Digby

NATIONAL AIR AND SPACE MUSEUM
RAND CORPORATION

JOINT ORAL HISTORY PROJECT
ON THE
HISTORY OF THE RAND CORPORATION

EDITORIAL USE FORM

PREFACE

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James F. Digby
(Signature)

Mr. James Digby
(Name, typed)

2 June 92
(Date)

Digby

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James F. Digby

(Signature)

Mr. James Digby

(Name, typed)

3 June 92

(Date)

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PREFACE

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James F. Digby
(Signature)

Mr. James Digby
(Name, typed)

2 June 92
(Date)

James Digby
January 23, 1991

TAPE 1, SIDE 1

TAPE 1, SIDE 2

Interviewee: Mr. James Digby

Interviewer: Mr. Martin Collins

Date: January 23, 1991

Place: RAND Corporation
Santa Monica, California

TAPE 1, SIDE 1

Mr. Collins: There are a number of areas that I would like to pursue with you, but I think perhaps the best thing to do is try to proceed roughly in a chronological way. To begin with, just a brief sketch of your personal background, where and when you were born, your educational experiences, and how it is that you came to the RAND Corporation.

Mr. Digby: I was born on August 11, 1921 in Farmerville, Louisiana. I went to school first at Louisiana Tech, where I got a bachelors degree in electrical engineering. Actually, it was called mechanical-electrical engineering because I learned how to handle a few large steam-driven machines and I handled lathes and things like that. Then after graduating in 1941, I spent a year at Stanford University with a quite good fellowship. I intended to get a two-year degree in electronic engineering, but World War II came along, so Stanford was nice enough to give a number of us one-year masters degrees in engineering. I got my degree in May of 1942 and immediately went into the US Army Signal Corps as a very green second lieutenant.

I went to Fort Monmouth, New Jersey for a little training about how to march and how to salute and also one week of Cooks and Bakers School, which has left a permanent impression on me. Then I went up to Harvard and MIT, [Massachusetts Institute of Technology] for three months (at each) of training in advanced electronics and exposure--my first exposure to the real secrets of radar. After that, I was posted down in Florida at Camp Murphy near Palm Beach and wrote radar maintenance text books for awhile. Then I went up to Wright Field, where I was part of a selected group of officers with fairly good educational backgrounds who went wherever there was a problem with radar any place in the world. I had two such trips overseas and one in the US. The first overseas trip was before D Day. I went over to help set up the radar in the C-47s that were going to drop the 82nd and 101st Airborne Divisions for the invasion. Then later on I put radar in some PBY flying boats that were used to pick up downed airmen in the English Channel. Then toward the end of the war, I was sent out to Hill Field near Ogden, Utah to learn about installing radar in B-29s for use in the Pacific. After that assignment the war ended and I went into civilian work for the US Air Force at an offshoot of Fort Monmouth, New Jersey, which was called Watson Laboratories of the US Air force at Eatontown, New Jersey. That's the place where I first encountered visitors from The RAND Corporation. I came to RAND on the day before Memorial Day, 1949, into the Electronics Division.

Collins: What was the nature of your responsibilities at Watson Laboratories?

Digby: At first, I helped write instruction manuals on some of the new equipment that was coming along. Toward the end of my stay there I was in something called the Plans Branch, which tried to plan how Watson Laboratories would deploy its skills with particular respect to defending the United States against Soviet air attack. We did some of the very first work on defending the US against ballistic missile attack.

Collins: It sounds like during this period your responsibilities were not of an experimental nature, but you were more on the side of the effective use of these technologies. Is that fair to say?

Digby: Pretty much, although sometimes I would write up the results of the experiments. I probably wrote up the first report on a radar in the United States trying to detect ballistic missiles. But I was not out in the field doing experiments.

Collins: What were your inclinations as an engineer at this point? You were somebody who had a good technical background.

Digby: It was more in explaining and not in experimenting. There was some quite interesting experimenting going on there. In the place where I roomed was one of the four people that first bounced radar signals off the moon. That was considered kind of a very experimental thing to do in those days.

Collins: I'm curious. In the period just after the war, did you have a number of options that you were considering about what you might do in the post-war period? Why did you elect to stay in the Air Force?

Digby: As soon as I was released from the Air Force, I began a trip out to California where, I had in mind possibly working for a movie studio as a sound man. I had been a sound system operator for a couple of years at Louisiana Tech, of a very advanced, big sound system that was installed in their auditorium. I rather liked that kind of thing and I thought I might come out to California and be a movie sound engineer. But on the way here, in Phoenix, Arizona, where I also was looking for a job with some of the electronics outfits that were beginning to settle in Phoenix, I got a telegram saying that I

was offered a job writing manuals for new radar sets at Watson Laboratories. So I took it and went back.
Collins: One of the feelings that you get from many of the people who are associated with the new wartime technologies is that there's a certain kind of enthusiasm for their potentialities and uses in the post war period. Is this a feeling that you had as well at this time, that it was going to be an exciting area?

Digby: Yes. I somewhat preferred working on radar at Watson Labs to working on sound systems for the movies, although the movies themselves had a certain kind of glamour and attractiveness and I rather liked California from my year at Stanford. I had some friends who had been in the movie sound business whom I'd met during the war. I don't remember exactly who they were, but I think there were one or two of them. It was the kind of thing that I would have known quite a bit about.

Collins: What was the thinking at Watson Laboratories in this period about the role that radars were going to play in military thinking?

Digby: We had two very lively people who were slightly senior to me, just a year or two older, and had come there sooner. One was Herb Sherman, who later went to Lincoln Laboratories, and the other was Jerry Freedman. I believe Jerry became head of Lincoln Laboratories. Herb and Jerry were my two closest professional colleagues at Watson Labs. They were very keen minded, system-oriented people. They thought about what Watson Labs should do in terms of looking at the whole picture of the use of electronics in the US. They thought years in advance, as opposed to just solving the problems that somebody in the Air Force brought in. We really were a quite forward looking group in the planning branch at Watson Labs. Also, Jerry Freedman during the time I was there, and I helped him with this a bit, wrote one of the very first technical pieces on estimating the range of a radar set. It was not the very first, but it was maybe the second, and it had more technical material in it that the first one didn't have. And Herb Sherman wrote some rather important technical papers about the same time.

Collins: Now Watson Laboratories was part of the Signal Corps during this period?

Digby: No, it was part of the Air Force. By that time the Air Force was a separate organization and the radar work that the Air Force had taken over from the Signal Corps was put into a separate organization called Watson Labs, but in the outskirts of Fort Monmouth, which was the home of the Signal Corps. Later the Air Force moved the Watson Laboratories research up to Rome, New York and in fact, the threat of having to move to Rome, New York

is part of why I came to The RAND Corporation in 1949. Somehow Rome, New York was not my ideal of a place where I wanted to live for a long time.
Collins: What was the nature, then, of your initial contact with RAND or the RAND staff?

Digby: Well, three things. One, we had a visit from Jess Marcum, one of the initial good technical people from RAND who did the first statistical theory of target detection. Secondly, we had a visit from Al Hiebert, who told us more--his talk at Watson Labs was more about what RAND was doing for the Air Force. And third, I was put on some kind of panel of the Joint Research and Development Board. It involved visiting various places around the country. One of the places I visited was RAND. At that point I decided, "This looks like a good place for me," so I left an application. I must say it was February and it had been a very cold, snowy winter in New Jersey. There was a full moon and I'd heard about La Cienega Boulevard and the good restaurants that were there. I was walking down La Cienega with a full moon shining on the palm trees and smelling the roast beef being cooked at Lawry's Restaurant. I made a decision right then -- why stay in a snowy place like Red Bank, New Jersey or, even worse, Eatontown. I might get a job at RAND. By that time RAND was three months old. By the time I came, RAND was six months old.

Collins: The corporation was.

Digby: Yes. I think the full-moon thing was at the end of February, 1949.

Collins: You left an application and obviously they got back in touch with you?

Digby: Actually, I don't think I left the application but got the form and filled it out when I got back home and mailed it in.

Collins: Were Markcum's and Hiebert's visits to Watson Laboratories also meant to be recruiting efforts in any sense?

Digby: I don't believe they really were. They may have been in part, especially Hiebert's. But it was fairly easy for me to judge RAND because three or four people I knew had come here to work by that time, notably Ed Barlow from Sperry. Ed was a leading figure in the Electronics Division. So in effect, I applied for the job to Ed Barlow.

Collins: Give me a little more detail, then, how the final marriage came about and you did indeed come out here.

Digby: One thing I did while I was out here, in my role of whatever subcommittee I was looking at things for, was to hear a briefing by Ed Paxson which I thought was a pretty impressive tour de force with a very broad systems kind of orientation, which I thought was sensible. So I liked that. I also got to meet four or five of the people, and I thought they were all very smart, people I would enjoy working with. Evidently I had about the right background to make them feel like I belonged here. I knew Barlow already. He's probably the one who basically decided to hire me. I got out here, and it was a very congenial group of people. I was almost exactly at the median age of the RAND Corporation, which was 28, and many of us were unmarried. Barlow was married already and so was his friend John Mallett. It was socially a very good group to be part of. They were smart people and some had rather odd habits, but most of us had been in graduate school a bit and were accustomed to people who were smart who didn't behave according to the norms.

Collins: The issue of RAND culture is something I'd like to go into with you a little bit. Perhaps we can save that for another point in the discussion. What were your initial responsibilities or activities when you came to RAND?

