Matloff: This is an oral history interview with Dr. Herbert F. York, held in his home in La Jolla, CA, on December 31, 1984, at 10:00 a.m. The interview is being recorded on tape, and a copy of the transcript will be sent to Dr. York for his review. Representing the OSD Historical Office is Dr. Maurice Matloff.

Dr. York, we will focus in this interview particularly on your role as Director of Defense Research and Engineering from December 30, 1958 to April 30, 1961. First, by way of background, I would like to ask you to summarize your previous service and assignments with scientific programs sponsored by the Department of Defense.

York: I had a great deal of contact with the Department of Defense prior to that time, all growing out of my involvement in the Manhattan Project during the war and then my continuing relationship with nuclear weapons, especially during the period 1952-1958, when I was the director of what is now called the Lawrence Livermore National Laboratory. As a result of my being the director, I was drawn in to a lot of advisory activities during the 50's, beginning in the period 1952-1953. The most important of these was as a member of what is sometimes called the von Neumann Committee, or the Teapot Committee, established to advise the Secretary of Defense on the strategic missile program. This committee was originally set up at the instigation of Trevor Gardner, who was in the Air Force. I remained a member of that committee right up until the time I became the Chief Scientist of ARPA in 1958. In addition to that, in fact just a little bit earlier, I was a member of the nuclear

panel of the Air Force Science Advisory Board, also chaired by John von Neumann. I was also a member of the Army Science Advisory Panel during about the same years, but not quite such a long time, that is, in the middle 50's. Then in 1957, right after Sputnik, when the American government was trying to decide, first, what the problem was and also searching for answers to it, the problem represented by the fact that the Soviets put up the first Sputnik, I was invited to join the President's Science Advisory Committee. As a result of that, I continued the same kind of involvement with respect to reviewing many of the key Defense programs, including those in the intelligence area, as well as the obvious strategic ones, and so on.

Matloff: Had you had any contacts with the Secretaries of Defense during any of these periods?

York: Not until I became a member of the President's Science Advisory Committee, although at the next level down I had some contact, because Don Quarles, who was, first, Assistant Secretary of Defense, then Secretary of the Air Force, and then Deputy Secretary of Defense, had previously been the President of the Sandia Corporation at the time we set up Livermore. We were on the same level at that time, so I had met him and certain Secretaries of the services, but I had not met the Secretary of Defense. I met Secretary McElroy within weeks of joining that Committee, and had a number of important and useful conversations with him.

Matloff: You had no dealings with Secretary of Defense Wilson?

York: No, I never met the man.

Matloff: I was going to ask you about his attitude toward scientific research. I remember that famous statement that's always ascribed to him, "basic research is when you don't know what you're doing."

York: There were some others--for example, Eisenhower's first Secretary of the Air Force, who gave a talk in which he made evident that he really knew nothing whatsoever about research, not just basic, but any other kind.

Matloff: Let's come now to the background of the appointment as Director of DR&E. What were the circumstances, who recommended you?

York: The essential circumstance was that I was the Chief scientist of ARPA at that time, which meant that I was already intimately involved with a wide range of high technology defense programs, especially those involving large rockets, long-range missiles, and matters of nuclear energy in space. That was precisely the area in which there was felt to be the greatest need for pulling things together, for better understanding at the top, and so on. That was the key to the next step of becoming DDR&E. But what I recall is that they offered the job to at least one other person and they felt out yet another. There may have been still more, but my recollection of what I knew is that they offered the job to Crawford Greenwald, President of Dupont. Then someone asked the Director of the Applied Physics Laboratory at Johns Hopkins (Gibson?) if he would be interested. I don't think they offered him the job. I suppose that everybody who shows up at the Pentagon for the first time is naive

about some things. When Secretary McElroy invited me to take this job, he said, "The President and I would like you to do this." I said, "Fine, I will go talk to the President about it." I was naive, I was not aware of the fact that most assistant secretaries of defense don't talk to the President. But he immediately called the White House, and made a date for me to see Eisenhower the next day, with Don Quarles present. I mention this because it has something to do with why I was selected. When I talked with the President, he had the good grace to tell me, and I think he was being honest: "You were my first choice, but they said they wanted somebody older and more distinguished looking. I wish you could have been a Republican." He was aware of the fact that I was registered as a Democrat. I said, "Mr. President, my wife and my father are both Republicans." He slapped his thigh, and said, "That's a new political concept, absolution by association." The point is that the President himself knew me because of my few months on PSAC, during which I essentially worked full time. PSAC is nominally a part-time outfit, but I happened to work full-time.

Matloff: So you had already met the President before that.

York: I had met the President in connection with my PSAC activities. Jim Killian was the President's science adviser and he and I had a very positive and fruitful relationship. Because high technology was the question of the day--what does Sputnik mean and what should we do about it--the thing that people talked about at the highest levels was precisely what I was working on. I was never present when the President

talked with McElroy or with Quarles about those issues, but I can well imagine that they were talking about precisely the things in which I was interested. With respect to moving on to the front office of DDR&E, the most important thing was, first, that I was Chief Scientist of ARPA and, second, that I had this White House connection. During the time I was Chief Scientist of ARPA, I didn't have a White House appointment but nevertheless I kept very close contact with Jim Killian and often visited the President during that time, under his auspices, rather than Secretary of Defense auspices.

Matloff: Did you receive any instructions, written or oral, from McElroy, Quarles, or possibly the President, when you took over?

York: Very little. They were still working out the boilerplate, the description of the job. We had some conversations about that, but I think in the main we just knew what the responsibility was and what ought to be done. There was one minor caveat, as far as I can recall, and that is that Quarles said, "Let me handle the nuclear airplane." It was a terribly controversial thing, and I regarded that as basically a friendly act, although maybe Don didn't want anybody else mucking around in it as well. It would clearly have come under my purview, given the function of the office.

Matloff: What contributed to the creation of this office? There was the Defense Reorganization Act of 1958. Had you been consulted on the creation of it, on the desirability or the need for this new office?

York: Yes and no. Not by the Secretary of Defense in any direct way. On the other hand, during that very brief period when I worked in the

White House, December 1957 through February 1958, I was involved in the whole matter of the organization of the space program. That automatically touched on other high technology issues. I think that I had probably made it clear that I didn't think the existing organization, or the existing people, were adequate. But I was not in any sense an architect of the defense reorganization plan. I had more to do with the creation of ARPA, even before I went there, than with the creation of the ODDR&E.

Matloff: How about your initial conception of your role as the director? What problems did you face when you took over? What was the state of Defense research and engineering, as you saw it, and what had to be done?

York: My concept was fairly simple: that I was responsible as a staff officer of the Secretary of Defense for every research and engineering program in the Department of Defense, no matter what component it was being conducted in. That included ARPA, the NSA, absolutely everything. And that in the role as staff adviser I actually had, in addition to the strict role of advice only, the authority, with respect to those research and engineering programs, to approve, disapprove, or modify. It's in the boilerplate. Then, in addition to that, I had the function of advising only, with no authority, the procurement of high technology equipment (airplanes, rockets, missiles, etc.). In other words, I had staff authority, advisory authority with respect to high technology, both procurement and research and development, but authority with respect only to research and development--all of it. And I took it perfectly seriously and regarded it as quite natural. I was, both for my own reasons as well

as external reasons, most interested in rockets, missiles, space, and nuclear questions. That's what I had been involved with; nuclear for my entire professional career, and missiles and space ever since I joined the von Neumann Committee six years before. In addition to my own interest, that was essentially the public interest. That was what the American body politic wanted somebody to do something about. There was a perception in the public at large that there was a mess that needed to be cleaned up. That was not my perception, and there were some problems I had with testimony in the Congress and so on, because not only would I not admit that there was no mess to be cleaned up, I didn't believe that there was one. I wasn't merely supporting my bosses. It was my own view. There were some loose ends that needed to be straightened out. But the point is that there was a widely held view among the public generally, the press, and the Congress that the reason for some of our problems was a failure of organization, and so on. It was my view that while there were some organizational problems that contributed to our not being quite up to where we could have been, that was not the main point--that, in fact, we had been doing pretty well, and that our predecessors were not a bunch of bums. That's so commonly the view. A new group who comes into the Pentagon or elsewhere so commonly has the view that those who came before them were a bunch of bums. I did not feel that way. I did feel that some of the people were less qualified than others, and that especially in the research area things had gone slightly sour, but it wasn't so much the organization

as it was other questions. The Assistant Secretary of Defense for Research and Development, Paul Foote, was really over the hill when I moved in. There were several others--the head of the Office of Guided Missiles really wasn't quite up to his job either. But the research people in the services, both uniformed and otherwise, were quite good, and the Deputy Secretary of Defense was a very broadly competent technologist, who was looked on by many people as being stuffy and unimaginative. Maybe in some ways he was, but there was no question of Don Quarles being a competent man who, by and large, did the right thing.

