feeling that maybe we might not have had to have the Cherenkov devices to make a success of the experiment. But I know that having two methods of measuring the velocity was very important to eliminating spurious effects.

There's a difference between seeing anti-protons in a beam where it's known that anti-protons exist, and the thing you have to do before that, which is to see anti-protons with such a certainty that you know you can't be fooled by something else that might be in the beam that you weren't suspecting. In other words, there's a difference between the first experiment you have to do to make it so over-convincing that the anti-protons are there that you can persuade people that were in doubt. Then it's much simpler, once you know the anti-protons were in such a beam, to say, "I can see them easily with my time-of-flight device."

Hale: You were fighting a huge background of mesons.

Chamberlain: Yes. One particle in a million coming out of those collisions was an anti-proton. By looking in the forward direction where we were looking, we could enrich that to about one in a hundred thousand; but in the beam we had to look at about a hundred thousand to find one that was an anti-proton.

Hale: And in fact didn't that turn out to be rather more frequent than you thought in advance?

Chamberlain: I think maybe we got two per hundred thousand, but it was very close to what we had guessed it should be. I think my prediction was based on very crude arguments and was still correct to a factor of two. We guessed very well, actually.

Hale: You mentioned how you somehow came back with the idea of hunting for anti-protons after you spent this time at Brookhaven in 1953. How was it decided it was sparked?

Chamberlain: I don't know. I think a number of people were aware that once you got to enough energy, the possibility of producing anti-protons was a real thing. I think a number of people talked about that at Brookhaven.

Hale: I mean, let's face it, it was already contained in implied fashion in the designed energy of the Bevatron.

Chamberlain: Yes.

Hale: And it was already predicted.

Chamberlain: Well, I must confess that at a time when I was interested in looking for anti-protons but I hadn't yet made it a consuming thing at all, I heard that there had been a bet between Hartland Snyder and Maurice Goldaber, with Maurice Goldaber betting some large sum--it could have been $500; it seemed like a huge sum at the time. Maurice Goldaber had bet that the anti-proton didn't exist, and Hartland Snyder had said it did.

Well, I have a great respect for Maurice Goldaber as a physicist and I suspect he made the bet when he was a little drunk, but even when drunk, Maurice Goldaber is a good physicist. So if someone of the stature of Maurice thought maybe anti-protons didn't exist, then this was a real spur to showing that they did. And I think it was at that moment that I decided, "By Jove, this is what I want to do."

Hale: When did you first hear that, the little bet?

Chamberlain: Well, I heard of the bet, I believe, after I had returned from Brookhaven, sometime maybe in the fall of '53 or the winter of early '54. I was already thinking about anti-protons, but this somehow led me to plunge right in. And I think it helps to be in the position I was. I was morally convinced that they have to exist. There was no doubt in my mind that they should exist. It seemed obvious.

See, part of the reason there was doubt was that Dirac had predicted the things about twenty-five years earlier, and something that has been predicted but hasn't shown up for twenty-five years is in danger of being declared nonexistent because it's never been found. It is a little bit like the man who hasn't been seen for twenty-five years. He's liable to be declared no longer existent.

I was going to say one more thing about my interpretation of Piccione. I think at the time he was

disappointed and upset with himself that he hadn't carried through on the experiment. I think he realized that he had dropped the ball, but I think as the years went by, this thing played as a record in his mind and gradually got altered. I think, you know, seventeen years later or something, when he finally brought this lawsuit against me and Segrè, that by that time he was confused about the fact of the matter. He distorted the image in his own mind of how it had occurred. And I know that our lawyer was certainly convinced that this might be the case. He said we'd be surprised how many convicted murderers had gotten convinced after twenty years in jail that they never murdered anybody. They play this thing through in their mind, and it gets altered a very small amount.

I don't know whether that's what happened, but I really think Piccione was misguided when he brought that lawsuit. The suit came to an end when the U.S. Supreme Court refused to hear it. It went through many stages and various levels of court. Just in the last three or four months, the U.S. Supreme Court refused to hear it.

Hale: Ed Lofgren had to have a file of everything that he could remember of it because he would have possibly had to have been a witness. Or I assume he made a deposition statement anyway about the organization of the time on the machines and things like that--how Piccione, in fact, had been an advisor or allowed to use the machines at various times as an outside user.

Chamberlain: He does so to this day. I mean, as recently as a year and a half ago, he was using the Bevatron here, as many outside groups do also. Since he was interested in working at the Bevatron and since he was known to be a good physicist, he was invited to come to the Radiation Lab in the fall of '55. I'm sure there were some unfortunate little accidental things that must have contributed to the way things turned out, but that's always true.

The Radiation Lab was under some criticism from the AEC and the congressional committees that advised the AEC, and so forth, in that it had quite a number of aliens, non-U.S. citizens, on the staff. And it was getting to the stage, about the time Piccione came, where we had to sort of individually justify each instance. "There's no U.S. citizen known to us that could fulfill the same thing," and so forth, and this took a little extra time. It wouldn't be surprising a bit if he was delayed a month or more in his arrival here because he was not a U.S. citizen. But, again,

that's hardly something that we could take into consideration. And the way it all played out was that he really didn't take part in the detailed design of the experiment or the setting up of the apparatus. It wouldn't have made much sense to take him aboard the experiment after we had gotten such a good start on it.