Digby: I was about the only person in the Electronics Division who knew about the military units that an air defense system would have. In other words, I knew that a flight was smaller than a squadron and a squadron was smaller than an air group. I knew things about anti aircraft defenses because I'd been in the Army for a while and then the Air Force, so I was basically the air defense radar system designer for RAND's first big air defense study. I also designed some Soviet radar networks for Ed Paxson's study of US bombers that had to penetrate that network. I was mainly the radar network designer in my first year at RAND.

Collins: One thing that you begin to address in your historical essay which is very helpful in tracing the evolution of the ideas associated with strategic doctrine, is the broadening or the increased appreciation of the wider contexts of developing these systems. When did that begin to become an insight for you? Was this something that you were thinking about at Watson Laboratories? Or was this something that was stimulated by The RAND Corporation?

Digby: At Watson Laboratories I thought of the broader systems as an engineer would think of them, as a big system that had to be designed. The costs had to be kept under control and designs had to be made so that complicated things would fit together. In fact, I remember cutting out a clipping in the <u>New York Times</u> about someone who had made a talk on the design of systems of organized complexity.

Collins: You're talking about Warren Weaver's--

Digby: Yes, Warren Weaver. So I'd been impressed by this kind of thing but always as kind of from an engineer's point of view. At RAND, Ed Barlow was very good at understanding the good points made by the economists and political scientists, whom at RAND we called social scientists. The political scientists were in something called the Social Science Division, and typically the economists and engineers thought the social scientists were long-winded and couldn't really come to very definite conclusions about things. But we found three or four of them whom we could really work with and rely on -- notably, Herb Goldhamer, who was trained as a sociologist. And there were one or two other of the social scientists who seemed to know the importance of numbers and statistics and so forth, as opposed to what we thought were rather long-winded essays about how things should come out. So, very soon after I got here, Barlow began organizing a big air defense study, and I was his number one assistant at first. Later he had Bill Graham [William B. Graham] also as an assistant. I helped write the report on the study. Bill Graham and John Mallett had worked with Barlow at Sperry, and the four of us were all on Barlow's study. We got about five people from the Missile Division five from the Aircraft Division, and two or three economists. So we ended up with about 20 people. The number can vary, by the way. I may have cited another number somewhere else and, the reason is that the fraction of time that these people put into the air defense study varied from month to month. But it was quite a large study and one that was hard to replicate in later years.

Collins: Hard to replicate in the sense of--

Digby: Organizationally. It was hard to find 20 good people from the six or eight skill pools who could be put into a study of the kind that Barlow organized. Paxson organized systems analysis a little bit earlier. He had a very different personality from Barlow, rather imperious. He was a hard drinking man--he would squeeze your hand when he shook it until your hand hurt. He just had a very different approach from Barlow. While he organized a pretty big study, I don't think he ever evoked quite the loyalty that Ed Barlow did.

Collins: There are all kinds of threads here, I think, that are worth pursuing. I guess the ability to bring together a large group of people to concentrate on a particular problem area at RAND was a continuing issue over time.

Digby: Yes, it was.

Collins: This exercise, to the best of my understanding, had a pretty free form character in terms of how it came about. Are you suggesting, in the Paxson and Barlow cases, that the critical element in bringing the people together was management style and personality, as opposed to specific organizational incentives to have an organized effort in this area?

Digby: Well, I'm not sure I can draw that distinction exactly, but let me tell you how it was. In Barlow's case, he had a division chief, Lloyd Young, who was much less forceful than Barlow was. Lloyd sat on the Management Committee and was head of the Electronics Division and backed Barlow very strongly. Barlow also evoked the support of several of the other division chiefs. RAND was pretty much run by the division chiefs, who included John Williams, Ernie Plesset, Jim Lipp, Gene Root, Lloyd Young, Charlie Hitch, and Hans Speier. What really mattered was that Barlow had a plan and a persuasive way of putting it. He talked the Management Committee into supporting him, and he got a lot of support from Lloyd Young. Paxson was himself part of the Management Committee and he had been a division head himself. But somebody decided, correctly, that his personality was not one for administering people or hiring and firing them. So he became a part of John William's division, and Williams supported Paxson. Later, Charlie Hitch supported some of the people from his division who started studies, like Andy Marshall. Williams also supported Igor Ansoff, a mathematician who did a tactical study later on.

In any event, all of the project leaders were people of very high intelligence, with an unusual degree of drive and persuasiveness and insight. The Management Committee saw that it was good for RAND to give these people teams that they could work with. The Air Force at that time was not at all forceful about what the program at RAND should be. That didn't come for ten years or so. Thus Barlow was able to recruit a team that may have had 45 people in it, with something like 20 or 25 full time equivalents. Paxson's team wasn't quite that big.

Collins: Could you describe a little more of the process of pulling the team together? You indicate as a background that there was management support in this particular case.

Digby: Let me just use Barlow as an example. Barlow would get the idea of what he needed. He would work out fairly detailed plans of what questions had to be answered. He would look over the corporation and decide who were the best people he was likely to get to answer those questions. So he would go talk both to the people and to their division chief. He might have got a hunting license first from the division chief, and then he would go and try to persuade the person that his project was one that they wanted to be associated with. So it worked out. He used the combination of his license from the Management committee direct permission from the division head, and persuasion of the person. But it was very seldom a case of Barlow going to [Frank] Collbohm and saying, "I need to get an economist who knows how to do R and D studies," or of Collbohm telling Charlie Hitch, "Find Barlow an economist." That would not have worked too well in the RAND environment at the time.

Collins: How did the individual researcher sitting in his office, assess whether or not he wanted to participate?

Digby: Early on, it was considered better, by most of the people, to work on one of the big systems studies rather than to do your own thing. The people who were really good in systems studies wanted to work on systems studies. The alternative was to do an individual study or a two-man or three-man study. And those were what a lot of people did, notably in the Math Division. There were people writing treatises on various forms of game theory, all by themselves.

Collins: This gets, I think, into the differences among professions in responding to the environment that you're describing. Were engineers by inclination more willing to be a member of a large team than, say, a mathematician?

Digby: Yes. Engineers somehow felt that the important thing was the big system structure and that you had to be on a large team. And also, I guess engineers in other places would normally worked as part of the team instead of as individuals. In the profession of engineering, there's a little bit less emphasis placed on doing brilliant individual work and more placed on doing team work. The economists turned out to be fairly good team people, although they did their individual work simultaneously in some cases. There was a fairly warm feeling between the engineers and the economists. Well, no. I don't know why I said that, because sometimes there wasn't. But both engineers and economists had a kind of economy of verbiage which the social scientists did not have; also the mathematicians had an economy of verbiage, and the three felt rather scornful of the way some of the political scientists would go on at great length without coming to a firm conclusion on what had happened or what might happen. They even wrote parodies about them.

Collins: Let's just pursue this a little further. Let's look at a case in which an engineer might decide that he wanted to work on his own. How would such an individual define a problem area and feel confident that he was making a contribution?

Digby: Well, for example, there was an engineer named Jack Connelly. He probably flourished at RAND about '51, 52, 53, somewhere in there. He came up with the idea that airline reservations could be put on a digital computer, and that had never been done before. Jack Connelly began exploring doing this, and he was able to do it at RAND because RAND let people have a fairly wide latitude. He probably ended up with one or two helpers on the project and the project may have--I don't remember it for sure--but the project may have had to move into RAND-sponsored research after it had gone along for six months or so. There was nothing, though, to keep him from doing the beginning part of that research under Air Force sponsorship. The Air Force was also interested in its military air transport service, and you can stretch a point a bit and call it that. As far as I know, Jack Connelly devised the first computer-based reservation system for airlines. He did it pretty much on his own or with a very small team.

Collins: You seemed to indicate --looking at the economists for a moment--that it was really a subset of the economics staff that took a key interest in working with the engineering side of the house.

Digby: Yes, some were much more system oriented than others. There were theoretical economists there who really didn't want to work with the engineers. Although I must say, by and large, the kind of economists whom Charlie Hitch hired were ones who wanted to work on large system problems. But many also wrote impiortant papers on their own occasionally.

Collins: I know it varied depending on the type of problem that was being dealt with in any given project, but ways of working within a project could probably be categorized in at least a couple of ways. One is being in at the inception, working closely, and helping to formulate the overall concept. Another style might be, in breaking down subproblems, that you get a contribution that feeds in. In this case, looking at the air defense study, Barlow or somebody else would synthesize and--

Digby: Barlow actually wrote out project descriptions of about 30 subprojects which he dictated. In R227 it says exactly how many there were. I may have written out five of them and he reviewed them. But he probably wrote out 20 of the 30 himself.

Collins: So how then, in this case, would the economists interact? Would they contribute a discrete set of information that was compounded with the other things, or were they part of the conceptualized--

Digby: It would depend upon who the economist was. If it was Andy Marshall or Charlie Hitch or Jack Hirshleifer, they would criticize the whole structure. If it was a more junior, more technical economist, he might simply work on a specific economics problem. Let me give you an example of that.

Harry Rowen came to RAND as a chemical engineer with a little bit of economics background. He was put to work in the Cost Analysis Department of the Economics Division. The first thing I remember about Harry is that he was asked to estimate the cost of telephone lines that were used for a Civil Defense Ground Observer Corps network for the United States. He had to look into the cost of telephone poles and wire and putting it all together, which is a lot different from most of the projects that you think of Harry Rowen getting into in later years. But he was the kind of economist who looked at bigger pictures. Before long he and [Albert] Wohlstetter discovered each other, and Harry became Wohlstetter's deputy.

TAPE 1, SIDE 2

Collins: The instance you cited is essentially one in which two people with an economics orientation used engineering input into a framework that they were trying to work out. In your experience, in studies that were initiated on the engineering side of the house, was it more often the case that economists provided this kind of discrete input, or were they partners in planning and conceptualizing the effort?

