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Wohlstetter initially reviews the recruitment of his wife by RAND in the late 1940s, the attraction of RAND for him, and his consulting work for the Mathematics Division of RAND beginning in 1951. He then describes various aspects of working on the basing studies completed in 1953 and 1954, his increasing involvement in work outside of RAND after their completion, RAND's work environment, and his concerns beginning in the late 1950s over the future of RAND's work product due to such factors as conducting joint studies with the USAF.

TAPE 1, SIDE 1

1-3 Factors that attracted Wohlstetter to work at RAND: J.C.C. McKinsey, Alfred Tarsky, Abe Gershik, Olaf Helmer, Roberta Wohlstetter

3-5 Begins work as consultant with RAND in 1951

5-7 Importance of interdisciplinary work and project officers at RAND; systems analysis methods; Ed Paxson, Ed Quade

7-8 Wohlstetter's conceptualization and analysis basis problem to be examined by the basing study; his emphasis on "framing the question" in problem-solving

TAPE 1, SIDE 2

9-10 Basing study (continued); freedom at RAND from deadlines; factors complicating basing study; RAND bomb damage assessment wheel, complexity of problem: his attitude toward interdisciplinary work

11-12 Influence of systems analysis approach used in basing study; Quade course on systems analysis for Air Force personnel

12-13 Lag time between performance of basing study and publication of reports

13-15 Goal of changing policy conflicts with prompt publication; importance in doing systems design (analyses) of respecting questions posed by policy makers and implementers

15-16 Collaboration within university settings

16-17 How RAND generated interdisciplinary work

17 C.E. Lindblom paper Bargaining: The Hidden Hand in Government

17-18 Excitement of working at RAND in the 1950s

TAPE 2, SIDE 1

18-19 Wohlstetter increasingly involved in work outside of RAND; R290 study; briefing of Gaither Panel; member of National Security Council Acheson Committee to reconsider European policy

19-20 Wohlstetter's concern with imperfections of RAND system; concern that changes occurring would discourage creative thinking

20-22 Development of idea for the silo during R290 study

22-23 Freedom and time within RAND to develop new even startling ideas; different styles of working within different sections of RAND; Nathan Leites; Herb Goldhammer; Roberta Wohlstetter; Fred Ikle and permissive action link

23-24 Fear in 1960 that positive RAND attributes were threatened by joint studies with Air Force, budget tightening, general lack of sympathy with idea that policy research must be independent

Interviewee: Professor Albert Wohlstetter

Interviewers: Martin Collins and Dr. Joseph Tatarewicz

Location: Professor Wohlstetter's home, Los Angeles California

Date: July 29, 1987

TAPE 1, SIDE 1

Mr. Collins: I think for this think for this discussion, we'd like to use as a basis the memorandum which you have provided us recently on the character of the research activity. The memorandum, as I understand it, dated abut 1960,[1] reflects a reconsideration of some of the basic research objectives of RAND as an advisory organization, the two foremost ones being this question of providing objective independent advice and how to make interdisciplinary work effective along those lines. I wonder if you might begin with that framework in mind, what you found along these lines when you came to RAND in 1951.

Dr. Wohlstetter: I came to RAND as a consultant. I had known some of the first people who were on the RAND staff because they also were mathematical logicians: J.C.C. McKinsey, and Olaf Helmer, and also M.A. Girshick, who was a mathematical statistician, whom I had met when I was first contemplating trying to apply methods of modern mathematics to social science subjects like economics, and had a fellowship for that purpose. I had met him then, and I had known Chen McKinsey and Olaf Helmer for many years before when I was a student. When I came to RAND, I was still trying to phase out of the work I had gotten into during the war, which was in industry. Roberta Wohlsteter also knew them and had been induced to do work at RAND. I had no knowledge of the classified work that was going on at RAND, but I did find several characteristics which were striking, one, that it was an organization with an enormous latitude.

The best example of that that I can think of involved one of the important contributions to metamathematics by Alfred Tarski, which had never been published, which showed that it was possible to get a decision method, an entscheidungsverfahren, for a rather large and rich section of classical geometry, corresponding

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[1] RAND'S Continuing Program of Broad Policy Study: Problems and Incentives, M-953, February 22, 1960

roughly to high school geometry. That possibility was something of a surprise, because Kurt Godel in his famous and great results had shown that classical mathematics was essentially incompletable; that in any mathematical systems of a certain degree of richness it would be impossible to get a finite method of deciding whether formulae were valid or invalid; or that there were always some sorts of statements in the system which would be true but not provable. So that was surprising. There were of course systems that were known to be completable, in which you had such a method of decision, such as the calculus of elementary sentences and elementary statements, which now in high school, I guess, you have truth tables for evaluating. But anything that was as rich in content as this, was not supposed to be. Well, that was very interesting to mathematicians. I was surprised, however, to find that RAND, this organization that worked for the Air Force, was publishing a research memorandum by Tarski which presented this result, because it didn't seem to me to have much directly to do with strategic bombing or anything of that sort. I was struck, for one thing, by the fact that for some very basic research it was possible to publish, and to engage the interests of some of the people there, and that the terms of reference then were quite broad, and a great deal of discretion was given. That was the first impression I had. The second impression I had was that there. And then I met some of the physicist and some of the economists and they were really excellent, very first-class people.

Dr. Tatarewicz: Was this after you had come to the area that you began to get this picture of RAND? Had you known much about it before?

Wohlstetter: I had come to this area not because of RAND. I had come out here, oddly enough, to tool up an industrial plant. I'd come out here in 1947 for that purpose. I was a plant that was going to produce modular panels for a prefabricated housing system devised by Walter Gropius of the Bauhaus and Conrad Wachsmann, also formerly of the Bauhaus. It had been an R and D project in the war plant that I had been production manager of, first in charge of mathematical quality control, and then in charge of production.

I was living in Santa Monica, I guess, in the first house Roberta and I got, and we just ran into Abe Girshick, Olaf Helmer and Chen McKinsey on the street, and they were overjoyed to see us. Mathematical logic was a very, very small world. There were only a little over a dozen mathematical logicians before the war in the United States, and two jobs in mathematical logic. For the rest, you had to teach either calculus, as Chen McKinsey did, or philosophy or something of that sort.