Matloff: About selecting and organizing your staff--obviously you inherited people. How much leeway did you have?

York: More than is normally the case. I really had a sweetheart arrangement, because of Sputnik, and because people really wanted to do something about it. So I had what I didn't fully appreciate at the time, but as I see, in retrospect, a much easier time with that sort of thing than people normally do have. What I did--partly I was being clever and partly I just lucked into the right modus operandi--was to expand the office, and create an entirely new organization, which meant that there were a lot of empty jobs. I kept the old structure, which was organized in terms of technologies, that is, an office of atomic and chemical warfare, an office for guided missiles, an office for aircraft, etc. I kept all of those and kept those people in. They fitted, I must admit, the description of what people usually have of a bureaucrat when they mean it negatively. I don't use bureaucrat universally as a negative word. I kept the same

job titles they always had--they were office directors. I was creating what amounted to a matrix organization, with which I was familiar, but I did it not because I thought matrix organization was so great, but because of the opportunity it gave me to create the other half of the matrix. I kept the existing office as one part of the matrix, organized in terms of technology, and created an entirely new office, organized on the basis of varieties of warfare. We had a strategic office, a defense office, a naval office, and so on. I then recruited people into those positions and did that again in a way that wouldn't be permitted today. In many cases I went to the heads of the industries that the Defense Department was dealing with and said, "We need some good people; help us find some." I talked with the presidents of many of the major air space companies, and said, "Nominate somebody." That's where I got most of these people who were the assistant directors. I talked with industrial leaders--for example, presidents of Hughes, Lockheed, Bell Laboratories).

Matloff: How about military personnel? Could you reach in and select people?

York: Yes, but there I let the system make the judgments. I did have aides who were general officers, one from the Navy and one from the Air Force, but not from the Army because by that time the Director of ARPA was General Betts. Of the three, General Betts was the one that I had known the longest, and I had a good working relationship with him--mainly in connection with his role as Director of ARPA. I did not make important

use of either the Air Force general or the Navy admiral. There really was no need. I probably didn't particularly know how to, but beyond that, there was no need, because I had good relationships with the service assistant secretaries, and with the uniformed R&D people in the services.

Matloff: The changes from the previous set up under the assistant secretary of defense for R&E broadened the activity, I take it?

York: Yes. The other change was that I assumed a certain amount of authority, whereas the previous man had simply had an advisory role.

Matloff: How large did the staff get to be?

York: I don't recall exactly because it did not grow numerically very much. Basically it was a couple of hundred when I came in, and in adding assistant directors and some immediate assistants for them, it only meant a 10 or 20 percent expansion, as I recall. But all the expansion was at the top, so that's how we beat the system with respect to rules.

Matloff: How about your working relationships with various elements in OSD and the services and the like? Starting with the top, with the secretaries--McElroy, Gates, and McNamara--how close were you with them and how often did you see them?

York: My relationships with all of them were very close. Typically, I either saw them or talked with them on the phone an average of better than once a day. I thought at the time one measure of the authority or influence of a certain office is in fact how often one talks to whom. I did see the Secretary essentially daily or more often, and played

a close staff role with the Secretary--with McElroy, and then with Tom Gates, and with McNamara as well. I was formally a member of the Armed Forces Policy Council, because the Defense Reorganization Act required it. So that was one sort of special access, and that is the one special point I want to make about McNamara. He simply decided that he didn't need the Armed Forces Policy Council. So the first day I went in there at the time it would have met and sat up at the table with him and the service secretaries. There was no place for me, so I got to sit with the assistant secretaries. So, in a sense, McNamara took the first in a long series of steps of making the ODDR&E a little bit less special. Even though in recent times they have changed the title to Under Secretary, I think that during those first years under McElroy and Gates the position had as high an authority and as wide a reach as it's ever had. From then on it was gradually downhill, with another step uphill, when Harold Brown became Secretary and made Bill Perry Under Secretary. But in addition to those formal things, like being a member of the secretariat, and being a member of the Armed Forces Policy Council, they took me along as a principal adviser in many of the meetings they had with the President if they involved high technology. During those times when we were making up the final budget for each year, along about December (it would go into force about seven months later), there would be many meetings with the President, in which we would hammer out the final disputed questions. We went down to the President's retreat in Georgia and I was often one of only a half dozen people or less who

went to these final meetings to arrange the final program. Within the Office of the Secretary of Defense I sat in on all of the decisions with respect to what should be done on budget decisions as well as other kinds of decisions. So my relationships were very good, positive, extensive, and deep. Everything was just great.

Matloff: How about the attitudes of the various secretaries toward defense research policy? Were there any differences among them?

York: No, it was really a time after Sputnik when everybody felt that something had been wrong, something left out, and they were eager to have us scientists involved. You didn't have to elbow your way in at all. The same thing was true in the White House. I had superb relations over there.

Matloff: Any differences between you and any of the Secretaries of Defense in the course of these years?

York: No, almost none.

Matloff: Sounds like an ideal situation.

York: Yes, it was just great.

Matloff: How about the deputy secretaries?

York: The first one was Quarles, then Gates, and then Jim Douglas. I had very fine relations with all of them. My relations with Quarles went back quite far; Gates and Douglas were new people to me. I liked them. My impression was that they liked me. I had great confidence in them to do the right thing, and apparently they had confidence in me. It was superb, really.

Matloff: Did you deal with some assistant secretaries more than others?

York: Within the Office of the Secretary of Defense, yes--but way above all the others, the Comptrollers. There I did have problems. They were trying to save money that I thought ought not to be saved.

Matloff: You had McNeil, Lincoln, and Hitch.

York: I had more trouble with Lincoln than with any of the others. I had great personal respect for McNeil. I was aware of how long he had been there, and in my first conversation with him I came to realize how much he knew. On the other hand, Lincoln came in not knowing so much and threw his weight around anyway. So I did have arguments with Lincoln. With Hitch, I didn't stay very long; I had perfectly good relations with him but didn't see a lot of him. The others I saw only very sporadically. There was an assistant for intelligence, whom I saw occasionally, a Marine general. But his authority was fairly restricted. He only had much to do with NSA and, I think, they bypassed him a lot in that regard.

Matloff: How about the ISA people, Irwin and Nitze?

York: There I had more, now that you remind me, because there were some overseas programs in which we had a kind of a share. I didn't think they worked very well, but I also didn't think they were terribly important. That was one of the cases where there were minor problems that had to do with turf--for example, over some of the NATO programs and mutual development programs. But I soon came to think that NATO was such a complicated rat's nest anyway, that a few more complications

in Washington hardly added to it. I knew the legal counsel. In fact, he had the closest office to me. But I didn't work much with them.

Matloff: How about the Chairman of the Military Liaison Committee, who was also the Assistant to the Secretary of Defense for Atomic Energy, Herbert Loper?

York: Some, not a lot. When I was director of Livermore, before I ever went to the Pentagon, I had met "Doc" Loper, as we called him.

Matloff: How about relations with ARPA and WSEG, once you took over as DDR&E? What changes did you and/or the Secretary of Defense make in their administrative relationships?

