#85

Final Transcript
8/19/86

This is an oral history interview with Professor Albert Wohlstetter, held in Los Angeles, California, on January 30, 1986, at 3:15 p.m. The interview is being recorded on tape and a copy of the transcript will be sent to Professor Wohlstetter for his review. Representing the OSD Historical office is Dr. Maurice Matloff.

Matloff: As we indicated in our letter of December 12, 1985, we shall focus in this interview on some of the strategic events and issues with which you were associated or of which you may have knowledge, particularly during the Eisenhower and Kennedy administrations. First, by way of background to your long and distinguished career as a national security specialist and strategic analyst, would you discuss the circumstances of your appointment at Rand and any previous experience in the national security or strategic fields before you came to Rand.

Wohlstetter: I had no experience as an analyst of strategic policy. During the war [World War II] I had had two sorts of jobs in relation to the war. One was at the National Bureau of Economic Research, where I had been an intern on a Carnegie research associateship. Simon Kuznets, of the National Bureau of Economic Research, had gone to the War Production Board Planning Committee to work for Robert Nathan. The Planning Committee was a project involving a few of us at the National Bureau to examine some things that were relevant to possible constraints on German war production. I did some studies of the German labor force. I had been familiar with some of the material because I had previously on the Carnegie Research Associateship, and on the Social Science Research Council fellowship which I had had before at the Bureau, been looking at materials of this sort in connection with the business cycle methodology of the Bureau and with the logic of that methodology. At first, the fellowships had been for me to apply some of the methods of modern mathematics to business cycle research and economics, and I had done that. In the course of that work I had become familiar with some German labor force statistics. So I looked at their experience in World War I as part of the evidence for the potential operation of strains on the labor force in WW II--that was one sort of experience. The second sort of thing also seems largely accidental but turned out to be useful for my work at RAND. I was successively in charge of quality control, production control, and production in a war plant. I got into that because my doctoral thesis had an application of probability calculus to the statistical control of quality and manufactured product. For me at the time it was a purely methodological interest, but during the war there was a big demand for quality control and nobody seemed to know anything about it, and while I didn't know much, that still gave me an edge.

Matloff: Where had you done your doctoral work?

Wohlstetter: At Columbia. In the course of my work in quality control during the war, given my nature, I had gotten a good deal of theoretical interest in how combat equipment performs in combat and how the controls that you impose and execute in production are related to that. Curiously, even the methods that I used in devising quality controls turned out to be very relevant later to some of the things, even some of the mathematical methods, that we used in studying the warning and decision problem for strategic forces. That is, when I came to Rand, many years later. And most important was the practical experience I had in working with engineers. I worked with them from two sides, so to speak, as someone who had been concerned with very abstract theory more basic than that familiar to design engineers, but, on the other hand, I was also concerned with production, and therefore generally trying to get them to do things which were more practical than they wanted to do.

Matloff: What year did you come to Rand?

Wohlstetter: I came to Rand in February 1951.

Matloff: That's just before the Eisenhower administration, then.

Wohlstetter: That's right. My wife, Roberta, had already been there, and the reason for that was connected with our prior careers, because Rand was started by a number of mathematicians from the Columbia Statistical Research Group, including Olaf Helmer, J.C.C. McKinsey, and also Abe Girshick, who was a mathematical statistician that I had interviewed at the Dept. of Agriculture when I was doing work in the application of the probability calculus to economics. Roberta had met them on the streets of Santa Monica and they had persuaded her in the very early days of Rand to come there. They had been working on me, with Roberta's help, for a long time before I came.

Matloff: In Rand itself, what kinds of problems did you work on during the Eisenhower era and then later on during the Kennedy era? Was there any change, in any way? Also in your relationships with DoD, with OSD in particular, in those two periods--were there any changes?

Wohlstetter: I guess the major difference was that near the time when I started, under the Eisenhower administration, I had no established track record in the field, and didn't pretend to one. By the time of the Kennedy administration, the fact that I had affected major policies several times, had been effective in bringing about a change in war plans of a major sort, and had briefed many of the figures in the Kennedy administration meant that I had a much easier relationship with the Kennedy administration--less of an arms-length one, though I always believed in keeping it at a certain arms length. I was invited to join the Kennedy administration in 1961.

Matloff: I was going to ask you about that. A number of your colleagues did leave Rand to join the official community when McNamara came in, but you elected not to come. Was there any particular reason?

Wohlstetter: Yes. I have a view of the ideal role of a science adviser which suggests that it's most effective if the adviser can detach himself from the flux of day-to-day decision, and the obligations to deal with the operational matters which are there all the time; to try to see whether the questions that are being asked are really the right questions to ask--whether they're the ones that are either currently the basic problems or the future problems that are likely to come up. If you are a member of the bureaucracy, you have very good reason to spend most of your time on just keeping things going. Since I've always wanted to work on very basic issues and policy, one of the best ways of doing that, in my view, is not to try to affect it from point to point and day to day, but to stand back and do a thorough study on the question as you define it, rather than as it may be being asked at the time, and then to present your results to people who have the responsibility--but not to have the responsibility for decision yourself.