Digby: Well, as I said, it depended upon who the economist was. If the economist were Andy Marshall, Charlie Hitch, or Jack Hirshleifer, they certainly provided overall conceptualization before long. They may have done some specialized work too. Andy has done a fair amount of specialized work in his day, but he always understanding the bigger picture. But then there were people who were more specialized who might work only on what the R&D system was doing. The RAND economists were, by and large, big system thinkers.

Collins: What are some examples of studies that were principally on the engineering side of the house, in which some of the economists worked as conceptional partners?

Digby: One good example was, some of the designs of idealized aircraft, which was a specialty of the Aircraft Division, led by Gene Root and his deputy, Bob Schairer. A notable example of that was the project that Tom Jones led on designing a new transport aircraft. That, as you may surmise, was a pretty successful study which led to Tom Jones being hired by Northrop, where he soon became preisent. There would probably have been one cost analysis person assigned to Tom's project,and maybe an R and D [research and development] person from economics, but it was not a large project. It was very much an engineering type of project.

Collins: It seems that the very large studies, such as the air defense studies and later the SOF [Strategic Offensive Force] study were more the exception than the rule. I wonder whether this reflects the nature of the problems that RAND was dealing with, and that is that you can only do so many of these big studies; there are so many of these very large problems in which you could integrate all these subskills as you put it. Or, in what this balance was when doing smaller studies with maybe three, four or five people--was it a reflection of the nature of the problems, or was it a reflection of the ability to bring the organizational resources to bear in an effective way?

Digby: Both. There is one other factor that I should mention, which is that as junior people began to mature and become very skilled in the systems analysis, they wanted to lead their own systems analyses. So by the mid 50s, RAND began to find that it had a lot of people who wanted to start very useful sounding studies and lead them themselves as opposed to being team members on somebody else's study. A lot of people made pitches to the Management Committee, and they were very attractive sounding studies; and sure enough, we ended up with a lot of studies without very many people on each. And there was a lot of division of people between the studies--so that you would get one-third of Bob Holliday, for example, to do some logistics work on a study because he had a third of himself on study B and another third on study C.

Collins: I think as you that examine your time at RAND, it seems impressionistically that most of your research activity was generally part of a group study. It seems that you, as an individual, did not go off and do a lot of independent, organized research, say as a mathematician would. Do you consider that representative of the typical engineering person at RAND?

Digby: Probably. My own personal history was to work with Barlow as his deputy for about six years or so, and then I became a department head. As a department head, I was also one of the triumvirs that led the SAP [Strategic Air Power] study, the others being Herman Kahn and Albert Wohlstetter. That went on for a couple of years while I was a department head, and it made it very hard for me to be both, I might say. Then there was a reorganization in which Dale Oyster took over my department, and I again became Barlow's deputy on the staff of the Research Council. Collbohm decided to move all of the division chiefs into something called the Research Council, and I was on the staff of the Research Council. Then I became a front office person for five or six years. With the departure of Harry Rowen, I went back into research and was project leader of fairly small projects, which almost every project at RAND was by that time.

Collins: I was interested in your observation about the mid 50s, as the people who had been there for a few years began to mature and have a certain confidence in their ability to deal with these problems and get their own ideas about where the interesting research areas were. It would seem to suggest there were perhaps more problems and ideas to pursue than the organization could actually do.

Digby: Oh yes, there were.

Collins: So the question comes up, your perception of the process of selecting out which of those things would get support and get done.

Digby: Well, originally in a formal sense, it was done by the Management Committee and the Research Council was formed in 1959 for that purpose. The Research Council was supposed to have meetings and decide which of the projects was worth pursuing. There was one other formal effort which we discussed before, which was the Strategic Objectives Committee (the 1960 Committee). It was a formal attempt to decide what was worth doing and what wasn't. That was less in the line structure of RAND, though, than the Management Committee or the Research Council.

The Research Council was partly influential because of the high powered nature of the members. It was chaired by Charlie Hitch at first. It had John Williams and Ed Barlow, the old division heads. They were a very smart bunch of guys, and they could be quite persuasive. Several of them had Collbohm's ear.

Collins: Which people did Frank respect and rely upon for judgement and advice?

Digby: Frank was very much a loaner. I would not say he was socially close to any of those people. John Williams was something of an adversary to Frank, but Frank listened to him very closely.

Collins: Adversary in what sense, philosophically, organizationally?

Digby: Yes, both. You see, Frank had come up from the Douglas Aircraft engineering kind of background and was very liberal minded as people from that background go, and Williams was an astronomer and mathematician. He was very much his own person and he saw RAND's duties in quite a different way from how Frank saw them. From time to time he would go in and make a démarche, and Frank would not really like it very much but he'd know he had to pay attention because he knew how smart John was. Frank probably got along more easily with Gene Root, but Gene left fairly soon and left Bob Schairer and Jimmy Lipp. Frank probably did not have quite so easy a relationship with Plessette or Williams.

Collins: But at the time of the Research Council, as you put it, who had his ear?

Digby: It's a little hard to say, really. I think Hans Speier often had things to say that were different from what the engineers said, and he would listen to Hans Speier. By that time Hans had moved out to California from the Washington office.

Collins: I think we could go more deeply into this whole question of managing the research enterprise, which the Research Council represents one stage of. We have been going about an hour, and I'd like to know how you feel about continuing.

Digby: Let's go for another ten minutes if that's agreeable with you.

Collins: Okay. Let's talk a little more about the Strategic Objectives Committee [SOC]. I've looked at your paper and there's some discussion of it in there.

Digby: But have you seen its report?

Collins: No, I haven't.

Digby: That's available. I finally got it cleared.

Collins: Do you think it would be better to talk about it after I read that?

Digby: Let me talk a little bit about the motivations behind it. I would have to look at my own paper to be sure just which year these things happened, but I think one of the motivations was that people started thinking quite deep thoughts about strategy. When we learned about the H-bomb, there was a group led by Charlie Hitch, with Jimmy Lipp and Ernie Plessette and Bernard Brodie, who did an analysis of the H-bomb, and that of course made people worry about deeper questions of strategy. So I think an important aspect of the formulation of the 1960 Committee (or the Strategic Objectives Committee) is that the RAND people began to think they had a responsibility for worrying about air strategy and national strategy. They began to feel that they were about as good at it as some of the people who had written books on this subject in the past.

Collins: This sounds like the core group of people who were involved in briefing [President Harry S.] Truman on the potential impact of the H-bomb. Was there some sense in which RAND saw this as an opportunity, an area in which it could provide advice that others couldn't?

Digby: I think it was considered a great opportunity to be invited to brief a president, or anybody in the White House, in those days. But I don't think that RAND people saw it as an opportunity for broadening what RAND itself did. In those years we had a very high fraction of our support coming from the Air Staff, and the Air Staff was riding high in terms of its missions and so forth. I don't think there was much feeling at RAND that, now that we've briefed a president, we should move on into more elevated strata. That came more with people like Charlie Hitch going back into the Defense Department. We began working not for the Air Force, but for the Defense Department, where we had a lot of people whom we knew and respected.

Collins: What was the tenor, then, of these early discussions? This was clearly virgin territory, in a sense, in terms of sorting out what these new weapons meant. How did the committee begin to grapple with the problems that were associated with this?

Digby: It was fairly organized. We had agendas and we listened to the outputs of the various studies. There was a conscious effort made to fit all of the various ingredient studies that RAND was doing into an overall picture. But I think the lurking issue of having the H-bomb was part of the motivation. We didn't quite understand what that was going to do to all of the other things we had been working on. It needed sequential thinking and it needed for us to put our other projects into a unified picture.

Collins: Just a question on the documentation that came out of this. As you put it, you were the rapporteur for the group. Did you, as you put it, establish agendas? Did you keep minutes and other written things that came out of the committee?

Digby: Yes, and those are available in that box that I left with Vivian [Arterbery]. In most cases they are not classified, and amusingly, in many cases they are done on ditto, I believe, or at least ozalid. We didn't have xerox machines in those days, so we had to use more primitive reproduction systems.

Collins: Was this a reasonably closed group in terms of their handling of these ideas and these deliberations, or was there free interchange with other people on the staff who interested in these questions and who were readily aware of what was going on in the committee? To what degree was it known and sort of swaying this discussion?

Digby: It was quite well known, and a lot of project leaders were invited to sit in on the meetings and give reports. There was, to some extent, a changing membership. So it was not at all held as a close thing. But everybody was very busy, so those who weren't on it had no great desire to take the time to be on it. There was some concern about having either Albert Wohlstetter or Herman Kahn on it because of their tendency to dominate conversations. Particularly Albert.

Collins: You described this activity in your paper; what was your judgement about its role in the mix of ideas that were being passed around within RAND?

Digby: Well, let me mention two things. One, it was supposed to have a role in helping the Management Committee decide which projects to endorse and which to put on the back burner. I think it had that role. For example, it gave some impetus to counterforce studies that I led. Secondly in terms of a specific idea, the main idea that came out of the Strategic Objectives Committee was the idea that was presented in this US News article: "No Need to Bomb Cities to Win War"--in other words, the importance of having a counterforce strategy. That was pretty much agreed on by all the people in the Strategic Objectives Committee and is in the report, as you will see.

Collins: I guess what I'm unclear about is the--although obviously the concept was in the air, I'm not clear about the label "Counterforce" was attached to it. You pointed out in your paper that you referred to it as obviation.

Digby: Oh, I think the label got attached, just months after that.

Collins: Okay.

Digby: We began to realize the Air Force had these Alpha, Bravo, and Delta missions in which Bravo was counterforce.