So that was when I first heard of RAND, and they enthusiastically tried to recruit me and Roberta, and did recruit Roberta. If I hadn't had this job that I was doing, I'd have been gladly recruited, because I felt very nostalgic about research. By this time, I guess you had Korea, and then a big expansion of interest and an expansion of my own interest also, in public affairs, in this matter. And since Roberta was working there--you ought to really have her in on this--I got to know the people. Then Roberta, knowing that I wanted to get back to doing research, was thinking of ways of entrapping me into staying in Los Angeles, where it was easier to raise children, and pleasanter all around.

Roberta Wohlstetter: I see you're taking my name in vain.

Collins: Please sit down.

Wohlstetter: Roberta got Charles Hitch, who headed the economics division, to ask me to lunch to see if he could persuade me to at least do some consulting. But meanwhile I had learned what was public about RAND. I've forgotten when John McDonald's article appeared, but I have a footnote in here [M-953], so we could tell. Then was that?

Collins: It was 1951. I don't have the exact date.

Wohlstetter: Then I had known about it earlier. But I learned a lot about some of the personalities that were involved, and the unclassified problems that RAND was working on. I was struck by the fact that it was very wide-ranging, that the research seemed to be very freewheeling to encourage people to think inventively. The mathematics department, which I knew best, and where I consulted first, was one of the best mathematics departments in the country. It was just very good. And it was [John] von Neumann's department, so to speak. He was the godfather of it.

I had started to consult with RAND in 1951, while I was nominally in the mathematics department. It was in a special part of it that had been concerned with logistics, with an applied mathematician who was very interest in system problems and in dealing with interconnections of things, which I have an interest in. I consulted on more or less methodological matters for a while, but I've always been a little suspicious of methodology when applied to places where what's needed is actual theory and empirical work. It's a sort of substitute for getting into the mud and working through the problems.

Charlie Hitch then at one point called me in to say that they had just had a request from the assistant for bases, and he wondered whether I would be interested in working on the question

that they posed. And that was another thing that I found very attractive at RAND: at least at some level, you were able to decide whether or not you wanted to work on a problem. And if you thought you wanted to work on something like that problem, you could redefine it. And that's what I did. I at first thought that it sounded very dull, like logistics, and then I spent a weekend thinking about it. I though I didn't really know anything about nuclear war and I had a consuming interest in trying to understand it, since it was clearly of great moment; and that this problem area was capable of raising some of the most essential problems in connection with a nuclear war. At the very least, I'd be able to think about it in a fundamental way, and I would end up understanding the problems of nuclear was a lot better--the most important problem being how to avoid it--to go beyond the sort of bague things that you learned in the papers. So that was the second sort of thing that I found; it wasn't just that you could work on things that seemed remote from the direct problems of the client, but it was possible to have a great deal of independence in defining the problem that you wanted to work on.

Tatarewicz: At this time you were still a consultant?

Wohlstetter: Yes. I remained a consultant. People would be rather surprised to realize how long I was a consultant. I was not yet committed to staying in Los Angeles--that was just Roberta's plan for me. And so I wanted to have just one foot here in this, put my toe in to see how the water was. And second, I had some other sorts of consulting jobs in industry I was winding up, and they were more remunerative. I thought that RAND would give me a chance to do something that I was fond of, something much more closely related to my professional skills and interests, and on problems of great public importance. It would be less remunerative but it would have that as a advantage. That was the way I started.

You'd have to check, but I believe it was only in the course of actually briefing the Base Study that Charlie Hitch said, "You know, this is absurd. You're still a consultant and I think that you ought to think about becoming a member of a staff." And it is my recollection that that was about June or July of 1953. I began with the briefings in April or May, 1953. You've probably heard that these expanded to become a huge series. But I remained a consultant that long, and I exercised even more freedom than RAND staff members might have exercised at the time, in the sense that I was kind of an outsider, and so I would go around asking questions of people in all the divisions at RAND--odd questions, not the sorts of questions they'd been asked before.

Collins: Was this kind of questioning among the division something that they readily accepted?

Wohlstetter: Yes. Well let me say something about that. Just glancing through the memo, I realized that it was written also before I had returned to the university. I realize I can now fix more precisely some of the facts. One of the things I also learned--and I had it explained, I remember, by Igor Ansoff, who was an applied mathematician working in that same part of mathematics that I first started consulting with--was the pecking order at RAND, and that the most important people there were the project officers leading the systems analyses. These were interdisciplinary, generally large studies, and they brought people in from several different divisions. RAND didn't have just one engineering division. It had an aircraft division at that time and a missile division under Jimmy Lipp, who was one of the key people in the space program. It had a mathematics department and economics and so on. In general these were the people who were mustering resources at RAND and focusing them on evaluating some alternatives for policy, and so they would be therefore the cutting edge of RAND, so far as policy was concerned. Not all the division took parts equally by any means, as it was obvious that they wouldn't. But in general, mathematics would provide some people who might work on the model that was used. Sometimes a mathematician led this study, as Ed Paxson led what may have been the first bombing systems, which resulted in the recommendation that the Air Force buy turboprop bombers. The Air Force didn't adopt that, but the Russians did with the Bear.

That had, I think, a great methodological significance, that study. Paxson in many ways should be credited with having invented systems analysis: the bringing to bear of research on a lot of different variables, and then unifying the work in a mathematical model which simulated combat and gave you results as to the potential performance of various alternative systems. I think Paxson deserves the major credit for it. Frequently there would be a collaboration of more that two people from two different departments: Steven Enke of the economics department and Ben Rumph of the aircraft division, for example, collaborated on another bombing systems analysis. So I had it explained to me that as far as relevance is concerned, this was the sort of thing that RAND was most about. And that the project officer or systems analyst who led a systems study was the prototypical RAND man. So that was the third that I had learned.

I guess then, when I decided to think about these problems, I read various RAND systems analyses to see that they were like, the methods used, the sorts of question addressed. And I decided that the sort of questions that I found most relevant were more

complex, and went beyond the systems analytic methods that had been developed. I described the methodology of the Base Study in a lecture that I gave beginning the winter after the Base Study, and that I gave year after year for quite a while, in Ed Quade's course called "An Appreciation of Systems Analyses."

Collins: In your memorandum, just to digress for a moment, you give a lot of credit to Quade in terms of developing the systems analytic approach.