Matloff: Did you find the receptivity for your studies and other Rand studies in the Kennedy administration greater than in the Eisenhower era?

Wohlstetter: Yes, and it was true that there were a lot of things that one would have had to prove very systematically against much resistance which met with almost no resistance at all. I can illustrate that. The base study was something which for good reason had to be briefed a great many times to every directorate, many field commands, and sub-directorates.

Matloff: Are you speaking about the Strategic Bases Study that came out in 1953?

Wohlstetter: That's right. It was carried on between May 1951 and 1953. That did effect a large change in policy but it took a great many briefings and exposure of the study in great detail to examination by various responsible persons in the Air Force. On the other hand, at the beginning of the Kennedy administration, both Dean Acheson and Robert McNamara had asked me whether I could be McNamara's representative on the Acheson committee to review policy in Europe. They both knew my work. Particularly, Dean Acheson knew some of the public material very well, and liked "The Delicate Balance of Terror" very much. I had in galley at that time a Foreign Affairs article called "Nuclear Sharing: NATO and the N + 1 Country," which dealt with some of the key problems that were going to be addressed in the Acheson committee. That was distributed in galley and Acheson liked it, even though some of it went against the grain of what most of the people concerned with the study believed.

Matloff: There was a change, then, in receptivity.

Wohlstetter: Tremendous. I told McNamara and Roswell Gilpatric that I didn't know what the Kennedy administration policy on Europe was going to be, and I would therefore just do what I thought was sensible and keep them informed day by day. That's a very different thing from having to present a study in the course of ninety briefings.

Matloff: Would you elucidate a little, and look at some selected problems in national security in the 1950s, starting with the Strategic Bases Study Report that came out in '53, which you mentioned in passing. What were the origins of that study--how did it come about and what role did you play? What instructions, if any, did you receive? and what dealings did you have with people in OSD?

Wohlstetter: I was asked by Charles Hitch, who was head of the Economics Department at RAND, to think about whether I wanted to work on a question that had been posed by Colonel Harold Maddux, who was the Assistant for Bases in the Air Force. The question that he had posed was what was the optimal way to base SAC, considering such things as time on target, and so on, and given the fact that SAC had a very short-legged force, even the planes that were nominally intercontinental like the B-36 or the B-52, which was not yet in the force. For public relations purposes, the Air Force tended to state the combat radius, the payload, the maximum cruise speed, and maximum penetration speed, as if these things could be realized simultaneously, which they couldn't. The B-36, as I recall, had perhaps a 3600-nautical-mile combat radius. The B-47, which was to make up the vast bulk of the SAC bombers, had a combat radius of 2100 nautical miles. Strategic targets ranged in distance from various U.S. bases to various places in the Soviet Union from about 3100 to 6200 nautical miles. It was clear that we needed some way of extending the range. We had a worldwide system of bases, some of which we had inherited from the island-hopping campaign in World War II, or which were left over from World War II among our allies, such as the very extensive British bases. The question was what was the optimal way to do it. I thought that it looked like a rather dull, mechanical study at first--a logistic study. So I told Dr. Hitch that it didn't look very interesting to me but that I would think about it over the weekend. Then I did think about it and thought that almost all of the questions that I had about a strategic force were implicit in the question that he had asked, viewed in the right way, and that I would have to learn a lot about the operations of the strategic force and what the technical possibilities were, and also a lot about the political considerations in getting bases, and the economics. It struck me as being a marvelous problem--very complex and very important. So I told him that I had changed my mind and found it a very interesting problem.

Matloff: Did you have any dealings with anyone in OSD in the course of doing that? I know you must have had dealings with the Air Force.

Wohlstetter: I had very extensive dealings with the Air Force and dealings with people in OSD only from the standpoint of information gathering, never from the standpoint of trying to influence them on the subject--and that was a deliberate matter in the base study.

Matloff: In what respect did you agree or disagree with official strategic thinking in OSD, as far as you can recall?

Wohlstetter: There were several respects in which the sorts of questions I was asking were different from the questions that were being asked there. And some of the conclusions we ultimately came to were generated by the difference in the questions. For example: the sorts of attacks that official strategists envisaged the Soviets might make on the United States, let us say, were generally very largescale attacks, as large as they thought the Soviets could make them at various time periods. We asked ourselves whether that sort of large scale attack, which followed direct routes and provided maximum warning for SAC, would best serve the Soviet purpose of finding and destroying SAC. We tried other Soviet strategies. We had decided that you couldn't effect anything immediately on presenting a study. It was clear to me that it was going to take some time to do a systematic and thoughtful job on this, so I knew that the study was not going to affect anything before 1955, and eventually we made it 1956. That turned out to be a little pessimistic--it did change operations before that--but I thought of it as something that would begin, in effect, about 1955 and have an effect for the rest of the decade. I asked questions about what sorts of attacks the Soviets might make in this period. I, of course, had no sources of intelligence other than those that were available through the Air Force--no independent sources of intelligence. So I looked at the forces and I found that the Air Force and OSD in general, and later on, the Net Evaluation Subcommittee of the National Security Council, were generally thinking about quite massive attacks, in which the Soviets would direct their forces at cities and industry, and incidentally, in some cases, might attack some SAC bases. Now that sounds like what we generally call the worst case, but in a way it was a rather optimistic case, it turned out, for SAC, because it meant that ~~since~~ the Soviets were using all the strategic force they could get together, which was much more shortlegged than ours. They had to come straight across the pole in large numbers at altitudes at which it was very easy to pick them up, with five hours of warning or so. Actually, by studying SAC operations, we found that SAC really wasn't ready to use even five hours of warning. The problem didn't look like such a difficult one. Now, what we did, was to ask some questions. It's clear that Detroit is going to be there, and the steel mills aren't going to move. SAC, on the other hand, could move. So suppose the Soviets were to make their attack in two stages, at least, the second step being not an urgent one, and suppose they would design that first step precisely to catch SAC by surprise on base. In that case, they weren't going to go blundering over the pole with everything they had. In that case, they'd skirt the radars, and so on. By asking the question, how would the Soviets do it if they wanted to destroy what was time-urgent--that was a different question from that which was being asked in OSD or in the Air Force at that time.