Hale: Yes, I have the impression that he had the idea at some point that he'd been refused the use of machines at Berkeley.

Chamberlain: Oh, I don't really think it was--

Hale: Somebody had the chance to refuse him as an individual?

Chamberlain: No. I think he did claim in the lawsuit that he was afraid to speak up because he thought he would be refused access to the machines, though I don't think that's the way the world works in practice. In an abstract sense, I guess, he could have such a fear. He spoke up the first week, really, so it's not as though he was necessarily much silenced. It was not so specific. I think he claimed that he would have spoken up more vociferously if he hadn't been afraid that his privileges of using the Bevatron might be cut off in some way.

Hale: I remember the things that Ed Lofgren showed me. Nobody had the ability anyway to refuse someone the use of the machine. Essentially, the final authority resided, I suppose, in Lawrence at that period, and nobody could refuse him.

Chamberlain: Well, you know how this is. The decisions, after all, have to be made by human beings, and I don't know how to make the judgment. I don't think there was any literal direct path by which, let's say, Segrè and I could have refused him Bevatron time because we weren't responsible for doling it out at all. There were later years when we were.

When the suit came it, it was quite a surprise. I had no idea that Piccione was harboring this notion at all. When the suit came up, from then on I had carefully to avoid trying to make a decision on Piccione's experiments. There were a couple of times when I was on a committee that had to make judgments about Piccione's experiments. I let them be made by the rest of the committee.

[tape interruption]

Chamberlain: I think in a way, from my point of view, one of the most important things I can do is to highlight the places where I took extra pride in myself in a development, where I recognize something, like this success of Tripp with Ypsilantis going off by themselves. You might not have that kind of documentation in other places.

More About the Anti-proton

Chamberlain: Now, in the anti-proton business, I think I took an idea which was laying around for everybody to fuss with. I ran with it, of course, with Clyde Wiegand because the two of us worked very intently on it. His quadrapoles, while they were a new idea that came from the Brookhaven Lab, were a good design, and he had put a lot into that design. Even then, the design seemed somewhat straightforward; the decisions weren't all that difficult. But it was his design.

I think Clyde and I had good luck more than good judgment when we decided to use the method that we did. Some of the other groups that were looking for anti-protons were having trouble because they put too much absorber in the apparatus. Clyde and I came extremely close to accepting one of those unsatisfactory methods.

For instance, one of the traditional properties that we used on particles was what we called the "range": how much material would they penetrate before they slowed down and stopped. The point was that most anti-protons were being absorbed in nuclear material before this reached the point of slowing down completely and stopping. It would be absorbed along the way, so that instead of having a definite range, even if their energies were definite, they would have a fluctuating range until they've suffered this catastrophic collision where the anti-proton is eaten up in the nuclear matter.

I've been unable to think back and sort of pinpoint what the decision process was that led us to remove the absorbent. We used time-of-flight counters, which put some material in the beam, but very little. We didn't try to put any significant absorber in the beam. And if we had, the anti-protons would have been fewer, and presumably harder to find.

So it was lucky for us that we chose to measure momentum through a series of magnets, which didn't put any material in the way and quit using medium-thick counters, which did put too much material. In fact, the greatest material in the beam must have been the radiator for these velocity-selecting Cherenkov counters, and that was maybe an inch and a half or two inches of glass, which might have been enough to get us into trouble. But anyway, it worked out okay.

I consider it good judgment to pick a matter that didn't absorb. Yet we didn't know that the anti-protons were going to be eaten up, so I can't tell you what hunch it was that Clyde and I had that led us to stay away from a lot of absorbing material. I think in some vague sense we realized we didn't know what they would do in absorbing materials, or any absorber, whether it be a piece of carbon or a piece of copper--we didn't know.

But we didn't sense that the anti-proton would have a big cross-section. We only sensed that we couldn't tell whether it would be big or small. You see the difference. I think that the use of these velocity-selecting Cherenkov counters was a help, at least psychologically. The idea of having two independent methods of measuring the velocity was very pleasing to me because I would have been suspicious that there could be some kind of a fault, or more suspicious there was a fault if we used time-of-flight twice, say.

Hale: It certainly wasn't overkill, anyway.

Chamberlain: I think I agree with you, though occasionally people have remarked that maybe it was a little bit overkill--and it might have been.

Hale: I mean not in the sense that the allowance in the original design of the Bevatron was made for a much larger gap than was eventually used.

Chamberlain: Oh, that's true. You know, the Bevatron had two versions right from the start. We've covered that quite extensively with Lofgren. They couldn't see ahead and know that the oscillations in the orbits would be smaller than expected so that you could use a smaller aperture.

Hale: By the way, you mentioned glass. It was quartz, wasn't it?

Chamberlain: It might have been fused quartz, but I'd call that a kind of glass. At one time or another, we talked about a number of kinds of glass: rare-earth glasses and fused quartz--and I

have forgotten what we used in each case. In fact, you know, ordinary glass is really based on quartz. You only change its melting point by adding impurities to it, but SiO_2 is the main compound in all the glasses I'm aware of, or most of them.

