INTERVIEWEE: DONALD F. HORNIG

INTERVIEWER: David G. McComb

December 4, 1968

M: This is an interview with Dr. Donald F. Hornig, who is the Special Assistant to the President and the Director of the Office of Science and Technology. The interview is in his office in the Executive Office Building, Room 203, December 4, 1968, 9:45 a.m.

Well, to start with your background. Where were you born and in what year?

H: I was born in Milwaukee, Wisconsin, March, 1920.

M: Where did you get your education?

H: My schooling was initially in the Milwaukee Public Schools through junior high school and part way through high school. Then I received a scholarship to Milwaukee Country Day School, where I did my last two years of study. I subsequently received a national scholarship to Harvard and took my bachelor's in chemistry at Harvard.

M: This was in 1940?

H: This was in 1940, that's right. I was graduated from Country Day in '36. I received the usual advice to go elsewhere after my B.S. but Harvard gave me a better graduate scholarship than anyone else, and being more or less impecunious, and Harvard not being intellectually unrespectable, I decided to stay on there and took my Ph.D. degree--a rather peculiar degree because the war intervened. There had been an episode when I planned to drop out of graduate school and go to the UK. In fact, I came within a week of shipping off to help with some of the problems connected with the bombing of London, but as typically happens in a war, they got into a wrangle as to who paid the costs of my crossing the Atlantic. And backwards from what you would think, the U.S. government insisted on doing it, and the British government insisted on doing it. Since that couldn't be resolved, I didn't go. I then got involved in the question of very large bombs, so-called blockbusters, and submitted a somewhat peculiar Harvard thesis entitled--I've forgotten the exact title--but it was the effects of large bombs, and a considerable amount of work was done at the Aberdeen Proving Grounds.

M: Was this degree in chemistry?

H: Chemistry.

M: And you did receive a Ph.D.

H: I received my Ph.D. in 1943, about six months behind the schedule I had set for myself which was to finish in June of that year. It was on that basis that I got married in July of that year, but in fact I didn't get my degree until September. I had started out in infrared molecular spectroscopy before getting shunted off into war problems.

M: Well, you got your Ph.D. and it's still the middle of the war. So then what did you do?

H: Well, immediately after I finished my Ph.D. I went down to Woods Hole where we had set up a laboratory. All my biographies say it was the Woods Hole Oceanographic Institution, but in fact that was just the home for a small laboratory on one of the small islands, Nonamesset, for testing or studying explosives--the detonation of explosives in air. I worked there through a rather dismal winter (Woods Hole is not a good place in the winter, it's a gorgeous spot in the summer). In the spring I had to make the next real decision in my life on the basis of absolutely no evidence. It was a rather interesting affair because the director of the laboratory grabbed me by the arm one day and said, "Come up to the attic." And I went up there and he said, "How would you like to leave this job?" And I said, "What the matter? Have I done something wrong?" And he said, "No, you've been requested for another job." And I said, "Oh, that's interesting. Who requested me?"' He said, "Well, I can't tell you that." And I said, "What kind of a job is it?" And he said, "Well, I can't tell you anything about it. It's a very secret matter." I said, "Well, what part of the country is it in?" He said, "I can't tell you that." "Well, can you at least tell me north, east, south, west, what have you?" He said, "I can't tell you that either." And I said, "Is it connected in some way with what I am doing now?" And he said, "I can't tell you that." And after a little bit more of this sort of thing, I said, "Well, I don't know how I can say anything on this basis." And he said, "Well, I'll tell you what, Don, you take your time and think it over very, very carefully and let me know what you want to do tomorrow morning."

So I thought about it and said, "On the basis of this information, no, I'm not interested. Unless you tell me that the war requires that I do it, that it's a matter of sufficient urgency." And he said, "Well, I can't take that responsibility." Well, that is typical of the security in the war. Later in the day the lab intercom said very loudly that I had a call from Santa Fe, New Mexico from Kistiakowsky, and Kistiakowsky said, "What the hell is going on down there? Everyone's mad that you said no. You are the first person that has said no." So I explained to him that his call had helped me a great deal already. A little while later Conant called me from Washington and wanted to know what the devil was going on. Was I unpatriotic? "Remember, Hornig, Uncle Sam is pointing his finger at you." I never forgave him for that remark, but it's a point. So, anyway, we went out to Los Alamos in the spring of '44. When I got there I found that despite all the urgency, I didn't have much to do because my main job was to plan for testing the device. The only trouble was that the device had not only not been built yet, but there was no material to build it with. In fact, it was still quite a ways off.

M: So, now, were you assigned out there when the lab was just being set up?

H: I was the first of what might be called the second wave. I got the very last of the original good housing before they went into prefabs. The first people had gone out the year before. I was the first of the group that came when they really decided to go ahead full blast.

M: Was Oppenheimer in charge?

H: Oppenheimer was already in charge when I got there, yes.

M: Well, then, were you directly responsible to him or what was your position?

H: At this point, I was a twenty-three year old new Ph.D. My immediate superior was a fellow named Lou Fussel from M.I.T., and we were in a group headed by Ken Bainbridge from Harvard. He in turn reported to Kistiakowsky who reported to Oppenheimer. So I was well down the list at that stage.

M: And then you had to wait until the planning had gone along further?

H: No. The turn came because Oppenheimer ran this thing as a very live-wire intellectual organization. There was very tight security around it, but very little internal security. This was always a source of war between him and the military people since he felt, I think, that the best security was just making progress. And so we had periodic conferences about all the big problems. One of the big problems was how to initiate this thing, and at one of these meetings I made a suggestion as to how I thought it should be done. This ended up in Oppenheimer's assigning me the job of trying to work out this approach to it. So then I got into a very active period, both scientifically and practically. This is tied to the use of spark gaps as a means of switching this thing in a period with an accuracy of fractions of a microsecond to start it at various places. It was doubly exciting because the alternative way of doing it had been proposed by Alvarez who had just gotten the Nobel Prize. So there was an element of personal competition. This was the period of the development of the X-unit, the firing unit, for the first bomb.

M: Did you have anything to do with the principle of using implosion to--?

H: That had already been thought through by the time I came out.

M: So your concern was the actual detonator--

H: Well, in order to implode a sphere uniformly, you have to start the explosion around the sphere at a lot of places simultaneously. Simultaneously means really simultaneously because what is drawn in a picture as a sphere implosion is actually a lot of detonations starting at a lot of points on the sphere. If they aren't accurate, then it is not spherical. In the extreme one side might collapse before the other side did.

M: And there's the problem of the waves, too.

H: That's right. The waves are made spherical by explosive lenses, but each explosive lens, and there were thirty-two of them, had to be initiated with a high degree of simultaneity. The problem I worked on was to get all of the explosive lenses reliably started within a time interval of a ten-millionth of a second.

M: Which would seem to be a formidable job.

H: Yes. That was a problem, to switch on a large amount of electric current so that each one of the thirty-two points was switched on within a ten-millionth of a second from first to the last. Well, my approach to the X-unit was the one that was chosen, so then I became an electrical engineer. Having made it work, we had to then make a unit that could actually be used, that would stand being dropped, and what have you. This was touch-and-go right up until months before the actual test. I had my first contact then with American industry, with the Raytheon Company, which was supposed to manufacture this thing. There were a lot of technical details but anyway they essentially said it couldn't be made for production as we had made it in the shop. They wanted to start all over and re-engineer it to be sure it would be reliable. Time wouldn't allow that, so finally we asked them to just make Japanese copies, which is what they did. Then we came up to the test at Trinity. This is, incidentally, well recorded in Lansing Lamont's book, Day of Trinity. He did, I think, the most thorough research on that test.

M: Is his book reliable?

H: His book is quite reliable, yes. He interviewed everybody on the site and discovered, what is most interesting to me, that on something as vivid as the events of the test, that twenty years later good people remember differently such questions as who climbed up the tower after who? I think he's gotten this straightened out as well as anyone could have. I think his account is reliable.

M: You were up on that tower?

H. Oh, yes; well, Kistiakowsky called me the martyr of Trinity because in the two weeks we were down there everything went wrong; the firing unit was just barely ready on time and these spark gap switches were rather fragile things, so there were lots of troubles. They started shortly after we got down there. About a week before the firing, we set up for a mock firing, and I learned something I hadn't realized, which was when a great big cloud came over it would induce a very large extraneous voltage. Since we were about a mile from the place where it was to detonate--then if you get 25,000 volts difference between the grounds and you string a wire between the two points, well, interesting things happen. To make a long story short, what did in fact happen on the mock test is that in principle the set fired accidentally and the bomb went off when it wasn't supposed to. So I was put under a certain amount of cloud, let's say.

