HANS A. BETHE
(1906-2005)

INTERVIEWED BY
JUDITH R. GOODSTEIN

February 17, 1982, January 28, 1993

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Abstract

Two interviews conducted at Caltech in 1982 and 1993 with theoretical physicist Hans Bethe. The recipient of the Nobel Prize in physics in 1967 for his work on nuclear reactions in stars, Bethe was born in Strasbourg and educated at the University of Frankfurt and at the University of Munich, where he earned a PhD in 1928 under A. Sommerfeld at the Institute for Theoretical Physics. From 1928 to 1933, Bethe held a variety of teaching positions in Germany, also visiting the Physics Institute of the University of Rome in Via Panisperna 89A in 1931 and 1932. Hitler's rise to power forced Bethe from the University of Tübingen in 1933. Two years later he became an assistant professor at Cornell University, garnering a full professorship there in 1937. In the 1982 interview Bethe speaks principally about his contacts at Caltech, including L. Pauling, R. Millikan, T. von Kármán, F. Zwicky, C. C. Lauritsen, W. A. Fowler, R. Feynman and R. F. Bacher. He discusses his relations with other prominent physicists, including E. Teller, N. Bohr and J. R. Oppenheimer. He also describes his first impressions of nuclear physics, the political climate in Italy in the 1930s, and the Rome school of physics, including E. Fermi, F. Rasetti, and E. Segrè. The 1993 interview

concerns R. Bacher at Cornell and at work on the Manhattan Project at Los Alamos during World War II.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH HANS A. BETHE

BY JUDITH R. GOODSTEIN

PASADENA, CALIFORNIA

TABLE OF CONTENTS

INTERVIEW WITH HANS A. BETHE

CALIFORNIA INSTITUTE OF TECHNOLOGY

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Interview with Hans A. Bethe **by Judith R. Goodstein**

Pasadena, California

Session 1	February 17, 1982
Session 2	January 28, 1993

Begin Tape 1, Side 1

BETHE: I didn't know Linus Pauling well in 1926. We both were at [Arnold] Sommerfeld's institute [at the University of Munich], but I was a beginning graduate student. I knew there was this man Pauling, and I knew some of his work on energy levels of atoms, which I thought was not good enough because it was based too much on the old quantum theory. So I didn't pay much attention to him.

GOODSTEIN: Did you tell him that?

BETHE: No. I was much too low down on the totem pole to do so. Anyway, he very soon adopted more modern methods. And it is true that the good methods were really not invented until '28.

[Robert A.] Millikan of course I knew. I knew about his measurement of the charge of the electron, which was really a marvelous piece of work. Then, he once came to Munich—I am not sure just what year it was—it was in the early thirties, but I don't know which year.

GOODSTEIN: He went to the nuclear physics conference in Rome in the fall of 1931. It might have been then.

BETHE: It could very well have been the same time. He gave a talk at Munich on his ideas about cosmic rays. And that, of course, was his most consuming interest at the time. I was not impressed by the talk. You probably know that his ideas about cosmic rays were proved wrong not very long afterward. At that time, there were already many people who believed that cosmic rays consisted of particles and not of gamma rays. Millikan's theory was that as the cosmic rays came into the atmosphere they consisted of gamma rays of a number of different energies, and these energies corresponded to the formation of the helium nucleus and the oxygen nucleus, and, I think, the silicon nucleus. He claimed that this would all happen in interstellar space, where there are almost no particles anywhere. That didn't make sense. And the idea that twenty-eight particles would come together to form the silicon nucleus made even less sense. So I was not impressed by this talk.

GOODSTEIN: Do you remember what the reactions of the other people were?

BETHE: I only remember the reaction of some of my contemporaries; we rather made fun of it. But I don't know what the older people thought of it. I didn't ask Sommerfeld what he thought of it.

GOODSTEIN: Who did you have in mind as your contemporaries?

BETHE: Well, you probably wouldn't know the names. And I'm not even sure now who they were. They are not important.

Then I came to this country [to Cornell] in '35. In particular, I heard about Caltech, and I found out how much Millikan had done for Caltech. There is just no question that he was a very great man. He stimulated people tremendously. In the case of cosmic rays, he had the wrong idea. But at the same time, his people—[Victor] Neher, [William] Pickering, and a few others—did a lot of good work which later on was very useful for everybody. Millikan was very much convinced of his own importance. I didn't meet him very extensively. I'm not sure whether I even saw him in 1940, which was my first visit to Caltech. I met him later, on more extensive visits, and he was very, very nice to me but also quite distant.

GOODSTEIN: Do you think it was because you were a theoretician?

BETHE: No, I don't believe so. You think he was anti-theoretical?

GOODSTEIN: As he grew older, he distrusted theoreticians. I don't think he felt very comfortable with them. But this is just an impression I have.

BETHE: I'm afraid I have no evidence one way or the other. Of course, at that time, the main theoretician here at Caltech was [J. Robert] Oppenheimer.

GOODSTEIN: Also, to a lesser extent, Paul Epstein.

BETHE: Yes, that's right. And then there was [Richard Chace] Tolman, who was in a slightly different field. I don't know anything about Millikan's relations to theoreticians. I know that he had a simply marvelous experimental physics department—Carl Anderson was here.

GOODSTEIN: Caltech was very strong in experimental physics.

BETHE: Yes. Now that was not so unusual in those days. Certainly until the mid-thirties, the United States was quite prominent in experimental physics, but not in theoretical physics. There were relatively few theoretical physicists. Oppenheimer was, perhaps, the best of them. He was here about half the year. And [John H.] Van Vleck at Harvard. And two or three others. But that number was relatively small. Working with their hands, Americans could do a great deal of experimental physics very well. And theory didn't really become prominent until the mid-thirties. So if Millikan was anti-theoretical, he shared that feeling with the best of all nuclear experimenters—namely, [Ernest] Rutherford. That's good company to be in.

GOODSTEIN: True. What was the occasion for your coming here in 1940?

BETHE: In 1940 I was very much concerned with the terrible things going on in the war in Europe. I was, at that time, teaching summer school at Stanford. And I wanted very much to do something for the war effort. It was pretty clear that the United States would get into the war. It was certain that the British needed all the support that was possible. So one subject that I thought would be very useful was aerodynamics. There was, here at Caltech, Theodore von Kármán, who was, for me, the leading aerodynamicist in this country, and maybe in the world, at the time. So I came down here with Edward Teller to ask von Kármán whether he had anything for us to do.

GOODSTEIN: Had you ever met von Kármán before?

BETHE: No. He was quite a character, as you surely know. I can tell you more about von Kármán, if you want.

GOODSTEIN: Yes, I would be interested in hearing more.

BETHE: He was deaf, but he was selectively deaf. He could hear what Teller and I asked him about physics, and then gave us an answer. He gave us a very good suggestion. Teller and I then jointly wrote a paper, which was never published but which was the basis of lots of work by lots of aerodynamicists. It was about the lack of equilibrium which can exist behind shock waves. Von Kármán said, "This is what we need to know. We know the aerodynamics, but you know the physics of how molecules behave at high temperatures. So make us a theory." And so we did.

Well, he was full of jokes as you probably know. He had left Germany before the Nazis came to power. And after the Nazis came to power, Goering was very much interested in developing the air force in Germany. So Goering said, "I decide who is a Jew. Von Kármán is not a Jew. So I invite him to come back, and we'll give him all the laboratories and all the money that he wants." So von Kármán got a letter from Goering, personally, to this effect. And then, as he told us, "I wired him my profile." Now, to understand this, you must know that one of the important things about airplanes is the profile of the wing, which determines whether the wing will have enough lift. And his profile, of course, showed very clearly that he was Jewish. [Laughter] So that was one of the stories.

Fig. 1. Edward Teller, Enrico Fermi, and Theodore von Kármán, Hollywood, 1937. Caltech Archives.