I had my private little group in my head of the physicists that I particularly admire, and I look for their approval to some extent. I remember just before we started looking for anti-protons, we had the apparatus almost set up, and Jack Steinberger, who is one of the physicists that I much admire, came for a visit to the lab. I showed him the apparatus, and he said, "By Jove, that's a lovely apparatus. I think you're going to find anti-protons with that." He was very encouraging, and that was kind of nice.

Career Moves after the Anti-proton Experiments, 1957-1960

Chamberlain: After the anti-proton thing I had a sabbatical in '57-'58. It was in Rome, and I mention it mostly because it was a little bit of a period of letup or change-up in my work. Then in the fall of '59 I was at Harvard for one semester as the Loeb lecturer. While I was at Harvard, Dick Wilson talked to me--he's another Harvard professor. He said, "The rumors are all around that it's possible to make a polarized proton target. Somebody's got to take the time to develop it. And, you know, somebody that's just won the Nobel Prize might be the person--because, after all, he doesn't have to produce a new paper every year; he can settle down and take a few years to one problem."

So when I returned to Berkeley in what must have been February 1960, I started fairly directly, really in response to Dick Wilson's remark, working on polarized proton target. Now, at that time, with the granting of the Nobel Prize, I thought it better to get a little bit more independence from Segrè. I felt that my work had been enough dominated by Segrè that I wanted more independence, and I think with the Nobel Prize I felt that I had a little bit more leverage to do so.

I had asked Ed McMillan to assure me that I could set up an independent group of a few people--not necessarily large, but some independent group. This was okay. I figured that it really took four physicists to do an experiment, as a minimum, so I had some kind of a verbal assurance from Ed

McMillan that that sort of core would be available. I don't think I expected that all of that group of four would count myself; I don't think that all the other three were expected to be brought in from outside the lab or newly hired, or anything like that. I did expect that maybe one might be.

I set up an independent group starting in 1960, and then a few years later, which might have been around '64--I don't know which year--both McMillan and Bob Thornton approached me, saying wouldn't I be willing to go with the Segrè group at least for administrative purposes if I had some assurance that my work could be rather independent. It was a nuisance for them to deal with so many groups.

Formation of the Segrè-Chamberlain Group, 1964

Chamberlain: I got Bob Thornton to agree that he would be a kind of an arbiter between me and Emilio if any such arbitration should be needed. And I got Emilio to give me assurance that it was intended that I have a good deal of independence in what I did and that I didn't have to work on the same things that he wanted to work on. So from sometime like maybe '64, '65, we called it the Segrè-Chamberlain group and have been together ever since.

In that period Segrè has really been working more on different things than I have. Herb Steiner, Gil Shapiro, and I have followed this thing centering around polarized proton targets. Segrè has gone into other things. In the last years he was putting a lot of effort on his texts, his books. And he's been more active in helping Clyde Wiegand with the mesonic X-rays than he has us with the polarized target things. The polarized target work is constituted as sort of a sub-sub-group with in this administrative Segrè-Chamberlain group.

Now, there was one development that I was sort of proud of in the polarized proton target group, a thing which I felt that I saw clearly and other people either didn't see or didn't respond to. I've been told many times that the idea was known to many people other than me. But I felt that we were the first ones to make a large polarized proton target. By that I mean much larger than half wavelength of the microwaves that are used in causing the protons to be polarized.

The usual way of making a polarized proton target for us uses a combination of magnetic field and low temperature; you shine microwaves on the sample. Well, the magnetic field puts a large energy difference between electrons which spin one way and electrons which spin the other. The low temperature is needed to get the electrons to respond to this force of the magnetic field. So in low temperature and high magnetic fields, the electrons get highly alarmed--many spinning one way, a few spinning the other. Then the microwaves are used to transfer this polarization to the protons. You start by polarizing electrons and use microwaves to transfer them to the protons.

Up to the time we built the first large target, I think all the targets that we knew of were of the size of a few millimeters, which was about the half wavelength of the microwaves we used. Now we made a target whose dimension was a few centimeters. This is only a factor of ten in the dimension, but it means a thousand times as much material in the target for the beam to scatter off. And this was not a difficult transition because I had studied electromagnetic modes of oscillation and cavities in the lab on campus.

This is one of the cases where the combination of teaching and research meshed well together because I picked up ideas quickly from my teaching, which were just what I needed for handling the microwaves. After all, I was not a microwave specialist except that I was a teacher. So that if I hadn't been teaching electricity and magnetism and been in the lab where some of these experiments were done, I might have had much more difficulty responding to this.

Carson Jeffries and the First Polarized Proton Target Experiment

Hale: What about the work of Jeffries?

Chamberlain: Jeffries was very important. Now, when we first started working on the polarized proton target, there was some encouragement from Carson Jeffries. We were relying on him to tell us, you know, how it might be done. But when I first started doing it, we thought we were making polarized proton targets not for rare-earth salts, which we first used in practice, but we thought we were using it for irradiated polyethylene.