It had an amusing consequence. Afterwards, Kistiakowsky asked me why had I been willing to go ahead with the test when I was having so much trouble because of these electrical effects. I said, "Well, here's me, and there is Oppenheimer who says it is going to

go. What do I do?" And Kistiakowsky said, "Don, you should have said, 'Oppy, I will not go ahead with this test.'" I don't think even in retrospect I would have done it, not at that stage in my career.

The second episode came about two days before the test. In the meantime we had gotten a mock-up weapon on the tower and had a real spare X-unit up there; everybody was testing everything, firing up to get cameras on, just testing whatever they had. Sometime about seven o'clock the news came in that the firing unit had failed, wasn't working, and I wanted to go out to see what had happened. I suspected that it was just worn out. Obviously, since it was going to be used once, it was designed to last through maybe ten firings. But Oppenheimer just said, "No, no, don't get panicky, don't get panicky, don't go out there tonight while you are tired; we will just wait until tomorrow morning." That was at about seven o'clock. At about a quarter to eight in the evening, he called a big conference to decide what we would do in the event that various things proved to be wrong. So without anyone having gone out yet to find out what was the matter, we then conferred from about eight o'clock until two in the morning.

In the meantime, somewhere around midnight, we phoned up to Los Alamos and got all the spare parts for everything we could think of and got them on a rush truck to be sent down. They put together a van--it was a beautiful job--a van with benches, you know, and with a generator in it so that repairs could be made in the field and had this thing down to Trinity by about five in the morning. In the meantime, Kisty had arrived so they got the unit off the tower and back out to base camp. I must say I had the highest level group opening it up--ever. I wasn't allowed to touch it. But it turned out as I expected. It had fired some three hundred and fifty times before it had failed; one of the spark gaps had burned out. Nevertheless, this contributed some more tension, even though it was all right. The test itself has been so well documented that I won't go over it. I did have a fascinating role in it, though. It was not only my firing unit that was in the device, but I had the only manual control on the whole thing. I was the only human being that could stop it once the automatic sequence was set in motion. And this was a matter of considerable tension because it meant watching the control panel and keeping on reminding yourself that in the event the voltage needle came down, in the last second, your response time was a half-second, so that if you saw anything start to happen, it was up to you to keep the bomb from going off. You see, the problem in its going off if everything wasn't right wasn't that people would get hurt, everyone was in bombproofs, but we didn't have the material to make a duplicate at that time. There was one other weapon on its way out to the Pacific, but if that test had gone badly for any reason there just couldn't have been another for several months. There were no spares. Well, it was a very hectic night.

I think I was the last person down from the tower although there might be a little bit of argument about that. I won't go into any detail, but Oppenheimer had gotten worried about nine o'clock the night before about how easy the thing was to sabotage by anyone who really knew anything about it, and so I believe it was Kistiakowsky, Bainbridge and I who each took a turn sitting with it up on the tower. My turn came from around nine o'clock until midnight, in the midst of a violent thunder and lightning storm. You get

philosophical in those circumstances. You know, either you do get hit by lightning or you don't and either way you won't know what happens.

M: Did you do your repairs the next morning, then, the morning of the firing?

H: Oh, the repairs had been done the day before.

M: I see. So everything was set.

H: Everything was set for the firing. This was just a question of babysitting it and being sure that nobody came near it except the scientific people who had the right of access.

M: And you then descended the tower and went to the control room?

H: Then I descended the tower. No, I didn't go to the control room. It was pouring, and I went back to the canteen, had some coffee, and sat around waiting to see what was going to happen next because nobody knew whether we could fire. Trucks were sliding off into ditches everywhere; in an area that has about three inches of rain annually, it rained something like four inches that night. The whole desert was under water, and the roads which were bad under any circumstances, were muck. It was very, very depressing, and I just sat around. It must have been something like four-thirty when I finally went back to the forward bombproof where I had my equipment. The thing that amazed me is that although you might think this should be a moment of great tension, in fact everybody was exhausted. I had been up for seventy-two hours at that point, starting with the episode of the failure. I walked in and there was water on the floor, people just lying in the water sleeping, other people curled up on the benches between equipment sleeping, and just an air of complete exhaustion and almost depression. We had to fire before sunrise in order to get photographs; somewhere about an hour before, we were still trying to decide on it. Then a Latin American opera started coming on our communications channel.

But at any rate all these things finally resolved, the moment came, and it was detonated. In the last five minutes, of course, everyone got electrified. For one thing, we didn't know what the fallout situation was going to be. We had halftracks around to get out if there proved to be a problem. There didn't. I wasn't able to see the weapon at the moment of detonation because my equipment was set up just inside the door of the bombproof. The minute the firing needle dropped off and I knew it had detonated, I dashed out the door in time to see the fireball rising into the sky. And that was it.

M: Were you elated with the success of the test?

H: I wouldn't say I was elated. I was awestruck, just literally awestruck. This thing was more fantastic than anything I had ever imagined and under the circumstance was aesthetically beautiful. As this thing went up, there were big clouds of peach and green and deep violet gases all in a violently swirling motion. Then, as they cooled off there were sort of big dark spots of smoke, and then they would break open and there would be another burst of peach color or green color gas. I just was overwhelmed.

M: Did you get outside the door before the shockwave hit?

H: Oh, yes.

M: So you could feel that, and then you would hear the sound rolling in?

H: That's right. But the shock wave was about as I had expected. We knew what to expect, and so that we just waited for it and it came by, a big wind really. I think if someone had taken a picture of me, I probably would have just been popeyed and with my jaw hanging down.

M: Did you have to wear dark glasses or anything like that by the time you got out there?

H: I didn't. Anybody who watched the original flash would have been blinded without them. It would probably have been about plus three seconds when I began to see it, about three seconds after the actual detonation, and the biggest flash is over then.

M: Was this test necessary before the dropping on Hiroshima?

H: Yes, if you wanted to know whether it would work. I suppose the betting was against this working, and there was an elaborate betting pool. When it came to it, no one could see any reason why it wouldn't work, but in a sense, just because of the awesomeness of the thing, everyone's gut feeling was that something would have been forgotten and something would go wrong. Mine weren't the only troubles in the week coming up to the test. They were the ones closest to the question of whether it would work, but everyone had pretty much the feeling that something was going to go wrong.

M: Was the fear that the bomb might get out of hand pretty well laid to rest?

H: That was laid to rest. I don't know of anyone who thought so seriously.

M: You were pretty sure what would happen if it did fire?

H: Yes. If it worked as was expected, you knew what was going to happen. And all the things you could think of would make it work a little less well rather than of any of these thoughts of igniting the atmosphere or the earth or somehow turning out to be much bigger than had been anticipated.

M: Well, then after the successful test, what did you do?

H: After the successful test, Kistiakowsky and I got into a jeep about an hour later and drove up to Los Alamos. We were both so sleepy and tired that we drove off the road a half-dozen times. I quite literally don't remember anything about that trip except every now and again being afraid of falling out because the jeep didn't have a sidestrap.

M: Were you driving?

H: We took turns. We'd drive until Kisty fell asleep and drove off the road and then I would say, "I'm going to drive, George," and then I would drive for awhile. We stopped in Albuquerque for a bite to eat, and that was a mistake. That made us even sleepier. By my wife's account, I walked in the door of my home, sat down in a chair and fell asleep. It wasn't until six hours later that she waked me up. As everybody's reaction to the test there was about an hour of real excitement while everybody sort of recounted to each other his part in the events and the troubles that didn't happen; we compared notes on what we thought we saw and how it looked, but after about an hour, as cars began to load to leave, the atmosphere was just one of intense weariness.

M: So you went home and went to sleep, and then did you have to return to your job?

H: After that, we had a big job which was--I had a group out at Tinian--after that the problem was to keep them supplied. After the test we concentrated primarily on the Far Pacific.

M: Did you have to go to Tinian?

H: No, I didn't go. We had to make a decision before the test. We split my group; part of it went out and part of it stayed for the test.

M: Did you have anything to do with the selection of a target in Japan?

H: No. Absolutely none.

M: Did you have anything to do with an attempt supposedly by the scientists involved to ask Truman to warn the Japanese what the consequences would be?

H: Not directly. There was, of course, a lot of discussion at Los Alamos, in which I participated, as to alternative ways of doing it--the question of a demonstration on a deserted island, of warning the Japanese first, and a variety of things of that sort. This led to the attempt to persuade him not to use it directly. But I wasn't directly involved in the approach to Truman.

M: Well, after all of this was over with, after the dropping on Japan and the destruction involved, did you have any second thoughts about your work and what you contributed?