Collins: There are a number of complexities there. Perhaps we can sort some of them out the next time.

Digby: I think I'm about ready to fold up if you are.

Collins: Okay. Why don't we leave it at that, and we can pursue some of these things at a later time. Thanks, Jim.

James Digby
May 17, 1991

TAPE 1, SIDE 1

TAPE 1 SIDE 2

29	Organizational changes made by RAND in which Digby became head of the Operations Department; reasons for these changes; Digby's duties as head of the Operations Department
29-30	The role played by RAND department heads in determining which projects should be supported
31-32	The Strategic Air Power Project; Herman Kahn's knowledge of Strategic air power; how the RAND staff developed credential in the area of strategic air power
33-34	Discussion of Bruce Briggs' and Gregg Herken's books; Herman Kahn's "greganons' style
34-35	Discussion of criterion for promotion and salary increases; structure of professional review process; how important contributions were addressed; role of department heads in these matters

Interviewee: Mr. James Digby

Interviewer: Mr. Martin Collins

Date: May 17, 1991

Place: RAND Corporation
Santa Monica, California

TAPE 1, SIDE 1

Mr. Collins: The last time we talked, we covered a number of subjects, but towards the end of our discussion we focused on your activities and the activities of the Strategic Objectives Committee [SOC]. From our previous discussion and from what I read in your historical paper, I'm still not entirely clear on the motivations for establishing this committee. Let me just follow that up quickly with a second point, which is that in looking at the minutes that you preserved in your papers, there was something comparable a little bit before that, perhaps a year before in 1953. This was either a continuation or a rebirth of that enterprise. So if you could just try to sketch out your understanding of how the first incarnation of this Committee came into being, and then the second one, for which we have these records.

Mr. Digby: Okay. The name of the first committee was the 1960 Committee, and it was an attempt to look to that distant future year 1960. The name got changed to Strategic Objectives, I guess, for two reasons. One, 1960 was approaching and we were beginning to look well beyond 1960 at things like ballistic missiles and defense against them. Secondly, I think, the RAND people were more consciously strategists after having argued about these items for a year or so, and we realized we were about as good at strategy for the current age as anybody else.

But it is a little vague in my mind as to exactly how the committee got started. But I would think it was probably because John Williams and Charlie Hitch, and perhaps Jimmy Lipp, felt that the discussions in the RAND Management Committee were not really substantively complete or satisfying -- the Management Committee was run by Frank Collbohm and Dick Goldstein. While it was quite substantive, there was a sort of--I won't call it a sniping atmosphere, but there was a tendency to make points by some of the members, notably Ernie Plesset, who was head of the Physics Division. Then, I guess it was called. John Williams was not above scoring points when he could, as you can tell from his writings. When Bob Buchheim became head of the Aeronautics Department, he was a great person for contentious interventions in the Management Committee. I think that had not happened yet at the time we are talking about, but there was Ed Paxson, who was not a division chief -- he was not temperamentally the kind who would hire people and do the administrative things that a division chief had to but Ed was a member of the Management Committee on a kind of grandfathering clause because he had been one of the original division heads at RAND. He was a great gamesman. In fact, he introduced the Gamesmanship books to RAND, and they were considered required reading by a lot of people.

Collins: Just as a digression, what were the Gamesmanship books?

Digby: Oh, they were by this guy who had been a professor at this university in Malaysia--I'm trying to remember his name. Stephen Potter. They were the rage of RAND in the early '50s.

Collins: What was their value or attractiveness to the RAND staff?

Digby: It was a laconic Englishman's way of simplifying the rules for getting ahead in the bureaucracy without really doing much work.

Collins: I guess I've seen in the RANDom News, which I have a stack of over there, excerpts from some of them or take offs on it.

Digby: There was a guy at USC [University of Southern California] who had a kind of similar success later on -- I'll think of his name later. Anyway, the Potter Gamesmanship books, of which there ended up being about four, were quite in fashion at RAND in the early 1950s. These were all very bright people who had been the smartest guys around in their college classes, which was not so long ago, and so they tried to score points in Management Committee meetings. That is probably one of the underlying reasons for Hitch and Williams, and maybe Lipp, getting Collbohm to agree to have a Strategic Objectives Committee; the membership could be made to have just the people who would be a bit more serious. Like Andy Marshall, for example, or me.

Collins: At this stage in your career at RAND, did you have occasion to participate in Management Committee meetings and see this milieu firsthand?

Digby: Yes, from time to time. I was not a division chief at that point, but I was Ed Barlow's principal assistant, and if Barlow could not attend, I would often attend. I also attended for substantive reasons, if something that Barlow and I were doing was going to be discussed. So I saw many Management Committee meetings. Later I was a department head and attended the Management Committee meetings because of that. In the early 1950, I was just Barlow's chief assistant.

Collins: This is a question that reaches a little bit, but did you have any sense that Frank or Goldy [Dick Goldstein] felt that this kind of "gamesmanship" was something that was good or bad or appropriate to the RAND character or contributed to Management Committee meetings, or had any value at all?

Digby: The best way to put that is that one of Frank's great characteristics was that he was tolerant of bright people. Frank himself, maybe partly because of his deafness, was never the sparkling repartee type of fellow. He was a good engineer, and in fact he'd had a very successful career at Douglas before RAND, which I guess is the reason that he was--I can't say whether he was self-selected or selected, but he was not a sparkling-repartee-type person. But he probably realized that it was a good outlet for people like Williams and Paxson. And, of course, the physicists, like Richard Latter and Albert Latter were--and Ernie Plesset thrived on, making points. This had been something that was important in their culture in college, and I'm sure they kept on doing it. If you've ever been around Harold Brown, you will observe a master at that kind of behavior.

Collins: You gave a kind of suggestion of how the membership of this committee was determined. Did you have any sense of what went into selecting members to compose the group, apart from seriousness?

Digby: I think three things. One was that there was an attempt to get a variety of people--like the animals in Noah's Ark. You had to have some engineers, some political scientists, some economists, some mathematicians. There was an attempt to bring together RAND's varying kinds of skills and be very much inter-divisional. Secondly, I think everybody who was on the Management Committee had a good track record at putting serious things into writing and into oral statements. And third, these were the people who were seriously interested in--making strategic advances and had shown that by what they had written.

Collins: One thing that's interesting about your historical papers is an absence early period, of the Korean War as shaping influence or important spur at RAND. What was your perception of how that event affected activities here at RAND?

Digby: I wouldn't say it was entirely unimportant. You have probably mostly seen papers relating to strategic matters, in other words, the design of SAC [Strategic Air Command], the design of Air Defense Command, and the Korean War didn't have all that much to do with that. The papers relating to tactical air war don't seem to have been released in quite the quantities; they're not as famous. There was a big series by Norm Peterson that really sprang from the Korean War.

One notable cross fertilization was that General [Laurence C.] Craigie, the father of Jack Craigie who is here at RAND now, was the Air Force Commander in Korea. He invited Herb Goldhamer-

-actually, I think there were more people than just Herb. But Herb Goldhamer was invited to go over to the Korean peace talks to serve as an advisor. I mentioned this, by the way, in my paper, and I do a quote from some things that were exchanged between Herb and, I think, Brodie or Speier at that time. So it was not ignored. But the main effect of the Korean War was to plant the notion that air power might have to be used in a measured way.

Collins: Did it feed in any way at all to this ideological discussion that came up between Brodie and John Williams about preventive war?

Digby: I don't recall that it fed in in a serious way.

Collins: Well, in terms of perhaps setting a tone, that war was indeed an eminent kind of possibility.

Digby: I think the Korean War convinced us that war could happen in the nuclear age. I happened to be at Air Defense Command, which was then on Mitchel Field, Long Island, at the time the Korean War had just started, and I participated in a hurry-up effort by the operations researchers there, led by a operatious researcher named Dick Blythe, to say what should be done on a temporary basis to improve the air defense of the United States in case we were attacked by Soviet bombers as a result of the Korean War. I don't think I felt this at the time, but I later found out that [Harry S.] Truman was seriously thinking of using a nuclear weapon on the Koreans and that [S. Winston] Churchill flew over to talk him out of it. The reason that we were having a hurry-up effort to improve the air defense of the United States was keyed to this feeling that the war might suddenly escalate because of Truman's feeling that he should use the nuclear weapon against the North Koreans and that the Russians might see fit to retaliate because of that. Anyway, that's just to say that the Korean War had its effect; but the written RAND record of the tactical air studies, which would be more relevant to lessons from that, just doesn't seem to have survived as much or been noted as much. The whole Paxson effort, playing war games down in the basement, derives largely from experiences like the Korean experience and explored in map exercises similar wars, a lot more than it did any big wars.

Collins: What was the intended relationship between the Strategic Objectives Committee, and the Management Committee and Frank, and, more broadly, the interests within RAND, at least at the administrative level, of coordinating or helping to plan the research enterprise of the organization?

Digby: Largely just that. It was intended as a way of making our research plan as relevant to the real big problems as possible, as opposed to letting plans bubble up from the bottom, which they always did, too. But the 1960 Committee was supposed to take a very broad look and say what kinds of research should be relevant.

Collins: Did it have an effect in that respect?

Digby: Yes, and one reason it had an effect is that three of the members were quite powerful department heads--Williams, Hitch and Lipp. Correction, they were division chiefs then, not department heads.

Collins: In other words, they were in a position to implement the recommendations or insights of the committee.

Digby: Yes, and they were also quite influential in the Management Committee, and they had Collbohm's ear. And Brodie had Hans Speier's ear, who had Collbohm's ear.

Collins: I guess it's not clear from the minutes as to whether specific projects or studies were recommended or--

Digby: Studies.