We thought we'd be lucky to get 7.5 percent polarization. Actually, we got 15 percent polarization in the first sample of rare-earth salt. We call it LMN, lanthanum magnesium nitrate. Jeffries first told me in the hall, "You know, on paper it looks as though if you put neodymium as an impurity, as a doping of your LMN, it's going to give you the best polarization." In fact, he said, "Lanthanum magnesium nitrate rather than ceramide magnesium nitrate with neodymium impurity ought to be the best polarized target of this kind." And he said, "We'll try it in the lab in a few days."

Then, a few days later, he came by and said, "It's great. Polarizes much more than anything else we've found." So right in the middle of our design, we were building up the parts. We could switch to an LMN target very easily because everything we made was quite consistent; we really didn't have to change much. We immediately switched to an LMN target.

I think our first target had 15 percent polarization, which was good; and we did the first experiment that you could call a high-energy scattering on a polarized proton target. The previous experiment had been something less than 30 MeV protons, and these were with scattering of ions and a good deal higher energy, and it was a first step of sorts.

When we submitted that paper to the *Physical Review Letters*, it was turned down. It turned out that the person who reviewed it for the editors didn't understand that this was the first polarized proton target experiment. You know, we said we constructed a polarized proton target and we've used it for this kind of a scattering experiment. He said, "It shouldn't be published until you've done the full analysis of these results." That would have taken another year to do, so it wouldn't have made sense. Here we've done something new and different and we wanted to tell the world about it, so we just sent it off to *Physics Letters* instead of *Physical Review Letters*. That was fine.

A few years later, I saw the editor, Sam Goudsmit. He said, "You should have phoned me." And I said, "But, Sam, you set up a system that doesn't require personal conversation with somebody in order to get something through the sieve." I happen to take the view that we should publish pretty much everything that the Physical Society members submit. This business of having screening of what's

published, I think just as many good papers are screened out as bad ones.

The percentage of good papers is not improved by this screening process, in my view. And it's because of things like this, where something new and different comes along and the people that are doing the screening can't recognize it. And it's bound to happen. I frankly would be glad to have the papers not refereed at all.

I admit that the referees have occasionally relieved me of some embarrassment. One paper we sent in had a whole lot of results and whatnot, and we forgot to specify the energy in which the experiment had been done. Fortunately, the referee asked us for the energy and we put it in the paper. It would have been kind of silly to forget to tell people at what energy these measurements were done [laughter].

Now, the polarized target experiments were in large part fairly successful. However, I'm really regretful that we didn't do a second and third and fourth generation of polarized proton target experiments. We should have kept at it with more vigor because we really quit before we'd achieved as much accuracy as we should have. I don't know why I didn't press for a later generation of experiment a little more.

Hale: Just to improve accuracy?

Chamberlain: Yes. You gain a lot by improving the accuracy. We were getting the general outline of what was going one, but just now, these months, there's a very nice set of results coming out of the Rutherford Laboratory in Britain. There, instead of doing ten different energies, they're doing thirty different energies in the same region where we were working, and in each case they're getting accuracy that's many times --maybe five times or something like that--better than what we were able to give.

I guess they've worked long and hard. They've been patient; they've taken many data that run for weeks and weeks. I think there were times when I could have had a little more encouragement from some of the people around, but I really feel it's my own fault that we didn't press for these later generations of polarized target experiments.

Then what happened, quite to my surprise, I lost the Bevatron out from under me. It switched over to heavy-ion work, and I no longer had the opportunity locally to do

those experiments. If I had been willing to put a higher priority on them, I could have finished them before the machine was lost to us.

Hale: And all those experiments are basically the pion collision experiments, is that correct?

Chamberlain: Yes. We did some proton collision experiments, but we were concentrating on the pions because we thought it would be easier to analyze them fully first.

Hale: Why would that be?

Chamberlain: Well, in the pions there are only two basic scattering amplitudes, which I believe means that there are four basic experiments. Whether it follows from the fact that it's two squared, I'm not quite sure--for, as in the proton-proton case, there are five basic amplitudes, and I think that means there are twenty-five basic experiments.

I analyzed a couple of years ago fifteen varieties of experiment that I thought were the first fifteen to do on proton-proton scattering. And of those, it seemed that twelve would be necessary before one could get a unique answer. So the proton-proton system requires, let's say, twelve experiments instead of three before you get the full answer. Or fifteen instead of four. It's a tougher problem, and you therefore don't tackle the proton-proton case first.

Now, I happen to believe that the proton-proton case, having more amplitudes, is richer with information, so I am in fact very enthusiastic about doing these things. Now, to do them completely requires a polarized beam being accelerated, I believe. There's a thing that I didn't bring out because it didn't cross my mind at the moment, but while we worked at the 184-inch cyclotron, we could polarize the particles by scattering. As the energy goes up, that becomes less and less effective.

If we had to polarize the Bevatron beams by scattering, we'd get only 1 and 2 percent beam polarization. That would be awfully hard to do accurate experiments. At the high energies it becomes important to accelerate the beams while they are polarizing, to keep them polarized while they are being accelerated. That's what they can do now at the Argonne Lab in the last couple of years.

So that's why the door is open at the Argonne Lab to complete analysis of p-p scattering. I'm pretty sure that they're well on the way to getting a complete description of the 6 GeV case and that they will be able to do the same thing at 12 GeV before the machine gets turned off. I think the results are going to be quite amusing and really quite interesting. Well, we'll see, we'll see. Can't tell yet.