Another was later on—I think it was after the war. Some company wanted him as a consultant, and he was already a consultant to lots of companies. He said that he didn't have any time to devote to that company. So he asked the person who invited him, "Do you want my name or my work?" And the person was somewhat embarrassed and said, "Well, Professor von Kármán, really we want your name." "Oh," he said, "that's all right; that I can give you. But my work I cannot." [Laughter]

GOODSTEIN: How was it that the paper you and Teller wrote jointly was not published in 1940?

BETHE: At the time, it was sort of secret. It did not have a secret stamp, because neither Teller nor I were citizens, so we didn't have access to any secret documents. But it was kept behind closed doors. It was published by the Aberdeen Proving Grounds in Maryland. They made copies available to people who might be interested and sufficiently reliable. But then, after the war, when there was no objection to publishing it, neither Teller nor I had time, and neither of us was sufficiently interested to write it up. It is available to the public. It was published by an outfit in Michigan which publishes otherwise unavailable documents. But it never went into any journal, which is a little bit of a pity, because it was a good paper.

GOODSTEIN: How long did you spend here, then?

BETHE: Here at Caltech?

GOODSTEIN: In 1940.

BETHE: Two days. And we mainly came to see von Kármán.

GOODSTEIN: In that two days, he suggested the topic?

BETHE: Yes.

GOODSTEIN: And did you do the work in the two days?

BETHE: No, no. Then we went home. We were at different places. He [Teller] was in Washington and I was in Ithaca. We discussed it for a few more days, because we were on a longish drive across the country. On the drive we discussed what we would do. Then he did some work and I did some work, and we put it together into a paper. But I'm sure we spent at least a month on it, each of us.

GOODSTEIN: I think I had asked you why it was that you didn't give your nuclear physics lectures here, when you went on that speaking tour in the late thirties.

BETHE: I didn't really go on a formal speaking tour. Are you referring to the tour sponsored by Sigma Xi in 1942?

GOODSTEIN: I learned about it by reading Jeremy Bernstein's profile of you in *The New Yorker*. And I think what Bernstein says is that you went around giving talks on nuclear physics and then wrote up the series of famous articles.

BETHE: Ah, yes. That was in 1935 and '36. But that was not a tour. I was just invited by a few places—like the University of Chicago, I don't remember if it was MIT or Harvard, and Columbia, and Purdue University. They just invited me, personally. And I never got west of Chicago on that occasion.

GOODSTEIN: Did you have much information about the nuclear physics program here at Caltech in the thirties? Did you know of it?

BETHE: I certainly knew about the work by [Charles C.] Lauritsen and [William] Fowler and their students. It was one of the important programs in experimental nuclear physics. There were a few others. There was important work done down in Washington, D.C., at the Department of Terrestrial Magnetism, and some at Wisconsin. Well, of course, the most important work was done at Berkeley. And then we did some at Cornell; we had the smallest cyclotron operating. But I read the papers, and I read them very carefully. They did very good work here at that time—I think especially about capture reactions in which protons are captured by nuclei with the emission of gamma rays. I had met Lauritsen; I don't think I had met Fowler. I believe I met Fowler for the first time here in 1940.

GOODSTEIN: When did you come back again to Caltech?

BETHE: Let me see. I was in Berkeley in '42, but did not go down to Caltech at that time. Probably I didn't come back until about 1950. Still, in 1940, I remember a lovely half day with Willy Fowler and his bride-to-be. They were not quite married yet. We spent half a day together in the garden of her uncle, which was a lovely time. The reason why I'm hesitant in placing this in time is that if it had been in '40, then the Tellers would have been present, and they were not. It was just before the Fowlers got married, and *Who's Who* says this was in 1940.

GOODSTEIN: Did you ever hear any talk about the fact that Caltech had never opened its doors to any of the physics émigrés?

BETHE: No.

GOODSTEIN: That's something I've just uncovered. In fact, Emilio Segrè spoke to me about it. He felt that this did not reflect well on Millikan. So I have thought about this a bit, and I was wondering if it was ever noticed?

BETHE: I never noticed it. And one of the reasons is that there were rather few of us to start with in the thirties. And so there were only relatively few places which did open their doors. I could almost count them on the fingers of my hand. So it didn't strike me as peculiar that there were no refugee physicists here at Caltech. There were none at many other places, too.

GOODSTEIN: And you also say that in the beginning there were very few of you.

BETHE: Yes.

GOODSTEIN: You see, when you look in von Kármán's correspondence files, you discover hundreds of letters from people who had to leave Germany and Austria, and other parts of Central Europe, asking him for help. It's always the same reply—"There is nothing here." Now, it's true, it's not only in physics; it's also in applied mathematics and engineering.

BETHE: Yes, well, this would be mathematics and engineering. And I don't really have any information about that.

GOODSTEIN: So it may not be a significant fact?

BETHE: Yes.

GOODSTEIN: That's one of the things I'm trying to clarify.

BETHE: Yes, it would be good to find out. Carl Anderson is still alive, isn't he?

GOODSTEIN: Yes.

BETHE: He might know.

GOODSTEIN: Yes he might, if Millikan talked to him about it at all.

BETHE: Yes, that is, of course, the question.

GOODSTEIN: One could put a less charitable interpretation on the coolness that you felt when you did meet Millikan. It might be a reflection of his antipathy to wars—

BETHE: I don't think so. No, I think it was much more on my side than on his side. I thought he was very pompous. And everything that I had seen or heard about him, including some of his scientific papers, was very pompous. And I didn't feel attracted to that.

GOODSTEIN: No, it's true. I take it, then, that Sommerfeld never spoke to you about his visit to Caltech.

BETHE: He never did. Well, I think he mentioned that it had been a very good place and so on. He was a good friend of Epstein, with whom he had talked quite a lot. But Sommerfeld didn't have many personal conversations with me. It wasn't likely that he would tell me. Do you believe that Sommerfeld and Millikan disliked each other?

GOODSTEIN: No.

BETHE: I wouldn't think so.

GOODSTEIN: No. They were from the same generation and the same kind of upbringing and the same kind of physics. They would like each other.

BETHE: I would have imagined so. In fact, they were born the same year.

GOODSTEIN: 1868?

BETHE: In ’68.

GOODSTEIN: Yes, they were well suited to each other, temperamentally.

BETHE: Yes—Sommerfeld also was a little bit stiff personally.

GOODSTEIN: I think for the same reason, it’s not likely I could find out the answer to my question from talking to Carl Anderson—because I suspect that Millikan did not take Anderson into his confidence that much.

BETHE: Yes, that’s quite likely.

GOODSTEIN: You see, when Segrè talked to me about this, it was with a particular person in mind. Bruno Rossi had to leave Italy, and it would have been a good choice on Caltech’s part to invite Rossi here, because Rossi did cosmic rays. And that’s how this came up.

BETHE: On the other hand, Rossi was perhaps the first person who proved Millikan wrong. So he may not have been against all foreigners.

GOODSTEIN: But merely against one person in particular.

BETHE: Right. And Rossi was quite remarkable. I think he was in his twenties when he did the coincidence experiments and also discovered the east-west effect, which showed that cosmic rays were due to positively charged particles. And I don’t think Millikan would forgive him that.

GOODSTEIN: Did you attend that meeting in 1931, the nuclear physics meeting in Rome?

BETHE: No, I did not. At that time, I was not yet interested in nuclear physics. It was witchcraft at the time.

GOODSTEIN: To you.

BETHE: To me. It became a science in ’32 with [James Chadwick’s] discovery of the neutron. And then I became very interested.

GOODSTEIN: So it was the experimental discovery.

BETHE: It was the experimental discovery. Then its theoretical explanation was by [Werner] Heisenberg, who wrote three very beautiful papers about the constitution of the nucleus in terms of neutrons and protons. Also the Russian physicist [D.] Ivanienko was involved in this. But in ’31, nuclear physics was witchcraft.

GOODSTEIN: ’31. Does that coincide with your first visit to Rome?

BETHE: Yes.

GOODSTEIN: Were they talking about going into nuclear physics at that time?

BETHE: Only during my second visit, which was in ’32.

GOODSTEIN: That was after Chadwick’s discovery?

