

INTERVIEW WITH

Hans A. Bethe

on

November 3, 1977

for

Dwight D. Eisenhower Library

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This interview is being conducted with Professor Hans Bethe at the Massachusetts Institute of Technology, Cambridge, Mass., on Nov. 3rd, 1977. The interviewer is Dr. Thomas Soapes. Present for the interview are Professor Bethe and Dr. Soapes.

DR. SOAPES: You came to this country in 1935 I believe.

PROF. BETHE: That's correct.



DR. SOAPES: And I believe you were involved in the work at Los Alamos during the war.

PROF. BETHE: That's correct.

DR. SOAPES: What was the nature of your work there?

PROF. BETHE: Well, I was the leader of the theoretical physics division. I had about thirty physicists, that is academic theoretical physicists under me and, at the maximum, about sixty technical personnel helping me.

DR. SOAPES: Did you have much contact during this period with Edward Teller?

PROF. BETHE: Yes, I had quite a bit of contact. We didn't agree very well. I thought the important thing was to get the job done and to get weapons designed and calculate their performance. Dr. Teller, as far as I could make out, was mainly interested in finding physics problems which were connected and interesting but not necessarily vital for the solution of the main problem. And in par-

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ticular he was interested in the hydrogen bomb at the time, which I believed was quite premature. He did, however, make some useful contributions, some of them are mentioned in the biography which has been written about him, and I mention two in particular. One was that, in a qualitative way, he discussed the implosion with Dr. John Von Neumann, and decided that there would be a substantial increase in density of the material, the uranium, which would decrease the critical mass and would therefore lead to an increased yield of the assembled weapons. The other contribution was with some other people at Los Alamos on estimating an equation of state of compressed uranium, or plutonium for that matter. Purely theoretically this estimate was made at very high density, using the Thomas-Fermi statistical method of treating the behavior of electrons in such a configuration. And he then gave some ways of estimating the effect of temperature on this. This was very useful for constructing the equation of state, which of course we needed to have in order to calculate the effects of implosion. So both of these contributions were connected with the implosion. He didn't want to have anything to do with the more quantitative calculations of the implosion.



SOAPES: Did you have continuing contact with the federal government and official research activities after Los Alamos until you came into the Eisenhower administration?

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BETHE: Not very close contact. I continued to consult for Los Alamos. In fact I've been at Los Alamos at least once a year every year. I was on at least one committee in Washington, directly for the government, that was a committee of the armed forces, committee on atomic energy, which was a joint committee of Army, Navy, and the Air Force. I was connected with that maybe for a year or two before I joined the Science Advisory Committee. I did not join a great number of committees or summer studies as many of my colleagues did, but I did not.

SOAPES: How did your appointment to the President's Science Advisory Committee come about?

BETHE: Well, at that time it was still just called the Science Advisory Committee, and was part of the Office of Defense Mobilization. So it was not directly responsible to the President. It was always a very good committee--had such people on it as [Lee] Dubridge, and [Isador] Rabi, and [Edwin H. ] Land, and [James R.] Killian, and several people of that caliber. Several of them knew me quite well, especially I would say Rabi and Dubridge knew me quite well and knew that I might contribute something and that I had some knowledge about nuclear weapons and a little bit about radar, so that they thought that maybe I could be helpful. So they asked me to join in, I believe



it was the fall of 1956. And, knowing that it was a very useful and interesting committee, I accepted right away.

SOAPES: Just before you came to PSAC, the Technological Capabilities Panel had issued its report. I think that one came out in 1955.

BETHE: That's right.



SOAPES: And one of the big concerns of that report, as I read it, was their concern over the vulnerability of the United States to surprise attack. Was this an ongoing concern in the administration?

BEHTE: As far as I know, this is so, but I was not really as much involved in it. I knew about many of the studies; I knew about this study. I didn't read this report, but I knew this was the conclusion of this report, and it seemed very persuasive, that indeed they were right.

SOAPES: As you look back on the various debates that took place within the advisory committee, were there important conflicting views over technological priorities?