York: The ARPA one is more complicated. Let me take the WSEG one first, because it's so simple. I used WSEG, but not a great deal, and there were some problems within WSEG and some problems in relationships between IDA and the Joint Chiefs. I did not have a lot to do with WSEG. Occasionally I used their reports as input on something. I don't recall giving them any tasks. I might have, but they certainly were not the top level things I was concerned about. The relationship with ARPA is a very much more complicated issue. The start of that complication was the way ARPA was set up in the first place. The Director was to be Roy Johnson, and then I became the Chief Scientist. There was even some small controversy at the very beginning, because I don't think Killian was too keen on Johnson. He didn't think that Johnson's background was right. Johnson had been the chief executive at the white goods plant of General Electric in Ohio, making refrigerators and so on. There

was, I believe, some concern on the part of Killian. He didn't make a big thing of it and talk with me about it, but I perceived it anyway. In a sense, the combination of Johnson as Director and me as Chief Scientist was a sort of a deal, in which the White House would agree with Johnson as the Director provided I, in whom they had more confidence, would be in there as Chief Scientist. So I think that on one side of the house it was looked at as a kind of a duumvirate of Johnson and me, but he didn't look at it that way. He had a Deputy, Admiral John Clarke, me as Chief Scientist, and then there was this extra peculiarity that I was actually employed without compensation by the Defense Department and paid by IDA as the Director of its Advanced Research Projects Division.

Matloff: This was before this post?

York: Yes, starting March of 1958 and ending when I took this up. So there I was in ARPA as either the number two or three person, depending on how you looked at it. Certainly, de facto, I was number two, but, de jure, probably number three. I was the one who carried all the heavy testimony on it when it had any substance, even in meetings with the Secretary of Defense or the White House. They relied on me for substance of program. I don't think Roy Johnson was ever entirely happy with that, although he always treated me reasonably. But at any rate, when I became DDR&E, essentially I was being jumped over my boss. That was the problem. Then, as these things always are, if you read the boilerplate describing the jobs of the Director of ARPA and the job of DDR&E, there

were contradictions. His boilerplate said that he reported to the Secretary of Defense and was responsible for the programs of the agency. My boilerplate said that I reported to the Secretary of Defense and was responsible for all research and development of the Department of Defense. I never for a moment doubted that that included ARPA. Roy Johnson was not entirely happy with that, but basically accepted it as long as I acted in a staff capacity, that is, reviewed everything he did. But where it really became a problem was when I essentially confirmed a decision that was already building up in my mind--that ARPA was not needed for running the space programs, that the organization would be neater and would better serve the purpose if indeed the Air Force was given, without ARPA in any involvement, all responsibility for launches, and if whatever service had a mission that required a payload could develop that payload. So essentially, I took ARPA out of space. In addition to that, ARPA also, while I was there, had started moving into the business of developing the biggest of all boosters. It was obvious to me that that belonged in NASA. So essentially, during the first year I was DDR&E, I took ARPA out of space by giving the military side of it mainly to the Air Force, but part of it to the Navy and Army, and by taking the things which were not military, like the big booster that eventually bacame the Saturn, the Apollo, and so on, and transferring those to NASA. Roy Johnson didn't really agree with those recommendations. So I did have some problems with ARPA, but essentially, to the extent that I had disagreements, I guess I won them all.

Matloff: How about your relations with the military services?

York: That's still in ARPA. I guess we went right from Roy Johnson to Sy Betts. I had superb relations with Sy Betts.

Matloff: How much contact did you have with the military services, the heads of those R&D programs and also with the service secretaries and the chiefs?

York: First of all, I worked very closely with the civilian assistant secretaries. The one in the Army was called the Director of Research, and was the most difficult, Dick Morse. He was a fellow with a lot of sharp corners on him anyway, and he was in a difficult position. My relations with the Army were by far the worst of those with any of the services. The only service with which I had bad relations was the Army, and even there it was essentially with the Secretary of the Army and the missile side of the Army, General Medaris and the Army Ballistic Missile Agency. The fundamental reason that I had bad relations with them was that my version of how things ought to be involved essentially taking them out of their most favorite of all programs, that is to say, out of long-range missiles altogether, out of space boosters altogether, and even to the point of transferring von Braun to NASA. Those were all ideas of mine, and the Army fought every one of them. I guess naturally so. That aside, my relations with both the Navy and the Air Force were excellent, but different. The assistant secretary of the Navy for R&D was Jim Wakeland. There were a couple of Air Force assistant secretaries for R&D, Dick Horner when I first went in, and then Perkins.

I had good working relationships with all of them, and I liked them all. In the case of Wakeland, his appointment is interesting. He was appointed just after I was. I remember Tom Gates (he was Secretary of the Navy at the time), with a certain amount of challenge in his voice, said, "Tell me why the assistant secretary for R&D should be a scientist or an engineer?" I said, "Mr. Secretary, I don't think he does have to be a scientist or an engineer; however, there are five members of the Navy secretariat, and one of them ought to be technical. It could be the under secretary, the assistant secretary for financial management, or the secretary himself, any of them; but there ought to be one, and the most plausible position is the assistant secretary of R&D." Incidentally, in an odd circumstance, Bob McNamara, five years after I was out of the Pentagon, asked me the very same question and I had the opportunity to give him the very same answer. I thought somebody in the Defense secretariat ought to be and it was the most logical place. We met quite frequently, we had good personal relationships. I don't mean we had parties and drinking, but good personal relationships. We had lunch together monthly or weekly, which we rotated around the Pentagon. I essentially chaired a group consisting of me and the three of them. Each of the services had a military chief of R&D and people rotated through there fairly rapidly. The Marines had one also, and I had a good working relationship with him. It was General Hochmuth, the highest ranking officer killed in Vietnam.

Matloff: How about your relations with the Chairman and the Joint Chiefs of Staff? Did you have many dealings with them? That would have been Twining and Lemnitzer on the Chairman level.

York: Yes, with both of them, and with the individual Chiefs as well--Arleigh Burke, Tommy White, and Lemnitzer. The Navy and the Air Force had more high technology, so I had more to do with them for that reason. My relations with the Army unhappily were mainly characterized by the negative fact that, in order to get things straightened out and rationalize the way we were running our programs, in my view--which prevailed--the Army had to get out of long-range missiles in space.

Matloff: You felt that you had won most of the battles with the Army, all of them actually?

York: Yes, probably all of them. I had three with the Air Force, really. One was over the nuclear airplane, one was over the Dinosaur, and another over the question of Man in Space. In the case of Man in Space, I felt that there definitely ought to be a Man in Space program but we didn't need two. NASA was headed that way and that was good enough for all of us. In the Air Force it would be diversionary. In the case of Dinosaur I simply regarded it as grossly premature. The same thing was true for the nuclear airplane. If it was going to have a time, it wasn't yet, and maybe it never would come. So we had those battles, but, generally speaking, the Air Force and I got along very well. To bring up a slightly delicate subject, the NRO was created in those days. I know there still isn't any such thing as an NRO, but in

those days we weren't quite so coy. Whether there is an NRO today or not, there was then, and that was the Under Secretary of the Air Force, Malcolm MacIntyre. But I was essentially the Secretary of Defense's contact with that, so that in a practical sense, and it's been the same way ever since, DDR&E and the Under Secretary of the Air Force somehow ran the reconnaissance.

Matloff: How about with Congress? Did you find the congressional committees sympathetic toward Defense R&D, or R&E, whichever way you want to refer to it?

York: Yes, in those days, during the end of the Eisenhower administration, the Democrats were in the ascendancy and running a campaign, and wanted us to do more than we wanted to do. It was one of those times when the Congress wanted to give us more money than the administration wanted. I loyally supported the President, because he was the President and also because basically I thought that he was right.

Matloff: Did you encounter any resistance in funding certain programs, for example, in basic versus applied research?

York: Not much. They were fussy about something they called unnecessary duplication, but it was mainly just words.

Matloff: When you appeared on the Hill, did the Secs/Def give you complete leeway in testifying?

York: Yes. I was loyal to them, and their attitude towards me reflected that. I wouldn't have crossed them up anyway.

Matloff: How about relations with NASA? What was the division of responsibilities between your office and NASA?

York: The line was military programs on our side and civilian programs on their side, and the only issue at all was defining where the line was.

Matloff: How about cooperation?

York: We didn't have any difficulty. Glennan was the head of NASA that whole time and we had an excellent personal working relationship.