Wohlstetter: Ed Quade deserves credit, I would say. The originator really was Ed Paxson, who was quite brilliant, much less sober than Ed Quade, and whose work I think primarily--and this is not to put it down--had a methodological significance. He showed that it was possible to bring together a lot of different variables. You had to get aeronautical engineers to develop curves showing how aircraft weight would increase with range for a given altitude, speed, etc., and you had to get physicists to tell you something about weapons weights and weapons effects. You had to get economists to tell you something about the industrial target system and how it functioned and how it might be damaged and what the effect of that would be. And then to weave that all together into a model which would give you concrete results. I think that Paxson developed that. Now, in fact, Paxson was not as thoughtful or as good at dealing with the actual problems. And when I looked at Paxson's work, I noticed that the results in some ways were arbitrary. He varied all sorts of things, a million variations of this and a couple of million of that and what not. But there were some things that were left arbitrarily fixed, like the radius at which the aircraft operated, which was 3600 nautical miles, as I recall--I haven't looked at this for 30 years or longer. And that was determined by not looking at actual targets in the Soviet Union, but at some nominal target there and a nominal base in New Foundland or some place like that, measuring it. And then there were all sorts of variations of everything else. But that radius wasn't varied.

But if you look--and since I was about to examine base systems, I looked--to see why he chose those. You don't just have one base and one target. What if the radius that was required was something much more extended that that? Or what if it was smaller? It turned out that you have curves that for turbojets, which he was comparing with turboprops, vary with radius differently from the turboprops, and there's a kind of crossover. And if you chose other radii, you got different preference, and so forth. So in the end, I thought that Paxson's was an extremely stimulating and useful thing, but that as far as the soundness of the results is concerned, I doubted it. You'd have to really make a very serious study of basing, and

distribution of targets and so on, in order to get a sound result.

Quade was a methodologist too, and was not really interested in determining whether turbojets were better than turboprops or something like that. He was interested in developing or showing that a simple differential equation model could pretty much approximate the results of the much more complex model (also developed by him) that was being used in the missile versus aircraft systems study. So you want to think of systems analysis in terms of the methodology, and in terms also of the conduct of the systems study which really affect policy. Both Paxson and Quade were more on the methodology side. Though Paxson appeared to be talking about policy, Quade was frankly concerned with methods. And Quade had very good, sound judgment in seeing what sorts of models might actually work. He did a very big, complicated computer model for the missile aircraft systems study, and then thought: isn't there a simple analytic model which give you more or less the same results? And that was typical of Quade. He was not concerned with the substance, but more with the method.

Collins: I'm wondering. It sounds like your approach was initially to do a critical analysis of the assumptions.

Wohlstetter: I was trying to understand systems analyses, the methods that were used. I recommend that people read all the RAND reports for the Base Study, and that's because I think you really need ore than an oral tradition. You want to develop some sort of critical methods. Now, I had my own predispositions. I believe you basically start with the same problem that the Air Force or your client starts with. Not the same <u>questions</u> necessarily, but the same problems. The Air Force wants to develop a base system. It's going to face a developing Soviet threat, which was going to be increasing, clearly, since the Soviets had acquired the bomb not long before, and had a big expanding program. The Air Force formulated the problem in terms of an emphasis on target destruction, you might say, but without having refined the question very much.

I was interested in looking at that problem, and I looked at the globe a lot and read hearings about the bases and so on, and found out the sort of problems that people were running into and what they thought the problems were. And I looked at RAND systems analyses basically to see just what methods they had used then. In the bombing systems analyses, they were all studies of how you penetrate defenses using alternative sorts of airplanes and so on. Whereas I though with bases, well, what's their function? What do they do? If your base is up close to the adversary, that means you may be able to get more advantageous

points of entry into defenses, or perhaps smaller planes. That sort of thing. But if you were close of him, he'd also be close to you. And since you might be a target, too, you had to think about that. So I thought of it just in this way, and saw that there were two sorts of things that work in opposite directions: one, that there were certain advantages that came with being up close. But there were certain disadvantages, too, because then he was up close also. And if it were all two points in some sort of abstract space, why it wouldn't be a very interesting problem. But in fact, it was not that at all.

The target systems were a very complex structure. So was our potential base system. And there were various functions that you could perform back far. For example, since weight of aircraft begins to increase exponentially with distance, if you could take on fuel up close, then that would be an advantage. Then you could avoid getting out on the steeply rising part of the J-shaped curve of fuel weight rising exponentially with increased distance. On the other hand, if you could keep your initial bases back far, you could, within the rear contiguous radar cover, avoid some of the disadvantages of being close--namely, of being subject to his attack without warning.

So what I did was think about it in physical terms of this sort and noted what made it an interesting problem. The reason I had been wrong in saying it was uninteresting was that intellectually there were some things that seemed to be working in the opposite direction, and how it all worked out in the net would depend on a very detailed and close analysis. I looked at the other studies, not from the standpoint of just being a critic of them--they were, after all, completed--but from the standpoint of understanding what they did then noticing in some cases that if I did that, I could kid myself on the results.

I have regarded the most important phase of any inquiry as being in the framing of the question. I think that's where you have the greatest difficulty. If you want to try to frame a question which preserves what's important in the problem but also makes it manageable, in some cases it will require some simplifications to make it manageable. But if you simplify it by really making it irrelevant, which is the usual way, them you've missed the point. So I think that, looking at the problem, trying to phrase relevant questions, relating what you're doing to some sort of basic considerations of what you really want to do, what's the objective, are key. I could give examples of that for space systems today.

TAPE 1, SIDE 2

Wohlstetter: Because I thought that this was the way to go about any problem as complex as this, and sometimes as deep, one of the things that I found attractive at RAND was that you didn't have an urgent deadline. It was not like a government problem, where you have to get something out in two weeks. And It was not like the advisory committees, which did studies generally; or the summer studies, which are held when classes are out and you can go up to a nice place on Cape Cod or San Diego, and you have to finish everything in the two month's summer vacation. This was a problem which I recognized was going to take me a while to understand because I'd have to understand the operations of the strategic force. The operations of the strategic bomber force are much more complicated than you might suspect from the sorts of models that dominate the field in studies of deterrence today.