BETHE: Before Chadwick’s discovery. But at that time, there were the very peculiar experiments by [Frédéric] Joliot and [Irène] Curie, who had found some radiation with mysterious properties and couldn’t understand what it was. And on that basis, [Enrico] Fermi decided that he wanted to go into nuclear physics. I think then Edoardo Amaldi and a couple of others went with Fermi.

GOODSTEIN: [Franco] Rasetti told me that when they discussed it, he and Fermi were ready to switch, but that Segrè was willing to stay in spectroscopy.

BETHE: Is that so?

GOODSTEIN: That's what he told me. So this must coincide with your second visit in '32.

BETHE: This was the second visit, yes.

GOODSTEIN: That was after Rasetti came back from Otto Hahn and Lise Meitner's laboratory in Germany?

BETHE: Yes, that is correct.

GOODSTEIN: Do you remember much about your visit to Rome in '31?

BETHE: Yes, a fair amount.

GOODSTEIN: It's interesting that they were not talking about switching to nuclear physics in your first visit; they were still doing spectroscopy, and atomic physics.

BETHE: Yes, yes. That is certainly my recollection.

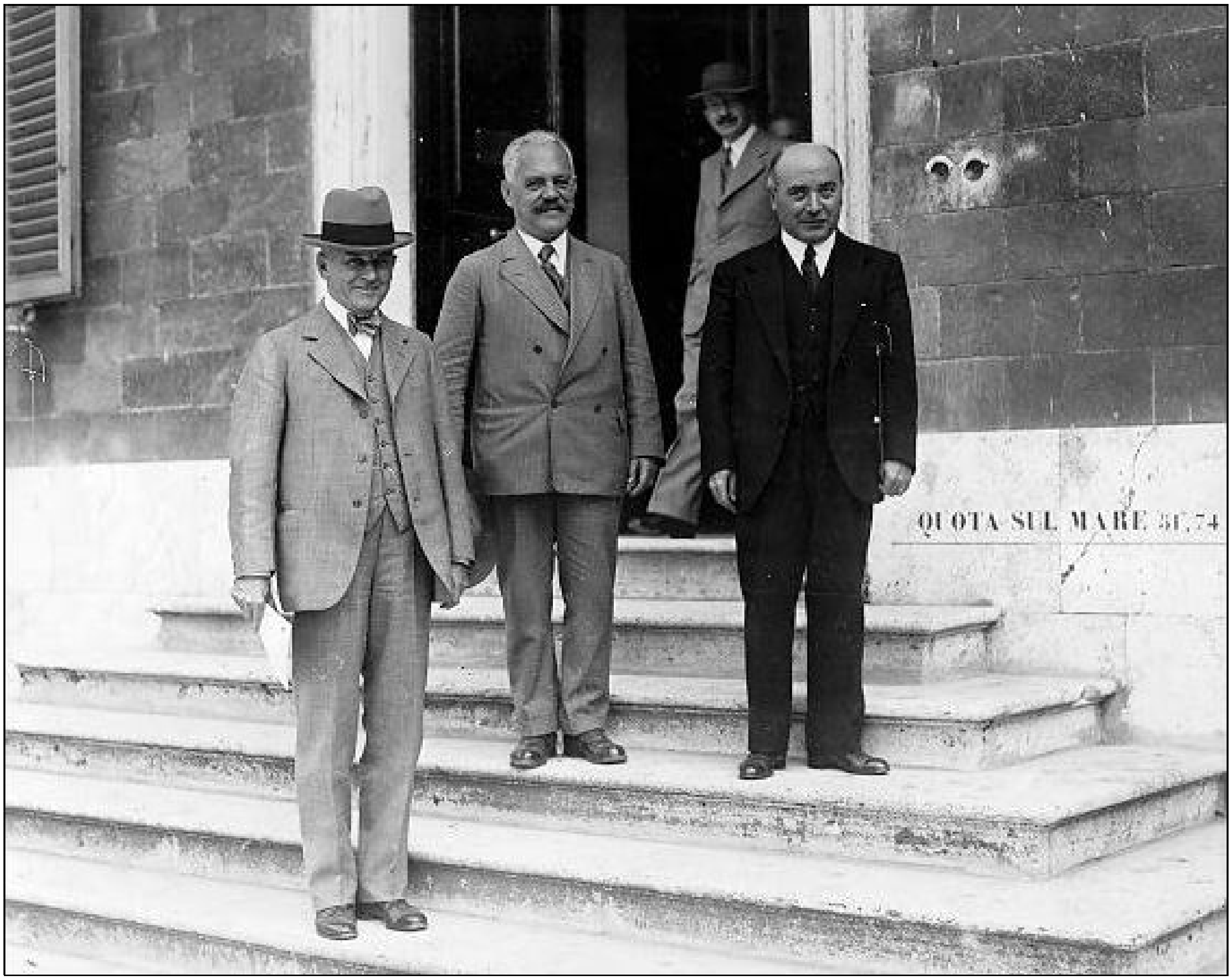

Fig. 2. Robert A. Millikan, Arnold Sommerfeld, and Orso Mario Corbino, Rome, October, 1931. Photo by FotVelo. Caltech Archives.

GOODSTEIN: That's what Rasetti said, too. The only reason I press you is that there is an article by a historian of physics that says otherwise. I have two loves in life at the moment: One is the history of Caltech, the other is Italian physics in the twentieth century. So I've begun to ask the people when did they actually make the switch.

BETHE: Well, in '31 Fermi was interested in quantum electrodynamics. And of that time, there is his article in *Reviews of Modern Physics,* which for the first time made quantum electrodynamics understandable. It took away all the terrible complications that Heisenberg and [Wolfgang] Pauli had put into the subject. And he did it by the simple means of making a Fourier analysis.

GOODSTEIN: Had this article come out before you went to Rome?

BETHE: No, I don't think so. I can't swear to that, but I don't think so. But Fermi was very much interested in this subject. I was interested in the stopping power of matter for charged particles. There had just come out an article by [Christian] Møller, which gave an approximation to quantum electrodynamics for weak interactions. So I was doing the stopping power for relativistic particles. And Fermi got interested in that, and we wrote together an article about the relation of the various formulations of quantum electrodynamics—Møller's and [Gregory] Breit's and the standard thing of Heisenberg, Pauli, and Fermi himself. So this was one interest. The other interest was atomic physics. Lots of atoms were being calculated, and I was very proud when he told me that a certain calculation of mine could be included in *The Treasury of Wave Functions*.

GOODSTEIN: How long did you spend in Rome on that first visit?

BETHE: It was February through June; five months.

GOODSTEIN: Did you live in Rome itself?

BETHE: Yes. I wouldn't have even considered anything else. In fact, I lived rather close to the institute [Physics Institute of the University of Rome].

GOODSTEIN: Did you speak Italian?

BETHE: Enough to order a meal, but no more.

GOODSTEIN: Did you speak to Fermi, then, in German?

BETHE: Yes.

GOODSTEIN: And the others as well?

BETHE: The others as well. And they all spoke German very well. Except Mrs. Fermi. When I was invited to the Fermi's home, we spoke English. She spoke English but not German.

GOODSTEIN: Do you remember having any political discussions with Fermi's group?

BETHE: Not with any of the people I'm most interested in nowadays. I had a political discussion with one person, but that was before I came to Italy. There was a young physiologist who visited my father, who was a physiologist. This man was strongly anti-Fascist. So I had a discussion with him. Then on my second visit, simultaneously there was [George] Placzek. He, of course, was no more Italian than I was, but he spoke Italian. We had a lot of political discussions. But I don't believe I had any with the Italians—partly thinking that it would be embarrassing to them.

GOODSTEIN: So you talked about physics and other things.

BETHE: Yes. There was enough about physics. I went with Segrè and Rasetti on, I believe, two excursions, which was very nice.

GOODSTEIN: Mountain climbing?

BETHE: No. We went into the countryside, looking for old Etruscan bridges and things like that. They were both very well educated.

GOODSTEIN: Did you feel the presence of Fascism on that first visit?

BETHE: You couldn't help it; it was everywhere. The police were everywhere. It was very visible. I was very much aware of it. I was very much aware that I should be cautious in whatever I said. And the police…

GOODSTEIN: Were they at the institute?