BEHTE: Not very much. As I remember it there was really a great deal of consensus within the committee. Surprisingly much.

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SOAPES: Could the committee then be criticized as being not sufficiently catholic in its outlook?



BETHE: I think it could be. On the other hand, it got a lot of things done this way. And as far as I know the committee at that time did not try to bring in people of widely different opinion. In addition to that, there were relatively few priorities. I don't know--there may have been only one or two in the early times. The committee members were mainly selected, as far as I remember, on their previous association with one or the other of the wartime projects, and for this reason they were perhaps very homogeneous. Of course we came from, originally, from very different backgrounds. I should mention one correction to what I said. I believe in '58 probably one of the people joining the committee was Dr. Herbert York of Livermore Laboratory--I think he was then still director of the Livermore Laboratory--who brought in a very different attitude. He was very close to Teller, and I remember quite a number of controversies between him and other members of the committee. In particular I remember that Rabi was quite annoyed by York at several times as being too much of a "hawk".

SOAPES: Didn't York, though, begin to change his views?

BETHE: He began to change his views when he left Livermore. And I think the main change probably took place while he was the director of Defense Research and Engineering, which was several years later. Several years? Maybe only one year later. And then he changed his views--so now he is quite a "dove". In fact, I believe more of a "dove" than I. I don't think I have changed.

SOAPES: But you think he came out from under the influence of Teller and was exposed to other viewpoints.

BETHE: That's right. Yes.



SOAPES: One of the startling events was the Sputnik in '57. Was this a great surprise to the scientific community?

BETHE: It was to me. I believed, and I think most of the committee probably believed, that the Russians were somewhat behind us. We had very little to go by. We really knew very little of what went on in the Soviet Union. There had been some international meetings --I think there had been a meeting with the Russians on space flight --and the Russians, as usual, were very reticent. And when the Americans presented their plans for satellites the Russians said, "Um-huh." [Laughter] So nothing could be concluded from this. So

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I believe that most people were very much surprised. In retrospect, I don't think we should have been that surprised, because I believe we had observed-- but I am not sure about that; other people know that much better--I believe we had observed some flights of something approaching an ICBM. We knew the Russians had medium range ballistic missiles which we didn't have. And so we really ought not to have been surprised, not so much so. They had done much more missiles than we had for obvious reasons. It was a much clearer need for them or much clearer way to go for them, because for them it made a lot of sense to have a missile which could cover a thousand miles or two thousand miles into Europe. For us this didn't make any sense at all. Unless we could hop the ocean, the missile was useless, and this by itself made a much greater motivation for them than us. Furthermore, of course, we had a very good set of long-range planes, which they did not have. So we didn't need it. So it was, I think, perfectly obvious that given the same effort in strategic weapons the Russians would go for the missile and we would go for the planes.



SOAPES: Did the Science Advisory Committee then have to consider international political considerations as well as technological

considerations in its reactions?

BETHE: I don't think we considered strictly political matters, but Sputnik was a really dramatic effect on the committee. In fact, this was because of President Eisenhower. It was a dramatic effect on him. He remembered that he had a Science Advisory Committee. He called us in for a session almost immediately, and we had one day to talk among ourselves, what we wanted to tell him, and then the next day we had an hour with the President. And this was a very memorable occasion, one of the most memorable in my life. We had a lot of complaints before this, and these complaints mainly concerned inefficiencies in the Defense Department and a near absence of relation between the Defense Department and the scientific community. It was true that some individual scientist consulted for some parts of the Defense Department, but their advice was often ignored. And there was nobody in the Defense Department who would bring the most modern technological possibilities to the Defense Department. So this was the area in which we concentrated. Our spokesman was Rabi, and he had much closer contact with Eisenhower than anybody except Killian and George B. Kistiakowsky. He had a lot of interviews with Eisenhower before, because when Eisenhower was President of Columbia,