One of the disasters that happened in the field, you might say, was that the ICBMs[inter continental ballistic missile] seemed to make the analysis very simple because you didn't have to worry about defenses, you didn't have to worry about aerial tankers to refuel things, you didn't have to worry about overseas bases. You just launched a missile, and no defenses would intervene, and so on. And the second disaster, in getting people to be thoughtful, was the RAND bomb damage assessment wheel. It was a little circular computer which made anybody with that wheel an expert. And that meant that people were able to get results that sounded very impressive. You could testify to Congress or give an interview to the New York Times without ever understanding anything very much or thinking very hard about what's it all for and in what contingencies would you be called on to use any of this stuff and so on.

But what I was struck by at RAND at that time was that here was a complicated problem. I was going about it in a very unorthodox way. This was noted by Robert Schairer, the head of the aircraft division, who noticed that I was asking all these questions and then I'd go do my own aeronautical engineering. I noticed that a lot of the equations were in families of curves. A lot of them were just substitutions in the range equations of [Breguet], a French aeronautical engineer, which involved things like specific fuel consumption, lift to drag ratios, and so forth. And if you could handle logarithms, well, you could get whatever the perceived opinion was on specific fuel consumption for this and that and you could vary it yourself. Well, that kind of worried hem. Let alone here was this fellow going off by himself and doing these things.

It is my view that it's very important to have interdisciplinary work, but I was not really congenial to these

huge teams. I think every project leader should be a kind of unit-mixed team in the sense that he should be able to do a lot of this stuff himself. He should then use a few people in a kind of gingerly way, make inquiries very widely, but go beyond the range of questions that they were thinking about.

Now, as I mentioned, I think today that when I look at, say, space programs, my approach is exactly analogous to what it was then. Namely, I find that the questions people have asked themselves generally have been, what is the optimal system cost, what is the least system cost for performing these space functions given the sorts of resolution you can get, and if you have very fine resolution, them you're not going to cut as wide a swathe. So you balance one against the other, pixels and focal length, and things of that sort. But not much critical attention tends to be given to under what circumstance you are doing this. Is this just a peacetime function? What objectives are you talking about? Do you want to have a robust capability which can last during war? Now, if you have actually marvelous resolution, as we do--let's say in microns--when it's that good, it's very relevant for monitoring the throws weight of a missile in an arms agreement--minor variations which may have no military significance, but which are specified so you have to determine if they're violations or not. You can then do trade-offs and so on about how often you want to be able to orbit, and what sorts of resolution, and look at systems costs. But you have to remember that if you got engaged in an extended non-nuclear conflict with Soviet union, you wouldn't really be worrying about whether he was violating an arms agreement, violating SALT [Strategic Arms Limitation Treaty] I say. You wouldn't need that sort of resolution for that purpose. There would be something you'd need which you might not be able to get with those few very powerful satellites that you use for the other purposes. So I find that formulating, thinking hard about the questions that you're addressing, is still the most important thing, and that was my view really from the very start at RAND.

Collins: Was that view shared by other RAND staff?

Wohlstetter: I think that there was a tendency at first, in the initial years of RAND, to take the questions more or less as they came, to modify them some so that they were more manipulatable. But there was less thought given to how you introduce the problems in the real world that these questions should address. And I think the systems analysis course which Quade ran, beginning in the winter after the Base Study, had a lot of influence in this respect. Quade gave a lecture in which he described the Base Study in careful detail, using it as the major example of a systems analysis. And I gave a lecture in which I contrasted systems <u>design</u> as a process for inventing systems

solutions for helping a government or other organization solve its problems, in contract to systems analyses which stresses comparison of off-the-shelf alternatives as a kind of judicial process of choice among them. Analysis, of course, is essential in both cases, but it is subordinate to design in a systems design. The Base Study, as you know, was probable the most successful study that RAND had done. It meant nine billion 1953 dollars in savings and an enormous improvement in effectiveness, and a decrease in vulnerability. Nine billion 1953 dollars is like nearly 40 billion dollars today. So that was more than I earned in a week. And I was sorry I hadn't taken it on a commission basis. In any case, its success made many thoughtful people look in a friendly way at the methods that had achieved these results.

And now I wouldn't want to lead you to believe that everybody at RAND was a genius or anything of that sort, because that's not true of any organization. There were a lot of good people and there were some very brilliant ones, and they were much fun to work with. Thoughtful ones agreed very much with our emphasis on design and invention rather than analysis of received alternatives. Perhaps the Base Study was one of the things that led to that point of view.

That was the predominant point of view in the Quade course, "An Appreciation of Systems Analysis," which wasn't a course about how to teach Air Force officers how to do a systems analysis. It was to get them to understand what systems analysis was about and how to understand a systems analysis, how to tell the good from the bad, how to ask questions and look for deficiencies and see how it could be related to their work. And I think--I haven't look at the book for a long time--that was the sort of predominant view, and Quade's introduction to it used the Base Study. He went through the whole Base Study, to use this as the principal example, to explain what systems analysis was to the Air Force and how to deal with it. That was the point of view that I think was pretty much presented in Quade's course. There is no way of judging how much that was influenced by the small group responsible for the Base Study--by Harry Rowen, Bob Lutz, Fred Hoffman and myself. The Base Study, because it was a good example of working on a problem of great importance involving huge sums of money and the feasibility and stability of deterrence and so on, and the fact that it had approached the problem in the way it did, was a help in propagating this way of looking at systems studies as designing systems to solve problems.

I haven't had time since to do much consulting with industry but I've consulted with various parts of government. The tendency in government and in industry is still to do a quick

study in which you look around for some model that's near at hand and you don't have the time to think very broadly about it. And so for the SDI [Strategic Defense Initiative] studies that were first done, in my view, that was precisely what was wrong, on both the proponents' and the opponents's side, was that there wasn't enough thought given to what you are trying to do and what contingencies are relevant. And you'd have very preposterous Soviet attacks assumed by both sides. But if you look to see what Fred Hoffman did in his panel, he raised questions that were more fundamental. And it's only now that Hoffman's sort of results are beginning to put that issue in perspective. It's only now that the government has really come around to looking at SDI in much the same way. So I would say, that sort of view will never be universal, because people are working against deadlines.

Collins: Let me just take that point a little farther. Your Basing Study was done in 1954?