BETHE: Only on certain occasions. The institute was located right next to the Ministry of the Interior, and they used the same driveway. When there was a meeting at the ministry, none of us could get to the institute. There were two policemen standing at the entrance to that driveway. You know the story about Fermi and these two policemen?

GOODSTEIN: Yes. [Laughter]

BETHE: He was always dressed quite disreputably. And so it was quite believable that he would be the chauffeur to His Excellency.

GOODSTEIN: Did you hear him tell the story yourself?

BETHE: Yes.

GOODSTEIN: And that's how he gained admission?

BETHE: That's right. And he emphasized, "I never told a lie. I *am* the chauffeur, to His Excellency Fermi. And His Excellency would be very unhappy if he couldn't go through."

GOODSTEIN: So it was nothing but the truth.

BETHE: Nothing but the truth. [Laughter]

GOODSTEIN: I take it he enjoyed telling this story.

BETHE: He very much did, yes.

GOODSTEIN: Did he tell any other stories along those lines?

BETHE: No, certainly not while he was in Italy. This was sufficiently harmless to tell. But I didn't hear many stories from him even afterwards.

GOODSTEIN: Rasetti says he was a very discreet man.

BETHE: Very discreet, extremely so.

GOODSTEIN: Did you ever have an opportunity to meet [Orso Mario] Corbino when you were there?

BETHE: I met him, but just to shake his hand. He was the director, after all. And we exchanged some polite words, but for not more than ten minutes. Now, from Segrè's book it is clear that Corbino was an extremely farsighted person, and that I really should have tried to have more contact with him. But I didn't try. There were the other young people with whom I had common interests in physics and lots to talk about. So I didn't feel any need.

GOODSTEIN: You said you felt the presence of fascism. Besides the police, were there other things that stand out in your mind? For example, were you in the habit of reading the newspaper there?

BETHE: Yes, I was. And it certainly was evident from that. I wasn't as sensitive probably at that time as I was two years later. But it was clearly evident from that. I was trying to remember whether there were lots of military parades. And there were a few, but they were not so conspicuous.

GOODSTEIN: Was there any change between the two visits?

BETHE: No.

GOODSTEIN: When you said you were much more sensitive, you meant after the rise of…

BETHE: The Nazis, yes.

GOODSTEIN: When you were in Italy, did it seem to you that that could happen to Germany?

BETHE: Certainly. Yes, even the first time, and very much the second time, because it was very much on Placzek's mind, who was politically much more aware than I was.

GOODSTEIN: So Placzek and you talked about it.

BETHE: Yes, almost constantly, because we had almost all our meals together in restaurants. We saw an awful lot of each other and talked very much about the impending… [tape ends]

Begin Tape 1, Side 2

BETHE: In 1930 was the first election in which the Nazis became a very strong party in the German Reichstag. They won almost twenty percent of the vote, so it was clear that they were very much in ascent. And then in '31 and '32, there were terrible economic times in Germany, and probably also other European countries. But the German governments, Brüning's in particular, seemed to be quite helpless.

GOODSTEIN: So your going to Italy was like seeing the handwriting on the wall.

BETHE: Yes. Now, fascism was not anti-Semitic at that time.

GOODSTEIN: You never picked that up.

BETHE: No.

GOODSTEIN: The racial laws in Italy came into effect in 1938.

BETHE: Yes. And by that time, of course, the Nazis had very much the ascendancy over Mussolini. I don't know when [Giurio] Racah left Italy.

GOODSTEIN: I'm sure it's before the outbreak of the war. Segrè leaves in '38, too.

BETHE: Yes.

GOODSTEIN: And Fermi leaves because of his wife.

BETHE: Yes.

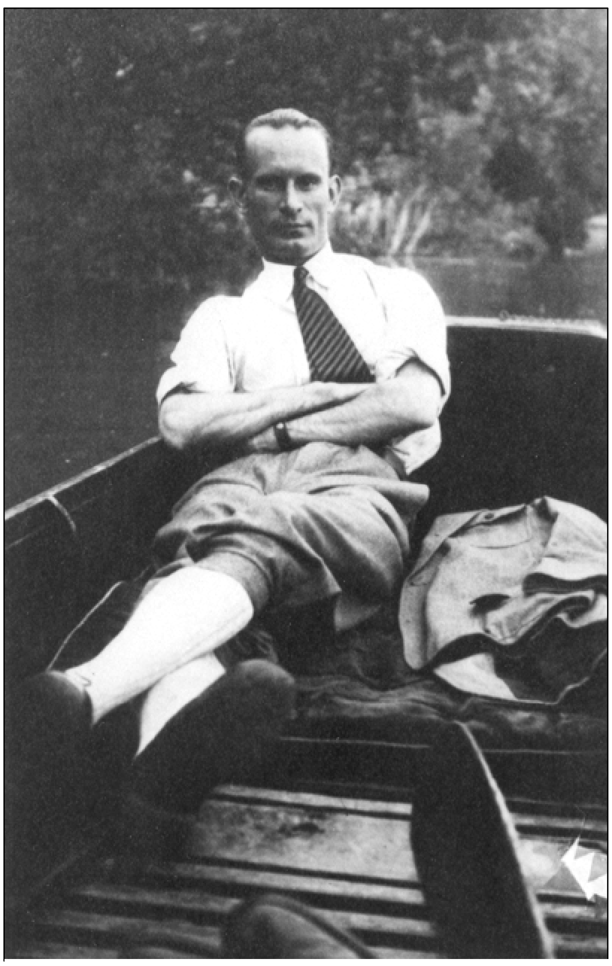

Fig. 3. Franco Rasetti, Cambridge, England, 1932. Caltech Archives.

GOODSTEIN: All those who had to leave left in 1938 and 1939.

BETHE: Yes. And Franco Rasetti, who did not need to leave.

GOODSTEIN: No. He's not Jewish. He left because he was disgusted. No, in fact Rasetti says that he was only mildly anti-fascist in the beginning—in the beginning meaning 1922—because it seemed to him, and to others, that the communist strikes were destroying the country, and that maybe a benign dictatorship would help.

BETHE: Yes. But then it didn't remain so benign.

GOODSTEIN: No. Back to 1924, it was quite obvious that it was not benign. Did you ever encounter [Ettore] Majorana?

BETHE: I encountered him; and again, it was almost the same as with Corbino. Well, I guess we talked a little longer, about a half an hour or so. But Majorana at that time did not speak either German or English. But Segrè was present as an interpreter. Segrè very much was Majorana's connection to the world, as far as I could make out.

GOODSTEIN: Not Rasetti, but Segrè?

BETHE: Segrè. I don't know about the relation between Majorana and Rasetti.

GOODSTEIN: And this is before Majorana went to study in Germany. I think he went to Leipzig.

BETHE: Yes, that's correct. That was, I think, in '33 probably.

GOODSTEIN: I think that's correct. Well, he's a very strange person.

BETHE: A *very* strange person. I hope you heard enough about him from these two people—Rasetti and Segrè.

GOODSTEIN: I have never talked to Segrè about him. I did talk to Rasetti.

BETHE: You should talk to Segrè. He was a tremendous admirer of Majorana. And he claimed that, fundamentally, Majorana was an even better theoretical physicist than Fermi.

GOODSTEIN: I think he says that in his diary. What do you think of that?

BETHE: Well, Majorana was a very different theoretical physicist. He may have been more fundamental. He discovered the Majorana forces, which, of course, are very much used in nuclear physics even today. I think today not many people know that they are Majorana forces, but they are.

GOODSTEIN: What name would they go by?

BETHE: None.

GOODSTEIN: Just forces?

BETHE: Yes, forces of a certain type. This was very important. And in this respect, he corrected Heisenberg. Heisenberg had made a mistake defining the forces between neutrons and protons. And Majorana discovered the wave equation like the Dirac equation for the neutrino. But Fermi did many fundamental things as well. There was, after all, the theory of beta decay and the Fermi statistics. I would say, even considering the short life of Majorana, Fermi was the greater physicist. But Majorana was very good, there's no question.