I'm going to a conference at the Argonne next week on polarized things, and I'm hoping to find out just how they stand, and maybe bring back a few tidbits that I can analyze here and see who far along they are. Of the twelve experiments that I believe they need, I think they've done ten now, or nine, so they're getting pretty well along.

Hale: You think that this possibly would just be accumulated amount of days, or will we see some possible quantum jump in understanding?

Chamberlain: No, I don't think it'll be too startling because the basic processes are known once you see the processes occurring without having any polarization involved. You get a kind of detail when you measure the polarizations that you couldn't get before, and that detail only comes into clear focus if you make a complete determination.

I'm terribly curious to see how these results turn out because there are still several possibilities in my mind that might turn up. What I'm expecting is that two of the amplitudes that are thought to be small will be small. The other three that are thought to be large could come out in any combination. I just don't know. I'm very curious to see how these things come out. I think it's at least going to tell us whether some of the idea about Regge-poles are correct or false. Something's going to emerge. But it's not going to be a huge surprise. I mean, you won't read it in *Time* magazine, I can assure you.

Hale: Is there, I assume, some sense in which the pion-proton experiments can be synthesized in some way to predict what you should get in the proton-proton experiment?

Chamberlain: Oh, well, you're hitting on a sore point. The theoreticians originally thought that there would be a great deal of hope doing just what you say. Once you know the pion-proton scattering, you ought to be able to predict the proton-proton scattering. If we could, it would give a great deal of help in the feeling of real confidence for nuclear physics. Then we'd feel, okay, we know the basic forces, we

know how they're put together when two particles collide, we see how nuclei are put together, and we had a kind of understanding it would be very satisfying.

It turns out that this has been, so far, difficult or impossible to build one on the other. Essentially because these processes occur so abundantly that you can't get there by first approximation and second approximation and third approximation, simply because each approximation turns out to be bigger than the previous one instead of smaller.

Physics has had great success where perturbation theory would work. And that's where a big effect might be taken into account, and then a slight correction to that, and then a still smaller correction to that. You have a series that with each turn gets smaller; then you feel that you're working closer to the right answer.

But in this strong interaction physics, successive approximation is more complicated than the one that went before. It's also giving bigger answers than the one that went before, and therefore you get yourself deeper and deeper into a kind of mucky situation if you try.

Perturbation Theory

Hale: Is there some main theoretical reason for that?

Chamberlain: Well, I'm really saying the same thing. I'll put it down in different terms by saying that perturbation theory is the most satisfactory theory for covering a complicated situation. It only works if the perturbation is small enough that in second approximation you get something that's a smaller change than what you got in the first approximation. So you get a series of decreasing contributions to the total.

Hale: In other words, you can't use it in a domain where the perturbation is relatively large?

Chamberlain: That's right. I think the reason is this: It usually turns out that the first approximation is easy to calculate; the second approximately is difficult but manageable; the third approximation often takes a great deal of care and time and effort, and may be unmanageable for a number of years but finally becomes manageable. Now, in the meantime, we've got

a good theory if the first approximation is the main effect. You do that early on. With effort, you show that the second approximation makes things come out even a little closer; and maybe the third approximation, if all is well, kind of brings within an _____.

But if you're in strong interaction physics, the first approximation gives you an answer that is crudely right but only very, very crudely. It doesn't look right in much detail. Second approximation turns out to be just as big as the first so that when you went to the extra trouble of doing the second approximation, you didn't improve the answer very much because you changed the answer quite a lot. You didn't look as though you were converging on the truth.

Nobody's had the patience to try a third approximation under those circumstances because it seems clear it's going to be a huge term, and instead of working successively closer to a right and more right answer, you're just dealing with big terms. There's no hope of calculating the fourth approximation yet, so you kind of give up almost before you've gotten too deeply into it.

Now, given that circumstance, my answer is--by the way, here I am very unconventional--that we experimenters ought to solve both the proton-proton and the pion-proton problems. In effect, lead the theoreticians to the right answers by showing them which terms are important in their theory and which ones are unimportant.

In other words, if the theory won't work on its own, I'm trying to get illumination from the experiments as to what parts of the theory are the most important. Now, this is speculation because I'm not sure we'll get anything but a muddle. It may be that at one energy the proton-proton scattering seems to show that one part of the theory is important, and at another energy it shows that another part of the theory is important.

Maybe I won't get consistent, easy answers, but I hate to give up, you know. It's like the mountain climber. When he sees the mountain there, he wants to climb it. I just hate to give up until we've done a complete analysis on the proton-proton scattering. I really wanted to extend it to call it nucleon-nucleon scattering, where nucleons are either neutrons or protons. That's a little more general context, which would be still more meaningful.

S-matrix Theory

Hale: Is this the background against which S-matrix theory comes to be more important?

Chamberlain: Yes, it is. It's the right background for S-matrix theory. However, S-matrix theory has been running into its own troubles. While it has a background of truth to it, its embodying methods aren't at the present thought to be particularly meaningful for making further progress. It's got a lot of correct things within it, things that we believe in and use, but there's not a lot of current progress that I know of in the field of S-matrix theory. So we're still stuck.