Matloff: How well was the first report received in OSD?

Wohlstetter: OSD didn't hear about it officially until the Air Force told them, because I was trying to affect a decision which was within the powers of the Air Force to make. At that time Rand was in an advisory role to the Air Force and I was a consultant to Rand, at the start of the base study, not even a member of the staff. In any case, it seemed that we were trying to present alternatives for a decision-maker in the Air Force to decide, so he should be given the opportunity to decide. We did not lobby for it in OSD or leak it, or do anything of the sort. Moreover, a crisis came up at one point when an Air Force officer who was in IDA in the Weapons System Evaluation Group [WSEG] was present as an Air Force officer when I briefed one of the many Air Force sub-directorates that I was briefing. He told some people in WSEG of this extremely important study that was being presented to the Air Force. And WSEG then requested a briefing. I then asked Clint Coddington, who was the executive assistant to the Director of Operations and one of the two senior working colonels who was crucial in getting the base study a hearing, whether I could tell them anything. He said, "Absolutely not." And I asked, "Do I have to brief them?" He replied, "Absolutely, you do." That was a dilemma. I then had to give them a briefing which left them looking dazed, because I wrote all sorts of formulae on the board and gave them a methodological briefing. They said that they had heard that this was a very major study with radical effects on all of their plans, and they didn't see it. That was a very sweaty afternoon I spent, giving them that, but I carefully avoided bringing pressure on the Air Force to do something which they hadn't yet had time to consider themselves.

Matloff: To take you to another problem in the '50s, in connection with the Gaither Committee report in the fall of 1957, "Deterrence and Survival in the Nuclear Age." What connection did you have with that committee? I think you briefed them, among other things.

Wohlstetter: There were several connections.

Matloff: Did your SAC vulnerability study have any impact on it?

Wohlstetter: The Gaither Committee had several connections with the work that I had done with Harry Rowen and Fred Hoffman. Rowan Gaither was chairman not only of the Ford Foundation, but also of the board of trustees of Rand. He had heard me brief the Rand Base Study in 1953-54 and had known what had happened to our war plans as a result of the study. And he had heard me brief the second big study "Protecting our Power to Strike Back in the '50s and '60s," which I had briefed to him in 1956. He knew what had happened with that study, that it had gone through all levels of the Air

Force much more quickly than the Base Study had and had been accepted by the Air Force Council very early. But he had known that, unlike the Base Study, this one required some action by the Secretary of Defense and the National Security Council, and that it had been stalled there. When the Gaither Panel was getting started, he called me and Frank Collbohm, the president of Rand, and asked me directly, and Frank had asked me for him indirectly, what I thought was important for the Gaither Panel to address. He told me the terms of reference of the study, and that it was beginning as a civil defense study. Isidor Rabi had been in the White House for some time connected with what is currently FEMA, the civil defense organization, as an advisor, and that was the major interest the panel had at first, with a subordinate interest in active defense of the population of cities, which was then the dominant interest of the atomic scientists movement. (Very different from today.) I was not at all unsympathetic to the defense of cities by passive or active means, but I did not think that it was of as critical importance as reducing the vulnerability of SAC and having a strategic force which was able to deter war altogether. So I told them that what they were doing was a useful thing in the national interest, but I thought that they should remember that it was easier to protect SAC than it was to protect cities, and that protecting SAC was a sine qua non of having a genuine deterrent, as Gaither knew from having listened to me for the preceding three years. So he agreed, and that was put on the agenda.

I was not involved in the internal workings of the Gaither Panel, but several RAND people did get to be involved in it. One was Ed Oliver, who acted as exec to the chairman of the panel. He was an able aeronautical engineer whom I knew quite well. Another was Andy Marshall, who, of course, you know is a first class man. Herman Kahn did some work on and off with the Gaither staff and he had gotten a strong interest in protective construction earlier in connection with the second big study--protecting our power to strike back in the '50s and '60s--which had recommended the silo and other blast-resistant complex shelters with big doors.

Matloff: This was the second strategic bases study?