GOODSTEIN: You know he vanished, and that adds to the mystery.

BETHE: Yes. He vanished and a large number of papers have been written about that.

GOODSTEIN: That's right, which fuels the mystery, that he may be alive.

BETHE: Oh, I doubt that. Well, he wanted to go into a monastery, so maybe he did, and is just no longer known.

GOODSTEIN: When you were in Rome, did you ever hear any discussion about the loyalty oath that the university professors had to take?

BETHE: No.

GOODSTEIN: It was significant in 1931, because there had always been a loyalty oath that people who worked for the state took and it was just loyalty to the state. The first Fascist loyalty oath did not change and so there was no problem with anyone signing it. But in 1931, the loyalty oath was changed to read loyalty to the state and to the Fascist regime. And so for ten or eleven people, that was too much. So they resigned.

BETHE: I see. Very interesting.

GOODSTEIN: And I was simply wondering if it ever was discussed?

BETHE: That I would remember, if it had been. No, it never was.

GOODSTEIN: There was one mathematician who did not sign—his name was Vito Volterra.

BETHE: Oh, yes.

GOODSTEIN: He left his position in ’31. Did you ever meet any of the prominent mathematicians when you were there?

BETHE: No.

GOODSTEIN: The two circles never intersected.

BETHE: No.

GOODSTEIN: Some of them were interested in physics; not all. Volterra was not particularly interested in physics. But [Tullio] Levi-Cività was.

BETHE: Yes, I know. But I never met him

GOODSTEIN: And another one was Guido Castelnuovo. Well, there wasn't a main campus, like we have here. So it's hard for people to meet from other fields.

BETHE: Yes, that's very true. The physics department was completely separate from everything else.

GOODSTEIN: In the German universities you were at, was there more of the concept of a campus as we know it in this country?

BETHE: Considerably more, yes. And of course, Rome now has one also. Yes, in Munich, there is very much a campus. I guess some departments were separate. Chemistry and medicine were outside the campus. But physics—both theoretical and experimental—was inside the campus and most of the other fields as well. It was even more concentrated than a campus. There was a huge building which housed the university. And right next to it there was a smaller building—the physics department.

GOODSTEIN: So you were very much in close connection with each other.

BETHE: Yes.

GOODSTEIN: If I may jump up to the present. How did you feel when both of your colleagues—Robert Bacher and Richard Feynman—left Cornell to go to Caltech?

BETHE: Well, those were very different occasions. Bob Bacher went first to the Atomic Energy Commission. And since he was the director of the nuclear laboratory, we had to replace him. We had no idea how long he would be away. So we got Bob Wilson. Once Bob Bacher resigned from the AEC, there was no suitable place for him at Cornell. He could have come back as a professor, and everybody would have been very happy. But we didn't want to displace Wilson from the directorship of the lab.

GOODSTEIN: So, in a sense, Bob had burned his bridges.

BETHE: That's right, yes. So we were not surprised in any way that it was not attractive to him to return just as a professor.

Feynman was a terrible loss—and a loss that we did not expect. After all, he had done his most fundamental work at Cornell. And we thought that he was quite happy. He said he had too much work with students. But then that was his own fault, in the sense that he was willing to accept far too many PhD students. He didn't need to. There were other professors. So we certainly lost one of the great theoretical physicists in the country, and our department never was the same after that.

GOODSTEIN: Did you make a concerted effort to try and keep him?

BETHE: Yes, but it was hopeless from the beginning, because he just wanted to leave. Money couldn't induce him to stay. "Well," we said, "you don't need to direct any theses that you don't want to, and you don't need to participate in the work of the department." But none of this was attractive to him.

GOODSTEIN: He wanted to go west.

BETHE: He wanted to go west.

GOODSTEIN: Well, he certainly put Caltech on the map after World War II.

BETHE: In theoretical physics, yes.

GOODSTEIN: In theoretical physics. And, if you look back now, I think you could argue that he embodies Caltech in the way that Millikan embodied Caltech in the earlier generation.

BETHE: Isn't that a little extreme?

GOODSTEIN: Possibly. Because there are very few people who were known outside of the physics community—and certainly Feynman's name was not a household word. Millikan had much more of a public persona.

BETHE: And Feynman has always rejected that—the public.

GOODSTEIN: Yes, he has. He certainly goes out of his way to avoid it. And I think Millikan liked it.

BETHE: Yes. Feynman has had no connection to national laboratories or to industry or administration.

GOODSTEIN: You're right. I would have to refine it and say that within the scope of people who know Caltech, they often identify Caltech with Feynman today, regardless of their field—even if they're not in physics.

BETHE: Very good.

GOODSTEIN: I think that's part of what came west with him.

BETHE: Yes. That is really marvelous. Well, he is so stimulating and so much of a person, even if he doesn't want to be.

GOODSTEIN: That was very much true at Cornell, too?

BETHE: Yes.

GOODSTEIN: The fundamental work that he did there—did you know then that it was going to win him the Nobel Prize?

BETHE: I didn't know. But it seemed likely.

GOODSTEIN: When he was at Cornell after the war, when you were there, did you often see him?

BETHE: Oh, sure. That was the point of it, after all. Not often enough, but I often saw him. We worked on somewhat the same subjects—quantum electrodynamics. I did it in a very pedestrian way. I made one contribution explaining the Lamb shift—at least its numerically major part, but theoretically a minor part. I knew perfectly well that Feynman was just miles ahead of me. But I loved to talk to him, and I often talked to him. He explained his theory to me before he explained it in public. One time I even had to hold his hand—figuratively. There were annual meetings—first the Shelter Island conference, then the Pocono conference. At the Pocono conference [March 1948], both Feynman and [Julian] Schwinger presented their versions of quantum electrodynamics. Everybody was tremendously impressed by Schwinger—who, indeed, had done beautiful work. It was connected with the old way of looking at it, so that people could understand it easily. And Feynman had a completely new way of looking at things—which I knew but most of the other people found strange. And especially Niels Bohr, who, after all, was the leader of us all. Niels Bohr couldn't understand it, wouldn't believe it, gave some very sharp arguments against it, and treated Feynman rather badly. And Feynman, of course, was very much disappointed because he had what he considered a beautiful theory. And here was the greatest of all quantum physicists, who wouldn't believe him. So when he came home, I had to console him. I was at the meeting; I heard the presentations, as well as Bohr's reaction. Unfortunately, Feynman likes to present his work—or did, at that time, like to present it—as paradoxically as possible. And this was just impossible for somebody like Bohr to understand.

Fig. 4. Hans Bethe and Richard Feynman at Los Alamos gathering in April, 1983. Caltech Archives.

GOODSTEIN: If Dick had presented it in a more straightforward manner, might Bohr have been more receptive?

BETHE: Yes, I think so.

GOODSTEIN: Did you say this to Feynman afterwards?

BETHE: Yes.

GOODSTEIN: What was his reaction?

BETHE: That he would try in the future to do so.

GOODSTEIN: And did he?

BETHE: I think he did. And very quickly his methods caught on. Today, nobody ever goes back to Schwinger's methods of introducing electric quantum field theory. But they always use Feynman's way.

GOODSTEIN: How long would you say did the changeover take?

BETHE: Two years. So it was not very long.

GOODSTEIN: No, but it must have seemed long then.

BETHE: It did to him.

GOODSTEIN: That means the papers were published, and it took a while for people to read them and understand.

BETHE: And to work with them and to find out that this was really a much easier way to do things.

GOODSTEIN: Can you pinpoint one physicist in particular who might have been very important in getting other physicists to use Feynman's approach?

BETHE: No, everybody did it after a while. I don't think it was anybody in particular. Everybody who worked in the field found out that this was just a much better way to do it.

GOODSTEIN: So Feynman left Cornell just about the time that—

BETHE: It had been accepted.

GOODSTEIN: So this was in the time when he was resting. And that's when he left.

BETHE: Yes.

GOODSTEIN: But, of course, that was due to Bacher, who made it his number one goal to build up the physics department here.