H: Well, you see, I think that my feeling was profound happiness in one respect; we were all convinced that it was absolutely necessary. People told us the Navy was building a 500,000 bed hospital on Tinian, for example, and people were out there in anticipation of the landings on Japan. There had been the problems on Okinawa, so that I think most of us were pretty well satisfied that, distasteful as it was, it would end the war and that it would save a lot of American lives. On the more general question, we were perfectly well aware that, if we hadn't done it, it wouldn't have saved the world from the weapon, because, for instance even during a period of high secrecy, there were--I shouldn't call them

amusing--but there were episodes to prove that just after the war, before there had been any breakdown in security, the University of Pennsylvania physics department caused us a lot of embarrassment because on an unclassified basis they set out to design a weapon themselves. What they designed in a fairly short time was so like what we used that it produced a public relations crisis. Our question was, if now you classify what they have done, that would confirm the approach. If you didn't, there would be a published description of something like the real thing when this was one of our very biggest secrets. It was all very delicate. They were finally gently dissuaded from publishing their designs. My point is that once the phenomenon of fission was known, the only question was when one or the other of the powers made a fission weapon. I don't think I ever took seriously the notion that has sometimes been expressed that if we hadn't done it, the world would have been spared the existence of the bomb. That I don't believe.

M: The atomic age would have been with us regardless?

H: The atomic age would have been with us no matter what. And in fact we might have been very much worse off because it's hard to believe that we would have been able to put together the effort after the war to match what was done in the Soviet Union which took only four years to make a weapon.

M: Well, then, with the end of the war, what did you do?

H: I stayed at Los Alamos until 1946, partly to put things in good order, partly to help them get the laboratory onto a permanent footing. I had no intention of staying, but neither did almost everyone else who was there during the war, so there was a real crisis in '46 until a permanent staff could be recruited. Anyway, then I mostly worried as to a job. I wanted an academic position. In the end it worked out very well. I had my choice between Chicago, Columbia, Rochester, and Brown, and somewhat to everyone's surprise, I decided that Brown was the one that offered the most opportunities. I was right too. So I went to Brown University as an assistant professor. That was in '46. From there on, it's the fairly usual kind of story.

M: At Brown, according to the information I have, you went from an assistant professor to associate, to full professor.

H: I was an associate in '49 and the director of the research laboratory. I guess it was in '51 I was made full professor. In '52 I became associate dean of the graduate school. In '53, I became acting dean of the graduate school. At that point, I was offered the permanent job of dean of the graduate school, and decided I didn't want it. Thus I became probably the youngest retired dean around.

M: You stayed on there at Brown until--

H: I stayed on at Brown until '57. In the meantime, and it may have been one of the events which eventually took me to Washington, I undertook a study in '51 on the application of infrared to warfare. It was the first real involvement with Washington after the war, and it

was of some consequence, because one of the things we came upon was that the Sidewinder air-to-air missile had just been killed as part of the general cleanup because of the fact that so many missiles were being invented that the situation was out of hand. But as usual the wrong ones got killed. I stumbled onto this fact, and I had a very direct and personal role in getting the Sidewinder back on the track. That mainly consisted of getting some money for it, but this is important because for ten years that was the only functioning air-to-air missile we had.

M: Did you have to deal with Eisenhower on this?

H: No. At that point, I was still a guy about thirty-one. I thought I was doing pretty well dealing with the top of the Navy and Air Force.

M: But were you able to get your ideas through?

H: Yes. Along the way I got an illustration of how, when you conduct yourself the wrong way, it can sometimes be helpful. I went, at the climax of this thing, to--I've forgotten the name of the admiral--but at any rate he was testifying on the Hill. I got to talk with Admiral Parsons instead. I had known Admiral Parsons in Los Alamos so it was a little better. I explained to him what the situation was, and he said, "Well, his word was that they had already, on the basis of our previous representation, funded the Sidewinder. I told him that no money was getting out to Inyokern." He called up some captain from down in the works who explained that for a lot of administrative reasons all sorts of things had to be done before funds could be made available and that I was in fact correct. Parsons explained to me how things had to be done in an orderly way, and I lost my temper completely. I just told him what I thought of the Navy and its capability for dealing with any enemy if it was going to operate that way. But I had an airplane to catch so that after I had lost my temper completely, I had to run. I caught my cab, and in the plane I had tears in my eyes because I said, "By gum, I really blew it. This was my chance to move the Sidewinder and what did I do? I lost my temper and shouted and raved."

Well, the outcome was okay. I have a letter I still treasure which I got from him about three days later. It was simply a diary of events after I had left him, which had been at about five-thirty. At something like seven-thirty, he had taken off for Inyokern to see for himself what was going on. Two days later they had been given completely open-ended funding to proceed on a crash basis. So I don't know, maybe I could have done better, but I have the impression in retrospect that probably losing my temper may have been right under the circumstances.

M: Incidentally, did you have any feelings about what happened to Oppenheimer?

H: Oh, violent feelings. My God, this is a scandal and disgrace that this country has never lived down.

M: You obviously had great respect for Oppenheimer.

H: Of course, I don't know of any man who could have done what he did at Los Alamos on the time scale in which he did it, who could have excited the kind of enthusiasm and creativity and hard work in so many kinds of people. It was a very, very difficult circumstance. He's also, to my taste, a poet, a philosopher, a kind of a man the country needs--you know, an intellectual through and through. It is perfectly true--that became clear in the hearings--that his judgment was questionable on some scores, and he probably relied too heavily on his own judgment about what was safe and what was unsafe. Still, there was no evidence that his judgment ever resulted in any transfer of secrets to the Soviet Union, for example. No, I regarded that as witch hunting at its worst. Actually, I suppose one of the things I admire my friend, Harry Smythe, for most is that he was the only Atomic Energy Commissioner who was willing to withstand the violent pressures and to vote against the withdrawal of clearance for Mr. Oppenheimer.

M: Was it frustrating to you that you just had to stay inside and watch this happen to Oppenheimer?

H: Of course it was frustrating, and it was to the majority of people who had ever known him, I'm sure.

M: Well, then, after your work with the Sidewinder, did you have any other government connection?

H: No, I didn't. I might have. I have always prided myself that I was in on a great many of the important developments one way or another. In 1954 I had a Fulbright grant and a Guggenheim grant, so I was spending the year in Oxford. At that time, I was asked to join the so-called Von Neumann Committee, which was the ballistic missiles committee. People had gotten alarmed at the intelligence about the Soviet Union and what they were doing with missiles; we were coming along pretty slowly here and the question was how to proceed. I was invited to join that, but I decided, probably wrongly, that I wanted to finish my year at Oxford, and so I didn't.

M: Who was the head of that committee?

H: John Von Neumann.

M: Then you finished up at Oxford.

H: I finished up at Oxford and came back to Brown, moved to Princeton in the spring of 1957 as a visiting professor and then in the fall of '57 I became professor there. In '58 I was made chairman of the department. My next connection with Washington was in '58, after Sputnik. I went on to the Space Science Board of the National Academy of Science to consider our own space program. NASA was just being formed then, and the initial job was to help formulate a program for NASA. NASA didn't have a staff yet so what was involved was not second guessing, but helping get it underway.

M: There must have been great political pressure then too.

H: Oh, sure.

M: From Congress and from the country in general.

H: That's right, but the problem was, you know, despite the political pressure to get things going on a sound and even keel rather than getting off on deadends. I guess it was in '59 I first met President Eisenhower, and he named me to his science advisory committee in '59. I joined it in January of 1960.

M: And at the same time you were doing your work at Princeton?

H: Oh, yes. I had a big and active research group at Princeton. I had four or five post-doctoral people and four or five graduate students. I taught undergraduate courses only off and on, not as a regular matter.

M: In this point in time, did you have any connection with Lyndon Johnson?

H: No. In fact, I didn't meet Lyndon Johnson until later. Once I got on the Science Advisory Committee, I spent about a quarter of my time in Washington commuting from Princeton. It was one of the things my wife disliked most about our life in that period. During the Eisenhower period, I became progressively chairman of two different space panels of the Science Advisory Committee--one called the Booster panel, which was looking at the problem of producing rockets; and the other, the Space Science Panel. I resigned from the Academy Space Science Board since these new jobs were internal to the government. So I got deeply involved in space.