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and they liked each other from that time. So Rabi was our spokesman and gave a list of maybe six points which we would like to change in the Defense Department and its relation to the scientific community. And within that hour Eisenhower approved all of them and gave orders to General Andrew Goodpaster to see to it that they were actually put into effect. And I think this was marvelous. That is, I think, here, Eisenhower showed that he had very quick understanding that he understood right away that this would be useful for the Defense Department. And then being, formerly, a general, he knew how to delegate this to somebody else. And indeed practically everything was done. Perhaps the most important for the immediate future was the establishment of the Advanced Research Project Administration, I think was the full name, ARPA. York was the first head of the administration, and ARPA has continued to play an important part in the Defense Department. Those possible future armaments which people could think about but which were very far removed from actual implementation were given to ARPA to investigate and our contracts to industry and universities to pursue these various lines of research and development. And this, for instance, was important for the antiballistic missile development, the radar part and also the missile part. It was important for advanced radar, then for



reentry problems of missiles. These are some of the particular phases that I came in contact with. But I am sure there are many more that I didn't know about. Then soon, I think within a year, it was followed by the director of Defense Research and Engineering, who has overall jurisdiction on all new developments, all new armaments developments, and can overrule the three arms of the Defense Department, he can overrule secretaries of the individual services. This was very important because it meant that there was much less duplication and it also meant, since the head of that organization was always a scientist or engineer, it meant that it was done by a real expert instead of by a general who might not really know anything about it.

SOAPES: That comment raises the question. In his memoirs Killian makes reference to the fact that PSAC had to spend a lot of time dealing with these interservice rivalries.



BETHE: It did indeed. And before that, this one committee that I was on which I mentioned, the Atomic Committee of the Armed Forces, we spent, I think, more than half our time on just that. Yes, this is absolutely correct. And these two organizations which I mentioned I think contributed to sorting out some of the rivalries within the Pentagon, not having them come up to the presidential level.

SOAPES: Was it your estimate that this competition among the services -- the Army had their rockets, the Navy had the Vanguard -- did this materially retard the United States progress in the field.

BETHE: Absolutely. It did indeed. It did very much.

SOAPES: One field that you worked in a great deal was disarmament. You headed the disarmament committee of PSAC in 1958.

BETHE: Yes. That is correct.



SOAPES: I think on that committee, Killian mentions you had representatives of DOD, CIA, AEC, Air Force, Los Alamos, Livermore-- was it tough to work--

BEHTE: State Department--

SOAPES: And State Department as well. Was it tough to have all of these different views on that committee?

BETHE: Yes. It was quite tough to do so. They were very often really opposites.

SOAPES: Opposite in what way?

BETHE: Well, the party line of the Defense Department was that any

test ban automatically would be against the interest of the United States, while the State Department representative very strongly had the opposite view. He said that it would really improve our negotiations with the Russians, and our standing with our allies very much if we did go into the test ban. The representative was much more strongly in favor of the test ban than his chief, Mr. [John Foster] Dulles.

SOAPES: Who was the State Department representative?



BETHE: We had two--Spires was one of them, and, Mc Farland, is that the right name? It may be Philip Farley, but I'm not --[Philip J. Farley, Special Assistant to the Secretary of State for Disarmament and Atomic Energy.]

SOAPES: We can check that out.

BETHE: We can check that. I'm pretty sure it was Philip and I think Farley, but I'm not sure. And I'm also not entirely sure whether he was in that interagency committee, or whether he came in later when the actual negotiations started, that is the expert's conference. He certainly supported the test ban very strongly, and was very unhappy when there were difficulties. But whether

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he was in the interagency committee I don't know. Now, as far as the laboratories were concerned, Livermore Laboratory, represented by Harold Brown, was strongly against the test ban, and the Los Alamos representative, at least one of them, Carson Mark, was for the test ban.



SOAPES: This is the Harold Brown who is now the Secretary of Defense.