Matloff: How about perceptions of the threat when you were serving in this role? What was the dominant attitude toward the Soviet threat that you found?

York: That was a complicated story. When I moved in, in early 1958, we within the administration thought that something like what was later called the missile gap was in fact developing. We did fear that there would be a missile gap. On the other hand, as time passed by, doubts began to grow in some of us as to whether there was, but confirming those doubts was quite another thing. All we had going was the U-2, and the amount of area it could cover was very small compared to the territory of the Soviet Union. So there we were in the spring of 1960 with a situation in which there was a lot of noise out there in the Congress and elsewhere about this terrible missile gap, but with us in the administration, meaning the President, me, and others, really doubting that there was a missile gap. The reason was that however hard we looked, we couldn't find any deployed missiles. However, we were looking over such a small territory. That in fact is why Gary Powers was given this assignment to fly a lot further than before, and in fact Dick Bissell and I were the two that briefed the President with regard to why the Powers flight should take the course it did.

Matloff: Before or after the flight?

York: Before the flight. It was Bissell and I who somehow got the duty, me for the Pentagon and he for the CIA, of persuading the President that the Powers flight needed to go where it went--that route up Sverdlovsk over to Bodö in Norway. The reason was that we couldn't find any missiles, but we had Stuart Symington and a lot of other people telling us there was a big missile gap coming. In fact, I myself had a year before been among those who thought that there was a big missile gap. Our search for missiles and our desire to settle this question became stronger. So there were a couple of splits with regard to the threat. One was, is or isn't there a missile gap? There were some people in military intelligence and some people in other places in the Pentagon who agreed that there was a missile gap. There was the President, who was more and more convinced there wasn't; there was me, Killian, and then Kistiakowsky, and others also convinced that there really was no missile gap. It wasn't just that we couldn't find them; they weren't there. However, at one point--and here I'm hoping to get a copy of the letter soon (it used to be Top Secret, but a student showed it to me)--I gave a briefing (and in effect I want to ask you to help me trace this a little better) to the Joint Chiefs and later to the National Security Council and the President himself in a special session, in which I presented the very first of what you would call "window of vulnerability" calculations. This was in the spring of '60. (I happened to run into an Air Force letter referring to this, which somebody else obtained through the Freedom of Information Act.) So I briefed the Chiefs. (It was simple enough;

one man could do it without a computer). I said, "Let us assume the CIA is just right--the numbers, accuracy, and reliability are right--and then I'll program that force out, I'll attack the American force as we've got it planned with that force, and ask the question, 'How much of our strategic force would be left?'" I concluded that somewhere in the middle of 1961, given the CIA projection, which was much less than the missile gap people were claiming but was still substantial, we would have nothing left. That is to say, they could put two on every SAC base and every Atlas by the summer of 1961. That would make up for inaccuracy and reliability. That would last for a period of about six months and then the Polaris submarines would be coming in and they couldn't get those so we would have them left. I briefed this to the Chiefs, and the Air Force loved it. I didn't know it at the time, but now I've seen this letter signed by Tommy White saying that Dr. York's got to give that to the Security Council. I made a series of recommendations, all of which were bought. One was that we should accelerate the BMEWS program and the Polaris program and another was that we should prepare for an airborne alert. I said that would solve the problem. The BMEWS will give us the opportunity to launch the missiles on warning if we need to. The acceleration of the Polaris will fill in this gap because they can't get those (by filling in I meant hit them with ten, which in those days seemed like a lot, not like today with 12 thousand). The airborne alert would take care of the fact that some bombers would survive. Actually they bought all of

those. I don't have any paper of my own that traces that. I made a chart myself, or rather made the calculations myself and got a staff member to make the chart, which I took with me to demonstrate this. It's the first calculation of a "window of vulnerability."

Matloff: I take it that the impact of Sputnik on the program was quite strong.

York: Yes, although it's been misjudged. What happened as a result of Sputnik was lots of reorganization, setting up all these offices we've been talking about, congressional reaction, and proposals from all directions about what to do. What finally happened, however, in terms of program, was not a lot. No new missiles were introduced. Technically speaking, Minuteman was approved after Sputnik and maybe would have taken longer, but we were headed straight for Minuteman just before Sputnik and the proposals were being generated in the Air Force Missile Office before Sputnik. Then we continued with the Atlas and the two versions of Titan. Organizationally, the military space program was modified a lot. It went to ARPA and then back to the Air Force, but the guts of the Air Force program (in those days we were free to say the reconnaissance and surveillance programs) were only changed modestly. The Discoverer program was modified. But the big changes were over in NASA. One of them involved building bigger rocket engines. The Air Force did start those but the responsibility was later transferred to NASA. So the substance of the Defense program was not greatly changed. The civil program was totally created as a result of Sputnik.

Matloff: How about the demise of the missile gap?

York: We in the Eisenhower administration were already convinced it wasn't there, or nearly so. But we simply didn't have the solid facts, because the very first recovery of a capsule from space was in August 1960.

Matloff: Were you getting any good intelligence before the attempted Powers flight, because there were other overflights?

York: Yes, there were a lot of U-2 flights before that. We were getting pretty well informed about what was going on at Tyura Tam and Kapustin Yar by that and other means. The question was deployment, and the intelligence was nothing like it is today, but we did have a lot of peeks under the iron curtain. We got a lot of things right, and a few wrong. We ourselves were becoming convinced that there wasn't any missile gap but couldn't prove it. It was not until the first year of the Kennedy administration that the facts became solid. Eisenhower's intuition was that there wasn't a missile gap, and that was no accident. He was good at that sort of thing.

Matloff: His son told us that he was quite angry when Gary Powers turned up alive.

York: I don't remember that I was angry, but I was shocked. In fact, I was feeling badly about the fact that he was dead. So I was utterly surprised, I won't say relieved, to discover that he was alive. He was supposed to be dead. I think that the first person who told me he was shot down was someone who didn't know the full facts.

Matloff: Was there much concern about the Soviet progress in other areas aside from those you have mentioned?

York: The other big argument was over nuclear weapons. There was a moratorium on testing in place, and I soon became, along with George Kistiakowsky, one of the main supporters of President Eisenhower's determination to stop nuclear testing. There was a lot of doubt about that in the Pentagon, especially in the Air Force, and in the Atomic Energy Commission. The Air Force was always discovering intelligence that indicated the Soviets were testing and I would go over it with as fine tooth a comb as I could, in my own head, because I was sort of the nuclear expert. With their data and the CIA data I always concluded the opposite--that there was no evidence the Soviets were cheating. The Chairman of the Atomic Energy Commission was John McCone, and he was convinced they were cheating. The Chief of Air Force Intelligence was convinced they were cheating, and one of his assistants was that same George Keegan that still gives us trouble today. They were all convinced that the Russians were cheating, and on the basis of no evidence, in my view. I insisted on this to the Secretary of Defense. I think he had some doubts about it, but I think basically he accepted my views on that. The President, of course, was ahead of me; he knew they weren't either. And George Kistiakowsky. So I was at odds with some people in the Pentagon over that particular issue. In one very small meeting involving McCone, Gates, myself, and a couple of others, when I stated that there was no evidence, McCone said that that was tantamount to treason. He accused me of treason for saying that I believed that the Russians were not cheating.

Matloff: Let me ask you about the interaction of R&E with strategic planning. To read from the boilerplate, the DoD Directive of Feb. 10, 1959, states that the director's responsibilities shall include consulting with the Joint Chiefs "on the interaction of research and development with strategy." How much consulting did you do with JCS in this connection?

York: I certainly did consult, but the context tended to be programmatic approval or disapproval. The closest I came to fulfilling that in the purely strategic sense is the episode that I just told you, where I briefed the Chiefs on my version of "the window of vulnerability" in the summer of '60. And I did brief the President. But generally I would meet with the Chiefs in connection with a programmatic budget decision--for example, the question of should we go ahead with Nike Zeus? When I was first in there, I thought it was right to do, but then, when I came to grips with it, I decided that it was far too premature to deploy. I talked with the Chiefs about that. We met and talked about a lot of things. I don't remember how often--it was nothing like the everyday affair with the Secretary of Defense--I don't even think it was monthly. On the other hand, I saw the individual Chiefs more often.