Wohlstetter: No. The question was posed to me in May of 1951.

Collins: The complete version was--

Wohlstetter: Well, I'll tell you the story about that. The report was finished on March 1, 1953. We began the study in May 1951 and R-244-S, the 30-odd page summary report, was published in March 1953.

At the time the staff report was published, there was a draft of the entire final report which was very thick--it came to many hundreds of printed pages--and minutely structured to deal very systematically with the issues by a method of successive approximations. That study was presented to an ad hoc committee of the Air Force to evaluate in 1953. About June they began to go over it, and they wrote their evaluation of it by October, as I recall. The Air Force accepted it after this long series of briefings in which we deliberately went to every part of the Air Force, every subcomponent that was likely to be a player and to have different views.

Then after the Air Force Council, which debated it for several weeks in my presence, accepted it, it was acted on. I then decided to make it easier for it to be implemented. We changed the wording. We were comparing several systems: (1) the system that the Air Force had programmed; (2) an improved operating base system because the Air Force was going to operate from overseas after the outbreak; (3) an intercontinental air refueling system; and (4) an intercontinental ground refueling system, in which overseas bases were used as ground refueling stations, if possible only with post-strike ground refueling or, where they were very extended, with pre-strike and post-strike

ground refueling. So we changed every mention of that four-point comparison to read as if it were comparing the "formerly programmed system' with three other systems, so that there wouldn't be any partisan use of the results to say "how could the Air Force be so dumb" or something of that sort. Because, of course, it wasn't a question of stupidity at all. It had been a very complicated and fundamental research and design problem. And we deliberately delayed publication of R-266, the final report, until the Air Force had had time to begin exercises and to begin to implement the recommendations we had made.

So R-244-S was released March 1, 1953. What the "S" in R-244-S meant was "Special"--Air Force eyes only. The reports with an "S" after them weren't listed even in the RAND catalogues because RAND didn't want anybody to ask for them even in classified form. The result has been that there's a mis impression that the Base Study was finished in 1954. When it came out, I was already hard to work on the successor system which dealt with command and control and missile and proposed the silo and fail-safe and other things.

But it's one of the problems in the history that if you're interested in changing policy you frequently have to avoid taking complete credit for what you're doing. What you want to do is get a change made, and if you can persuade the decision maker that it was his idea, that's even better. And you certainly want it blatantly clear so that bureaucratic adversaries of the services will not use this as a sort of critique. Our purpose wasn't to act as critics, but to suggest the best policy. But if you do this, then you frequently have to obscure your tracks somewhat and that's the way we did it. That means that in history there are an awful lot of things which you can be misled by if you read only the easily accessible public record. But the actuality is quite clear. It left many landmarks: the air Force Council Decision and the report of the Ad Hoc Committee of the Air Staff, which had been appointed to evaluate the Base Study.

The essential research had been done and the report completed by the spring of 1953, when the Summary Staff Report R-244-S was distributed. It was agreed I would not go on a trip to brief the results of the Base Study to the Air Staff until we had a complete draft of the final report and not just the Summary Staff Report R-244-S. We wanted to make available to the Air Staff the detailed study in typescript for examination while we made a series of oral presentations.

Collins: That's very useful to have that caution in that explanation of the history of it. What I was starting to get at was the memo that you wrote on the research activity at RAND was done in 1960, and the Base Study was done much earlier and served

really as a kind of beginning model for systems analysis in a very broad sense.

Wohlstetter: The broad conception of systems analysis treated it as a problem of systems design.

Collins: Yes. But it's clear that the underlying theme of this 1960 memorandum is that we need to be careful about how we do systems analysis.

Wohlstetter: That's right.

Collins: So there's a sense in which, this period of time--a course you've cited a couple of modern example where the full concept was not even applied at a place like RAND--where there were the time and the latitude to do a full investigation. That seems to be the implication.

Wohlstetter: That's right. It's a hard line of work. For one thing, remember, the purpose of systems analysis is not publishing a book; you're trying to get a policy changed. It's not enough to be right. You really have to have answered every question, legitimate and illegitimate, that an important player has to have, or makes. I have said this in several places, but I'll say it again. As you can tell from the memorandum, I do not have and did not have the sort of arrogance about the military that almost inevitable some intellectuals have. I found the questions that I was asked in each of this enormous number of interviews to be very serious. That is to say, these were fellows who had responsibility. But some questions seemed almost laughable. For example, given the scope of the study and the huge range of questions that it dealt with, once I was asked: "What's this going to mean for me?" Whereas frequently in intellectual circle--you give a talk at a university or at RAND, for instance--the questions by the Air Force a tremendously useful thing. And it's you ability to answer the questions of the people who are really going to affect the decisions that's the key.

I could give you a long list of things I still remember. There was a Major Hanley who showed up not only at the directorate of operations briefing, but at the directorate of intelligence briefing. He showed up again and again, and he always asked the same question, and I got better and better at answering that question. I said that it turned out that this intercontinental ground refueling system not only was less expensive, but it got off more sorties of aircraft than the overseas operating base system. Now, this struck Major Hanley as being very strange, offhand, because if your aircraft are very close up, War Planning Manual 50-A says you can get seven sorties

a month; whereas if you're back in the continental United States, you only get off two. So he said, "How is it possible?"

Finally, I think about the fourth time he showed up, when he asked that I took him very seriously and said, "Look, if you have an overseas operating base, you have to buy a large infrastructure--you need the overseas base and the warning and active defenses for it, in addition to what you have if you've just using the base for ground refueling, which is the bare thing. Out of a given budget, that means you're going to buy fewer airplanes. That means that those planes that we don't buy don't get off any sorties. Second, you're still going to get close up, and so for all the ectra defenses you have and so forth, you'll still going to be a lot worse off and suffer more attrition on the ground than if you were all the way back in the United States within the cover of our continual defense system. Those planes that get killed on the ground don't make any sorties." And I gave him about four points like that. When I finished that fourth time, he was completely happy. Well, that is more important, if you want to get policy changed, than just publishing a book, we'll say.