BETHE: That's right. And he did.

GOODSTEIN: I don't know if he concentrated only on theoreticians.

BETHE: Not at all, no. He either retained or got a lot of experimental physicists, who turned out very, very well.

GOODSTEIN: Were you at all surprised that he could do this?

BETHE: No, I thought he was really a superb administrator. He had shown that at Los Alamos. He had probably the most difficult division to run in the whole laboratory.

GOODSTEIN: His division was actually assembling the bomb?

BETHE: Well, all the divisions were somehow concerned with that. But his division had to do physical measurements by which you could decide whether the assembly was satisfactory. You know that it is done by implosion—that is, by setting off an explosive. This will only work if the implosion is completely uniform—that is, you have to have a perfect sphere on the inside of the explosives. And so his division had to invent methods to ascertain that this was so. There were many very good experimentalists in Bacher's division at Los Alamos. But some of them were rather difficult to deal with. Somehow Bob dealt with them.

One of the things that Bob told me once about his operations here was the quickness of decision. Somebody asked him, "How long does it take from the time the physics department decides to get a new faculty member to the time when you make the offer?" "Oh," he said, "anything between half an hour and twenty-four hours, depending on whether the president is here or not."

GOODSTEIN: That is no longer true.

BETHE: Bob is very deliberate. But once he has reached a decision, he sets out to do it right away.

GOODSTEIN: I think it's a very good story he told you. And it helps to explain how he built up as good a physics department [as he did]. [Laughter] Actually, there were other departments that were rebuilt after the war, and he played a role in that, too. The decision had to be ultimately his, and you see his handiwork all over the campus.

BETHE: Yes.

GOODSTEIN: Have I left anything out? I don't know if you had anything to say about [Fritz] Zwicky or Tolman?

BETHE: Not much.

GOODSTEIN: It occurred to me that you might have seen Tolman at Los Alamos.

BETHE: I saw him here, and I saw him at Los Alamos. Not very much. The person who came more often to Los Alamos was [James B.] Conant. But I knew Tolman, and he always seemed to me like a very kind uncle. He always wanted the right thing, but perhaps was not as energetic as some of the other people who supervised the project, such as Conant.

Zwicky I met a couple of times on visits here. He was quite a character. He really did superb work on the supernova observations. He was not the only one. Walter Baade was involved even more. But Zwicky predicted, very early on, many things that we learned twenty years later—theoretical matters, too. He said, "Well, the remnant of a supernova will be a neutron star." Nobody else would have dared to say that at the time.

GOODSTEIN: That was in the thirties?

BETHE: Yes. There was a theory which was published by Oppenheimer and [G.M.] Volkoff on the structure of neutron stars ["On Massive Neutron Cores," *Phys. Rev.* 55, Feb. 15, 1939]. But nobody but Zwicky would believe it. And he said, "Well, that's what it will be. And it will have a radius of ten kilometers." Which is right.

GOODSTEIN: Did you believe it at the time?

BETHE: I thought it was pure fantasy. How could such a thing be? So where supernovas came really into my knowledge was only with the work of Hoyle, Fowler, and the Burbidges, when they said that this is the way all the elements are formed. But Zwicky had predicted it all before.

GOODSTEIN: From first principles?

BETHE: More or less, yes. At the same time, he was so extremely Swiss. Swiss never lose their national characteristics, which other people do. But Zwicky was as Swiss as one could be. Not only was he proud of Switzerland but of his own canton, which is one of the less known cantons. He would go back there and vote from time to time.

GOODSTEIN: He did not become an American citizen?

BETHE: That I don't know. You can be both a Swiss and an American. And you cannot lose your Swiss citizenship, no matter what you do.

GOODSTEIN: He sent his papers back to his Swiss canton.

BETHE: He did? Well, that's very much his style.

GOODSTEIN: Was he an easy man to get along with?

BETHE: I didn't have occasion to observe, because I only saw him twice at lunch, and maybe for a half an hour or an hour in an office. I wouldn't know.

GOODSTEIN: Did you ever run across H. P. Robertson?

BETHE: Yes. Him I knew a little better. And he was a nice person. I liked him. We had very little in common scientifically, but I liked to talk to him, and he seemed a very friendly person to deal with.

The people I knew best were the Lauritsens, because we had very much in common scientifically. And also, Charlie Lauritsen was very much interested in political matters, trying to keep our Defense Department from swallowing the whole country, and at the same time he contributed very much to defense. So we had quite a number of conversations outside of physics, many of them in Oppenheimer's house. He and Oppie and [I. I.] Rabi were very good friends. And we discussed the deplorable state of the world many times. It has become far more deplorable since.

GOODSTEIN: At that time, the deplorable state would be the early fifties?

BETHE: Yes.

GOODSTEIN: It's become much more so? Do those days seem much more sane compared to times today?

BETHE: Yes, in some ways. Well, there was one difference which was more insane at that time, and that was [Senator Joseph R.] McCarthy. Fortunately, nowadays we don't have a McCarthy.

But Charlie Lauritsen was a delightful person.

Fig. 5. Charles C. Lauritsen, ca. 1933. Caltech Archives.

GOODSTEIN: I didn't know that he had a great interest in political matters.

BETHE: Yes, very much so. He always was so extremely sane. I think he had quite an influence in some Washington circles. They consulted him. I spent several evenings at his house. It was wonderful how relaxed a person he was, with all his interest in politics and all the physics he was organizing. He was one of the most—"relaxed" is not quite the right word—he was one of the most integrated personalities I have known.

GOODSTEIN: When he was at home, he was at home.

BETHE: That's right.

GOODSTEIN: He didn't bring his work home, so to speak.

BETHE: Not as far as I know. It was just so easy to get along with him.

GOODSTEIN: Do you think it was his Danish upbringing?

BETHE: Yes, that had a lot to do with it.

GOODSTEIN: Did you see this in Niels Bohr, too?

BETHE: Yes. But Niels Bohr was not as easygoing as Lauritsen was. He was always searching for something new.

GOODSTEIN: Lauritsen wasn't?

BETHE: Not as far as I know. He was very much interested in his physics, but it was rather straightforward—what experiments to do next and what would be interesting. Bohr was always searching for some new ideas in physics. And here is one advantage of an experimenter over a theorist. But there are also experimenters who are always searching—probably not as much as Bohr.

GOODSTEIN: How would you characterize yourself?

BETHE: I'm not searching that much. In fact, I have never tried to go into the really deep fundamentals of physics, into the philosophical part of physics. I'm much more interested in phenomena that you can observe. Now, I am searching for solutions to some equations also, but that's not the kind of thing that Bohr was doing.

GOODSTEIN: Would you say that Feynman searches for things the way Bohr searched?

BETHE: Yes. But Feynman is a much more exuberant person and much more of an extrovert, really. Life is just happiness for him, even if he's had all these medical troubles. He just is a fundamentally happy person.

GOODSTEIN: Well, he's a bit irrepressible.

BETHE: Totally. [Laughter] And Bohr was not a fundamentally happy person—though he was not an unhappy person. But coming back to the Lauritsens: Tom was quite different, and I think much less relaxed and self-contained than Charles. But he was a very good friend and a very kind person, with whom I loved to spend time. Also a very good physicist. It's just very sad that he died so young.

GOODSTEIN: You don't think he had any conflict doing physics, when his father had done physics before him? And then coming into the same group?

BETHE: That may have been a conflict, I don't know. It didn't show in anything he ever said to me.

GOODSTEIN: Did his father very much want him to follow him into physics?

BETHE: Yes, I'm sure of that. But that doesn't always work. I would very much have liked my son to follow me, and he didn't want any of that.

GOODSTEIN: He just didn't do any science, period.

BETHE: Well, he was quite interested in it; he would have been good at it. But he's a banker, and he does that very well. And so that's fine.

GOODSTEIN: Yes. I see in my own children how easy it is to want them to be like you.

BETHE: For my son this was a terrible conflict. And in his student days he was quite confused. I think he had somehow to resolve that conflict, and he had to find one subject in which he was obviously much better than I. And that was playing bridge.