Matloff: Do you remember the great strategic ferment in the late '50s and early '60s, in the think tanks, on questions of deterrence, limited war, civil defense? Did you and your staff try to keep up with this debate, did you get drawn in on the debates?

York: Yes, but you see I had been in on it before I had ever gotten into that position. So for me it was a natural thing to do. But even

there most of the time the issues involved the budget. Other questions come up in the budgetary context, and the theoretical arguments, if any, came up during that time. If you were supporting some kind of a new missile, arguments about its role and deterrence would be used as part of the budgetary arguing debate process. But the fact is that in most instances (civil defense is a special case), especially in strategic offense, the issue was "which one", not "whether". There were so many different ways of doing it that the theoretical arguments about deterrence and so on didn't enter because the argument was a smaller one; for example, which 5, out of 8 possibilities. With regard to defense, it wasn't the theoretical side of defense that was dominant; it was the fact that the technology was inadequate. Regarding civil defense--there it was the doubts about its utility that were dominant. I recall one curious argument, involving Jack Irwin from ISA, myself, and Tom Gates, the Secretary of Defense. Somehow Jack Irwin got the bee in his bonnet that we really ought to be doing more about civil defense. He had been given some kind of technical briefing and wanted to talk with Tom Gates, and either Gates or Irwin suggested I come along. Jack Irwin was trying to persuade Tom Gates that we ought to do more and started talking about gamma ray absorption, dirt, shelter covers, and things like that. Tom interrupted him and said, "You know, it's not gamma ray absorption that's going to make any difference; it's chaos. And it isn't going to work. I don't want to hear about it." But there were other people who bugged us about that--Nelson Rockefeller for one.

Incidentally, he had political reasons--I don't mean partisan politics. He said his view was that the American people were too afraid of nuclear war (nothing ever changes), that because of this, they were liable to do the wrong thing and not react correctly, and that we needed civil defense in order to stiffen the backbone of the American people.

Matloff: When you took over in DR&E, this was the heyday still of the Eisenhower/Dulles New Look, massive retaliation period.

York: Yes, although we came along later.

Matloff: What was your attitude in general toward nuclear weapons, strategic and tactical, their buildup, use and control?

York: I generally went along with the inner nuclear consensus, that nuclear weapons were extremely important; that the threat of retaliation was what was maintaining peace. Remember, those doctrines didn't grow out of nothing. We'd had that miserable Korean War and the idea was that we weren't going to let them choose the time or place next time. We were going to use weapons that would be to our advantage, instead of theirs, the means that we were best at, and so on. So I was generally sympathetic to the basic line of the nuclear in-group. However, very early in my Washington experience, when I really was just getting started, I became very much interested in and finally a strong supporter of Eisenhower's views that the nuclear arms race was intrinsically a bad thing and would ultimately cause us a great deal of trouble--which is a kind of understatement. So I became an avid supporter of Eisenhower's

attempts to do something about it that I mentioned earlier, in connection with the test moratorium. Kistiakowsky and I became his main supporters. It didn't happen immediately, but after six months, both in the White House and then as Chief Scientist of ARPA, that's the conclusion I reached. But it wasn't that I felt that arms control or a test ban was the answer, but I did feel that arms control, including such things as the test ban, were proper alternatives in the sense of options--another way to go, if you could succeed, that might be better than the way we were going.

Matloff: Did you go along with the thesis of Brodie in those days, that strategy had hit a dead end in the nuclear age, one of his lines?

York: I knew Brodie, but not very well.

Matloff: Did you know Wohlstetter and Kahn?

York: Kahn, but not Wohlstetter. One of what I've come to regard as false lines in the history of those days, especially in the period just after this, the Kennedy period, is the overblown notion of the role of strategists like Wohlstetter and others. The fact is that in the Office of the Secretary of Defense, and even, I think, in the higher offices in the Air Force, they were barely aware of them or what they were saying. It was a more amateurish group of technical people, who were not professionals at operations analysis, arriving at almost the same conclusions, whose conclusions mattered. It was the technical people at Rand (E.H. Plesset and Richard Latter, more than Herman Kahn) who were influencing what happened, rather than the social scientists--and the same thing in the Pentagon itself.

Matloff: When the Kennedy administration took over, people got to talking about flexible response, the successor to the massive retaliation notion. Did you get drawn in on those discussions?

York: Yes. I didn't think that it was very different. I did think of it as being a different way of saying it, but in fact, not very different.

Matloff: This is the Kissinger/Kaufmann/Osgood concept of limited war.

York: But it had been around in different words. I think what was new was that it was being intellectualized in a more formal form. Most of what McNamara did and most of what those defense intellectuals did was really to restate what was already there in a more formal way.

Matloff: Some historians have various questions on whether President Eisenhower adequately promoted the development of long-range nuclear weapons; whether he should have maintained a better balance among the competing elements.

York: What do you mean?

Matloff: That he should have done more about long-range missiles, for one thing; that he was so concerned about the budget that he wanted to keep Defense costs down.

York: I don't agree with that. In fact, when the Kennedy administration came in, after all this talk about the missile gap, they didn't make any net increase at all. They only made minor adjustments. So I think that it was quite adequate. It could conceivably have been more, but it was during his [Eisenhower's] administration that the long-range

missile programs were finally given the highest priority in '53 and '54. He could have thrown a little more money at them before Sputnik, but I don't think that it would have made six months of difference, or three months of difference.

Matloff: So you wouldn't go along with these arguments?

York: No.

Matloff: We talked earlier about service differences, particularly with the Army. Let me ask you about those service advisory boards in science. How would you evaluate their contributions from where you were sitting? Did they have any impact on the program of the OSD office?

York: Yes, but it was an indirect impact. In other words, they advised the services and the services digested that advice, used it in various ways or ignored it, and made their programs based on it. My impression is that the Air Force Science Advisory Board had a substantial influence on what the Air Force did; that NRAC did not, except in certain peripheral areas, maybe in basic research; and that the Army Science Advisory Board didn't have a lot of impact either. But the Air Force SAB did. The Defense Science Board had very little impact in those days.

Matloff: How about your board?

York: No, I didn't use them. They played a role in the reorganization, however, but you see quite a few of the members were ex officio, and Jim Killian was one of the members. It was one of his entrees into defense policy.

Matloff: You spoke about the budget earlier, that you had an important role in the formulation of the R&E budget for defense. How about in the appropriation of the funds going to the service programs, did those come through your office directly to the services?

York: They came through, but only in a huge package. Every year I would write a letter that had just one or two pages to each of the services, authorizing a following long list of the biggest programs. One letter would cover 90 percent of the funds, or something like that.

Matloff: On the allocation of the funds in Army and Defense, how much would be between basic and applied?

York: Basic would be pretty blooming small, by the usual definition of basic. The total that I was managing was running six billion. Within that what you would call basic was a few hundred million, the level of 5 percent at the very most.

Matloff: How much was going to the OSD part, and how much to the services?

York: The great majority went to the services, virtually all of it. Of the several hundred million that were going into basic research, virtually none was OSD other than ARPA, and in ARPA it was a very small part of the total. What academic scientists would define as basic research would be just a few hundred million.

Matloff: How about the Army part of the Defense budget, I gather it was relatively small compared to other things?

York: That was running 15 percent of the budget.

Matloff: Did it increase during your period?

York: Yes.

Matloff: You must have been very persuasive.

York: No, it was the times.

Matloff: We spoke earlier about your relationships with foreign R&D and I think you were playing down the amount of activities or the effectiveness, and possibly I misunderstood, but certainly in the boilerplate, in the directive of February 10, there was a provision to work with the ISA man in developing programs of assistance to friendly countries for military research and development, exchange of information, and the like. What kinds of programs did you get involved in?