I think the process of doing systems analysis really involves making sure that you understand the question, the vague problems sometimes felt, and the serious ones. And being able to answer them and to do that convincingly, because not many people are going to read a 5--=page volume that is printed--not just typescript, but a 500-page <u>printed</u> volume, closely reasoned and in great detail. You have to be able to answer the questions that each important player has. And that's again something that takes a lot of time, and it's not something you can easily do in a summer study, or in most of the advisory committees. They listen a lot. They don't do original work. They listen to a lot of briefings of studies that were already made." But that's a different line of work. I don't say that it isn't something that's also use, but it's a different line of work from doing genuine systems analysis--which i prefer to call systems design to stress that the problem, which means you have to design alternatives. You don't just take alternatives off the shelf and make a judgment. You have to invent.

I've had a lot of personal experience since at universities and the way you can do systems analysis there. I taught at several universities, and spent fifteen years at a great one, the University of Chicago. Chicago is a place where, much more than in other universities I've seen--in comparison with Berkeley or Stanford or UCLA or Harvard-- it's easy for professors to get to see each other. Therefore, If I had an economic problem and wanted to talk to Harry Johnson, one of the most brilliant theorists of international economics, I could call him and I

could see him for lunch and pose it to him, or try out something. Or if I wanted to see a geophysicist, I could do that too.

In a university in general, and that includes the University of Chicago,f you can do that at a lunch. But you can't ask somebody to take time off from writing papers in his field and really spend some time working on a problem that's one essential component of what you're doing, but not an essential for him. It's very hard. At a university they're organized by disciplines, and so what I say about the departmental organization at RAND--which I think is important for keeping up professional qualifications--at a university, all the means of advancement are on the basis of publication and with your peers in that discipline. If you're in another discipline, you're kind of a distraction.

I worked on race differences in income, looking at income distributions, and I had some ideas in the mid-sixties which were novel on some of the new measures that might be illuminating. 'Well, that was a popular subject at the time. Nonetheless it was inconceivable that I could go over to any of the mathematical statisticians at the university and say, "Here, I have the idea. Can you spend a couple of months investigating this?" And so it's very hard to get that sort of interdisciplinary work that you could get done in RAND at that time.

Tatarewicz: And what allows you to be able to get that at RAND? You mentioned the strength of the project leader in the RAND pecking order, yet that all sounds a little coercive.

Wohlstetter: Well, you see, it's not so much rape as seduction. Remember, that's the point. The most successful project, you could go about it by negotiating with each of the department heads, and if it sounded important enough and it was plain enough, you might be able to get quite a large crew. I worked by myself for a while. And then there were two young guys in the cost department. Harry Rowen and Fred Hoffman. I would go around and talk to them, as I would go around and talk to Ed Quade or Bob Specht or Lloyd Shapley in the mathematics department, and I would pose questions. I didn't know anything about cost in this field, so I'd questions. But thin I would also try and intrigue them, because it was a new kind of question which they hadn't been asked before. And they would go and work on it just on their own. They were volunteering, you see, for some time, and then they formally joined in. Well, this is the process of seduction by intriguing people in a problem. But it was easier to do that at RAND than it is in a university. You can do some of it, of course, within a university, but it was easier at RAND. I certainly benefitted by the success of both studies and a lot of things adopted as a result. And obviously

the studies were interesting and solid, so that gave me a lot of prestige.

I don't know how easy it is now, but at the time, people weren't as worried about "What project shall I charge this to?" But the Air Force was a new service, and it was clearly the service that was growing and most important. The Navy had had a solid intellectual backing for a long time. For the Air Force, a lot of this had to be created. So that was one of the reasons that they gave RAND a lot of latitude, I think. Then, when some of the decision began to go against the Air Force--and the budgets were tighter-- I think there was tendency to look more sharply. That was sort of the occasion for this memorandum, you see, because there was a feeling of budget tightening, and Ed Lindblom's pieces [2], which I refer to, were really very brilliant. Now, as you see, I disagreed with him in a very, very fundamental way. You know who Ed Lindblom is, C.E. Lindblom of Yale. He had written a very brilliant wayward paper [3] called "Bargaining: The Hidden Hand in Government," sort of the idea that in government there's something like Adam Smith's market working, just in the bureaucracy, as a kind of hidden hand in which, like the fable of the bees, the private vices are public virtues. When a key person pushes his own facts and his own interest as fact, it all works out. Now, as I indicated, I have my doubts about that sort of thing, and that was the reason I wrote M-953. But it's a brilliant series of memos, because Ed was certainly a very provocative and interesting guy. But there was a lot of turmoil at RAND at that time, and that was the reason I was concerned. I was not interested in any experiment in advocacy, you see. As they say, there are no experiments in the loss of virginity.

I haven't been formally on a contract at RAND since I left it nearly 25 years ago. I have many friends there and I have very friendly relations, and sometimes at RAND I will have exchanges with people. RAND is doing work in connection with the Integrated Long-Term Strategy project. But in the fifties, I can testify, it was probably the most exciting place to get in the country, not because everybody there was first-class, because that never happens. Even in a platoon, studies have shown that most of the firing is done by one or two guys--Hamilton Hause made such once in Korea. But there were enough people so that there was a sort of critical mass, and it was possible to have very wayward ideas, and have a chance to show that they weren't as crazy as they seemed. It's very hard to create that precious

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[2] M-7486, 7487, 7489,7490, 7491, Dec. 19, 1959.

[3] RM1434-RC, February 22, 1955.

atmosphere, and it requires really a certain percentage of first-class people who are venturesome, and the ability for them to interact. But it also requires a body of solid work, because none of this would be possible without being able to draw on a lot of component work that's being done by various professional disciplines.

TAPE 2, SIDE 2

Wohlstetter: Even before I left RAND formally, I was spending a lot more time outside of RAND. This was partly because I avoided publication while I was at RAND, until "The Delicate Balance of Terror," which I published in late 1958. It came out in the so-called January 1959 issue of Foreign Affairs. But I was spending a lot more time outside of RAND and outside of RAND clients, partly because of the facts that the problem I was working on now had public prominence.

I had briefed the second big study that I had done, Protecting US Power to Strike Back in the 1950s and 1960s, which I led with Harry Rowen and with Fred Hoffman again, but without Bob Lutz. That dealt with a series of questions which in some respects were left over from the Base Study, and some of the hardest ones--like how you deal with the command and control problem, and the problem of going to war by mistake. This first led, among other things, to our proposal for the silos, and the second led, among other things, to our development of fail-safe procedures and improvement in warning and repeatable responses to ambiguous warnings and so on.