Wohlstetter: It wasn't called a base study this time because it was just as much concentrated on the warning and decision problem and command and control. It was called "Protecting Our Power to Strike Back in the 1950s and 1960s" and it was report No. R290, by myself, Fred Hoffmann, and Harry Rowen. Herman had been associated with that study, although he was not one of its authors. But that was the way he came into this line of work. He paid attention especially to the civil defense part of the Gaither Panel. My own interest was in briefing the Gaither Panel, as I was asked to do, on the results of the 1956 study. The vice president of Rand in the Washington office, Larry Henderson--a very important figure in getting these things into the decision process--had felt, with me, that it was not something which should be left stalled, and that this was an opportunity to inject it at a very high level and very legitimately to get it started. My briefing and the study affected the first part of the Gaither report, which was an attempt to formulate some of the same sorts of things that we had said. But it had a few twists. There was a long appendix to the Gaither Panel study which quoted extensively from R290, which was done basically by Spurgeon Keeny, but the first part of it was by Bob Sprague and others.

R290 is the report of the second major Rand study.

Matloff: Let me ask you about the followup on this--the article you wrote January 1959, "Delicate Balance of Terror," which is a landmark in the literature on strategic thinking in the post-World War II period, the one that appeared in Foreign Affairs. What led you to write the article and to what extent did it reflect dissatisfaction with official thinking on strategic policy and on the presumptions about the stability of deterrence?

Wohlstetter: The exact history was that Rowan Gaither was chairing a study group in the Council on Foreign Relations at the time. The vice chairman, as I recall, was Jim Perkins. Bob Sprague and a long list of people were members of the Council Study Group. As you go down the lists of people on both the study group and the Gaither panel, you'll notice a very large overlap. Philip Mosely, who was the director of studies at the Council on Foreign Relations, asked me to do a paper of my choosing to present to that study group. Phil Mosely was also a member of the Rand board of trustees, so he had for four years heard me give talks. I presented the talk that was later to become "The Delicate Balance of Terror" in May 1958 and it had a very striking effect. Many leading figures in the national security community, decisionmakers, and so on, were present, and several of them wrote letters to Frank Collbohm to say how important they thought that talk had been. I was asked by Ham Armstrong, the editor of Foreign Affairs, immediately thereafter, if I would write an article on it. That was the origin of "The Delicate Balance of Terror." I gave the talk--I still have my notes--and then, as I always do, I went through several drafts and published a longer draft as a Rand unclassified paper. Then, working over it with Ham Armstrong and Phil Quigg, I reduced its size, leaving out some of the things on the importance of improving conventional forces in the shorter version. After that, I went off to the surprise attack conference, which had gotten started in connection with a Soviet misunderstanding or misrepresentation of fail-safe which was one of the principal recommendations of R290. While I was at that conference, it reached the galley stage and they decided to hold still another meeting of the study group on it. I asked Charles Hitch to represent me on the strategic and political-military side and Herman Kahn to represent me on the technical side. That was a very distinguished group at the Council meeting--Paul Nitze, George Kennan, and many others. In December 1958, when it came out, the article was already well known. There had been many antecedents, and that was the reason that it was immediately translated by the Japanese, the French, and so on.

Matloff: Do you recall any reactions from OSD on this provocative article?

Wohlstetter: Yes. Let me think about that. What had happened when my briefings for the second big study reached the OSD level was part of the background for this. I mentioned that while R290 went through briefings in the major places in the Air Force, it did not go through all the sub-directorates. So it was accepted relatively quickly by the Air Force leaders, and then a collection of three- and four-star Air Force generals went with me when I briefed Charlie Wilson, Quarles, Douglas, and the civilian heads of OSD. At that briefing, when Wilson asked the first question my heart sank, because it was clear to me that he hadn't understood a word that I had said. He said, "You have forgotten that we have overseas bases," and then after a few other questions of Wilson's, Quarles asked questions. He was a very intelligent but rather arrogant man. He said, "This is an excellent study, but you seem to have omitted this consideration." I said, "No, as a matter of fact, I'll show you what we considered on the that matter," and I went over it with him. And he was satisfied on that point. But at the end of the day, it was clear to me that they had not been persuaded. If I were a drinking man, I would have gotten drunk that night, thinking," Lord, is this the way the Department is run?" In any case, I realized that it appeared that we were at a point at which the Air Force recognized the importance of making these changes, and, on the other hand, it was going to run into a problem at the OSD and NSC levels. Given the fact that I was sure that Quarles hadn't heard anything as crucial as that before it, I was very disappointed, because I had heard good things about his sharpness. I got a phone call from Larry Henderson later indicating that Quarles had called him and said that he wanted to hear that briefing again. So this time I decided that I would make it very hard for him to evade the issues or not to see the point, and I got out a couple of quotations from some speeches that Quarles had made about the importance of deterrence, and so on, and used them at the beginning as an epigraph. I made it as forceful and as blunt as I could as to what the problems were with the existing plans. He kept rubbing his forehead and saying, "This is very urgent and the President simply has to hear this."