BETHE: That's right, yes. Harold Brown technically brought in the major difficulties with him, with test detection and the underground possibility, and I think he represented his point of view very ably, but it was quite difficult to sort all this out. Well you read in Killian's book the three points we had to discuss. One was the problem of test detection, and what would it do to our weapons development, and they included Russian weapons here. I knew a bit about the Russian weapons development and perhaps I should have mentioned that as one of the activities that I had been engaged in. There is an agency of the Air Force which observes the foreign test, and in those days they collected the radioactive material in the air. And then in the early days, let's say 1948,1950,'51, until about the time we are talking about, '58, much of the evaluation was done by a committee which was known under my name, that is I was the chairman of that committee. This committee I think was

appointed jointly by the AEC and the Air Force to evaluate foreign tests. On this committee we had, again two representatives from Los Alamos and two from Livermore. From Los Alamos, that's the same Carson Mark who knew about the design of weapons. He was my successor as the leader of theoretical division. And then we had at least one other person from Los Alamos who was in radiochemistry, and similarly from Livermore. So we knew pretty well in those days what the Russians were up to. The Air Force could also determine by the strength of the acoustic signal, they could determine the size of the explosion, probable yield, and then we tried to figure out a possible design which might correspond to observed radioactivity and to the yield. So this was very useful for the purpose and so we knew fairly well where the Russians stood relative to us. We knew in fact that they were, in '58, appreciably behind us in development of thermonuclear weapons. And that is what we put in our report, we said---, and this I managed people to agree on, and that was quite an achievement. [Laughter] But there I had the Livermore representatives with me, and the AEC director--

[Interruption]

BETHE: This in fact was the same General Alfred D. Starbird who is still at it, the one who is still director of military application.



But, by and large, our committee could agree on that part. There were varying degrees of concern that it might trouble our laboratories. Los Alamos didn't seem very concerned; Livermore was somewhat concerned; and the division of Military Applications was terribly concerned, and the Defense Department even more. And then finally we had the detection problem, and I think on the detection problem we agreed pretty well what could and could not be observed.

SOAPES: Did you feel in 1958 that detection was sufficient, that in a treaty situation you could detect violations?

BETHE: Yes. We did. We felt and wrote down that test in the air could be detected and identified down to quite low yield--I don't remember what it was, maybe two kilotons. We were less certain about underground tests and there our limit was considerably higher. I don't know what we wrote, but later on in the expert's conference I think it was set at twenty kilotons. We felt doubtful about tests in space. Most of us felt that this was a crazy thing to do, but that one might test high in the atmosphere, and that high in the atmosphere we would presumably also be able to detect fairly well. And we wrote that all down quantitatively. And later on, the Conference of Experts in Geneva wrote a report which in most respects duplicated the one we had written. We didn't give them our report.



But somehow by agreement between the Russians and the American experts it came pretty much the same.

SOAPES: You said that the State Department representative in the committee was much more attuned to test bans than was Dulles. How can we account for this difference between the two?

BETHE: Well, everybody has his individual opinions. I think Dulles generally, later on, in the Committee of Principals, generally supported Killian or Kistiakowsky and voted in favor of the test ban. So he was in favor but he wasn't terribly hard in favor.

SOAPES: A couple of people in the State Department who were interviewed for the John Foster Dulles Oral History Project at Princeton, said that they felt that at the time of Sputnik, Dulles began to change his view, that he became more interested in disarmament and bans. Were you able to see any signs of shift?

BETHE: I didn't know anything about his attitude before. In fact the only time I ever saw him was when I reported to the Security Council on the result of our interagency committee. And at that time I had the impression that he was mildly in favor. There wasn't much discussion when I gave that report. Dulles was about the only



one who said anything, and this was mildly in favor. President Eisenhower, I believe, didn't say anything, but just thanked me at the end. There were questions, technical questions, quite a number of technical questions, but very little opinion expressed except this one. But obviously Spires and, even more, Farley later on, had personally formed the opinion that this would be a good thing. Farley was assigned to this particular job, and so it was his business to be in favor of it.

SOAPES: You said Eisenhower didn't have much reaction when you gave your report, but could you, from the years in which you were associated with the administration, detect Eisenhower's personal view toward test bans and disarmament?