Hale: But is that the sort of problem they are trying to address?

Chamberlain: Yes, although what I think of typically with S-matrix theory is that it's attempting also to introduce scattering of protons by anti-protons. That's yet another extension which I wasn't going to mention. S-matrix theory would be applicable to these, would be most important, I think, if we extended our study into studying proton-anti-proton collisions. But that's a ways down the road yet for me. I'd like to see a complete analysis of p-p scattering first.

Hale: That series of pion-proton scattering papers had about thirty papers, as far as I can tell, in a period of about ten years?

Berkeley Physics Department's Golden Era, 1955-1960

Chamberlain: Of course, our golden era was around 1954 and 1955, when we were in the middle of the triple-scattering experiments and still also able to do the anti-proton experiments at the same time. I had the feeling that those years everything we touched turned to gold a little bit. I don't think there's ever been a period in our research life that was like that.

I liked very much the idea of trying to make a fairly complete study on one kind of process. I think you learn more from complete studies than you do from a lot of partial studies. But the number of times we've been able to achieve that goal has been most limited. We aim at complete studies; we often don't get there. Some of the magnets that

we would have needed for a complete study I've had on the drawing board for six or seven years. But each year there wasn't enough money to go ahead with them, and we didn't see the polarization experiments going rapidly enough ahead so that we'd get into them. Now we've lost our chance. Too bad [laughter].

Hale: Between '55, when you knew that anti-proton was essentially established and solved, and '59, when you won the Nobel Prize, there was a mixture of both?

Chamberlain: Well, let's see, it's almost hard for me to remember. It includes '57, '58, being away on sabbatical. And prior to going away on sabbatical, I'm pretty sure that a lot of hard work went to some polarization experiments.

Hale: The Italian connection? The things that you had with the Italian group, using emulsions.

Chamberlain: Oh, yes. But they preceded the anti-protons more than followed. They certainly were concurrent, but we made some measurements, I believe, of how often anti-protons turned into anti-neutrons in going through a target. We made a few supremative absorption cross-section measurements, I believe, for anti-protons. Now all of those experiments have been completely redone. They've been improved upon enormously.

This machine was the first machine that had enough energy to make anti-protons. The higher energy machines make anti-protons more copiously. So within a year or so, we found ourselves in the position that the higher energy machines, such as the Brookhaven A.G.S.--alternating gradient synchrotron--were going to produce anti-protons much more copiously. The best beam down here was likely to have maybe two anti-protons per hundred thousand particles, whereas at Brookhaven Lab, the best beam had 1 percent anti-protons. It's quite a difference.

So the real experiments that need anti-proton beams were never going to be best done at the Bevatron, even though they had been discovered here. That is perhaps a little different from what you might first expect. It's a really obvious fact of life. This machine had barely enough energy to make anti-protons.

All right, '57-'58 I was away; then in '58-'59 I was here. Frankly, at the moment, I don't really remember what I was doing in '58-'59, but in that one year--it was a short

year here, being away again in the fall of '59. In '58-'59 I think we were doing some more polarization experiments, probably involving the pion-proton scattering, but I've forgotten just when those came in.

Hale: I noticed one that stood out here to me in 1961, the search for a neutral-meson in hyper-spin. It seemed to stand out like a sore thumb.

Chamberlain: Well, that was another of these artifacts. I suppose we were doing some polarization experiments in that period because there's a September 1960 thesis by Clift on that subject, the search for a hyper-spin meson. Years later it was found; it was called the eta meson. But the eta meson was too massive to be made by the 184-inch cyclotron. I had the idea that if the eta had had a lower mass, I think we would have found it. However, it happened that our method was not necessarily the most sensitive. I enjoyed looking for the thing, but if it had been in that mass range, I think we would have found it. I do believe that other more sensitive methods would have shown up sooner or later, and if we'd missed it somebody else would have found it.

Hale: Would you find it at the Bevatron?

Chamberlain: It was purely my hunch. I had no sure knowledge that the thing existed. It turned out that it did, so the hunch was good. It could have been found at the Bevatron. It probably was found at the Bevatron. At least it was found in bubble-chamber pictures, which are likely from Bevatron exposures.

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Owen Chamberlain Oral History Presentation

The Bancroft Library, University of California, Berkeley - 24 October 2000
Remarks by Herbert Steiner, Professor Emeritus of Physics, UCB

Owen, Senta [Chamberlain], Ladies and Gentlemen:

It is an honor and a privilege for me to say a few words about Owen on this occasion. We have known each other and worked together for 47 years. As an illustration of how far back we go I remind you that I knew you when it was intellectually and socially fashionable to have a pipe clenched between your teeth and a slide rule protruding from your shirt pocket. I watched your crew-cut brown hair recede, only to appear again in a salt and pepper hue elsewhere on your face, and then, later still, even your beard faded quietly into the sunset.

Owen was born in San Francisco on July 10, 1920. He graduated from Dartmouth in 1941 and came to Berkeley for his graduate education. In his autobiography Emilio Segrè says:

> In one of my optics courses there was a student who amused himself in finding flaws in the lectures. His objections, always polite, were often well taken and showed a critical and alert mind. I appreciated the young man, who obviously was interested in the course, and used his head, and I made friends with him. He was Owen Chamberlain.