So we seemed well on our way to seeing the President on the subject, but then about two weeks later Quarles called Larry Henderson and indicated that the President's health--this was between his diverticulitis surgery and his heart attack--was too precarious and that he had decided not to do it, which made me feel not too good at the time. That was 1956. It indicates something as to what our relationship was then with OSD and above the level of the Air Force. The Gaither panel came as a second shot, then at affecting things at the National Security Council level.

Matloff: Let me focus for a moment, if I may, on strategic concepts and planning, on which you've written a great deal and worked on while you were at Rand, and, I'm sure, since. What was the impact of the Korean War on your thinking about strategic concepts and planning? Did your thinking differ in any way from the official national security policies in the wake of the conflict? Remember, the administration was talking about massive retaliation and the New Look policy; and all the rest of the strategic theorists on the other hand--Kissinger, Osgood, Kaufmann, and the rest--were talking about limited war in various manifestations.

Wohlstetter: There was one very direct and simple connection between the Korean War and my work. I read with great interest and meticulous attention to detail the Hearings on the Situation in the Far East. It was one of the most illuminating ways of getting a look at many of the issues connected with developing a base system. I also then read the hearings on the B-36 and the aircraft carriers, in which the Air Force and Navy were pitted against each other, and there the problems created by the difference between the point of view of SAC and the point of view of the Navy and the issues they were debating were matters which I had very much in mind. But here again, I felt that they were usually debating the wrong questions. They were talking about such issues as whether SAC would be able to get through. General LeMay thought SAC would always get through, whereas the Navy was talking about the enormous improvements in active defense and surface-to-air missiles, which would confront SAC with a tremendous defense problem in penetrating. SAC would respond that some would get through and the destruction would be so great that it would be decisive--and so on. There was no hint in any part of these rivalries of any questions about whether SAC would survive attack. You can imagine that, if the Navy had been aware of the problem, they would have raised it. But there was no discussion of that at all. They were discussing it in terms that were essentially those that had been generated by a world during the interwar period and World War II, in which the problem of SAC's survival on the ground had not been a key one. The hearings on Korea did raise a lot of important issues about restraint and about what we were targeting but without resolving them. Other hearings demonstrated to me the sorts of issues which were being debated and those which, unfortunately, were not.

Matloff: The McNamara administration marked the change from massive retaliation to flexible response. Did you ever have a chance to present your views? Were you called upon to present your views to McNamara in this connection? Were you drawn in on the official discussions in connection with the shift? You mentioned in our earlier discussion your role on the Acheson Committee.

Wohlstetter: That shift took place in connection with the Acheson report. Acheson, a wily old bird, had made sure that there would be no "Acheson Report" but only a National Security Council report. He prepared the report as a draft NSC document. That meant (in those days) that it was not something which a reporter was likely to get. It would be much more closely held and it would have a different status from just a report of an advisor.

Matloff: Who appointed you to the Acheson committee?

Wohlstetter: McNamara. I was his representative.

Matloff: You were still at Rand?

Wohlstetter: Yes.

Matloff: This was a rather unusual appointment, then?--a secretary of defense appointing a consultant from the outside to serve on an official committee which is making a recommendation at the NSC level?

Wohlstetter: It hadn't occurred to me that it was unusual, but it was clear that both McNamara and Acheson were eager for me to do it.

Matloff: As you have written, this committee recommended a formal change from a policy of massive retaliation to flexible response. This is a point you described in the recent article on "Bishops, Statesmen, and Other Strategists on the Bombing of Innocents" that appeared in Commentary in June 1983. On your view of Secretary McNamara as strategist--can you shed any light on the development of his strategic thinking? Did you ever have discussions with him on the counter-force/counter-city problem before his Ann Arbor speech in 1962?

Wohlstetter: Yes, I talked with McNamara on a number of such things. I guess I've described in several places the change in McNamara from before the Cuban missile crisis and thereafter. Before the Cuban missile crisis I found Bob McNamara quite extraordinary as a Secretary of Defense. If you contrast him, say, with my experience with Engine Charlie Wilson, you can see what I mean. He didn't accept anything that he was told on faith; he was loaded with questions that he was asking. On the whole, I thought the first two years of McNamara were by far his best years and that they were really of great importance. He shook up all the operating organizations, getting them to think, to justify what they were doing. They hated it, but it was very healthy. I remember one general from a revolutionary war family, a very good fellow, "Tick" Bonesteel, sourly recalling the name of a current TV program, in connection with McNamara's 135 questions (or however many there were). He called them "Youth Wants to Know." They were a rather young group, who were then heading the Defense Department, but on the whole they were very good. Also, it was odd suddenly to find a Defense Secretary to whom I had given a short explanation of what I was doing, saying, when I met him in the corridor, "Albert, can you tell me the order of magnitude of such and such that you had mentioned?" and so on. That was not the sort of question I had been used to getting in the past from high officials. So I found him very stimulating and quite admirable in the things that he was doing at that time.

Matloff: But you say there was a change after the first two years in your perspective? In what ways?