York: All of them peripheral, and minor, but nonetheless important; of importance mainly because they helped to give NATO somewhat more substance. We had a number of programs that were NATO programs. That is to say, there was an ASW laboratory in La Spezia, Italy, and some kind of an air laboratory in Belgium. So there were some NATO laboratories and then there were bilaterals. We had an office in Paris that ran those bilateral programs. My memory of my view at the time was that this didn't amount to much. As far as American national defense was concerned, there was almost nothing going on that mattered, we were so far ahead of everybody. There were some exceptions, but they were really fairly rare. The British were doing some interesting work on short takeoff, even zero takeoff aircraft. I think the work that finally led to those fighters they used in the Falklands was going on back then, something

called the Harrier. Then there was a little work relating to cryptology that the British were doing.

Matloff: Nothing with SEATO, no relations with the Southeast Asia pact?

York: In the R&D area, none at all. There were some interesting personal things. There was a Defense Council, a council of heads of defense research. The man with the longest record of activity there, that I recall, was a Norwegian. Even a few years ago (20 years later) he was still the Norwegian member. They had annual meetings, but they just didn't do anything important. SHAPE would have an exercise every year called SHAPEX, and I went to that, but it really wasn't terribly interesting.

Matloff: There was no irritation with the SHAPE member-countries, for not doing more in this field?

York: The bureaucracy of NATO was the biggest shock I had in Washington. I knew about bureaucracy generally, I had been dealing with it, but the pervasiveness of bureaucracy within NATO and the inability to get anything done was the biggest surprise. We also had a trilateral arrangement with the British, French, and Canadians. That's how I first got to know Sollie Zuckerman, whom I still consider a close friend. I see him at his lordship's digs in East Anglia nearly every year.

Matloff: To go to the question of area problems and crises, were you consulted by intelligence agencies in connection with various area problems--for example, Cuba, the Middle East, or Vietnam?

York: No. Not consulted, but with respect to Vietnam there were various kinds of studies and other things going on down there that related

to Southeast Asia, but that war didn't get hot until three years after I left. We were already involved in a minor way; there was an important military advisory group down there. Curiously enough, a number of the studies we made with respect to chemical warfare, especially non-lethal chemical warfare, involved its possible use down there, including some scenarios which now seem pretty bizarre--the idea of the Chinese somehow marching down through Laos and Burma and cutting us off at the pass in some strange way. With regard to Cuba, virtually nothing. There w

As amended: delete for security

some incidents-[redacted]--but with respect to the Bay of Pigs, I was never clued in on that. It might have been good if I had been, because I was the senior holdover in the Defense Department. The result was that the civilians who came in and who did deal with it were all totally inexperienced. I don't mean that I had experience with Cuban military or anything like that, but I did have some experience with making judgments about where you got good advice and where you got bad advice, and who was reliable and who was not. If I had been involved, it's possible that I would have told somebody that you are listening to the wrong people. But they didn't involve me, even though I was the acting Secretary of Defense during the transition. That was probably because it was a CIA operation rather than a Defense operation. It happened just before I left. If there had been an Armed Forces Policy Council meeting on that and I had been involved, I might have said something different. Now, I say that

with lots of doubt, but some feeling, because in connection with the Teheran affair they also didn't consult the R&D people, like Perry, who might have said something sensible about those helicopters. Each time the theory was that the R&D people really were not involved, no matter what their rank might be, that it was an operational question.

Matloff: To get back to the U-2 flight with Gary Powers, after that plane was shot down, was there any hullabaloo about getting a different kind of plane? Were you pulled in on that one?

York: Yes. That's where the SR-71 came from. That you will have a hard time finding in the records, because I usually dealt orally with the Air Force on that issue. I was the Secretary of Defense man on that sort of thing, but I very often dealt with it orally.

Matloff: Where did the pressure come from to do something about that?

York: It was not really being pressured; it was just the thing to do. But satellites were coming along anyhow, so that the question of another airplane was not as vital as it would have been if we didn't have the satellites.

Matloff: Let's talk about something that must be very close to your heart--the impact of DoD R&E on university research. What impact did the policies and programs fostered by your office have on university research, either temporary or long-range?

York: There was some, but not a lot. The materials research program, which was set up on my watch, of establishing a number of laboratories around the country, had the biggest single impact. Otherwise, it was

my view that the Science Foundation was the right place to be sponsoring basic research. The Defense Department should sponsor enough so that it is involved--it has a bureaucratic and progammatic connection there--but I had a definite internal dispute with some of my staff over the issue of whether the Defense Department should be responsible for the health of basic research. My view was that the health of basic research was a very important issue, but that it was wrong to make the Defense Department responsible for everything that is very important. If you follow that line, we would be running the tax collections, because that's even more important. Therefore, the fact that something is important to national security does not mean that the Defense Department should take the responsibility. In fact, the man in charge of my office of research, the one I inherited, not long after left because he didn't like my views on it. He got a good job somewhere else in the Washington bureaucracy. So I did not work to expand the basic research program of the Defense Department, only the applied research program.

Matloff: Should universities, in your view, have more control over DoD research projects?

York: More than what? There are some problems between DoD and universities now, but it is not so much the matter of control. It has to do with secrecy and these issues that have come out of Perle's office in recent years having to do with prior censorship. I gather that's finally been straightened away. That's the only thing that I have regarded in years as being an important issue. There my overall view

is the one I stated, that a healthy research and development community is essential to American national security and life, but it is not the responsibility of the Defense Department, but of NSF, NIH, and so on. The Defense Department should be involved so that there are always people there who know what's going on and have contacts and so on but they should never assume the responsibility. So I think that's handled about right. There are these occasional things going wrong, like this matter of prior censorship of basic research, and control of who's at meetings, which I think is grossly overdone, as is almost everything Perle does. Then some time back there was a flip the other way when the idea was that the Defense Department shouldn't be doing anything that didn't look as though it had a weapon at the far end. I thought that was wrong, too. I think the Department's basic role in basic R&D should be enough so it is involved but not responsible, and non-intrusive.

Matloff: Let me ask you a few general questions about Cold War policies. You believe that containment was a realistic policy, that its assumptions were valid?

York: To the extent that I thought about it, the answer is yes. The things that we were doing to implement it that involved R&D I thought we were doing OK, as far as theory was concerned.

Matloff: The opposite part of the question would be do you think detente is a more realistic policy?

York: Yes, but they are not contradictory. I think the idea of containing Soviet influence is correct, but detente doesn't mean the unleashing of Soviet influence. It's just another way of maintaining relationships with them, so I believe in both detente and containment.

Matloff: To get back to arms control and disarmament, I gathered your office was drawn in--at least you were--on proposals for arms control and disarmament. What were your views at the time and have they changed any since that period?

York: The big arms control and disarmament issue during the time I was DDR&E was the test ban issue. There I soon adopted the President's point of view and was one of his staunchest supporters, against maybe the consensus within the Defense Department that it was a bum idea.

Matloff: This was in the Eisenhower period?

York: Yes. But then just as the Eisenhower administration was coming to a close, my views broadened on that in a rather special way. It had to do with the ABM, the Nike Zeus program, and a growing feeling on my part that it was't just Nike Zeus that was not going to work, defense wasn't going to work, period. It wasn't just this program that wasn't working out, nothing would work out; there wasn't going to be a defense against nuclear weapons. That was all crystallized when Jack McCloy called me up in January '61, during the lame duck period. He had already been appointed by Jack Kennedy as his arms control adviser, and he made a date to see me. I never knew him before. He's a fine guy, with a 50-year history, but I didn't know it at the time. I barely knew

who he was. Things were quiet in the Pentagon during this lame duck period because nothing was being decided. We were waiting for the new group. So I had time to relax for the first time and undertook to think about the question before he came over, instead of putting out tomorrow's fire. I came to the conclusion essentially that there was no technical solution to the dilemma we were in. The dilemma is that steadily national security keeps going down and national power as our capability to cause damage keeps going up. But the damage that can be done to us without our being able to do anything about it is also going up. I have always interpreted that in a particular set of words that some people like and some people don't--"that national security keeps getting less." There isn't any technical solution of that dilemma. That's the main point I made to Jack McCloy. Later I put that into an article that Jerry Wiesner and I published in 1964 in the Scientific American, although I said something like that earlier in connection with my testimony supporting the partial test ban of 1963. I've said the same thing in a great many other places since then.

Matloff: You were drawn in on the limited test ban treaty?