It wasn't long before Owen became Segre's graduate student. Owen's beginnings as an experimental physicist at Berkeley were far from auspicious. Again I quote from Segrè:

> During the summer of 1942, a theoretical group under Oppenheimer's direction met in Berkeley to try to design a nuclear bomb. Hans Bethe, Robert Serber, Edward Teller, E. J. Konopinski, and two younger physicists, Stanley Frankel and Eldred Nelson, worked on this project. As they proceeded in their calculations, they needed more and more experimental data that had not been measured, and we tried to help them out as much as possible. To proceed with a concrete plan for a bomb, it was necessary to know, among other things, the fission cross section of uranium, as well as many other cross sections, as a function of neutron energy. At the time such data were few and unreliable. It was hard to obtain monoenergetic neutrons of known energy between a fraction of an eV and a couple of MeV. Some specific energies could be reached using photoneutrons.

> Chamberlain, Wiegand, and some other students, and I used photoneutrons generated by gamma rays of Na^{24} on beryllium or deuterium. During these experiments we had a nasty accident when Chamberlain dropped a strongly radioactive solution of radiosodium. He was seriously irradiated and his blood showed sufficient alterations to require a vacation.

In 1943 Segrè, with Owen in tow, moved to Los Alamos, where they made important measurements of spontaneous fission half lives. At the end of World War II in 1945 he followed Fermi to the University of Chicago to join what surely must rank as one of the most impressive groups of physics graduate students ever assembled in one department. His thesis, submitted in 1948, was on neutron diffraction from crystals. He joined the Berkeley Physics Department as an Instructor in 1948. I had just started as an undergraduate at that time, but I was never in any of his classes. Our paths really didn't cross until the summer of 1953, when I joined the Segrè/Chamberlain Group at the Rad Lab (as it was then called) to do my Ph.D.

As one who had been brought up in the Germanic tradition to believe that physics professors usually sit directly beneath the right foot of god, or visa versa, the first name informality that Owen preferred was a major cultural shock. There were typically 6 or so students in the group at any one time, and there was really no distinction between the Chamberlain and the Segrè students. We all worked together. We were all just a little intimidated by Segrè, who didn't suffer fools lightly, but who also taught us a lot from his extensive experience as a physicist, and his ability to focus on the essence of a problem. Owen was the person we would go to when we didn't understand something or when we wanted to learn more. More often than not his explanations were original, and sometimes even unconventional, and we learned a lot. I wish we had collected all the Chamberlainisms that came out of these discussions. They would make a marvelous upper division physics course. The other key member of the group was Clyde Wiegand, who was the superb experimental physicist who turned our crazy ideas into reality, and who taught us by example how experimental physics should be done. He was also the one we would go to ask the questions we were too embarrassed to ask even Owen. One student, in particular, of those times deserves special mention, Tom Ypsilantis. He was always full of ideas, some of which even worked, and he played an increasingly important role in the activities of the group as time went on. Unfortunately Tom passed away a few months ago in Geneva.

At that time (1953-54) the main thrust of the physics program of our group was the study of the nucleon-nucleon interaction, and a number of scattering experiments were then underway at the 184" cyclotron. The most innovative part of the program involving the production of polarized beams and their subsequent use in scattering experiments had just been launched by Tom Ypsilantis, and quickly became the major

focus of this program. It may have been this initial contact with polarization phenomena that stimulated Owen's later research interests with polarized targets.

In 1954 the Bevatron was nearing completion, and it was clear even to lowly graduate students like me that searching for antiprotons would be an early experimental objective. It was also clear that our group was not alone in making plans to do such an experiment, so a here-to-fore uncharacteristic sense of competition and secrecy entered the picture. It wasn't unusual to see Owen and Clyde hunched over Clyde's desk in Room 203 of Building 50, earnestly engaged in conversation, or to see the "big four" (Owen, Clyde, Emilio and Tom) getting together for discussions behind closed doors. But the group being what it was, and physics being what it is, it wasn't possible to insulate the rest of us completely from comments about the "secret weapon" and other parts of the project, and it wasn't very long before I, too, found myself working on this experiment. By the way, the "secret weapon," which was later also known as the "Pickle Barrel," was a very innovative velocity-selecting Cherenkov counter designed by Clyde and Owen, which played a crucial role in identifying the very rare antiprotons in the copious background of other particles.

The weekend before we made a serious first attempt to look for antiprotons in the fall of 1955, Owen decided to insert a water-filled detector into our beam as an additional means of rejecting background. Of course such a counter did not exist at the time, and I remember quite well working with him late into the night on a Saturday to couple an empty orange juice can to a photomultiplier tube. We finally succeeded in making it light and water tight, and installed it in the experiment. Initially it worked like a charm but over the next days and weeks it worked less and less well, until one day we opened it to find a brown, murky goo inside. We learned that orange juice cans do rust. The antiproton phase of Owen's research career was relatively short, only from 1954 to 1959, almost certainly because the flux obtainable from the Bevatron was not competitive once the AGS and the CERN PS became operational.