Collins: --what rather broad areas that RAND ought to be concerned about.

Digby: It was more that either a broad area or a broad study was reviewed and suggestions made right on the spot and also internalized by the members for later recommendations. In effect, the main recommendation output is in D-2700, The Next Ten Years.

On recommendations, we also made recommendations directly to influential, friendly people in the Air Staff. That was part of the output of the Strategic Objectives Committee and one of the people who was really very receptive was a man named Teddy Walkowicz, who later went to work for Laurence Rockefeller. Walkowicz was a lieutenant colonel when we were first working with him, later a full colonel, and he was in the planning part of the Air Force Development Planning Office. So he was in just the right spot to influence the shape of the Air Force, and we made an effort to bring him on board. one of the main outputs of the Strategic Objectives Committee was to come out strongly in favor of designing the Air Force to do counterforce attacks as opposed to anti-city attacks, and Walkowicz was the source--well, first of all he wrote a piece for the intellectual Catholic journal Commonweal. That led in turn to a big story in U.S. News and World Report in which they had bright red headlines, in the fashion of U.S. News of those days, that said, "No Need to Bomb Cities to Win War."

Collins: That was Richard Leghorn's piece that you're referring to.

Digby: Yes, Leghorn was the other one who was much affected by this.

Collins: If I recall, the U.S. News and World Report article is about '59 or '60, isn't it?

Digby: No it was earlier, about '56. There's a reference to it, with the date, in my note.

But Leghorn and Walkowicz were good friends, and both had the same kind of general function in the Air Force. Leghorn, a reconnaissonil specialist, was a particularly good friend of Amron Katz at RAND, and Katz channeled RAND things to Leghorn. Both of them later ended up with a connection with the Itek Corporation. Leghorn went to work for them, maybe helped to found it. Walkowicz, I think, saw to it that Rockefeller money was put into it.

Collins: Are there any other examples of ideas or recommendations that the SOF group passed along to the Air Force besides the counterforce idea?

Digby: Well, we were strong on protective construction and protecting SAC. So the Wohlstetter-type ideas were favorably included in the recommendations. I think really you'd better just see that "D" because it was a carefully drawn "D" written by Hitch, Brodie and Marshall. It did not include all of William's sometimes off-the-wall ideas. But it is a rather densely packed document. So you've really got to read it rather than just have me recall.

Collins: How did Albert Wohlstetter's work fit into the things that the committee was evaluating and considering?

Digby: Well, it was one of the inspirations for having the committee because Albert introduced some new things for us to be concerned about, and Charlie Hitch in effect represented Albert's views very eloquently in the committee. We deliberately did not invite Herman Kahn or Albert Wohlstetter to be members because they had this tendency to dominate conversations, and it would have been very hard to have a serious back and forth between eight very smart and rather vocal people if Herman and Albert had both been there.

Collins: This is a quote from one of the minutes that struck me as interesting, and it's something apparently attributed to Charlie Hitch. This is from a memo of July 1954, minutes from 1954. And it goes as follows:

"From the point of view of making predictions, we are at an unusual position in world history. Technological developments which can be generally predicted appear to be dominant in determining the course of events. The paper in here"--I believe they're referring The Next Ten Years--"will try to predict these developments and trace their implications."

I guess what strikes me as interesting about that, at least one element, is the emphasis on the strong determining agency of technology in these kinds of issues; of diplomacy, or political negotiation and interaction, seem to be considered a secondary agency or force here. That struck me as kind of interesting, especially when reading the Brodie pieces which seemed to argue for the primacy of political considerations as opposed to technological ones.

Digby: Well, you get different views from Bernard at different phases of his career, as I think several people have pointed out. Early Bernard tended to reflect the lore of past strategists, the heritage of Douhet, and tried to superimpose the technological revolution onto that. Mid-period Bernard was very affected by the technological thing and that's the Bernard who is on this committee in 1954. The later Bernard reacted against technology to some degree, by the time he got to UCLA.

Do you remember the rest of your question? It seems like I only answered one aspect of it.

Collins: I was asking about the sense of the committee with regard to the principal, problem areas to grapple with--the implications of technology as opposed to, say, the political environment.

Digby: First of all, the one big diplomatic thing that was going on that seemed to be working was NATO [North Atlantic Treaty Organization]. Some members of the committee were somewhat interested in NATO and we sent RAND people over to work in NATO, and so forth, around that period of time, but it seemed to be under control. Dealing with the Soviets had been a completely dry well at that point. You have to remember that [Josef] Stalin was still alive or just gone from the scene, and the Soviets and what they would do in a diplomatic sense were just very beyond any ability of scholars to deal with. Now RAND had some people who studied the Soviets very carefully, both their economy and their politics, and the works of Nathan Leites were very well known to all the people on the committee and particularly to Andy Marshall. So I would guess that the reason we dealt more with technology than with political trends was that it just did not look very promising in the mid-1950s.

Collins: Okay. One thing that struck me about some of John Williams's writings, and I think must have been part of the frustration that comes through in Brodie's responses to him, is the crudity of his sociological or political analyses. As Brodie points out in turn, the analogies and the metaphors he frequently uses are pretty simplistic in terms of understanding a country. They're reduced down to animal or savage metaphors.

I guess in terms of the deliberations of the committee itself, it's difficult to sort out what the point of view was. Here you mention you have people in the organization like Nathan Leites who devote their careers to a very careful analysis and articulation of Soviet political theory, Soviet ideology--

Digby: And there were a number of lesswell-known people too.

Collins: Yes, so I guess the question is how did the almost visceral feelings about the situation that John Williams represented balance with the more studied approaches to understanding the Soviets and their potential behaviors?

Digby: Well, that was the interesting thing about RAND, that people went from a kind of a deterministic constructs of things, and John was a mathematician and astronomer. Now astronomers typically think things are going to work in a very precise kind of way. I'm an engineer. Engineers temper this precise way that things work with a knowledge that you need safety factors and that there are unknown things, like the vibrations that knocked down the Tacoma Narrows Bridge that we didn't know about when the bridge was designed, and others. And fatigue factors in the early jet airplanes that we didn't know about until they caused crashes. And then there is, finally, the political scientist, who is accustomed to a world in which predictability is not the usual order of things.

So RAND was a mixture of all of those people, and both RAND itself and the Strategic Objectives Committee were melting pots for putting these different views together. The one thing that was common to all of these people was that they were all very smart. John Williams was smart. Bernard Brodie was smart. Charlie Hitch was smart. But they had rather different approaches.

Collins: This is a groping question that refers back to the objectives of the SOC. Was there some sense in which you wanted to attempt a system--maybe this is answered in <u>The Next Ten Years</u>--a systematic listing of the problems that needed to be dealt with?

Digby: Yes, we did.

Collins: And was there a belief that those problems could be enumerated and defined, or was the field in such a state that it was even difficult to define what the problems were?

Digby: It required a certain finesse in writing about them to avoid being too deterministic about those problems. In other words, that's one of the things that Brodie and Hitch and Marshall brought to <u>The Next Ten Years</u>, the ability to state things in words that did preise predictions. That was well realized. And, in fact, we tried sometimes to make mathematical statements such as, Here we have a problem that consists of ten different stages. If we know each stage was a ten percent certainty, how well do we know the outcome after ten of these stages? So we consciously thought about convolving uncertainties. That's one reason the gaming technique grew, because it let you go through a series of uncertain steps. But when you got through, you had to realize that what you had gone through was not a predictive process but a learning process.

Collins: Okay. I think there is probably more to talk about there, but we'll perhaps come back to that.

Digby: But this was known to the SOC people at that time and talked about.

Collins: Now, though Herman Kahn was not originally on the committee, he apparently was added--

Digby: Yes, added late.

Collins: --added late in the committee. Did that affect the dynamics of the committee in any way or the problems that it focused on?

Digby: Not too much, I think. I guess there are two reasons. One, Herman was just making his reputation as a generalist when the committee was first organized, and secondly, I can't remember just what the arguments were for adding him, but we did. He did not disrupt it when he was finally added.

Collins: Okay. One other issue that occasionally comes out in the minutes is a sense that one of the elements that was required to emergency or address the sense of the problems that existed was more money for defense.

Digby: Yes.
Collins: Now, in the aftermath of the Korean War, at least for research and development money, there was a fairly dramatic increase in the amount of money that the military was spending in this area. I think overall, military budgets were certainly substantially higher than they were before the Korean War. Was the issue ever addressed of how much was enough in terms of meeting these problems that the committee forsaw?

Digby: Yes, to some degree. Let me mention two things about that. One has to do with the Korean War. A group of us went back to Washington for a secret meeting in either mid 52 or mid 53, in which the Air Force was getting a much larger budget for R and D, [research and development] and we were to advise on what new things should be instituted because of this new chunk of money. You're quite right that the Korean War had a major effect after the Louis/ Johnson administration. It really brought in a lot of money for new things. Among other things, that's when the U-2 was first authorized, and a pitch was made for the U-2 at the meeting that we attended. I can't remember if it was Kelly

Johnson or somebody else. Another pitch was made by Stark Draper of the Draper Labs at MIT [Massachusetts Institute of Technology] for better autopilots and guidance systems, and that was well received.

Collins: Was this a meeting of key Air Force contractors?

Digby: No, it was more like an offshoot of the Air Force Science Advisory Board. But somebody on the SAB thought we need to get some additional help on talking about this, so they called four or five of us from RAND and some from MIT and some from other places. It was a mixture. Generally not airplane contractors. RAND was more like part of the Air Force than a contractor at that point. So the Korean War, did have a lot to do with getting more money for R and D.