Wohlstetter: I think that he was very shaken by the Cuban missile crisis and from having moved in the direction of taking the possibility of war seriously. He had felt that he was very close to the brink of war. I don't think he was. Roberta Wohlstetter and I have written an article called "Controlling the Risks in Cuba," which included a lot of the memos that we had been writing to the EXCOMM during the crisis, and which were sent to it through Harry Rowen and Paul Nitze. Our view was that this was obviously a tremendously important crisis, but that we were a long way from losing control, and that there was no likelihood that Khrushchev was going to let things get out of control. So we believed that one should behave cautiously. I sat on the Quarantine Committee during the Cuban missile crisis which dealt with a minimal use of force. But because people were acting very cautiously on both sides, we weren't anywhere near the brink. McNamara and most of the principals really thought they were practically on the edge of war. That is the way it is conventionally written about. You can see the difference if you read our piece, "Controlling the Risks in Cuba." I think that he was very shaken by that. Before the Missile Crisis, he was taking seriously the idea that there might be a war, and that if there was an attack, we would actually have to respond. Therefore, he was taking seriously the notion that we should respond in a selective and discriminate way against military forces, rather than a thoughtless way of responding against population and ensuring the death of your own population.

After the Cuban missile crisis I think he found it hard to contemplate that we might actually get into a war. The obvious and highest priority was to prevent a nuclear war. But the only way you could prevent it was by assuring an adversary that not only <u>could</u> you retaliate, but you <u>would</u> retaliate. I think he found it hard to think about that after his experience in the Missile Crisis. I could give you a few details. I think that then he moved towards Deterrence-only, which is deterring without intending to do so, rather than putting yourself in a position in which it could be in your interest at the time to retaliate. So that was, I think, the major difference. He's a very complex man, and he contradicted himself many times, I think, in this later period, and still does. The sorts of things he did afterward--some of them I feel were in exactly the wrong direction to go.

Matloff: Did he appoint you to the Quarantine Committee?

Wohlstetter: No, that was done much more informally. John McNaughton was chairing the Quarantine Committee in the Defense Department. Paul Nitze was mostly off at meetings of EXCOMM and John was the one who was running that for ISA. I was naturally included as a valued adviser, sort of a junior version of Acheson at that level, compared with Acheson in the White House itself.

Matloff: Did you get drawn in on the controversies of the McNamara administration on technology and weapons questions: for example, on the ABM system, the TFX, the carriers, the B-70 bomber, and all the rest?

Wohlstetter: Not on all of them. I actually avoided being drawn in as a sort of general wise man, because I feel that it's important for a science adviser to distinguish between those issues on which he can speak on the basis of evidence and careful reasoning from those on which he may have a hunch, good or bad, or a prejudice, or, at least, (to be kinder), a predisposition. In fact, McNamara suggested that he would like more frequent meetings, Paul Nitze told me, and I told Paul that I didn't have anything to say to Bob McNamara that frequently. I felt that if I did see him very often, then when I had actually done a lot of work, reflection, and reached conclusions that I thought were well-evidenced and made a recommendations to him, he wouldn't be able to tell the difference between that and when I was just telling him something off the top of my head. So that's relevant, I guess, to some of the questions you have about how science advisers, in my view, ought to behave. But I did talk with him about the ABM. I did a lot of work on that, and sent him a long letter at one point, which was supposedly part of the intellectual background in deciding to go ahead with the "thin" defense of the country.*

Matloff: Did you ever get into a discussion with him about this question of nuclear superiority, parity, or sufficiency, vis-a-vis the Russians?

Wohlstetter: I can't recall any specific conversation, but on the other hand, I read carefully what he had to say and it drove me to despair, as all of such discussions drive me to despair, because you're talking about comparing very complex aggregates. They're what mathematicians would call vectors, with many components, rather than simple arithematic quantities, so you can't really say that A is greater than B. Am I larger than, say, Herman Kahn? I was taller than he, but he was heavier. Now when you're talking about an aggregate which has hundreds of thousands of components, to talk about which one is better or worse is generally at the best vague. You can talk about it sometimes in a very crude way when somebody has a case of dominance. For example, a large, husky, young heavyweight can beat a sickly midget who is very old. When you talk about these things in terms of "parity" or "superiority", or "inferiority", it's only at the very least unclear; it doesn't have any operational meaning.

It has a political effect, and in general McNamara's statements on the subject of "superiority" or "parity" would amuse you to re-read. In 1963 or '64, testifying before Congress on the test ban, McNamara was asked whether, if we had such a test ban, this wouldn't mean we'd lose our superiority, and there'd be only parity. He said, "I don't know what the word "parity" means. I know that we're superior and we're going to stay superior,

*See Appendix A for Letter, Albert Wohlsetter to Sec/Def McNamara, Feb. 21, 1967.

and the Soviets understand that." Now, he didn't say, "What in the name of God is the meaning of 'parity'," as a certain Secretary of State later said. But he was definitely in favor of superiority and said that that's what we would have and keep. In some sense I believe that the whole discussion is rather vague, but it's kind of amusing to see that many of the people who were talking that way now regard it as one of the great menaces for the United States to attempt to get superiority. So I feel that it's rather sterile to discuss it in terms as vague as that. McNamara is emphatic. He doesn't like to, and finds it very hard, to qualify--I've always noticed that about McNamara--and so he makes downright statements on that subject which I don't believe would bear examination. That has always bothered me.