BETHE: I think only through the medium of Killian or Kistiakowsky. They told us that Eisenhower was very interested and much in favor. They told us later--us meaning PSAC--in April they told us that Eisenhower felt that he had been put on the spot by the Russians. Khrushchev challenged the United States to stop all testing without any special safeguards at that time. So Eisenhower was, at that point, apparently very happy that the interagency committee had done its work and had come to that conclusion just about two weeks before

Khrushchev's note. And it isn't clear, well, it certainly was not connected but it was a fortunate accident that we had done our work. Now, his main, as I understood it, his main interest was that the Russians should not get away with a diplomatic victory. And so he was happy that he could then answer, "Let's have a conference," and discuss first the technical matters and then the diplomatic problems. But from Killian and from Kistiakowsky I had personally, the impression that Eisenhower would very much have liked to have more arms control, but he didn't want to do it without consent among all his advisers. I haven't read the two books, but I'm sure the two books talk a lot about the Committee of Principals?

SOAPES: Yes.



BETHE: And that Eisenhower would act only if there was a strong majority in the Committee of Principal in one direction or the other. He very much relied on staff work. If the staff couldn't agree, nothing would be done.

SOAPES: That's a very interesting point. One theme that one gets from reading Killian is that the panel favored the test ban in '58 because it would freeze in America nuclear superiority. From what historians have written up to now about Eisenhower on this subject, I can see that some would immediately interpret that as the ad-

ministration's objective in disarmament negotiations or a test ban negotiation, as being an attempt to secure American nuclear superiority. Is that a fair judgement in your view?

BETHE: I'm afraid I couldn't read Eisenhower's mind, but I would imagine that this would be strongly in his mind. It was in mine. And I think it was an argument which weighed in my mind favoring a test ban. I wrote about it in 1960 in the Atlantic Monthly. And I was firmly convinced of it and still am, that indeed it would have frozen in some nuclear superiority. It was also an argument which I used in our interagency negotiations, and I think it counted to some extent with some of the people on the committee.

SOAPES: This was an argument that was appealing to people like Harold Brown or to Department of Defense people.



BETHE: Yes. Well, General Herbert B. Loper of the Defense Department I think never believed it. But I think Starbird did, to a considerable extent, and that made a lot of difference. If he hadn't believed this I think he would have opposed the committee reports very violently. And as it was, he only tried to modify its statement somewhat.

SOAPES: Was this a realistic objective, to freeze in American nuclear superiority? Could we anticipate that the Russians would agree

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to a treaty that would accomplish that?



BETHE: I think we could learn from everything the Russians did in the later negotiations, and from the fact that the Russians offered a test ban to begin with, I think they would have agreed. The reason they didn't agree in the diplomatic negotiations was that we were so terribly fussy about control. If we had been a little more relaxed, and if we had said, "Okay, most of the underground tests we can see from the United States, at least the big ones we certainly can see," I think they would have agreed. But they objected to our wanting eighteen stations inside Russia with foreign operators to run the stations and then, in addition, a large number of on-sight inspections. This was an intrusion which went far beyond anything that they could tolerate. If it hadn't been for that, I think they would have agreed. And I have that opinion also from talking to the Russians during some of the negotiations. We didn't talk terribly much with them but occasionally we met socially-- they gave a cocktail party, we gave a cocktail party-- and they generally were very much in favor of an agreement. During the expert's conference in July and August of '58, in Geneva, they first spent two or three days with declamations in which they said, "Well, if all the nations of the world yearn for cessation of the tests, why don't we just sign

a treaty that we'll stop." We wouldn't buy that, and this I think rightly. But with a moderate amount of surveillance by the atmospheric detection and flight of planes and so on, for atmospheric tests, and by some seismic detection, they were all set to agree to that, and they were all eager to sign that. Now it is of course a matter of opinion whether they wanted to sign this in order to cheat. I don't believe that. But many people believed it. But they were much more eager to have a test cessation than we were, knowing, which they surely did, that we were in better shape than they were.

SOAPES: Why do you think they would be willing to allow American superiority to be frozen in?