On the subject of how much is enough, Charlie Hitch was the most knowledgeable and eloquent spokesman for not getting more and more and more. Charlie perceived very clearly the dangers. He put defense matters into a perspective that the rest of us who had never worked on anything else didn't quite have. At one point he said specifically, "No Jim, you don't really want to ask for more total money in the defense budget, you just want to allocate it better." But the economists tended to understand why that should be done more than the engineers or others. In fact, Enthoven's book <u>How Much is Enough,</u> is the kind of the culmination of that line of thinking.

Tape 1 Side 2

Collins: One thing that's unclear during this period when you were working on the Strategic Objectives Committee is your awareness, either general or detailed, of what Air Force war plans were. The historical accounts indicate, with respect to Brodie anyway, that you knew in the early 1950s when you served some time working with [General Hoyt] Vandenberg, that his exposure to being able to read war plans and look at their assumptions led him to revise his thinking and have a greater concern about what the Air Force's attitude toward conducting war was going to be. What level of knowledge, then, did the committee, or members of the committee, have with respect to what the Air Force was actually planning and how this related to potential recommendations or areas of study that the committee might suggest?

Digby: Well, first of all, in terms of exposure to exact top secret Air Force plans, we probably had very little at that point because they were very closely held. [General Curtis] LeMay was the head of SAC and did not have a very tolerant attitude toward civilian intrusions, although he was a very smart guy who listened and sometimes picked up points of his own from civilian pitches. So I would guess that the people had little direct access to the fully classified current war plans; however, we had very close relations with a lot of smart lieutenant colonels and colonels who were in the Air Force and who wanted us to know how things were in a general way. So we knew the nature of the war plans even if we didn't have an example of them, and the Air Force would from time to time give us simulated war plans for the purpose of going through a study.

Now, one reason RAND could operate on that sort of system is that, right from early on we tried **not** to be an organization for dealing with the next year or two, and war plans do deal with the present and the next year or two. We tried to be an outfit for dealing with five or six years, or eight or ten years in athe future. That meant that our formulation of what the Russians were like was just as good as the Air Force's. Now there was this problem of getting good intelligence which Andy Marshall and Joseph Loftus did a lot of work on. We learned about the Russians, in part, from our systems studies that designed Soviet attacks, like the Heymann-DeHaven-Raymond study.

So I think the answer is that in general, the people in these meetings did not have direct access to complete top secret war plans, although occasionally they may have. Ernie Plesset may have been shown one at Los Alamos on some occasion, or Charlie Hitch may have. But officially, RAND was not privy to the immediate official war plan. Also RAND people worked on the Air Force Science Advisory Board, and occasionally SAB committees were given inside information on those things.

Collins: In your judgement, did this hamper your ability to articulate strategic issues?

Digby: We overcame the problem pretty much, partly by having good friends who were in the Air Force or who had just come from the Air Force who kept us realistic. You will notice, in some of the things that have already been written, that Joe Loftus was upset at some of the RAND beliefs about the Russians, having just come from a very secret part of Air Force intelligence. He tried to do his best without violating the various compartments to get RAND to be relevant. So there was a problem, and people were aware of it, and it was partially overcome.

Collins: This question may be more difficult to answer, but viewing this from the Air Force side, the presumption was that RAND was there to help the Air Force analyze these kinds of problems. Why wouldn't there be a better flow of information to make your work potentially more productive and useful for the Air Force?

Digby: There were a lot of people in the Air Force who did not have access to war plans. The RAND people worked most closely, by and large, with the development offices. At first we were attached to one at Wright Field, but later to AFDAP, the Ben Schriever Development Planning Office. People in those offices did not necessarily have access to war plans. So it was not considered all that unusual.

But I think the main thing is that we were working on a period of time for which war plans were no better than what we were conjuring up, at our best, and Joe Loftus tried to make us more realistic on what we were conjuring up. I think Loftus is uneasy about some of the RAND assumptions on Soviet basing and forces. It shows that we were not too perfect. For example, on special intelligence clearances--very few people at RAND had them, and yet to have the best knowledge about where the bases were and what was on those bases, you had to have special clearances.

Collins: What was the nature of RAND's contact with the Plans Office and the DCS operations, and what was your sense of the connection between that operation and the AFDAP[Air Force Office of the Assistant for Development planning] Office?

Digby: I don't have any strong recollection on that at least not much. RAND actually had people back in the AFDAP Office most of the time, so that was our close connection.

Collins: One of the things that you alluded to as an important product of the committee was the idea of counterforce and its strategic implications. One sees this notion of counter-force essentially still trying to be sold by the late '50s and into the early '60s.

Digby: In fact, in the 1987-88 Wohlstetter=Ikle Commission, it was still being sold.

Collins: Why was this a strategy or a framework for thinking about strategy that the Air Force, given the timeframe, found it difficult to embrace?

Digby: Okay, let me give you two things. One, LeMay's first feeling was, "we will go and blast those guys out of their control centers, even if it means destroying Moscow in the process." This was a typical caricature of a LeMay feeling. And SAC referred to the civilian deaths that would occur when a control center in a city was hit as bonus value. So there was a general feeling that you wanted both to hit the military targets and injure the other side very seriously. The RAND view, and Wohlstetter probably expressed this most clearly, was that you should only attempt to do what was necessary to get the other side's military force neutralized. And his so-called dual criterion, "Do the least damage that is unintended and the most that is intended," also played an important role in this recent commission. You can see it actually won out in the Iraqi War to a large extent, and those times when it was a failure, like the so-called "baby-milk factory," were problems. I don't know the true story of the baby-milk factory, but that RAND view on avoiding unnecessary damage was quite important for the Air Force.

Collins: This again may be difficult to characterize, but I think the LeMay perspective was part ideological and part operational He felt more comfortable giving a massive attack or a retaliation, and he felt that operationally it would be very difficult to locate all of these highly specified military targets and carry out a mission against them.

Digby: Yes, I neglected to mention a very important event on this. The real crux of the RAND advice on counterforce was in the 1962 Ann Arbor speech of [Robert] McNamara, which was largely written by Bill Kaufmann who had given a counterforce briefing for RAND. [The talk had been given earlier in classified form to a NATO meeting in Athens.] McNamara figured out that the Air Force would have an almost limitless ability to ask for more bombers and missiles if it had a counterforce strategy, where as it would be quite limited if it was only to destroy the cities. So McNamara, for a very practical reason, backed away from the reliance on counterforce of the Ann Arbor speech and SAC--by that time LeMay had sort of passed from the scene--SAC began to be very strong for counterforce, because it saw it as a way that could not be constrained on how many ICBMs [intercontinental ballistic missile] it would get.

Collins: Did the budget implications, or potential force structure implications, of counterforce become evident to anybody at RAND, prior to this?

Digby: In general, yes. I was the leader of RAND's first counterforce study in 1957, and I could see that it had an almost limitless target list. I was in effect preaching the value of going after the enemy's force as a strategic matter, and my briefing was not heavy on economic consequences. That came up later during the McNamara period.

Collins: I guess there's a potential nuance here with the Air Force response, whether they were adopting this internally as a way of structuring a potential attack versus whether they were making some an official announcement of whether or not this was a strategic approach of theirs. Was that kind of distinction ever?

Digby: I think, in general, the SAC view was, "we will emphasize counterforce, but we won't give up completely on bombing cities." And they called them by these names: the Alpha targets, Bravo was the counterforce, Delta--I've forgotten what they all meant, but they had lists for all of these things.

Collins: It's not indicated in the minutes or any of the documents you pulled together, why the Strategic Objectives Committee seems to have dissipated and concluded its work?

Digby: I think the production of D-2700 was the apotheosis of what it was trying to do. Everybody was very busy doing other things and I think, we had gone through the whole list of the important RAND studies. We were all very busy people. The notion of having it was not rejected, it was more just that we had done what we set out to do.

Collins: Was it some sense that The Next Ten Years had laid out a map that RAND was more or less following in the projects and problems it was pursuing in the next three to four years?

Digby: Well, somewhat, but it was just that everybody was very busy doing other things. In fact, in my own case, I've forgotten just what the sequence was, but we reorganized RAND and I became a department head not too long after that. The other thing that happened is that the systems analysis course was organized by Ed Quade, with a big input from Herman Kahn, and that was considered a similar kind of activity where we codified many of the RAND views. It seemed appropriate not to have the committee just because we had had it last year.

Collins: Okay. Why don't we talk a little about that organizational change in which you became a department head?

Digby: My first line job was as a group leader under Barlow in the early 1950s. Barlow was the head of the Electronics Division and I was head of the radar group. Then when Barlow became head of something called the Engineering Divisionin 1956, it had several departments and I was head of what was called the Operations Department.

Collins: Do you have any sense of what motivated that organizational restructuring at that time?

Digby: I don't have any clearcut idea. I could probably think of something if I thought about it long enough. RAND from time to time has felt like reorganizing itself. There had been a major series of layoffs in 1955, I think it was, and that may have had something to do with reorganizing another way. I know the reason for the later [1960] reorganization where the division heads were moved onto the Research Council. The reason for that was, I think, that Collbohm felt the division heads had become too entrenched as power centers and he wanted some of the power in the structure to devolve to the department heads, the division heads were all put on to a Research Council. That was an interesting period. That was 1960.

Collins: Right. What were your responsibilities and area of purview as the head of the Operations Department?

Digby: Well, the thing that was new was that I had to hire people, fire people, do salaries and a hell of a lot of reading. I had to review the external publication drafts of everybody in the department who wrote one. I had one assistant George Gompf, who helped me with these things. I think one problem for department heads that has continued right up to the most recent reorganization is that there is one hell of a lot of reading of reports, and ever some of the even very smart people don't write very good reports. Or else they won't write them, and you have to persuade them to write them.