Matloff: On some of the area crises and problems that arose during the period of the Eisenhower and Kennedy administrations--in connection with NATO, you mentioned earlier your role on the Acheson Committee, were you drawn in on any other discussions on the OSD level on NATO problems of strategy, policy, or buildup?

Wohlstetter: I saw Paul Nitze regularly and also his deputies. Harry Rowen was his deputy for plans and policy. I saw John McNaughton then, and also when he became Assistant Secretary for ISA. I would see them very frequently and I wrote various memoranda for them, some of which I still have. For example, when McNamara decided to send [redacted] more nuclear weapons to Europe, (the total got to be about [redacted] and most of those were put there under McNamara), I felt that it was quite inconsistent with his views on the conduct of a nuclear war, and wrote a note to him through John McNaughton to that effect.

OSD 3.3(b)(4),(5),(6)

Matloff: Were you being drawn in through the OSD/ISA people, mostly?

Wohlstetter: No, through a lot of places in OSD, e.g. in Systems Analysis, Alain Enthoven, who had worked with me at Rand and whom I liked very much, had asked me to be a sort of critic of the DPMs and the strategic memos. I did review those and wrote comments on them. I knew people in Systems Analysis at several levels. Fred Hoffman was Deputy for Strategic Systems there. I knew him, of course, intimately, and several others in other parts of OSD. Adam Yarmolinsky, the Special Assistant to the Secretary, would ask me questions from time to time. I saw people at every level.

Matloff: To add a personal note on this, in early '61 I had just returned from a year's leave from the Army Historical Office as a Brucker fellow to study NATO. As soon as I returned I was told to get over to the Pentagon and help brief you on what I had learned. Rowen and Nitze were there, and I briefed you late in the evening.

Wohlstetter: That must have been early in 1961.

Matloff: Yes. You were going to go on and brief somebody in the White House at that time. Is there anything more you would like to add on your role in the Cuban missile crisis?

Wohlstetter: Roberta and I were writing memoranda during the crisis. They arrived there in two ways.

Matloff: You were doing this at Rand? Were you drawn in officially into the OSD thinking and discussions?

Wohlstetter: Yes, by Harry Rowen and, of course, with Paul Nitze's knowledge. I had seen Harry before the discovery of the crisis, and shortly after the President had made his speech in which he drew a line, saying as long as the missiles the Russians deployed were for active defense, which was the essential point--he meant surface-to-air missiles and so forth--we would tolerate it, but we would not tolerate their putting offensive missiles there. When he said that, I remember saying to Harry, "The President's sure putting himself on the spot," because this was an election year and he couldn't back down from a statement of that sort. I remember Harry saying, "You don't think that Khrushchev would do that, do you?" Well, he did, and, of course, was planning to at the time, and as soon as it was possible for Harry to call me legitimately and tell me, he did, and asked me for help. I did come in and began to work with him, and Roberta and I began writing memos almost from the start. Roberta had been working on Castro for some time and she was simply incredibly clairvoyant about what Castro was going to say. She was almost able to predict his speeches in detail.

Matloff: Did you attend any of the EXCOMM meetings?

Wohlstetter: No, I attended meetings essentially in the Defense Department, through Rowen, McNaughton, Yarmolinsky, and Paul. I was seeing some people from State at the time, too, but I was not in the White House. We did communicate with the White House, but through these memos, which did get to the EXCOMM in two ways: one, directly through Harry and Paul; and then later through Herb Goldhamer, a splendid sociologist in Rand. A lot of people at Rand were working on it and he gathered up memos and sent them along as a collected set. Ours had already been sent.

Matloff: Were you drawn into any of the discussions on Indochina, inevitably the Vietnam question, during the McNamara period particularly? Did anyone officially seek your advice and views on the problems?