GOODSTEIN: And you think that helped matters, actually?

BETHE: That helped matters enormously. It gave him confidence.

GOODSTEIN: And then he became a banker.

BETHE: Then he became a banker, yes.

GOODSTEIN: Well, the Lauritsens left behind a very strong legacy, which I guess Charlie Barnes and Willy Fowler continued.

BETHE: Yes, very much so.

HANS A. BETHE
Session 2
January 28, 1993

Begin Tape 2, Side 1

BETHE: Now, I told you I would tell you about Bob [Bacher] at Los Alamos. He was the most important person next to Oppenheimer.

GOODSTEIN: Why do you say that?

BETHE: Because it's true. He was the leader of the experimental division that made the experiments to prove the implosion—that is, to prove that implosion would work—and that was really the fundamental problem at Los Alamos. [George] Kistiakowsky did the explosives work, as you know, and my division did some calculations. But the real proof of the pudding was in the so-called G division, which Bob Bacher was the leader of. Without the implosion, the plutonium [bomb] would not have worked. So that's the main reason why I say he was the most important person.

Secondly, he, I think, had the full confidence of Oppenheimer. And remarkably the full confidence of [General Leslie R.] Groves. And that came about partly because Bob had the answers—[inaudible word]—which were better than Groves's himself. There was one important crisis—namely, when [Emilio] Segrè discovered that plutonium has spontaneous fission and that meant that we could not use the gun method of assembly. Also it meant that the plutonium had to be produced in such a way that it would not be exposed to neutrons any more than absolutely necessary. So Oppenheimer and Bacher decided that the Chicago project [Metallurgical Laboratory, University of Chicago] had to know this. And so Oppie told that to Groves and after a while Groves agreed: yes, the Chicago project should know it. But the only person who would go to Chicago would be Bacher—we had implicit confidence in him. And indeed he also talked to Groves and persuaded Groves that not only Fermi and [Arthur H.] Compton should know this but all the senior people in the laboratory. So that's just one incident.

Later, when this division got going, Bacher held it together, and there were many famous experimental physicists in the division, each of whom was a prima donna—including [Luis] Alvarez and [Edwin] McMillan and [Norman Ramsey?] and a few other people. And they were willing to work with Bob, but probably not with anybody else. In particular, I don't think Alvarez would have been willing to work under McMillan or vice versa.

The thing I observed most closely was that if Oppenheimer had a problem, he would discuss it with Bob.

GOODSTEIN: By "problem," do you mean technical, scientific problems or problems of personnel also?

BETHE: Technical problems, mostly, but also policy problems.

GOODSTEIN: Is Bacher a calm person? I've never seen Bob get really excited.

BETHE: Right.

GOODSTEIN: Was that always characteristic of him?

BETHE: Yes, yes. It was, and that was very important with respect to Oppie, who easily got excited. He didn't show it, but he had to restrain himself—very much. And Bob had very close relations with me and with Robert Wilson, the leader of the nuclear physics division. That was very important. I guess none of us had very close relations with Captain [William S.] Parsons, who directed the ordnance division. But it certainly was essential to have a close relation between these three.

GOODSTEIN: And Bacher was the link among all these divisions?

BETHE: No, I think Bacher and Wilson and I.

GOODSTEIN: The three of you.

BETHE: The three.

GOODSTEIN: There's a story that I've heard that Bacher actually put the [plutonium] bomb together, he assembled it. Is that true?

BETHE: That is true. There's a story that it wouldn't fit right and that it was difficult to do it, and [people say] that he kept his cool when the bomb was hot. I mean the plutonium was hot, because it's fairly radioactive. And the two halves wouldn't fit well. And then he invented a way to do it better, for the Nagasaki bomb.

GOODSTEIN: Why him? Why was he the person chosen to put the bomb together?

BETHE: Because he was the leader of G division.

GOODSTEIN: So it was obvious that he would be the one to do it?

BETHE: It was reasonable, let's say.

GOODSTEIN: Were other people in the room watching him assemble the bomb?

BETHE: I believe lots of people watched him. I wasn't there. Lots of experimental physicists were there, but I wasn't there until the [Trinity] test itself, and then I stayed for the evaluation.

GOODSTEIN: You and Bob were there from the beginning of the Los Alamos project?

BETHE: He came a little bit later. Oh yes, he was also involved in another aspect: Oppenheimer had underestimated the number of scientists who would be required—I think his original estimate was fifty and then he revised it upward gradually—and there was a meeting at the beginning of the project to which Bob came. Another person there was I. I. Rabi. Oppie had left the lab to some extent unorganized—in particular, the theoretical division, which didn't exist.

So Rabi and Bacher went to Oppie and said, "You have to organize the lab into divisions." And that was very essential. In fact, it was organized very much after the pattern of the Radiation Lab at MIT—into divisions, which were big divisions, and each division into groups.

GOODSTEIN: What was the number of scientists connected with the project from the original fifty that Oppenheimer had thought—

BETHE: At the end, there were about five thousand people working, of whom maybe at least a thousand were actually scientists.

GOODSTEIN: A much larger number than Oppenheimer had anticipated.

BETHE: Right. Lots of technicians, in addition, which he had not provided for, and then two secretaries.

GOODSTEIN: You didn't tell me how you first came to know Bob Bacher. How did you first come to know him?

BETHE: Well, at Cornell. I came to Cornell in February 1935. And the department was engaged in building up nuclear physics. We had a very good experimental nuclear physicist, Stanley Livingston, who built a cyclotron for a minimum of money—I think not much over a thousand dollars and months of his time. And it was worth far more than a thousand dollars.

GOODSTEIN: I'm sure.

BETHE: And Livingston was extremely good in making that machine go, but not as good in actually doing experiments and thinking of the proper things to do. So the department chairman thought we should have another experimental physicist who was more of the thinking kind and less of the building kind. And so we thought for some time and he consulted me a lot. And we finally decided on Bob Bacher, who was at Columbia. An offer was made to him, and he came in the fall of 1935.

GOODSTEIN: Shortly after you came.

BETHE: Just one semester after. So we had a lot of conversations right away, and we got very close to each other at that time. And when the war came—in fact a little before that—Bob was asked to go to the Radiation Lab at MIT.

GOODSTEIN: I was wondering about that.

BETHE: That was, I think, Christmas 1940. And I stayed at Cornell. And then in the summer of 1942, I was asked by Oppenheimer to join the—well, I had joined the Radiation Lab, and I was asked to join the uranium effort, and the idea was conceived of the Los Alamos Laboratory. I was asked to go there, and Oppie asked me who would be a good leader for the experimental physics. And so I suggested Bob. And so Bob came to Los Alamos.

Now, I came to Los Alamos at the very beginning; Bob came a few days later for a big meeting we had at the beginning in which we developed programs and we made that suggestion of divisions. And then he was persuaded to come as the leader of the experimental physics division. At that time, that included nuclear physics, and Wilson was serving under him as group leader. But it took Bob, I think, two or three months to straighten out things at the Radiation Lab, so he actually came in July instead of April. This was July 1943. And then the following July we had this problem about the plutonium spontaneous fission. And then we had a fission of the experimental division into Bacher's and Wilson's.

GOODSTEIN: Now this is not about Bacher per se, but I have read, in some accounts of the Los Alamos days, that people there used to refer to the [plutonium] bomb as the Christy bomb.

BETHE: Only at the very end. That bomb was referred to as the gadget.

GOODSTEIN: OK.

BETHE: And so Bacher's division was called the G division, for "gadget."

GOODSTEIN: Oh, that's what the "G" stood for.

BETHE: We had thought, to begin with, that we would have a shell of material and implode it with high explosives, but then the implosion didn't seem very symmetrical, whatever—there were all sorts of irregularities, and so we were not sure whether it would work. And then I think it was only around New Year's of 1945 that [Robert] Christy suggested that we just take a solid sphere of plutonium. Implosion will compress it, and thereby make it supercritical. And that will surely work, because it's symmetrical to start with. That was then the Christy gadget. And only after the Japanese were bombed did we go back to the idea of a hollow sphere.