A more fun part of being a department head was planning the research program and getting projects started, and I had more influence in doing that.

Collins: Okay. I'm interested in this of balance, as you it before, of things that bubble up from the bottom and the effort to try to plant something at the department level to establish a coherent program.

Digby: First of all, as I've said a couple of times, RAND was full of very smart people, and many of them in any other organization would have risen within a year or two of joining it to be a group leader, then a department head, then a division head, and so forth. Well, that wasn't possible at RAND, because there were so many smart people and there was very little turn over. So one outlet was for people to propose projects because as a project leader they saw others--first of all, it was exciting and second, it involved travel to Washington and briefing important people. You were recognized, so if you weren't promoted on the line organization, at least you could propose a project and do a good job and get a salary increase and recognition far leading a good project. So there soon were too many projects, and by the time of the Strategic Objectives Committee, there was a conscious desires on the part of by management to hold down the number of projects. One way of doing that is to identify some very good things to do. That did not really completely solve the problem. It's a constant RAND problem.

Collins: What role, then, did the department head, as you experienced it, play in deciding what were good ideas for projects?

Digby: We had a voice in Management Committee, and could say who we would support to lead a project. But if we did not support him, we could probably kill it.

Collins: But in terms of somebody coming up with an idea for a project, was the first person they turned to for support is the department head?

Digby: The department head. For example, Wohlstetter got a great deal of support from Charlie Hitch in doing his basing study. Wohlstetter is not the kind of guy who ingratiates himself with hard-core aeronautical engineers, as he had to in order to get a multidisciplinary project going. So Charlie Hitch used his authority and sagacity to help Wohlstetter get his project going. Ed Barlow was a very different kind of guy. He was not a division head at first but was very suave and a very hard worker, basically an engineer and not a logician. He was able to operate bureaucratically very well. Then he became a division head.

Collins: As a department head, what criteria would you employ to decide whether or not a given project was worthy of support and proposal to the Management Committee?

Digby: One of the things that several of us organized was something called the Strategic Air Power Project, which had the happy acronym SAP. I liked that because I thought it was a pretentious idea and to have a slightly silly acronym was appropriate. Wohlstetter did not like that acronym at all. SAP had what was called a triumvirate of Wohlstetter, Kahn and Digby. I was considered the more practical of the three. During the time I was department head I met with those two people, or in Wohlstetter's absence, Harry Rowen. We planned projects on strategic air power. We listened to pitches from people who wanted to start them and we encouraged the right ones, we thought.

The Management Committee also listened to pitches of the people who wanted to have projects, and if somebody was poo-pooed too much in The anagement Committee and made to look like he didn't really know what he was talking about, he probably wouldn't get his project.

Collins: Just to be clear, was the Strategic Air Power Project a vehicle to encourage research by others in this area and filter the projects, or was there a specific project?

Digby: No, it was more to coordinate RAND's work on strategic air power.

Collins: I see. How did Herman Kahn become a member of this group?

Digby: Well, he had already done his part of the systems analysis course and had the beginnings of the Kahn briefing, I guess. Herman was recognized right from the beginning as being a very smart person, and by 1954-55 he was recognized as knowing something about strategic air power, which was not true in his early days at RAND.

Collins: This may sound like a naive question, but how does somebody establish those credentials, at least among his peers here at RAND?

Digby: I think first of all by solid knowledge of what has happened, what the issues are, and what the arguments are on the two or three sides of the issues. Herman really got to understand that. Wohlstetter, I might say, was very good about going and sending his close cohorts out to all the SAC air bases and radar stations and so forth etc. Barlow did that a lot, too. It was part of my way of doing business to go to radar stations when we could, a attend exercises. We would go to a radar station in the state of Washington, for example, and watch an air defense exercise going on. Then maybe we would be invited to Colorado Springs to hear the critique of what happened three months later. Maybe we would even help write the critique. So that gave us a knowledge of what the Air Force was trying to do as seen by the operations colonels who ran the different parts of it. But Barlow and Wohlstetter were both very good at doing that.

Collins: Okay, this is the beginning of something that helped establish your credentials as being knowledgeable about the subject matter.

Digby: Yes, and the RAND people, being somewhat gamesters, would--maybe they'd be back in Washington talking to a brigadier general who was in charge of operations, and he would say, "Well, I can't really see how those B-47s at Robbins Air Force Base had any risk of not getting off the ground." Then Fred Hoffman would say, "Yes, but when I was there, the status board had three of them red lined for no communications. How could they possibly know quickly to get off the ground. If they got off the ground they couldn't be controlled." The RAND people were very good at knowing these details which were key to whatever it was we were trying to do strategically. And Wohlstetter, surprisingly for a mathematical logician, became quite superb in getting this kind earthy of work done.

Collins: This is a bit of a digression from the credentials question. But was this something that Herman Kahn also was attentive to, this empirical understanding of operations?

Digby: Herman was an extremely gregarious fellow and he liked to travel. He would go and talk to people every where. I would say Herman absorbed a lot of this through anecdotes, and he could play back the anecdotes and make up new anecdotes or parables. Herman's talk is replete with parables.

By the way, let me tell you about this book for a minute. I don't know if you want to turn the machine off while I do or not. I found it in a distressed book sale across the street, and you may want to buy a copy. It's only $2.98. It's by someone who worked for Herman for 15 years, Bruce Briggs. He interviewed me, and I had never seen the book before until I saw it on this table of books being sold at very low prices. I thumbed through the index and found I was referred to about seven or eight times and I thought, well for $2.98 I can't go wrong. It turns out that while Bruce-
Briggs never really had great credentials as a scientist in this area, he's got a lot of the arguments down quite well. He makes a few mistakes. He talks about my changing our fallout shelter into a wine cellar later on. Actually, it was a wine cellar **<u>and</u>** fallout shelter from the very beginning, so there are some mistakes like that. But I'm surprised at how well he did,

compared with some of the other histories, like Gregg Herken's. Herken was trained as a historian, and apparently his first book was very good. But the second one had lots of mistakes in it. Bruce-Briggs seems to have many of the things we've been talking about, done in pretty great detail and fairly correctly.
Collins: Actually, I have scanned it. Our museum [National Air and Space Museum] library has it. I guess what I noted about it was that most of the text dealt with the period after the 1950s.

Digby: Well, no. I would say that one-third of it deals with the period up to 1950. I think I'm just at the point where McNamara is trying to get control of the Defense Department, and I'm a little over half way through. Anyway, I recommend this for an additional source.

Collins: When you say that Herman Kahn was gregarious, are you referring to his interactions with people in RAND or in terms of him going out to bases?

Digby: Well, both. He had a lot of energy and he was always traveling. He would become very friendly with people. He would remember a person a year later and still be his best friend. He was truly a friendly, gregarious kind of person. That's how Herman absorbed his realism, I think, by talking to a lot of operators, away from RAND as well as at RAND. Albert got his by having a team that he deliberately sent out to ask questions and do things and write reports.

Collins: How are we doing on time? I know you have to get going fairly soon.

Digby: I would like to knock off in seven minutes or less.
Collins: There are a number of things that I would like to discuss about the cultural aspects of RAND and the formation of groups of interests, clustered around certain ideas or approaches to problems. I think that might be reserved for another discussion.

Getting back to your role as a department head--I think you suggested how this was done, but I'm interested in the criteria for promotion or for getting an increased salary or recognition by the organization. What were the incentives for people to do particular things, and in what way was the professional review process structured to encourage people to work in certain directions as opposed to others?

Digby: Some people didn't like to travel and they were really at very much at a disadvantage. There was a lot of travel, since RAND was in Santa Monica and the Air Force was in Washington, Wright Field, Colorado Springs and on a lot of air bases. The people who visited the Air Force and asked questions got to know things that were really very useful in advancing their career. Among other things, the Air Force people were fairly friendly. You would go out to dinner, or you might be at a meeting and go out to dinner with people from Air Force Operations Analysis in Washington while you were both in Colorado Springs, and so you exchanged a lot of information, sharpened your wits, and made your pitch. All of that was very good for writing a report that would be relevant and well received. So traveling and getting around was quite important to the careers of RAND people, and those who just didn't like to travel, or who somehow didn't arrange to be authorized to travel, suffered.

Collins: Let's take a hypothetical situation. You're sitting down as a department head, and it's time to review your staff. What are the things that go through your mind as you look at their work product or their activities and you make a decision about whether or not they're going to get a promotion or a salary increase?

Digby: Well, you pretty well know how smart they are in general from arguing with them about some past project and some future one that they want to start. So that is in your mind: what is their potential? Being a department head, you are responsible for getting them to do the utmost of that potential, so you think about that a bit. Now you have read their written reports because, as a department head, you were required to read through all of that. Some of them have expressed their views clearly and some have not, and you've seen how they reacted to your criticism of their expressing their views. I guess most of all, though, the ones who became the stars are the ones who saw important problems and had the self-starting ability to pick on that important problem and say, "I want to work on that," whether it was part of somebody else's larger project or a proposal for starting something. That gets to be fairly clear.

Collins: This may seem obvious, but I'm interested in the question of what constitutes an important problem or an important contribution. How is that asssessed?

Digby: A department head would sit in on the Management Committee and participate in the RAND-wide discussions of what was important. Also, he probably wouldn't have been a department head if he wasn't fairly broad-gauged, so he would use that feeling about what was important to judge the people who worked for him. But RAND had a--with the tone set by Collbohm--quite tolerant idea about different styles of research. The research didn't necessarily have to be in a discipline that the boss liked, as long as it was good.