Between the 1960 election and inauguration, I was on President Kennedy's task force trying to reformulate space policy. It must have been either December of '60 or January of '61 that I first met Lyndon Johnson because when the task force reported we met with President Kennedy and then had a longer session with the incoming Vice President. This was a very interesting experience for me because the task force was a very energetic one. It had Din Land, president of Polaroid; myself; Trevor Gardner, Secretary of the Air Force; Jerry Weisner; I've forgotten who the other members were, but at any rate we were very opinionated about the space program, and what's more we were very positive in our notion of what the role of the Vice President ought to be as chairman of the Space Council. During this meeting the thing I remember best was Din Land, and to some extent Trevor Gardner, delivering an impassioned lecture to the Vice President on what his responsibilities and duties were. He obviously wasn't taking any advice from those guys. He sort of sprawled back, listened with his eyes slightly squinted, listened very carefully, but was absolutely impassive. You wouldn't tell whether he had soaked up every word or whether he was thinking about something else. They left sort of disappointed.

M: Did he have a response to this?

H: No, very little. He listened carefully but was completely noncommital. Actually, and this is something that I've felt ever since, I don't think he was at ease with these people, most of whom, you know, are pretty high-level types, all somewhat academic, at least scientific intellectuals. This lack of response, I think, was in part that he didn't feel at ease with this kind of person. I don't know--I've felt it ever since though--

M: You think this still holds true?

H: Oh, I think that this still holds true. I couldn't have been better treated by our President. He listens to me very seriously; he's responded on what I thought were major issues and such, but there has never been any easiness about it. At the ranch, yes, but then mainly because all of the business falls aside. But I think all of my own contacts have always been marked by this reserve. This may be personal, but it also seems to hold with the kind of people I have brought around to see him.

M: To go back a little bit, are you impressed with the usefulness of task forces to solve problems?

H: Oh, this depends on the kind of task force. There are many kinds of task forces in the government; most of them are a waste of effort. But let's say there are some kinds that are useful. There are the internal task forces, and in general they are useful because the jobs are short term and they assemble ideas that are already around; if there is a leading idea, like when the poverty program was first being born, out of all the things you might do, their job is to pull together a package for the President to consider. This kind of task force, essentially the legislative task forces, ones like those in the summer of 1964, whose job basically is to pull together a concrete package which he can consider and which can form the basis for a legislative proposal or initiatives on his part, these have been very fruitful as a way of integrating and assembling ideas into actionable form. When I said that many are useless, I was referring rather to many of the commissions and task forces brought in from the outside. Many of these consist in getting either creative people or elder statesmen to shoot from the hip, to draw on their experience, and to sort of put down what they think without some effort to work their ideas through. On the whole they have relatively little lasting impact. In short, I'm not impressed by the kind that doesn't do any work.

On the other hand, starting with the pattern that Killian set, the kind of task forces we've had in our office have been enormously useful because they are quite differently conceived. You see, from the beginning we didn't identify publicly the existence of task forces; we refused to divulge publicly the membership of task forces, panels of the President's Science Advisory Committee. The reason was just to give people a chance to work on problems without being exposed to political pressures personally. For instance, the members of the President's Science Advisory Committee, I figure, on the average spend thirty-five or forty days a year working for him. Considering that they are all top-notch people, this derives from the fact that they think they are effective and they think it is worthwhile.

The Science Advisory Committee covers everything from health to military intelligence. On any specific problem the approach has been to set up a panel or task force. The notion is that a group works on a major problem, works intensively for a year and works as part of the Presidential family. They are outsiders, but in fact they are given very high security clearances, made reasonably privy, to the internal pullings and haulings of the government, at least in the areas with which they are concerned. They have brought in objectivity from the outside, but they have also been insiders in the sense that they have been part of the White House family.

M: And this has been successful?

H: I think this has been successful, yes. But, of course, you can't draw universal conclusions from this experience because the whole existence of this office is tied to the existence now of lots of problems in which there are complicated and involved technical considerations. The matter of arms control, the whole question of the future military posture of the country, for instance, these are the issues around which it was founded obviously highly technical questions. The President was being pushed around by promisers who promised beyond any possibility of accomplishment on the one hand and people who said they didn't want to play space basketball, to recall a former Secretary of Defense on the other. What happened when this office was established was that the need for detailed technical consideration as the basis for general policy judgment in a wide variety of areas was realized in the White House. But this required cool, objective, detailed analysis in order to present the President with well-reasoned sets of alternatives. An awful lot of the problems in a democracy just can't be analyzed to produce an answer, you know, but the technique I just mentioned to you has been applicable to introducing this new element into policy considerations. The element of analysis was possible particularly in scientific and technical areas.

M: Well, now, when did you get connected with this office?

H: In a sense, when I was appointed to the Science Advisory Committee by President Eisenhower. President Kennedy reappointed me to his Science Advisory Committee when he came in. During the Kennedy Administration I continued to head the two space panels of the Science Advisory Committee. Actually, this office wasn't created until '62; it was part of the White House staff until '62. After '62 the Special Assistant stayed a part of the White House staff, but the rest of the office became the Office of Science and Technology in the Executive Office. And so then finally, the history of my getting in here is that on November 7 of 1963 President Kennedy asked me whether I would be a Special Assistant, and talked to me about it at length. On November 14 of that same year he announced publicly my appointment as Special Assistant. And then, a week later, the assassination followed. At that point, I was in no man's land.

M: Between your appointment and your confirmation?

H: No, there is no confirmation for a Special Assistant. It is a personal appointment of the President. So I didn't know what my status was. I just wandered between Washington and Princeton and didn't know whether I was going to be a Special Assistant or not. Obviously,

with a new President, although it was sort of a continuation of the same administration as far as Special Assistants were concerned, he wasn't bound in any way, shape, or form by what President Kennedy planned to do. I guess it must have been some time in December that I saw President Johnson at the time he gave the Fermi Award to Oppenheimer, which, incidentally, I considered a politically courageous and wonderful act. But nothing happened. We just talked briefly then. But afterwards Bundy came to see me and wanted to know what was going on; was I or wasn't I coming down here. I told Mac that, well, I just didn't know. I didn't know whether I should buy a house in Washington or what I should do. As a matter of fact, if I had known how many other people who thought they had appointments were eventually not going to turn up here, I guess I would have been more worried than I was. But I was aware that the President had a lot of things on his mind and so I was inclined to wait, although I was personally unhappy about not being able to make plans.

Well, at any rate after the talk with Bundy, he said, "Well, let me talk to the President," and I presume he did. At least shortly thereafter, some time in early January, I don't know the exact date offhand, but I had a talk with the President and he asked me all over again whether I would serve as Special Assistant. I actually came down here, I guess, about January 15, more or less immediately. And on January 24 he formally appointed me as Special Assistant. He also sent up my nomination to the Senate for the post of Director of the Office of Science and Technology. As far as the government is concerned that is a separate job although the first two people to hold it have held both and that is as it ought to be. It was fortunate for me since at that point the funding for my salary had been transferred from the White House to the Office. I remember on Monday morning, the twenty-sixth, Senator Hill called me in a rather amazing conversation and said, "Can you think of any reason why anyone might object to your appointment?" And I said, "No, I couldn't think of any." So that without holding any hearings the committee voted that day, and so I got a salary starting on the twenty-sixth. So that was that.

M: And you've been in this position ever since?

H: Yes.

M: Since January of '64 to the present time?

H: That's right. In fact, I now have four different Presidential appointments simultaneously. One is as Special Assistant, one is as Director of OST, one is as--no, I only have three actual sheets of paper--one is as chairman of the President's Science Advisory Committee. The fourth job is Chairman of the Federal Council for Science and Technology which by executive order is automatically held by the Special Assistant.

M: To clear up my ignorance on this, it would seem that this office has a rather unusual scope. If you are going to cover and be an advisory office to the President in science and technology over the whole spectrum of government activity, this would seem to be quite a bit. Is that right?

H: It is quite a bit. It is the source both of the strength and I suppose some of the weaknesses of the office, but it is true. President Eisenhower originally defined the job of the special assistant very simply, to advise and assist him on all matters affected by or pertaining to science and technology. That particular description was carried over under President Kennedy into the legislation that set up the Office of Science and Technology. There are lots of 'for instances' in the legislation, but the key words are in this phrase "advise and assist the President in all matters affected by or pertaining to science and technology." Now, originally, this meant primarily defense, the thermonuclear weapons and rockets; it also meant disarmament. You remember the Geneva experts' meeting in 1958, and the surprise attack meeting. This office provided the intellectual leadership for those two negotiations, hopefully also for the nuclear test ban. The third thing originally was graduate education in science and the general basic research foundation of the country. This subject, which is very different from the other two, arose largely because of the concern after Sputnik that somehow the Soviet educational system had gotten ahead of us, that they were producing more scientists, which incidentally they are by a factor or two, but at any rate there was a fear that a great cross-over had just taken place and they had just demonstrated that they were moving ahead on a broad front. So that there was a general concern with education.