This reaches, not quite the public, but the defense community more broadly, as the result basically of my briefing the Gaither Panel. The Gaither Panel had started as a response to a proposed 50 billion dollar FCDA [Federal Civil Defense Agency] shelter program; that is, a shelter program which was proposed by FCDA which would be partly private but mostly public, but in total cost was 50 billion dollars. This in turn led to the proposal of a study, what became the Gaither Study, which would deal with how active defenses, combined with passive defenses (the shelter program), might work, with the active defenses simplifying somewhat the problem of civil defense.

Rowland Gaither was the chairman of the board of trustees at RAND, and he had heard me brief both the Base Study and the second big study, R-290. He called me to ask what I would suggest, and whether I thought the terms of reference were good. I proposed that the Gaither Committee broaden the terms of reference to consider not only active and passive defenses, but to consider the problem of the vulnerability of strategic forces

and the problem of the deterring attack, and that was the reason they broadened them. And then I briefed them. The Base Study in another way had inspired some of the questions that were posed in the Killian Panel in 1955, but they weren't very well understood in the Killian panel, in my view. But the Gaither Committee was much closer to it, and brought these questions into the interagency context. It led to the Conference on Reducing the Danger of Surprise Attack, the ten-nation conference. And I was deputy chief science advisor there.

I was spending a lot more time outside of RAND, and then I was becoming, not entirely willingly, a public figure. So I was spending a lot more time away from RAND on that. First I was going to take a year, but then I only took a few months at the Council on Foreign Relations. Then I taught at UCLA and , most important, at the beginning of the Kennedy Administration I became Secretary of Defense Bob McNamara's representative on the Acheson Committee of the National Security Council to reconsider policy on Europe. I did the first draft for the Acheson Report, "Ending Massive Retaliation." So I was spending a lot more time outside of RAND on that.

And there was also, at the end of this time, a big budget tightening, and there were a lots of roles and missions questions, and a lot of soul-searching therefore as to what RAND's roles would be. And the picture I've painted I think is essentially right. But as this long memorandum suggests, you had to realize that it was a very imperfect system nonetheless. It was an exciting place to be. You could do very good work. On the other hand, there was some good work that got neglected. There were some good people who didn't ever get to do the good work they might have done because they fell by the way-side. And the set of problems that I'm referring to there were all based on seeing how we could increase the incentives for doing inventive work on systems, not just hardware. How we could encourage that sort of thing, in a period when there was concern about whether might get harder than it had been.

There had been two studies, one a joint penetration study, and I've forgotten the name of the other one but I mention it in the memo, which worked on a very different principle from the way RAND did normally. Good or bad, systems analyses were done by RAND people. There would be Enke, Rumph, Paxson, Ed Barlow, doing an air defense study and so on. But the joint penetration study was done in collaboration with the Air Force and it was done on a time schedule. Now, that's much more like the sort of thing that gets done in IDA [ ], WSEG [Weapons Systems Evaluation Group], you see, in which you're working on some things with the military man who represents the customer. That sort of thing also happens in quite a few places, and it's harder

to get an idea. It discourages getting some sorts of ideas which are wild and whatnot. And so I was worried about that, and I didn't think it was a very good study, the joint penetration study. I don't want to imply that there was anything dishonest or anything like that, because there wasn't. It was just that that sort of collaboration doesn't encourage the sort of privacy that you need for having ideas in a very tentative state. That's a very precious moment, when you're departing from the norms, and you really can't defend some of the wild ideas. Some things seems so obvious today that it's hard to believe that they were wildly heretical at the time.

The initial idea for one very important component of R-290 was the idea for the silo. Now, that came at a point in which people suddenly realized you were going to have H-bombs in ICBMs, so even though they were inaccurate, they would have such a huge area of devastation that they would destroy the target. On the other hand, getting an accurate defense against them--because of the enormous speed with which they trace--was going to be very hard. Now, most people were thinking, as they always were, of the active defense of the population centers of the United States, but of course we had a somewhat different view. We weren't opposed to that, but we were thinking about how you keep the strategic force alive and able to respond to attack. And so lying in bed at the Shoreham while I was finishing briefing to the Air Force Council, I was thinking: isn't there some way I could shrink that big bomb? I had heard of Bruno Augenstein's result, namely, that with a recent nuclear test he had showed that you could et an H-bomb small enough to put in an ICBM. I had heard about that, and I was thinking, well, how do you deal with this? And I thought, what had happened was you had something that was wildly inaccurate, a bomb that was big enough to fill that whole area with destruction, so that now you'd be able to destroy missile on the field and so on.

There's a point coverage formula, which is an approximation to the probability of destruction, which says "one minus one-half to the lethal radius over the median inaccuracy squared" is the probability of destruction. Now, whenever you have a squared term like that, that's always interesting to look at. What is says is that the radius of destruction is one thing, but the probability of destruction is going to be much more sensitive to the CEP--the median inaccuracy or the radius of error--that it is to the yield of weapons or anything of that sort. So the fact that you're getting a big bomb, you have a lot of leverage there. If you have a radius of destruction, because you get the bomb very big, of a mile, or say two miles, and you have median errors which are a mile--or as they were thinking of it, two to five miles for the CEP of the initial ICBM, and five miles for the SLBMs[ ] to be sure--you have a radius of destruction, let's

say, of five miles and you have errors of five miles. Then it's one-half, one over two to the destructive radius of a CEP squared. That's two to the one squared, which is two, so the probability of survival would be .5 and the probability of destruction would be .5. That says that, Aha! if I could get a way of shrinking that destructive radius, given the very, very huge inaccuracies in the early ICBMs, well, if I could get the radius of destruction to be half the radius of error, I'd begin to get destruction probabilities which would look outlandishly small. For example, looking at it the other way, suppose I had errors in destructive radius which were twice the median error, then the probability of destroying it would be very high, .94. But if it's three times, it would be .998.