York: Yes, both because I was a former director of Livermore, who was sympathetic to the idea, and I was a member of the general advisory committee on arms control and disarmament at that time.

Matloff: Do you agree with the article written by Kissinger that appeared in The Washington Post just two weeks ago?

York: I was in Ecuador, so I didn't see it.

Matloff: He wrote about the relationship ~~been~~ between contemporary weapons technology and arms control theory in the late 1950s and early '60s compared with today and made the point that in the earlier period the missiles were stationary, and relatively inaccurate warheads were assumed. That contrasts with today, when the launchers can carry ten or more highly accurate warheads and some missiles are becoming more mobile. He argued that equality in numbers of launchers today is less relevant to strategic stability. Does that strike you as being a valid linking between weapons technology and arms control programs?

York: Yes, but I think he's slightly wrong in that in those days it wasn't all that precise a connection either, because actually in 1960 the great bulk of the force was on airplanes, where simple counting relationships are not all that important. Then we went into a period in which most of them were on land. When you have fixed land-based missiles, then various kinds of counting diverse things is very important. But we never had all the force that way. We've always had a lot of it on airplanes, and a lot of it on submarines. That's why the window of vulnerability argument was wrong, and it's why numbers even today are not all that crucial. So I tend to agree with what he says, but the historical contrast you are making is not quite so. I wouldn't have said the '50s and '60s. I would have said the '70s was the special time. The great weight was at the beginning of the '70s, about the time the ABM treaty was being negotiated. When you had single warhead

systems and most of them were fixed and on land, then these various rules of all kinds for counting missiles became especially important. Earlier it had not been all that important, and now we are moving away from that again. So to say what he says is certainly true. It's a question about the exact historical trends, but it certainly is true.

Matloff: Which directions appear most promising for future exploration in this field of arms control? You mentioned a political solution. Do you see anything in weapons technology that might be useful?

York: There are things which contribute to stability and I think that anything that does that in a sense improves the prospects for arms control, because it reduces the tensions that always surround this question. Anything that improves survivability tends to make these numbers less important, and making these numbers less important means you don't get so uptight about them. So things that improve survivability are important, and the best way we know how to improve survivability is through mobility. Making missiles mobile on both sides, or continuing the trend to make them mobile, is probably not a bad thing from the point of view of stability, although it's sometimes seen by people as bad. Things like submarines, cruise missiles, etc., especially cruise missiles that are carried on either submarine or aircraft platforms, in my view do not make things less stable. Just the general idea of improving survivability could still include active defense, in my view, in spite of the fact that I opposed the ABM in the political sense in the 1970s period. I think that we became too dogmatic about the role of active defense. The role

of accuracy is a difficult one to assess. Many of my friends in the arms control community regard higher accuracy as anathema. I don't. It has pluses and minuses. I don't really quite agree with Wohlstetter either, but I think that he is more correct when he talks about the fact that higher and higher accuracy allows you to go to lower and lower yields. It doesn't mean that you will necessarily go to lower yields, but it is basically right. On the other hand, when you're talking about stability, it's a question of the ability of machines to kill machines which counts, and then making them more accurate makes them more lethal with respect to machines. Less so with respect to people. So I think the move towards higher accuracy is a net plus precisely because, and I put it differently from almost anyone else, high accuracy kills machines, and high yield kills people. It's a good thing to be moving away from high yield for that reason, even if it does mean that you make the killing of machines better.

Matloff: As you look back on the national security organization and management in science and technology, on the basis of your experience and subsequent reflection, what is your judgment of the structure and working relations in DoD in this area? Do you see any need for changes in the relations between Sec/Def and the Director, whether he be called under secretary or what, in this field? or in the relations between the White House and Defense? or between OSD and the services?

York: I think that it was better in the past, and in terms of when the

DDR&E or the Under Secretary saw the Secretary every day it was a better world than it is today, when he can hardly see him at all. That's independent of the boilerplate; it hasn't changed at all. It's just a fact that Weinberger hardly saw DeLauer, and whatever his job description may have said, he didn't have much influence. I think that it's better when he does, and that the notion that they had back there in the '58 reorganization act that the third ranking person in OSD should be the technical person is still true. But the question of how you accomplish that is not all that simple, because there are so many possible ways to change the title.

Matloff: Would you characterize the styles, effectiveness, and personalities of the people with whom you worked, particularly the Secs/Def and other officials? Do you want to add anything to what you already said about McElroy, Gates, or McNamara?

York: I liked them all, and I thought they were all equally effective, despite the fact that there are a lot of other people who don't think so. In particular, I thought that Gates' seat-of-the-pants and intuitive approach was every bit as good as McNamara's formal approach in terms of the final result.

Matloff: About the military-industrial complex and the farewell address of President Eisenhower--did you, or do you, share that concern?

York: Yes. And just the way he said it. He said we've got to have a military-industrial complex, but there is potential trouble there. I would add that he also said that we have to have a scientific and technological

elite, but there's also potential trouble there. I've written a couple of brief essays on that, including the introduction to my book, Race to Oblivion, and then more fully later--that Eisenhower had two warnings and he gave them equal status but everybody remembers only one of them. One was on the military industrial complex, and an exactly parallel one on what he called scientific-technological elite. He had in mind, and I know this from talking with him later, von Braun and Teller as people who are just out there selling and selling, and telling you that if you don't do what they're telling you you ought to do, you are going to be doomed. The hard-sell technologists are what he had in mind.

Matloff: Since you mentioned Eisenhower, I take it that he was the President you knew best, is that right?

York: Yes.

Matloff: There's been much discussion in the literature about whether he was an activist or passive president, delegating all the important questions and just sitting in on the final conclusions or actions. From where you were sitting, could you get any feel for this? for what kind of president he was?

York: I thought he was active, but I didn't know past ones, and I didn't have any basis to compare. There are a number of things that wouldn't have happened if he hadn't been: for example, the test moratorium, which is the basis for everything in arms control that has happened since, would not have happened, because the bureaucracy would never have produced that result.

Matloff: I'm getting the same kind of conclusion from people who worked closest to him in the administration, but the scholars on the outside are still having a big debate.

York: Yes, but some of them agree. At least they are having a debate about it. The basis of it may be that he was an expert at using staff. It doesn't mean that he waited for them to come in. He put the bug in the bonnet to start with, in many cases.

Matloff: The final question--what do you regard as your major achievements during your tenure as Director, R&E, as you look back on it? Of what are you most proud?

York: I did participate in working out a major reorganization in the whole national security establishment that did change the role of technology in the decisionmaking and administrative processes. In doing my two jobs as chief scientist of ARPA and DDR&E relatively successfully, I helped to make that possible. If I had failed, then the future would have been different. Harold Brown, Johnny Foster, and all the others had a better situation. Maybe I could have been more successful and they would have done better. But there was a major change which was being called for in the Defense Reorganization Act and by the general public, and we accomplished that change without doing a lot of things which were crazy. There was a demand for reviewing the entire program and changing the organization. We did review the entire program and I think made sense out of it, got rid of some dumb things as well as got some right things started, and made the organization work. So just simply succeeding in the basic assignment was the important thing I

did. Beyond that, there are certain programmatic things. I think that my support of President Eisenhower's desire to do something about the arms race mattered. And then I played a major role in working out the programmatic division between NASA and DoD. I was responsible for changes which greatly strengthened NASA's role, and I think that was right also. On the Defense side that was right only in a kind of negative sense. We got rid of programs which would have been highly diversionary so that the Air Force could carry out its mission, and the same thing with the Army. We took programs out of the Army which were proving to be highly diversionary. I think that one of the reasons the Army was so ill-prepared to fight a limited ground warfare was the fact that it was so completely smitten with the von Braun program that it took a lot of money and attention. Working out the organization of the national space program, getting the split between NASA and Air Force, and then getting it arranged within Defense--I played a major role in all of that, programmatically and administratively.

[Note added in proof: In retrospect, perhaps the most important thing I did in the period 1952-1961 was bring (or recruit) certain good people into the Defense Establishment. I mean particularly Harold Brown, John Foster, Gerald Johnson, Michael May, John Rubel and Jack Ruina.]