M: Incidentally, is that still a concern?

H: Oh, yes. Sure, it is of concern. The situation is very clear. The Soviet Union produces about twice as many Ph.D.'s in the sciences as we do; they have a stock of about twice as many Ph.D.'s in the sciences; we are convinced, however, that the quality of what they do is sufficiently lower than our own so that in fact we have an edge. But the reason it is of concern is that there is a very rapid learning curve in progress. When they built their first big accelerator at Dubna the thing just barely worked and although it was, for example, the biggest accelerator in the world for several years, nothing of real scientific consequence ever came out of it. On the other hand, they have just started what is now the biggest accelerator in the world, the seventy billion volt accelerator at Serpakov near Moscow and it worked the first time it was turned on. Many Americans have seen it. It is well engineered, well built, and they have obviously come a long way in the five or six years between them. I think it is also characteristic also of their military technology which originally tended to be crude and cumbersome, so that I would say that what is of concern to our country is that they are getting more sophisticated and they obviously are learning; they are learning from us.

M: Was Sputnik indeed a crossing point?

H: No, I think it is quite clear that it was not a technological crossing. It was more a political crossing point for this country than it was for military affairs. No, in retrospect, Sputnik demonstrated that they had built much bigger rockets than anything we had in mind, and they did work. But as to the much more general implications , I would say it became clear that, after the smoke settled it did not signify that their technology was competitive with our own on a broad front. But in this country, of course, it provoked an intense reexamination of everything from our educational system to our science to our military establishments.

M: Then under Eisenhower when this office began, its genesis, the concern was with military affairs, test ban--?

H: The intense concern was with military affairs and the test ban, yes.

M: And with this emphasis on graduate education especially in the sciences?

H: That's right.

M: Now, in the '60's then, this office was established formally.

H: Well, a special assistant and a staff for him were established by Eisenhower, but it was in '62 that reorganization plan number two made it permanent.

M: Now, has it broadened?

H: Yes, very, very much. If you look at the range of things that the office deals with now, it will run something like this. In the first place, there still is a strong group working on military problems, but since one of the first recommendations out of this office was the establishment of the Office of Director of Defense Research and Engineering, that was spun off and that lightened the load. Now the office is involved in, besides military affairs and intelligence affairs, it is involved for example in problems of health. For instance, one of the reports that the President made public was a big report on our biomedical programs and their administration, the Wooldridge Report; it has been involved particularly in health manpower and medical education; we've gotten persistently involved in the problems of pollution; that actually goes back to Eisenhower. The first involvement of that sort was the great Christmas cranberry crisis.

M: Ocean Spray?

H: Ocean Spray. And although that was the momentary thing, it had quite a big effect because the studies which were done here resulted in the practical abolition of the concept called zero tolerance, that there must be no carcinogens left at all. In fact, this is a meaningless concept because if you use sensitive enough analytical methods and recall that under some circumstances even salt can be a carcinogen, the country would have to stop eating. So we got together some better working definitions of what the law meant which has been tacitly accepted although the law has never been rewritten. We got heavily involved in the question of pollution in '64--

M: Is this water pollution or air pollution?

H: Water and air pollution, both. And in '64, when the task forces were being organized I wrote a memorandum to Bill Moyers in which I opined that pollution and the problems of the long-term maintenance of the environment were going to be one of the big political problems of our time and that it was time this administration got moving on it. I must say

that that is one of the prognostications I've been proudest of. We did set up a task force that summer; a lot of the ideas from the task force found their way into new legislation in the succeeding years. After the task force finished we thought it was important enough so we published their work. The report which the President issued is "Restoring the Quality of the Environment."

M: Well, now, in your--

H: Let me just mention some more areas. In the year HUD was established, we ran a six-week, summer conference involving architects, city planners, engineers, social scientists, psychologists, sociologists, physicists on "Science and the City." They attacked the general question of what did science and technology have to do to contribute to the long-term development of the city. That conference laid the groundwork for subsequent programs in HUD. The problems are hard, and the answers don't come easy. One of my people William Hooper has played a principal role, I would say, in stimulating the current push toward the production of much lower cost, quality housing for the low-cost housing market. One of the shocking things in this country is that whereas through mass production the unit cost of an automobile is enormously lower in real terms than it was in 1905, there has been no decline in real costs of producing housing at all.

We've also tried to coordinate the Federal water programs and helped the President with problems in the Northeast drought. There still is one man on my staff who spends all his time on water problems.

Until the formation of the oceanography council, we generally coordinated the oceanographic activities of the government.

The range of our activities is dictated by the sense of significance, either by what matters to the President at any given time, or perhaps more important--is to try to anticipate for him what is going to matter. This leads us in many directions. It has even gotten us into Treasury because we proposed three years ago that it was worth taking a look at whether we might not be able to find and produce gold a lot cheaper than we had. That led to the formation of the so-called 007 committee on which Ackley and Schultz and myself and Joe Barr from the Treasury were asked to look at this problem. Then the Geological Survey and the Bureau of the Mines got into it. I can't say we have produced billions of dollars worth of gold yet.

M: But at any rate, your office has covered a broad spectrum.

H: The way the office works is sort of unique in the White House. There is a staff here which is quite good and which is now quite diversified. My deputy director is an M.D. Aside from the staff there is the President's Science Advisory Committee which represents our primary source of external advice and assistance from the scientific and technical community.

M: You have a two-way flow of ideas; from the scientific community, you bring in new ideas to the government. If the President needs advice on something, he asks you for it. It flows that way.

H: That's right. There are a variety of channels of flow to him. In a number of his messages, he has formally asked me to undertake things like the study of the world food problem, some of the water and pollution problems. More commonly, this happens informally. Sometimes there are formal reports to him, most of which are not published, but some of which, the Medical Manpower Study, the Wooldridge Study, the one I mentioned on restoring the quality of the environment, the oceanographic study, are subsequently made public. They are really sort of planning studies for the future. There is the informal talk with him. Then of course the President is not only a man but he is the center of the thing we call the Presidency which is all the rest of the--the rest of the White House staff, and then the Cabinet members and heads of executive agencies so that there is also a lot of contact in both directions with all of these people.

M: But you are more than just a staff assistant. You are a liaison in effect.

H: That's right. In effect I am a liaison between the President and the scientific and technical community.

M: Is there any program that you have worked on that Lyndon Johnson has as President been especially concerned with? Or does this include everything?

H: Oh, no, obviously he is more concerned with some than with others. On Defense things I have primarily dealt with the Secretary of Defense. With Bob McNamara I had, aside from other contacts, a luncheon every two weeks. We just kept a sort of running contact, and I just dealt with the President on special problems. But I would say air pollution and environmental quality, largely originated during his Administration, and he has had a very personal interest in that. The world food problem and the population problem were certainly very much his own. Before the 1965 State of the Union address though, I remember I gave him a long paper urging him to include a paragraph on the population problem, and I talked with him for about an hour about the population problem on the way down to the ranch. I was originally disappointed when he only included one sentence. But in fact, though, that was one of the more important single sentences because it turned the matter loose. It made it legitimate for the government to begin to think creatively about this. Now, I don't doubt that lots of other people talked to him about the population problem, too but I'm sure that the discussion that we had on Air Force One was certainly one of the things that persuaded him that the time was right to move. Anyway, that is one he was interested in.

Many of the things we are doing, he is interested in at a greater distance though. He encouraged, for instance, the efforts we have made on stimulating low-cost housing technology. He has referred to it after I first suggested that the Department of Defense ought--since it built houses--to act as an agent. He suggested I go see Clifford about it, which I did. I think that it is at least one of the ingredients in the decision for his new

program. This office has tended, I think, to be reasonably close to him but not close in the sense that NSC and Rostow's establishment are not on a day-to-day basis. It has tended to be occupied with longer range things which he has encouraged, which he keeps abreast of, but doesn't get involved with day-to-day acts to speak of--except that we have contributed to a lot of messages he has sent up to Congress. That's one way in which our efforts reach some fruition.

M: Are you satisfied that this office is working as it should? Or can there be areas of improvement?

H: Well, I guess--since we are being very candid--my own personal discomforture is the one I mentioned at the beginning of the interview, that I have been reasonably close to the President but have never felt that he has been plugged in quite as tightly as I think would be ideal.

M: Now, this may be personality.