I was thinking, well, that's interesting. Now, how do I shrink that bomb? Could you get a complicated structure with large doors to house aircraft or missile that would resist hundreds of thousands, or higher, blast overpressures and other nuclear effects?[4] But then I went to all of the experts and committee members were off on other problems. They were noticing that H-bombs were different from A-bombs because they not only gave you more pounds per square inch, but they lasted longer. The pulse would be longer. They were looking at how paper houses-well, I'm exaggerating--<u>wooden</u> houses would be knocked down still further than you expected. I said, "Well, here, I have another problem. I'd like to know whether you could get resistances on the order of several hundred PSI [pounds per square inch], or even a thousand PSI or so forth," and I was told to forget it--that on any structure as complex as a silo, with huge doors and so on, you're not going to get anything more than 30, 40 PSI resistance.

So Fred Hoffman and I then sat down and wrote something in which we said we don't know if you can get such high blast resistances, but this is what it would be worth if you could get 100 PSI, this is what it would be worth to get 200 PSI, this is what it would be worth if you could get shelters that would protect their contents, even at the lip of the crater. In short, we did a break-even analysis. And when that came out, there was silence. If we hadn't had this big success with the Base Study

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[4] During the base study, we had looked at the sheltering of strategic bombers as one alternative, but had found the state of the art not very advanced. We also found that if such shelters were feasible, they were neither necessary nor very useful against the 1950's threat of attack by manned bombers with median inaccuracies of 1500 feet. The ICBM threat was something else; it might make shelters necessary because an ICBM would give very little warning and would involve much larger median inaccuracies.

they'd have said, "What the hell are these fellows talking about?" Now, I had to go call a Hungarian-American fantastically inventive structural engineer, saying, "Do you know anything at all about protective construction?" And he answered in his thick Hungarian accent, "Not a zing." "You're my man," I said. I had known Paul Wiedlinger very well. And he then designed a 200-PSI structure with a huge door, and that was the end of the controversy. Now you have 2000-PSI American silos, 10,000-PSI Soviet silos, and Paul has since designed an 80,000-PSI silo. But the point is that there's a moment there when, if you had to justify this before a large skeptical group, there would be no way of doing it. And in fact there was a lot of resistance to Paul's original design although, as I said, it ended this controversy. But there were a lot of people who resisted it to begin with and it took some time before Paul did it.

So that moment of privacy, when you can have a wild idea which may very well be wrong, is an important one because at that point the leader of the study should be someone who just uses his judgement, saying, "This guy has a good track record so I think we'll let him try it." But if you're working with people who are not research-oriented and need results right away, and who have built-in prejudices, then it isn't that they might say you can't--but you tend not to have those ideas as frequently. So that was one of the great advantages of RAND, that you could have wayward ideas, and you could have them in the middle of the night and convoke a very splendid group of four or five people of diverse skills to think about it with you the following morning. You could bring them together. That was enormously valuable. I think that is about the best place I knew of for that sort of thing, dealing with so broad a set of topics at the time.

Now, I can mislead you if you don't remember that there were very different parts of RAND, too. Roberta, who did this splendid study of Pearl Harbor, was in a part of RAND that published a lot. And there were some really very good people there--Nathan Leites, Herb Goldhamer, and so on. But in general, they weren't in the mainstream of RAND in the sense that they would never be a part, or they were seldom a part, or if they were a part it would be a kind of nominal part, of one of these systems analyses in which you might design alternatives. It was partly because that division generally didn't have much mathematical equipment, and so a lot of the things they did, like Nathan Leites's work, was really very relevant, but it was hard for them to bring it to bear. Nathan was the most keen analyst of Soviet, French, German views--a very subtle analyst. But he was not an integral part of the system as an analyst. And Bernard Brodie was not really a part of that either. Herb Goldhamer, as a sociologist, was. And Fred Ikle was different, because Fred as a demographer was able to work with physicists

and mathematical statisticians. He deserves the chief credit for something that I was frequently credited with in various books and so forth, but which he deserves, and that is the permissive action link that prevents nuclear weapons from being used without release from some remote source, as a way of preventing unauthorized acts. That was basically Fred Ikle's idea.

But in any case, RAND had differing sorts of functions, and various parts of it took part in different ways. And some of them were more like a university. For instance, the social science department generally was like that. But you did have some cases, as in the case of Fred Ikle, who could collaborate with Al Medansky, an excellent mathematical statistician now at the University of Chicago, and some people in the physics department and some people at Livermore, on the whole problem of preventing accidental detonations. Unauthorized detonations are a very serious and important problem, and it turned out very fruitfully. But I guess you must have had enough.

Collins: Just a quick one, as a closer. Was there something that you felt in 1960 where you felt some on these attributes were in danger?

Wohlstetter: Yes. Yes, there was. I felt that studies with the Air Force or any other client involved serious dangers to RAND's independence, its willingness to take risks, and its disposition to think up alternatives that were likely to meet opposition at a point in which ideas are tentative and vulnerable. Two such studies, both very massive, had recently been tried.

I think there were always Air Force people there, and that was fine--they were valuable--but when you had somebody who was not stationed at RAND, when you had an official collaboration with a part of the Air Force, I felt that was not good for encouraging inventiveness and creativity. Second, there was the question of support, whether the budgets would be decreased. Third, there was the brief, which you'll still find in the Congress, that invention is something that's limited to hardware and that you need independence for that. That is, a hardware inventor can use independence, but his work is not nearly as sensitive as that of somebody trying to invent a whole new system that's much broader. That involves questioning objectives. And I was concerned about that. There was a study in 1961, as I recall, by the Director of the Budget on the importance of independent policy research. Adam Yarmolionsky, who was then special assistant to Bob McNamara, asked me to write down some of the reasons why independence was itself a very valuable thing. You never get independence once and for all. It's more like staying thin after you're 40: it doesn't take one heroic act, it takes a lifetime of care. I think it's especially true for any

organization that designs broad systems with very ramified effects. It has to be a constant battle for independence, and it's always going to be. Not only will victory never to final, but it will never be complete either. You're never through once and for all; the technologies are changing, the political concerns and therefore the policy concerns are changing. It takes continued alertness to change and its meaning. And then there are some people that don't really want independence terribly much. It makes them feel uncomfortable. But that's human. So, in any case, the RAND that I'm referring to was clearly a place where there was a lot of independence and a lot of opportunity for invention and risk-taking, if you wanted it and were willing to do a little battle and so on.

Collins: Let's close out here. You've been very generous.