GOODSTEIN: After the bomb was dropped?

BETHE: Yes. And subsequent tests were made with a hollow sphere, plus [words inaudible].

GOODSTEIN: And the results of those tests were also successful?

BETHE: And the tests were successful.

GOODSTEIN: But for the bomb that was dropped on Nagasaki—it was actually the Christy idea.

BETHE: Right.

GOODSTEIN: My professor of elementary physics at the University of Washington was Seth Neddermeyer.

BETHE: Indeed!

GOODSTEIN: Yes. He was not a great teacher.

BETHE: I can believe that.

GOODSTEIN: But I have heard—much later; I didn't even know this when I took physics from him—that he was responsible in part for the idea of implosion.

BETHE: Chiefly responsible. It was his idea. And I know that in later years he regretted this very much. He thought he should not have made the atomic bomb go. Without the idea, it would not have worked.

GOODSTEIN: Is that true?

BETHE: That's true. Uranium would have worked, but not plutonium. Another point—he, Neddermeyer, had this very important idea, but then his experiments of assembling material with implosion didn't work. And we got the most horrible jets and irregularities you could ever have. And that was the case until a Britisher by the name of [James L.] Tuck came to us and told us how to make it a smooth shock wave, with so-called [explosive] lenses. And then Kistiakowsky came and took charge of the explosives division and Bacher took charge of the observation division.

GOODSTEIN: And so you got Neddermeyer's idea to work.

BETHE: Right.

GOODSTEIN: He did not, at the time, regret the idea—only in hindsight?

BETHE: Right.

GOODSTEIN: Well, a collaborator of mine went to interview him, and Neddermeyer was very disturbed about what he had done during the war.

BETHE: Right.

GOODSTEIN: Very disturbed about it. I took the physics course from him in 1960. The students didn't do so well in the class. And it bothered him a lot that we hadn't understood him. It bothered *me* a lot. [Laughter]

BETHE: Indeed.

GOODSTEIN: He used to scratch his head a lot. He could not; he just couldn't get the ideas out.

BETHE: Right.

GOODSTEIN: Was he that way in Los Alamos, too?

BETHE: Right. He was.

GOODSTEIN: Sweet man.

BETHE: Yes.

GOODSTEIN: I wanted to ask you, what was Bob Bacher like as a teacher at Cornell when he first came there?

BETHE: He was supposed to be very good. I didn't go to his classes, but I think he was very good. He taught partly graduate courses and partly elementary physics to liberal arts students.

GOODSTEIN: The kind of course I took from Professor Neddermeyer.

BETHE: Right, right. That's what he taught, and I'm sure you would have understood Bacher much better.

GOODSTEIN: Right. Now, your collaboration with Bob—the articles that were published in the *Reviews of Modern Physics*—

BETHE: Right.

GOODSTEIN: That collaboration began at Cornell?

BETHE: It was begun and ended. We wrote that article in 1936—well, even in 1935. It was published in 1936. When he came, I found out that he knew a lot about nuclear spin and magnetic moments and how to measure them spectroscopically, and much of his experimental work, at first, was spectroscopy and some of it was nuclear.

GOODSTEIN: There are some people who are naturally gifted in the laboratory.

BETHE: Right.

GOODSTEIN: Is Bob one of those people?

BETHE: I think he was somewhat gifted. I don't think he was as gifted as, let's say, Livingston or Alvarez.

GOODSTEIN: But, of course, what you told me about Livingston is that he lacked something on the idea side.

BETHE: Right, right.

GOODSTEIN: And Bob never lacked for ideas.

BETHE: He never lacked. And he had a complete appreciation of theory. So we had a very good time at Cornell—as also after the war.

Fig. 6. Robert Bacher and Hans Bethe, ca 1983. Caltech Archives.

GOODSTEIN: He went back to Cornell after the war.

BETHE: He went back to Cornell. And, in fact, it was due to him that Cornell got a synchrotron.

GOODSTEIN: Ah, OK. After the war you got bigger.

BETHE: Yes.

GOODSTEIN: And that was due to him.

BETHE: Right. But then he very soon disappeared. He was first asked to help Mr. [Bernard] Baruch, who was preparing to present the [American plan for the control of nuclear energy] to the United Nations. And so Bob went to New York, four days every week, I think, and worked at Cornell the other four days. [Laughter] I deliberately said it that way. [Laughter] And then while that was going on, very soon the Atomic Energy Act [1946] was produced, and he was asked to be the scientific member of the [Atomic Energy] Commission.

GOODSTEIN: These were tasks that some people might not have enjoyed—essentially going into policy work, where there's a lot more politics.

BETHE: Right.

GOODSTEIN: Did Bob enjoy this?

BETHE: He enjoyed it. He enjoyed it, but he didn't want to do it forever. He did, however, do it from January 1947 to mid-1949. At one time—and we talked about that just last Sunday when I visited him—there was a big test of nuclear work at Eniwetok, and he was asked to be the representative of the commission. And he really was the supreme authority at that test. He won't ever say that, usually. There was the four-star general who was the test director, but none of us would do anything without Bob's permission. And after the first one or two tests, Bob had to decide which of the many devices should be tested next. So that was quite an experience.

GOODSTEIN: Right.

BETHE: But then in mid 1949, maybe even early 1949, he turned in his resignation to President Truman.

GOODSTEIN: He'd had enough.

BETHE: Yes, he had had enough. So he didn't get involved in the terrible discussions about whether or not to develop a hydrogen bomb.

GOODSTEIN: He really was outside at that time.

BETHE: He was totally outside of that. He was here by then.

GOODSTEIN: That's it. When he left the AEC, he came to Caltech.

BETHE: Yes. He had an offer from here. I don't know who—what people, maybe Tolman.

GOODSTEIN: That could be.

BETHE: That's very likely. They knew each other well during the war, and even before the war. And in the meantime, Wilson had been appointed director of the lab [Laboratory of Nuclear Studies] at Cornell, and Bacher didn't want to go back, either to work under Wilson or to displace Wilson.

GOODSTEIN: So he really cut his ties with Cornell, as it were.

BETHE: Right. We couldn't stay without a director.

GOODSTEIN: I understand that. What you're saying is that life goes on, even if someone's doing important work someplace else.

BETHE: Right. He was still on leave from Cornell, but he then didn't want to continue.

GOODSTEIN: Did he have a lot of graduate students when he was at Cornell?

BETHE: Three or four.

GOODSTEIN: Was that about the right—

BETHE: That's about the right number. And one of them, [Boyce D.] McDaniel, became the director of the lab after Wilson left. Another, [Marshall G.] Holloway, was very important in Los Alamos and developed the H-bomb. And then went into industry. And the third, [Charles P.] Baker, was a very bright young man and after Los Alamos went to Brookhaven, and I think died just recently. There must have been one or two more, but I have lost track of them.

GOODSTEIN: You're allowed. So for Bacher do you think that the Los Alamos experience was central?

BETHE: Yes. I forgot one more, Willy Higginbotham.

GOODSTEIN: What about him?

BETHE: Higginbotham was a wizard with electronics and never took a doctor's degree, but at Los Alamos he was in charge of electronics [words inaudible], and after the war he became very interested in arms control and civilian control of the atomic bomb. So he went to Washington and was the representative of the Federation of American Scientists. And then went to Brookhaven again, to do electronics.

GOODSTEIN: And how did he interact with Bacher?

BETHE: I don't believe he was Bacher's PhD student, but they were fairly close.

GOODSTEIN: OK, but he was with Bacher at Cornell?

BETHE: Yes.

GOODSTEIN: What would you say was the central point in your life? For Bacher, it was Los Alamos. What would you say about yourself?

BETHE: There were so many essential points in my life. First of all, coming to Cornell, where I got much more into contact with other physicists, including experimental physicists. I did pretty well before—otherwise I wouldn't have come to Cornell. But I really expanded greatly at that point. So first, that; second, Los Alamos—the discovery of the thermonuclear cycle in stars, and then my work at Cornell after the war.

GOODSTEIN: Right. There are a number of high points.

BETHE: Yes.