H: It may be personalities; it may be the President's interests. I really don't know. Of course, there are more fundamental governmental problems. There is a big problem in this government which occurs in any large organization. You can set up an organization any way you like to accomplish a set of purposes. You have a set of Cabinet agencies, and then along comes a problem which cuts across the whole of the structure. Pollution is one point. Consider water pollution. Agriculture is involved. Agriculture likes to foster the use of insecticides, naturally. They like to foster the use of fertilizers. HEW, on the other hand, also is heavily involved. They look askance at the use of insecticides, and they see fertilizers as something that run off into lakes and streams and fertilize the algae, and so on. Then Interior sees these things as things which run into the Mississippi River and kill fish. The question is how do you pull these differing attitudes and interests together at a high level.

One of the charges of this office is to evaluate and to advise and assist the President in the evaluation and coordination of these programs. But the tools, even for the President, for this coordination are relatively weak. I don't really know what the managerial invention should be or I would have proposed it before now. This problem has been met in big industrial enterprises through the device of the program manager, who generally cuts across organizational lines by handling some of the budget for a specified purpose. Maybe some day we will have to find a Constitutional way to do it. One of the difficulties in simply proposing an administrative solution, though, is that the government structure parallels committees in the Congress so that you would have to invent some way to crosscut the committees as well as crosscutting the agencies. This is going to take a lot of doing. Anyway, I think some things still need to be done in that direction.

When you talk about future evolution, there is another point I might make. Last year, for the first time, the President asked us to undertake a job which is not science and technology at all. He asked me to set up an energy policy staff to take over the coordination of energy policy for the country. Now, energy policy involves the prognostication of the

future demands of the society for oil , coal, nuclear energy, what have you, and the shape of industrial development. So there are strong technological components in it, changes in the technology of extracting these things and of the costs. But it also concerns such things as tax matters, depreciation allowances, a whole lot of things that are surely not scientific and technical. And so the fellow who heads the energy policy staff here is a lawyer. One can ask, "Why is this done in the Office of Science and Technology?" And the answer is that when the President's energy task force worked on this, they couldn't find any other place to do it. What are the alternatives? The Council of Economic Advisers and the Bureau of the Budget. The Bureau does a lot of very important things, but as a positive policy developer it can't escape the fact that it is also the Bureau of the Budget when it deals with agencies. That was the objection of the task force. The Council of Economic Advisers, while it has gotten involved in these questions, is principally concerned with monetary, tax and fiscal policy on a macroscopic scale. They didn't feel they had either the staff or the ability. So in effect what is happening here is an adaptation of the general technique which had worked for science and technology providing an office of analysis for the President. I think that one day it is possible that the OST won't exist, but instead there will be another office in the Executive Office of the President that might be called the Office of Planning and Analysis, or something like that which would have a science and technology section, but which might also have a section on cities, or maybe a section on state relations.

Of course, the problem is how to keep it from just sprawling indefinitely. The biggest problem--the central issue--is to define the right boundary line between a small, nimble top group which is very responsive to the President, and a more formalized structure which can in fact do something in greater depth to study the complete range of things which from time to time he gets into. There are pressures in both directions. The Congress, which wants much better business management, presses hard on us all the time to assume a bigger role, not so much as advisers to the President in the ongoing sense, but as coordinators on problems that go across many agencies. I have resisted that. I think it needs to be done, but I'm not sure how to do it anyway. There are obvious liabilities in replacing a small staff responsive to the President--too many. This may be part of the problem I mentioned to you. The more the office becomes a bureaucracy, the more it tends to move away from the President.

I'm not entirely sure when you talk Office of Science and Technology whether even the President understands that the Office of Science and Technology has no clear assigned job. You see, unlike the Bureau of the Budget or the Council of Economic Advisers which have some statutory functions--the Office of Science and Technology has only one function, to advise and assist the President. But it's already unique in that we are bigger than the Security Council. As opposed to how Califano operates with a small staff around him, I have here about fifty--well, that includes the non-professional--about twenty-five professional people on the staff. So it's by far the biggest single staff that is close to the President. The Bureau of the Budget, of course, is enormously bigger.

M: It seems that your office then is in a period of evolution.

H: Very much. It has been ten years since the first Special Assistant was appointed. It has been five years since the Office was created. There have been four special assistants, and each one has done things differently although one can discern a very clear trend which, is the broadening from military affairs to a base which now spans just the entire government, dictated only by the President's tastes and interests.

M: Well, now, I have exhausted most of the questions I have. Is there anything that I should ask you that I haven't that you can think of?

H: I might just mention a coup le of things because they are interesting sidelights. One of the things we have been very much involved in has been foreign problems. Let me tick off a few of them. In 1964 one of the deputy premiers of the Soviet Union invited me to come to the Soviet Union as the first half of an exchange in which he would be the return visitor. As a result I went to the Soviet Union. What was interesting about it is that I think I was the first senior American government official to see Breshnev and Kosygin after their change of government. I went immediately after our election. The President gave us his plane for the visit. It was a most interesting occasion. The outcome of it was to raise some real questions as to whether our policy of trying to inhibit the flow of industrial technology to the Soviet Union was productive or might not be self-defeating. I think this visit was one of the principal things that led him to set up the East-West Trade Commission which deals more broadly with those questions. We didn't.

Our approach to the two week trip was this. I took with me a group of American industrial people. Piore was a Vice President of IBM; Hershey was a Vice President of DuPont; Holloman was Assistant Secretary of Commerce; and Fisk was President of Bell Labs. The scheme for the visit was to work our way down the Soviet hierarchy. We talked down the chain of command in two areas--communications and electronics. We started at the center of government and ended up talking to plant managers in some factories. Well, that was the first time the President really became clear that science and technology had a role to play in the conduct of foreign affairs. This came up again a little later in the spring of '65 when President Park Chung Hee of Korea was visiting the President here. It was the day before his meeting with Park that he called me and said that the stuff he had from State was a lot of crap, and he wanted something creative to propose to Park by the next morning. It happened to be on the day when the Science Advisory Committee was meeting, so during the day I kept thinking about this. Gradually the rough concept of the Korea Institute of Science and Technology emerged. It would not be the kind of pure research laboratory that the underdeveloped countries like to think of, but rather be organized on the model of a good industrial research laboratory, but would be good enough so that it would be able to attract back some of the Koreans who come to the U.S. You know, their entire college output in engineering goes to the United States, which is a very shocking situation.

Well we discussed it at the P.S.A.C. meeting. After P.S.A.C. adjourned, I collected a rump group which I remember included Frank Long from Cornell and Ken Pitzer, the President of Rice. We talked some more about this and firmed up the general description of--we didn't call it a lab--at that point we called it the Institute for Applied Research and Industrial Development, I believe. But at any rate we sketched out the general ideas and I

took it to the President the next morning, approximately one hour before President Park arrived. He liked it all, and we had a desperate time getting enough copies of the plan produced before the meeting. He invited me to sit in with him on the discussion with Park and I described this idea. Park was completely surprised because it hadn't appeared in any of the stuff they'd been given in advance but he was pleased and by the end of the meeting President Johnson had agreed to send me to Korea to see whether the idea could be implemented. Of course, there was a terrible row with AID and State who quite correctly, I might say, since--it was their money that was being put up, don't like to have these things happen without having ever heard about them or been able to comment on them. This ended up as an enormously successful venture which has been a model for at least one aspect of development in a lot of countries. As of now, three years later, it's in being--

(End of tape)

INTERVIEWEE: DR. DONALD HORNIG (Tape #2)

INTERVIEWER: David G. McComb

H: This thing is in existence. They have done economic and power surveys of the country. The first laboratory building is just being finished now and to our really great pleasure it has resulted in a flow of Korean scientists and engineers back to Korea.

M: But this idea can be applied elsewhere?

H: Yes. Indeed. The Chinese, for instance, in Taiwan have used their own monies setting up, I guess I had better call it a Chinese copy. It is a model now for several other countries.

This event that I have been elaborating on obviously gave the President ideas because the next summons came when Prime Minister Sato was here. The President called me the night before his meeting with Sato and again wanted a new idea. That time I proposed the Japan-U.S. medical cooperation program and worked all night on it. And this led to a very amusing incident. In the morning again there was another desperate effort to get organized. I talked to LBJ on the phone and outlined in general what I had for him and he thought this was fine. We started putting it down on paper but didn't make many copies. I got over to the White House with this thing at just about the same time Prime Minister Sato arrived. I gave the President his copy on the porch while he was waiting for Sato, and he read it and liked it. But the amusing consequence was at the table where I was with him when he talked to Sato. We passed over the President's copy to the Japanese. Beside that I had a copy and Ambassador Reischauer had a copy; these were all the copies that existed. Since this came as a surprise to the Japanese, members of Sato's staff wanted to see the text, so we passed those two copies across the table, too. After the meeting was all over, the discussions over, and Sato had departed Washington, the Japanese Embassy called over to see if they could get some copies of the proposal because Prime Minister Sato had taken with him all the copies they had. At that point we woke up to the fact that we no longer had any copies. Everybody thought someone else had one, but they had all been passed to the Japanese, which was potentially--I'm not sure this has ever been recorded--it was potentially terribly embarrassing. Fortunately, we went through our wastepaper baskets here and found the handwritten copy from which they had all been typed, and so the whole thing was bailed out.