Wohlstetter: Partly in line with the practice that I mentioned--that I don't like to be taken as an authority on things which I haven't done any honest work on--I tried to avoid that, though on several occasions I guess I did speak on the subject. My first major connection with it was at the end of 1961 or at the beginning of '62. The gaming facility at the Pentagon--it has had several names, one was SAGA--was running a Southeast Asia game, a very high-level game. I was asked to be game director. As far as I could tell, my qualifications were: (1) that I was very sceptical about the value of gaming; and (2) that I was not an authority on Southeast Asia. So I naturally became the director of the control team. That turned out to be a very useful thing, however, and the most interesting game I have ever participated in. I don't recall whether it was one or two weeks, but it was played at a very high level, with the Director of the CIA; the Chairman of the JCS, General Lemnitzer; Ros Gilpatric, the Deputy Secretary of Defense; and so on, as the senior team. Then there was a red working team and a blue working team. A policy had just been announced by Harriman, who was then Assistant Secretary of State Far East. He had made a number of speeches and written some articles which announced the basic policy. We were deciding not to go into Laos. That had been debated very much and I knew about that debate. It was decided to make a stand in Vietnam and defend South Vietnam. The way it was phrased by Harriman was that South Vietnam was both more defendable and more worth defending than Laos. That was the answer to some questions, but not really to the relevant questions, which were: 1) could it be defended at all? and 2) could it be defended without Southeast Laos, which has the Ho Chi Minh Trail going through it? So I decided to probe how much sense that policy made, given what were visibly some of the strategic considerations in Southeast Asia. Harriman sent over his deputy, Bill Sullivan, later on our Ambassador to Iran. Bill was there clearly to represent Harriman and to show that this was the only policy. He wanted therefore to be a member of the control team, but I explained to him that I was God in this case. I made him captain of the red team. So he had to think about how to beat the Harriman Policy. It was a very illuminating and prophetic game. The way the thing was run was interesting. Because it involved such high level people and went on for so long a time, they could only devote an hour or two a day to it. And that meant that all the work had to be done, even on into the wee hours of the morning, by the working team. But that sort of represented the way things normally happen in the bureaucracy. Because then they would go in and make their presentations to the seniors, and they would behave just as they normally would--bend the facts a little bit, overemphasize some things, leave out some--trying to get the right decision. When I finally forced the situation into one in which they had to start drawing lines on which they would stand, one drew it vertically and another drew it horizontally. And what they had in common, what they were going to fight for, was--guess what--Southeast Laos! That was my first real connection with S.E. Asia. The second was when I took an around-the-world trip in May, June, and July of 1962, during which I stopped in South Vietnam.

Matloff: Under whose sponsorship was this being done?

Wohlstetter: Rand, you must remember, was a freewheeling organization. I had a lot of latitude. I was part of Rand and I was taking the trip because I've never had the same view of strategic issues as certain of my friends and most of the people who write about it, where country A and country B are deterring each other and making threats, and so forth. I have never seen country A or country B. I believe that these issues really have to be looked at in terms of concrete countries and plausible contingencies, to judge how wars are likely to start, what the objectives of the combatants are, etc. So I was making this trip because I was very interested in various unstable areas where I thought that we might get involved. In that connection I made this trip to South Vietnam and I talked with a lot of people there, including our ambassador and Thompson of the British office there. I talked to all the key people. I was immensely disturbed. I made this whole trip with the remains of a bout of pneumonia, still having a slight fever. My fever shot up to 104° in Bangkok. I wrote a very long letter, which Harry circulated in the White House, indicating all the things that I thought were delusive about our policy in Vietnam on the basis of these observations. I made several trips thereafter, but, unlike some regional problems which I have studied in great detail and systematically--e.g., the four-volume study of the Persian Gulf that I directed--I never regarded myself an authority on Vietnam. I had a good sense of smell about several key points on Vietnam. These were based in part on what I had observed on trips I had made there. I was asked by Alain Enthoven, after one such trip, to raise these issues with Bob McNamara and to make these observations. However I turned the suggestion down because I didn't feel I had done enough work. I was not a key player at any point on Vietnam.

Matloff: Is there anything that you would like to add on the role of the consultant, the qualities that he might possess, and where he could contribute?

Wohlstetter: One of the places I've talked about the role of science advisers in these decisions is in my paper called "Analysis and Design of Conflict Systems." I say there that I feel that there is a great deal of inertia in large organizations; they just don't turn around on a dime. They're a little like battleships, at best. That goes for any large organization--military organizations, GE, etc. Therefore, if you're detached, if you're not on that ship, if you're not under obligations which day-to-day prevent you from looking at it, you can sometimes see the directions of technology and the sorts of problems that are implicit in the methods of operation we have, and in our future plans and large-scale political changes, long enough in advance to be able to make some fundamental suggestions. And you have a better chance of doing that, if people regard you as someone who doesn't have any turf to defend. It has the disadvantage in the sense of "who is this tall blond guy [I used to be skinny], who is coming in to talk about this?" You are an outsider. But on the other hand, if you have done a good job, you ought to be able to present the evidence. There's every reason that they should question it. You should be able to answer the questions.

Matloff: What do you think was your greatest satisfaction in your dealings with the Department of Defense, particularly during your tenure at Rand?

Wohlstetter: I think that the greatest satisfactions were, first of all, in the insights that we sometimes got in seeing relations among very complex developments. I am basically a research man, so I like to find things out. That's the reason this little group that I'm associated with is called Pan Heuristics, "finding out about everything." The second thing is having the satisfaction of doing a rigorous, objective, and reproducible job of showing that your hunch or insight was sound. The third is then being able to persuade key decisionmakers. The fourth is actually seeing it happen.

Matloff: What were your greatest frustrations or disappointments in dealing with the Department of Defense, particularly at the OSD level?

Wohlstetter: In OSD in the mid-50s I felt that the Republicans were defending themselves against Democratic charges of all sorts, and therefore they were not disposed to look at the problems they had and find out that there had been serious errors. Even when you got people to recognize them, it was devastating suddenly to realize that this was something that really had to go to the NSC and had to be seen by the President, but the President was just not accessible at that point. Those were the main frustrations. But I've never been under any illusions that the basic way I look at things and the basic changes I want to recommend should be taken quickly or lightly.

Matloff: Thank you for your cooperation and your willingness to share your recollections, insights, and impressions with us.