In 1960, upon his return to Berkeley after a sabbatical year in Rome, Owen pioneered a project that was to occupy him for the next 25 years. Anatole Abragam at Saclay and Carson Jeffries at Berkeley had independently just developed the technique of dynamic nuclear polarization. Owen immediately realized the importance of this technique for particle physics experiments. Together with Jeffries, Gil Shapiro (then a post-doc who had just come to Berkeley) and Ray Fuzesy (the technical wizard, who played a crucial role in all of the experiments of our group during the next 30 years), they set out to build a polarized target. I think this project exemplifies very well Owen's talents as a physicist. He had to master a whole series of new techniques which were not the bread and butter of high-energy physicists, such as cryogenics, magnet design, microwave generation and transmission, and the development of RF systems to measure the polarization. Soon every major high-energy physics laboratory in the world was engaged in one or more polarized target experiments. Suffice it to say here that these

experiments produced important breakthroughs in our understanding of the basic interactions between particles.

Etienne Barrelet, who was a post-doc in our group in the early '70's, and whom I saw just a few days ago at DESY, reminded me of the seemingly chaotic and yet incredibly effective filing system used by Owen. All of you, who have shared the adventure of visiting Owen in his office, be it on campus or on the hill, were surely awed, if not frightened into abject silence by the towering piles of binders, papers, journals, books and other junk, which occupied just about every square centimeter of table, desk, floor and cabinet space, and which threatened to inundate the unwary visitor at the first flap of a butterfly's wings. As one measure of the filling factor in Owen's campus office, space was so tight that the blackboard used to explain concepts to students during office hours had to moved to the hallway. Anyway, Etienne and Owen were discussing some physics problem of common interest at LBL, with Owen calculating on a piece of mechanics bond paper as was his wont. Eight and a half years later, during a short visit by Etienne to Berkeley, they happened to return to the topic they had discussed many, many years before, and Owen without hesitation reached deep down into one of the many piles on his desk and retrieved the paper.

In the middle 1970, although his interest in spin-related physics never wavered, he involved himself in two quite different projects. The first was devoted to exploratory measurements with high-energy nuclear projectiles at the Bevalac. The other project was the so-called Time Projection Chamber (or TPC) which was initiated by Dave Nygren. When Dave came to Berkeley he was put into an office in our group, and it did not take him long to notice the superb technical talents of Ray Fuzesy, whom he quickly attracted into helping him with the early prototyping. So it was quite natural, when the TPC became an actual project, for Owen to get involved in this work. Once again Owen confronted new technical problems of considerable complexity and solved them elegantly in the typical Chamberlain style.

What is that Chamberlain style? I think many years ago Owen must have learned a few basic concepts very well, and in the interim he has developed an uncanny ability to put this basic knowledge together in his own unique way to address whatever question or problem may have been posed. Many of his students and co-workers have been exposed to Chamberlainisms of one kind or another. How often have we been at lunch with Owen when some mystifying question came up? While most us would sit around the table pondering and chewing on our hamburger, Owen would make some seemingly irrelevant comments, which upon closer scrutiny were the answer. I suspect Owen must often have wondered what was wrong with the rest of us when we didn't immediately come up with his "obvious" solution by ourselves.

I have focused here primarily on Owen's scientific accomplishments, but of course Owen's activities extend far beyond physics. He has been an inspirational teacher, with

a deep interest in working directly with students. A few years ago our faculty colleague, Art Rosenfeld, recounted the following incident:

> It was a warm evening during the Free Speech Movement in the mid 60's. Students had taken over Moses Hall, demanding some UC action which Owen and I thought was partly reasonable, partly overdone. And of course we thought we could talk them into a compromise. But we couldn't get in to talk to them because the UC police had sealed off the building. So we shouted at the students on the 2nd floor that we wanted an audience, and pretty soon they lowered a hefty rope. We tied a knot in it for a foot hold, and hung on tightly, and first Owen, and then I were hoisted to the 2nd floor window. After an hour or so, with lots of coffee, beer, and doughnuts, we did talk them into a compromise; after which they courteously lowered us back to ground level, and we both went home, satisfied. I cannot remember anything about the issues, but I know we both enjoyed it.

One of our students in the late '80's, Matt Kowitt, was a "Dead-Head," i.e., a dyed-in-the-wool fan of the Grateful Dead. One day he invited Owen, who is actually a closet "Dead-Head," to a concert at the Oakland Coliseum. Before doing so, through a complex network of intermediaries, he contacted the "Dead" and explained who Owen was, and that he would like to meet them. I think one of the highest points of so many high points in Owens career was when he was invited to be on the stage with the performers during the second half of this concert. Shortly thereafter Mickey Hart, the lead drummer, asked Owen: "What did the Big Bang sound like?" which must be a question dear to the heart of any percussionist.

Owen has been a driving force in the sphere of human and civil rights, as an advocate for peace and disarmament, and as one deeply concerned with offering opportunities for professional development to young people. The list of these accomplishments is long and his commitment is great. I am sure I am not alone in saying that Owen has enriched my life enormously, and I know you will want to join me in wishing him and Senta and their children all the best in the coming years.

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