Then, there was another time when Pearson was Prime Minister of Canada and was visiting with the President at Camp David. By now he'd gotten fairly assured that he could always pick up something, so again I got a phone call and a request to brew something.

M: So your idea went through?

H: The Japan-U.S. Medical Program went through and it has been implemented. It's going strong now and it's good. In the case of the Canadians, we talked to them about Great Lakes pollution; that is still going ahead but is only faltering along. And then when President Ayub of Pakistan was here, again the President proposed to send me to Pakistan

to look at the problems of agricultural development and rural medicine, so I did that. He sent me to India to look at secondary school science education, and then, largely as a result of the Korean thing this last fall he sent me to Taiwan to talk to the Chinese. In this case, AID has terminated. This raised a problem which is more general than Taiwan; how do we deal and how do we cooperate with and assist the AID graduates who still have a long way to go but have graduated from assistance? That trip turned out to be a very good adventure which resulted in a new experiment. The Ambassador in Taiwan now has a special assistant for science and technology who is a top-notch American industrial and former academic scientist who is working very closely with members of the Chinese government in their internal development.

M: You have, then, become a--

H: A general troubleshooter.

M: That's right. And you have run across lines of state and new adventures as well. We probably would send a State Department official to, say, India but he sent you instead.

H: Well, State doesn't have a strong internal capability on matters that deal generally with scientific and technical things. I was also called on for more formal diplomatic missions. With Foreign Minister Gromyko I signed the Soviet agreement for cooperation in desalting water in Moscow. I also signed a cooperation agreement with the Romanians about six or eight months ago, which has led to an atomic energy arrangement and the cultural agreement. When Prime Minister Holt was here the President agreed to send me to Australia. There wasn't any specific scheme that time, but it was just proposed that he would send me a high-level team to see how we could improve cooperation with them. So I signed a formal science cooperational agreement with the Australian Minister of Science and Education about two months ago. So this pattern has continued.

There is an incidental and unrelated matter I might mention. Every once in awhile one discovers with surprise, when you bemoan the fact that a Special Assistant has no powers, that in fact you've got quite a lot at your disposal if you only use it. This I first learned in 1964 when Nick Katzenbach and I jointly chaired what I think was called the Telecommunications Planning Committee. This was primarily to coordinate the government point of view with respect to the INTELSAT negotiations and COMSAT questions. One of the big questions was whether the military would use COMSAT for our military communications. Bob McNamara wanted very badly to do that. He thought that was the best way to support COMSAT without subsidizing it. All of us were very uneasy about this, partly because of the problem of the security of communications, partly the question of whether it was really sound. At any rate there were no good solid objections so the plans progressed to the point where one Saturday afternoon, Cy Vance, the Deputy Secretary of Defense, was going to sign the agreement with COMSAT and commit Defense to carry their communications over COMSAT birds rather than fly their own satellites.

Well, during the morning, an idea which had been gelling for a long time finally settled hard, and I had several conferences here. What bothered us was that in order to have

secure communications two microwave bands were needed. One was around 3-4 thousand megacycles, and the other around 9, I guess it was. The upper one had never been in any commercial use. It was reserved for governmental use but by this device it would be handed over to commercial use. I said to myself, gee, this is much more fundamental than these general debates, because in effect we hand this whole frequency band over and start training the Europeans in how to work in the high frequency band, whereas the only existing equipment was American. We would have, in effect, traded off a big piece of the frequency spectrum into the commercial spectrum for no purpose and this is just wrong and dangerous.

This was within hours of the agreement being signed so I called Katzenbach and said, "Look, we've got to do something about this." Well, he's not a technical man, but he generally appreciated the arguments. But he said, "Well, now look, Don, we've been having formal sessions for six months. The committee has signed off on it. It's just too late." And so he he didn't want to have anything further to do with it. But the more I thought about it, the more upset I got, so finally I called Cy and just told him flatly, "You can't do it. I was mightily surprised myself when his reaction to that was, "Okay, I won't do it. But let's convene a group right away to start looking at the questions you raised." By three-thirty that afternoon, we did have a group convened to start looking at the details. And eventually the original agreement was discarded. That was the first time I really learned that when you call over from the White House and just say, "Don't," whether you've got authority or not, something tends to happen at the other end of the line.

M: The prestige of your office does hold some weight?

H: That's right. It's not any different, maybe less so, than if Joe Califano or Walt Rostow made a similar call.

M: Does it bother you to have so much government subsidy in science in the military area?

H: I'm not sure I know what you mean when you say that.

M: Well, government contracts going to universities for research on military programs.

H: Well, in the first place, there is practically no government money that goes to universities for support of military programs. The military got into the support of universities because after World War II, being aware of what happened with respect to radar, nuclear weapons, lots of things, being aware that very important things had happened which came out of purely academic science which had nothing to do with military affairs just before the war. The key people in our radar development were nuclear physicists at M.I.T. DuBridge, the President of Cal Tech, who had been a nuclear physicist, was head of that laboratory. They were aware that most of the staff of Los Alamos were academics, and that they didn't exist anywhere within the military or industrial community--people who could have put together the nuclear bomb. In short, they came out with an item of faith that the future security of the country wasn't going to depend only on our manpower strength and our industrial

strength which was the base in World War I and Civil War, but also on the intellectual base of the country.

And so it was that the military took on the role of being a science foundation. The Office of Naval Research made a fetish of the fact that what they regarded as significant to the Department of Defense was just a strong academic science base which would play the same role in any future war as it had in World War II. So almost all of the DOD money that is spent in universities is spent on the basis of proposals made from the universities for what people consider good science. We now have a National Science Foundation; the difference in what is called military research is that there is obviously a selection of fields. They support a lot of work in electronics and solid state physics which is not directly connected with any military objectives, but which the people in Defense say are foreseeably more critical areas looking twenty years ahead than, let's say, the identification of marine organisms would be. I would reject violently the notion that this orients universities to military purposes. I think if you were to check the Defense contracts, for instance, at the University of Texas, you would find that what I say is literally true.

Now, as to the soundness for the country in the long run, I would put it this way. I think that the DOD position has to be this--if no one else does it, then the Department of Defense has as part of the Defense responsibilities, the responsibility of assuring that there is a strong, viable basic science and science education establishment in the country. I think it is an undesirable place, nevertheless, to do it from in a democracy, but not for the reasons that some of the people at the University of Montana or other places have claimed. Doing it in Defense is always too easy a way of getting at all sorts of problems. But look at what has happened historically. In 1949 the Department of Defense provided 90 percent of the federal money in universities, and now it's about 15 or 20 percent. In my view as fast as you can take the general support of basic science and a higher education via NSF, NIH, other agencies, you surely should.

But this President has, for example, gone to the Congress for several years with proposals for substantial increases in NSF and he has been turned down. He's somewhat disillusioned by now. Both in the spring of '65 and the spring of '66 he tried very hard to pursue this course. So anyway I think that's the problem. I would prefer to see much of this support come via NSF, National Institute of Health and the civilian agencies. But at the moment I do not believe that this is perverting our universities to military ends in any way. There have been isolated examples that I think were bad. This business of the biological warfare work at the University of Pennsylvania is the wrong thing to get a university involved in. People tend to think these questions are new. I wrote a policy for Brown University when I was there in the 1940's to clarify just this problem long before it was an issue. We took the position that the university would not get involved in any classified work except if we determined that we had a unique capability and that there was no question that it was a national problem, not just someone saying that it was important. And that was '49. And at Harvard we have had a policy of no classified work.

M: You would have no misgivings about government support of science in general, even outside the military?

H: If you can name me an alternative, I might prefer it. No, of course, I would prefer pluralistic support where there was a number of different sources of support. It would give the country more options, more flexibility, more freedom of choice, but there just aren't any at the moment. No one has thought of anything else that can come close. There is a considerable amount of industrial support, but in one field chemistry where I know the number it amounted to 7 percent of the support. There is a considerable amount of support by the States through faculty salaries. This is not thought of as research support but since the faculty members are the key men, certainly through faculty salaries there is support by the States. But all of these things add up to about 25 percent of the total and as far as I know no one has any ideas as to how you could make that 25 become 75 percent. So I guess what I would say is that other ways might be better, but I don't think I have misgivings at the federal government's doing it. I think that we have to be careful as to how the federal government does it so as to preserve the autonomy and freedom of the universities in the process.