Matloff: Did you leave the post with any major frustrations, or disappointments with tasks not completed?

York: No.

Matloff: One last question. Jumping a few years to 1977-81, when you came back as a consultant to OSD during the Harold Brown period as Sec/Def, what was the nature of the service that you played in that period and how would you compare the differences in the type of service?

York: It was entirely different. My relation with Harold Brown is primarily personal. My relation with the other secretaries of defense was primarily professional. There were lots of differences. I didn't have any authority the second time around. There I do have one regret, and that is that I was so sympathetic to Harold and felt personally the burdens that he had, that I didn't argue with him enough. There were a couple of times when I wish that I had been more firm in agreeing and disagreeing with him.

Matloff: Is this the period when you were doing mostly arms control work?

York: During the first two years I did a variety of things, both arms control and other matters. I was an advisor on the B-1 decision--I advised going ahead with it. I was mixed in a little bit with some of the missiles in Europe questions. It didn't go my way. I was in favor of putting them in Europe but I didn't like the Pershing. I would have picked a different missile.

Matloff: You went for the cruise missile?

York: Yes. The Pershing is an adaptation of a Martin program. I would have built a smaller, more fragile system. I wouldn't have made Pershing II out of Pershing I. I think that was a bad technical base and that Martin did that, but that's a technical detail. The biggest thing was with respect to the MX, which I think was a mistake, because I think they should have done the Midget Man. I just didn't press hard enough on that.

Matloff: Were you accountable during this period to the Under Secretary, or to the Secretary directly?

York: I was accountable to the Under Secretary, but, as I say, it was more of a personal thing. Then I was Harold Brown's personal representative in the first anti-satellite talks, but the last two years essentially my Defense Department connections were severed because I became the chief negotiator at the test ban discussions. So it's really only 1977 and 1978, not '77-'81.

Matloff: Is there any other question I should have asked you which I haven't, in the course of your long and distinguished career in the field?

York: We could have talked about ARPA.

Matloff: Is there anything I should have asked you on ARPA that you would like to say, that we haven't discussed?

York: I did say a lot about it, because I got into the question of their jumping me over Roy Johnson.

Matloff: I did want to make sure that I covered your role as DDR&E.

York: It was a special period. It was a more dynamic period than one usually has. There were a lot of decisions to be made. But a lot of them did turn out to be confirmations of former decisions because while there were individuals who were there before who weren't up to the job, nevertheless the system worked much better than the public generally thought at the time of Sputnik.

Matloff: I didn't ask you about guidelines toward industry. Did you get in on any of those questions?

York: Yes, but not so much as on programmatic questions. There were some other hassles, one of which continues to go on and which was finally lost by Dick DeLauer, probably because of his relations. That was the question of test and evaluation. In my day there was a proposal that came in from a special committee chaired by Walker Cissler that test and evaluation should be handled independently of DDR&E. I thought that it was a dumb idea, and I still do, and in that particular time the secretary supported my view on that. Now finally that's been split away. My relations with Secretary of the Army Brucker got down to being virtually personal. I mean personally bad. It had to do with that issue I mentioned before, where I concluded that the Army should be taken out of long-range missiles and space, which were Brucker's favorite programs. But the Secretary supported me there also. I had just superb relationships with all three Secretaries; the more difficult with McNamara, because he did take the first steps to cut back on the status of the Director of DR&E--small steps, but they were the first ones. I didn't like it, but I was a lame duck myself. I had only agreed to stay for a few months. One of my greatest accomplishments was introducing Harold Brown to Bob McNamara. I turned in to Bob a set of thirteen names to be my successor. He had asked me to do that, with evaluations of each. I stated that Harold Brown's name was first because I had put them in alphabetical order, but that, in fact, he was my first choice. Bob said to bring him in the next time he was in town. That was only a few days later, and he immediately offered him the job, because Gilpatric

had met him. Both Gilpatric and Harold Brown were on the first set of the board of directors of the Aerospace Corporation and Gilpatric had met him there a couple of times and was impressed by him. Harold was, and is, an impressive person.

Matloff: When you served in the transitional period, when you were running the Department in effect, you were the lame duck holdover from the other administration. Did any problems arise then that gave you any trouble?

York: No, because Bob McNamara came to work before he was confirmed anyway. I checked out at the War Room to see whether there was anything going on. I was acting Secretary several times during Eisenhower's administration, when both the Secretary and Deputy Secretary were gone. Technically, I suppose it should have been Secretary of the Army who was acting Secretary of Defense, but there were so many problems between Brucker and all the rest, not just me, that I actually filled the role. Usually there isn't anything that happens that can't wait. There was only one occasion when I made a decision as the Secretary of Defense. That was a case where here on the West Coast the Air Force had a satellite it felt needed urgently to be launched and the Navy, which was responsible for range safety, felt that there were safety considerations mitigating against it. In order to get it off--things were so marginal in those days--they had to give it a certain amount of English, that is to say, they had to shoot it east of south in order to get enough of the effect of the earth's rotation. That brought it over a small country

park northwest of Santa Barbara, a very small population. The Navy said, "You can't shoot that way," and the Air Force responded, "We've got to." Secretary of the Navy Bill Franke called me up and said, "We've got a problem and you're the Secretary of Defense." He told me about the problem, and I spent most of the day on it. I talked with people in the Air Force and the Navy and I decided to shoot. So my only decision was a positive decision at that time. I overruled the people who said no.

Matloff: That is an important footnote in history.

York: When I was interim secretary, there was much more to it. Lemnitzer came and told me about how to go to war. They put a special telephone in the bedroom, and other things like that.

Matloff: It must have been quite an unusual role for a scientist to be playing.

York: One other thing that happened to me during that period that was unusual, but not much national policy was involved, was a heart attack in August of 1960. So I ended up in Walter Reed. Every dark cloud has a silver lining. Eisenhower visited me twice over there. He came to see his granddaughter on one occasion and Richard Nixon on the other, and stopped in to see me both times. The Secretary of Defense and George Kistiakowsky came over. I had a marvelous time.

Matloff: I hope that you didn't attribute the heart attack to the job.

York: It probably was connected, but in an accidental way. I had been working hard.

Matloff: Did you try to keep regular hours in that job?

York: I did after my heart attack. That's the reason I did not agree to stay on. I felt that I could not keep up with McNamara. I felt that the way the bureaucracy works and the question of turf works you have to work as hard or harder than the others in other to keep your turf, and I felt that I couldn't do that. But before my heart attack I might have been the hardest working member of the secretariat. I think that I put in more time than the Secretary or the Deputy Secretary, but maybe the same.

Matloff: Long hours and weekends?

York: Yes, eight to seven, that sort of thing; and then a half-day on Saturdays.

Matloff: Rough on family life, too, I imagine.

York: Yes, but we had three kids at home to help keep my wife busy. I cut back on speeches and parties. I didn't accept in advance more than two engagements a week, because every once in a while one would come along that I had to go to. I didn't accept more than two speeches a month in advance, because every once in a while the president or somebody would tell me that I had to make one.

Matloff: You've been very kind and I'm certainly obliged to you. If I may, I would like to incorporate what we've just discussed.

York: That's fine.

Matloff: Thank you very much for your cooperation and willingness to share your recollections and observations with us.

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Herbert F. York
Director

IGCC CENTRAL OFFICE, Q-060
UNIVERSITY OF CALIFORNIA, SAN DIEGO
LA JOLLA, CALIFORNIA 92093
(619) 452-3352
TWX #910337127

November 18, 1985

Alfred Goldberg
OSD Historian
Office of the Assistant Secretary of Defense
Washington, D. C. 20301

Thanks for your note of November 15. Sorry I forgot to reply with regard to categories when I sent back the marked-up interview. I don't know of any reason why it simply shouldn't be in category 1. Unless you advise me otherwise, that is what I choose.

I enclose herewith copies of Chapters VII through XI of the current draft of my memoir. Please advise as to just plain errors as well as misunderstandings and omissions. Look especially at the section "Eisenhower's Legacy" in Chapter XI. I may expand it. Most outside analysts have that part wrong.

Please let me know soon what you think.

Sincerely,

Herbert F. York

HFY:aw
Enclosure