BETHE: They probably said to themselves, "It doesn't matter." And in fact some degree of superiority really doesn't matter. What difference does it make if we can make a given megatonnage in five thousand pounds if they need ten thousand pounds, what difference does it make?

SOAPES: The leadership of PSAC changed from Killian to Kistiakowsky. Did that create a major change in PSAC, the way it worked, the results of this work?

BETHE: As far as I could tell,no.

SOAPES: You were a scientist, not a politician? The White House, the presidency is a political institution. How did scientists get along in that political atmosphere?

BETHE: I think for quite a while very well, under Eisenhower I think very well. It was to some extent political confidence--Eisenhower had confidence in Killian and later in Kistiakowsky. And I had the impression that he liked Kistiakowsky even better than he liked Killian. I'm not sure. You'll probably get other opinions about that. But he trusted these two people and he trusted PSAC generally; so we didn't really get involved in any political controversy, which might easily have happened. Kistiakowsky afterwards got very deeply involved of course in this Committee of Principals. And that was really a political battle of a very high degree, which I think he suffered from. He didn't like it at all. So if we got into political battle, I think we probably were not very well equipped to deal with this. But most of the time, it was just between the science adviser and the President, and as far as I could judge these relations were very cordial.

SOAPES: Of course at the other end of Pennsylvania Avenue, the



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Congress, Lyndon Johnson was the head of the Senate Space Committee, he's shooting for the presidency. Did you see any influence of his partisanship?

BETHE: Only in the space business. I wasn't involved in that, but PSAC had a space committee. In the beginning, this was I think less important than the military committee, but gradually it became more important. And in the space committee, the space committee made certain recommendations. In fact as far as I remember it wanted to emphasize unmanned flight and deemphasize manned flight. It also had a different opinion about the way to get to the moon once it was decided to go to the moon. That was of course all in Kennedy's administration and not in Eisenhower's. And there were a lot of conflicts between that committee and then Senator Johnson, and later Vice-President Johnson, and later President Johnson. And there the committee always lost, as you can imagine. I had very little to do with this. I had more to do with matters of science education. I was on the Military Strategic Committee of PSAC. This strategic military committee operated very happily and had good relations through the Pentagon DDRE [Director, Defense Research and Engineering.] Certainly as long as it was York and later Harold Brown. Still later when President Johnson came in, the whole of PSAC deteriorated



and there was not very much contact between the science adviser and President Johnson. And still later even less between the science adviser and President Nixon, until Nixon abolished the whole committee.

SOAPES: You mentioned that you worked in the Science Education Committee.

BETHE: Not terribly much.



SOAPES: I see. I was wondering what your evaluation was of the response the administration made in the late fifties, early sixties to this heavy emphasis on science education. Was it an appropriate response and was it effective?

BETHE: It was, I think, appropriate and for a few years it was effective. I know that very well because our children went to school just at that time. My wife paid a lot of attention to the Ithaca, New York schools. The Ithaca schools improved tremendously after Sputnik, which was just the time when our daughter entered the critical years, I think, eighth grade or so. And so she got a very good education which she never would have got without Sputnik. And I think this was quite happily supported by the President's statement on it and by the support from the federal government. Then,

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about six years later it all went to pieces again. And there's nothing of it left as far as I can tell. But at least for a few years it was very good.

SOAPES: We've gone on at some length and covered a number of important topics. I'm wondering if there's anything that we haven't touched on that you think is of great importance for historians to recall about the work that you were involved in.

BETHE: Well, I told you about the meeting with Eisenhower which was my main contact with the President. We then had a second meeting a year later or so which was not nearly so impressive. We had less to say, and he had less to answer. You know about the long, drawn-out negotiations to have a test ban and not to have a test ban. I was unhappy that Eisenhower didn't take a firmer stand, because apparently he was in favor of a test ban, but didn't want to take a strong stand unless his Committee of Principals were with him at least four to one. They never would have got the Defense Department to go along, but the rest might have. So, this made me and several others on the PSAC quite unhappy that he did not show more firmness. I think these are the main remarks that I can make.

SOAPES: Thank you very much for your time this afternoon.