M: I have kind of a farout question to ask you just to see if you respond. Did you have anything to do with the so-called flying saucers or unidentified flying objects?

H: I stayed meticulously away from that subject.

M: The question keeps nagging the public.

H: The question keeps nagging the public, I know. From time to time, just as a matter of personal interest, I looked at some of the reports. I guess all one can say is that there surely are some phenomenon that have been described by competent observers. In those cases where enough detail has been described to really track them down, there has been nothIng peculiar at the end of it.

M: As a scientist--

H: I thought you were going to ask me next if I had ever gotten involved in parapsychology.

M: Well, I could ask you that.

H: No.

M: Do you have any misgivings about the direction of our space effort? Enough emphasis? Not enough? Or what?

H: Well, this is a problem. No, I don't have any general misgivings. We've written one report which the President made public on the directions of the post-Apollo space program. I suppose that defines my views quite a lot. I'll tell you where my misgivings lie. Space is a symptom of a much broader problem. The thing that goes 'way back to de Tocqueville--who describes this as a country with a spirit of adventure, a defiance of tradition. I remember particularly an account in de Tocqueville where he talks to a sailor in

Boston and he called him, as I remember, a crude, untutored man, something like that, maybe it's a rude, unlettered man, I don't remember. At any rate, he asks this fellow why it is that since American ships are in some respects the best in the world they are so badly built? And this fellow says, without a moment's hesitation, "Sir, the arts of navigation and the arts of shipbuilding are advancing so fast that any ship we build today will be out of date in a decade anyway." He cites, and I think quite correctly, this as an example of what he considered a uniquely American approach, and the lesson he draws is that this attitude extended right down to the uneducated in America. This has seemed terribly important to me. We have slowed up in a lot of ways in this country, but it does seem to me that the spirit of enterprise and more so of adventure is still an important ingredient of what I consider the American national character. I think that space, along with other things, has been one of the important recent embodiments, and this President had a lot to do with that, with the 1960's expression of it.

I am disconcerted by the many fronts on which we seem to be a pulling back, by an air of caution which, in many things, seems to me to exceed what we have experienced in recent times and which is of concern to me. You see, recently I had, for instance, direct contact with this question in a different way that may seem unrelated. When the question was raised by Prime Ministers Fanfani and Erhardt of the technological gap, the President asked me to put together a study of the technological gap, what it was, and what it wasn't, what its implications were, and how we dealt with the European governments on it. In the course of it, this whole thing came quite clear to me. McNamara said it was a management gap, some of us said it was an education gap, but Pierre Marsailles in France, I think put it together best. He said, "It all adds up to an attitude gap." We educate more people; we educate them to a higher level; we find our management is more enterprising; but if you go further you find the availability of risk capital, the way banks conduct their businesses compared it with Europe, there is just an enormous difference. The American consumer expects to find a new model of everything next year, and if there weren't any previous models, he expects to find some inventions which will make things go better in the kitchen or wherever. This is why I was intrigued by Marsailles' characterization which really goes back to de Tocqueville. This country has thrived on a spirit of adventure, in a sense the spirit of the frontier, in a variety of ways.

And so to get back to your question, I am disconcerted at our pulling back from space as a potential manifestation of a general loss of exuberance. As for the details of the space program, my own view is that we could have done and should still do more with instrumented unmanned space vehicles rather than focus on manned flight. Mostly I think that we shouldn't think separately about manned space and unmanned space. We should rather focus on the missions and then ask, "What does a man do best and what can you do best for him with instruments?" This is just saying that in an airplane no one is going to take out the pilot, although no pilot ever flies his plane to speak of. Practically everything is done by the autopilot, the pilot is there to exercise judgment and throw the autopilot out any time it misbehaves. During--of the evolutionary part of the space program there has grown up the feeling that anything unmanned was a threat to the idea of man's role but that's human.

M: I have another general question for you. There is an old argument in at least our generation that science has outpaced the humanities, that we in effect have weapons and technology to destroy ourselves and yet not the knowledge to settle our human problems and to make peace. Now, do you have any thought about that?

H: In the first place, I don't know whether it is true. For instance, at the time of the invasion of Europe by Genghis Khan, the proportion of people killed were greater than anything we have ever experienced since. Still, this problem makes sense to them. We have the weapons to kill people. We surely can kill them more efficiently than we ever could before. I have faith in people. I don't know, in the first place, how to advance in the humanities, so I'm not sure I know what the question means. It may be easier, if you confine yourself to political science. It's the human character you are talking about, and so I don't know.

Certainly, science in one sense poses new threats, because what it really does is pose new options for the human race. I have sometimes said, it enlarges the stage on which the human drama is enacted. This is, I think, the fundamental thing. It makes possibly new ways of killing, yes. We have computers that will make possible new ways of organizing society, some of which, incidentally, are a threat to privacy. It has also been said that the computer is the greatest force for honesty since the Ten Commandments. So you have a moral factor. It has also, by the indirect effect of providing wealth, enabled us to educate many, many people farther than was ever possible in any previous time in history. Now, I would think that this ought to be one of the great humanizing influences. I don't know what 'outpace' means. Science offers more tools to solve problems as well as constantly producing many more possibilities for doing unsocial things too.

M: The whole episode of the atomic energy training bomb would also have possibilities for nuclear energy in desalting.

H: That's right. Medical advance through isotopes.

M: This would tend to support what you are saying.

H: Well, the world is entering a period now in which we will have to cope with a bigger thing than nuclear energy. This is the understanding of the biological process. If you ask what has happened in the last ten years, for the first time we have reduced, or have begun to reduce, heredity to a purely mechanistic status. One has begun to get both from the psychologists and from the neurophysiologists of the beginning of an understanding of the brain. We have begun to learn how to make artificial repairs, provide parts for the body, and to transplant organs. When you ask about the kind of problems one can from the purpose of the control of disease, it's surely true that the only way one will ever get scientific mechanism is to understand the thing so that medicine can be cause and effect rather than an empirical treatment of the symptom. And that's great. But if simultaneously it becomes possible, as I think one day it will, to in varying degrees control heredity--I don't know. In one sense, man has always been able to control heredity through selective breeding, and has had sense enough to stay out of that with a few rare exceptions like the Nazis. But to repair people indefinitely, I mean, one can think, for instance, that the day

might come, you know, when you would like to keep attaching the head of an Einstein onto successive generation of spare parts underneath it.

Now, I submit that this ability to begin to tamper with the, if you call, like the soul of man and say possibly to prolong life for selected people indefinitely, it is going to pose more moral and ethical problems that are possibly--and possibly even threats to the existence of man, that we have never, never ever faced before. And so it illustrates your problem. I don't know how to answer your problem basically, except as an item of faith and to say, "Well, let's push on the humanists and social scientists and give them all the support we can give them to not let that become a situation."

M: But science is posing tremendous problems for the humanities in the social sciences such as in heart transplants and brain transplants and what not and in heredity?

H: Sure. It surely is. This isn't a new phenomenon. After all, when Galileo was summoned before the Inquisition, one had science posing an enormous threat to what was then to the Church and to the Establishment. The thing that isn't commonly recognized is that how much science is a humanity and how much it has contributed between Galileo and the twentieth century to our whole concept of what man is, what his place in the universe is, what kind of a being--and I mean, through Freud, what kind of a being he is. Surely the whole idea of the subsconscious which is now solid experimental fact whether you like the details, and most people don't. Nevertheless, it has changed our whole concept of criminal justice and in dealing with people. The whole picture of evolution has settled down, I think. Nevertheless, when one sees man now, almost everybody sees man as the end result of a long, long selective process, and this is a world whose history you can trace way, way, way back in a very coherent form. And so that one thinks quite differently about the question of where are we going and who are we. Well, this is going to continue, I'm sure. But I don't think it's a new problem.

M: Then the dividing line between science and the humanities is not so strong as some people imagine?

H: I would assert over and over again that science is a humanity, and that it is one of the intellectual disciplines that contributes to our understanding of man. You know, if you want to see this, one of the most interesting things is to look, for instance, at people's attitudes toward fossils in, say, the late eighteenth century. Fossils have been known for all eternity. This was simply not assimilated into the intellectual spirit. I mean, if two guys wanted to use them as proof of the great flood, there are a lot of sort of wacky notions.

I don't know if this has been any help to you. It has been a very rambling discourse.

M: Well, maybe on this philosophical note, we should call it to an end. Thank you for your time.

H: I doubt that this corny philosophy will contribute much to your history.

M: Well, who knows! Thank you very much.

H: Okay.

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