Collins: One other catch phrase you used was "well received." In what context do you mean that? Do you mean by peers in the organization or by the Air Force officers?

Digby: In both. The peers in the organization tried to criticize things and judge them with regard to their relevance to the Air Force and what kind of impression they would make and whethert they would make a practical difference. RAND had enough from engineers its birth to really want to make practical differences how the world worked, not just to make theoretical contributions, which some of the mathematicians tended to want to do. But the RAND leaders said," Well, is that going to matter? How can we get that across to a reluctant Air Force?" By and large, Air Force people being operators, they wanted to keep doing better in exactly the patterns they had had all along. That's why things like the Wohlstetter study were so hard to sell. The Air Defense study was less hard to sell because it was just describing doing more of something that, at the time, was a growing job the Air Force had to do.

Collins: Why don't we conclude on that. Thanks very much.

James Digby
August 19, 1991

TAPE 1, SIDE 1

36	Discussion of "The Next Ten Years," an overview of RAND work and a consideration of long term issues.
36-37	Authorship of "The Next Ten Year" (Hitch, Brodie, and Marshall).
37-38	The Brodie-Hitch relationship; contrast in Brodie's, Wohlstetter's, and Marshall's intellectual approaches.
38-39	Digby's involvement in the Strategic Air Power Project; SAPP's purpose--to review and generate studies; integration of SAPP substudies; the relationship between SAPP, the Management Committee, and department heads.
39-40	Digby's responsibilities as head of the Operations Department; terminology question--operations search versus systems analysis; RAND's relationship with operations research in the outside world.
40-41	Digby's notable accomplishment during his term as head of the Operations Department; Operations Department staffing and structure; staff background--electronics, aircraft, missile, and later, operations research; Operations Department as a resource for doing other studies within RAND.
42-43	Establishment of the Research Council; reasons for its establishment; responsibilities of members of the Research Council.
43-44	Power struggles and RAND in the late fifties; why some key people became so powerful by this time; why the power issue became critical late in the fifties, rather than earlier; the effects of the reorganization of 1956; the issue of Frank Collbohm's control over RAND and his desire to change how things worked, as a factor in the power struggles; the effect of the establishment of the Research Council--divided authority, diminished role of department heads.
44-45	Reactions of department heads to Collbohm's maneuvers; Digby's role as a staff member of the Research Council; RAND work with the Department of Defense in the early period of the Kennedy Administration; Wohlstetter's firing and reasons for it; Digby's subsequent appointment as program manager for International Security Affairs (ISA) studies.

TAPE 1, SIDE 2

45-46	The Research Council's attempt to develop a preferred map of research activity; results of the Research-Council's deliberations.
46-47	The contrast between descriptive and

quantitative/analytical styles, the role of empiricism in RAND studies.

47-48 Factors in the RAND social scientists' anti-Wohlstetter" posture; Williams' "Hunting the Tiger' document; extent to which the work of the Research 22-Council was taken seriously by its members.

48-49 The Strategic Offensive Forces Study (SOFS) and how it evolved as an idea in the Air Force; "The Letter to General White;" what SOFS was trying to achieve; strategic aspects of SOFS--counterforce and protective construction.

49-51 Digby's 2/11/59 memo, sketching subject areas of SOFS; organizational problems faced in putting SOFS together; SOFS' strong Air Force blessing how Ed Barlow's low-key style and practicality facilitated putting SOFS together, Ed Lindblom's observations on SOFS during summer of 1959.

51-53 Digby's perceptions of how well SOFS was conducted; the quality of Lindblom's critique of SOFS' how the SOFS audience (the Air Force) affected how it was conducted, Sherm Wilkins' importance as a SOFS team member.

54 Digby question the overall influence of SOFS. as compared with earlier, smaller RAND studies.
The question of whether RAND was better suited to deal with limited problems in its studies, or larger, more integrated studies like SOFS. A strong Air Force presence during the SOF Study: was this a positive or negative element?

53-54 Lindblom's observation about the existence of "biases along party lines' with regard to strategy at RAND: what was the character of these different points of view, and what were the resulting tensions? How were the different points of view built into a big study like SOFS?

55 Strengths and weaknesses of SOFS; what it did for RAND organizationally; questions of whether it brought about a better mutual understanding between RAND and the Air Force.
SOFS recommendations against the B-70, and the question of manned bombers versus missiles; Digby's division of responsibilities during the course of SOFS.

TAPE 2, SIDE 1

55-56 Digby's involvement in post-SOFS briefings; Digby's assessment of the relative importance of SOFS in RAND's history; Digby's work with ISA after SOFS.

56-57 Changes at RAND (if any) as a result of the departure of key people to the Kennedy Administration and elsewhere; closer relationship with DOD, and negative effect on RAND's relationship with the Air Force; how Frank Collbohm responded to these changes.

58-59 Nature of Digby's interaction with Collbohm while heading up ISA' organizing RAND trip to Paris at

Interviewee: Mr. James Digby

Interviewer: Mr. Martin Collins

Date: August 19, 1991

Place: RAND Corporation
Santa Monica, CA

TAPE 1, SIDE 1

Mr. Collins: Last time we explored in some detail the activities of the Strategic Objectives Committee, but we didn't have before us one of the principial products of that group, a D document entitled "The Next Ten Years." What struck me about it primarily is the breadth of the treatment in the document. It seems to be a synthesis of a lot of the RAND effort up to that point, an attempt to plot future directions based on previous work. Was that to your knowledge the first attempt at an overview of the RAND enterprise and what it meant for its involvement in national security affairs?

Mr. Digby: Let me consider later whether it was really the first because there were in some ways precursors to that. However, it was a very important occasion in which senior people, some of The smartest in the RAND Corporation, had consciously considered major long-term issues and debated them among themselves and then came to a conclusion and appointed Charlie Hitch, Bernard Brodie and Andrew Marshall to write up what was more or less a consensus. Anyway, it was not done on a consensus. They were appointed because people trusted them to do the right thing, and they were not required to have everyone's point of view represented precisely. For example, John Williams had some non-consensus points of view that were not represented in that document.

Let me expand a little bit on why this was. There were a few important wide-ranging papers that had come earlier, notably some of the ones relating to the H-bomb. Those were an attempt to take a lot of factors into account. But they concentrated just on what would happen if the United States went ahead and developed the H-bomb. So they were not as broad gaged as "The Next Ten Years."

Collins: Four authors are listed on the document. Did any one of them or grouping of them take more responsibility for drafting the document? Do you know the circumstances of its composition? The reason I ask that, is judging from Williams' responses to it he directs all these principally at Brodie and not at anyone else who was author of the document.

Digby: First of all Brodie responded to some things that Williams wrote, and the Brodie/Williams exchanges were not necessarily based on things that were in "The Next Ten Years." That was a separate set of exchanges and you should study the dates on those with respect to the date of "The Next Ten Years." As to authorship, all three of the authors were quite literate people. I would guess that leadership really came from Charlie Hitch. Charlie had the highest rank in RAND. Let's see, it was Hitch, Brodie, Marshall and--no it was just those three. Marshall is likely to have--he was the quiet, but a steady and determined member of the group. So he is likely to have done more correcting than initial writing. And I would guess that of the initial things that were done, probably Marshall did the intelligence appraisals, Hitch did the economic and world picture appraisals, and Brodie did the political appraisals.

Collins: Do you have any insight into Brodie and Hitch's relationship? I ask that because they had different styles of scholarship, economics and political science, at least as represented by the work that they did.

Digby: Their relationship was quite cordial. Excuse me, I keep coming back to a little modulation of some of these answers. Hitch was in every way a gentleman, very thoughtful, slow of speech and I think he got along with Bernard very well, although there was of course the matter that Wohlstetter, who later did not get along with Bernard, was in Hitch's Economics department earlier on.

Collins: One thing that has been pointed up as a contrast by you and by others with respect to Brodie and to Albert Wohlstetter is, as I think you put it in a volume you shared with me, narrative approach. The juxtaposition of approaches on the surface seem distinctly different from a heuristic approach, and one that relies more on empirical underpinnings and elaborating conclusions.

Digby: Well, mathematical logic, notably.

Collins: But in this document, "The Next Ten Years," you see an interesting combination. It rests on a substantial comprehension of a large body of work that RAND's done. It's rooted in an empirical picture of the military situation and a drawing out of possible political conclusions and strategies and that kind of thing. So when you look at this particular contribution that Brodie made, there doesn't seem to be a real distinctive difference between perhaps what he was attempting to do or the intellectual frame work within which he was working, and what Wohlstetter may have been trying to do.

Digby: Well, you can't really judge the difference between Brodie and Marshall from something that the collaborate on, because I'm sure that almost all the quantitative parts of "The Next Ten Years" were done by Marshall and Hitch, both of whom were trained very thoroughly in statistical economic analysis. And I think Brodie understood a lot of that, but he would not normally generate quantitative studies. So to draw the contrast you have to look at things that Bernard wrote all by himself and things that Marshall wrote, some of which are quite technical economics papers. For example, in the first issue of The Journal of the Operations Research Society of America, Hitch wrote an article with a mathematical appendix by Marshall. And it is very mathematical!

Collins: Okay. I'll move on but we may want to return to the implications of that later on. One other thing I wanted to follow up from our previous discussion was your involvement in the Strategic Air Power project. Our discussion bifurcated when we were talking about that last time and we never finished. I didn't really have an appreciation of specifically what that project was designed to do. Perhaps you can elaborate on that for me.

Digby: Okay. The RAND culture, by the mid-fifties, had begun to reward people who were project leaders. So there were a lot of smart and energetic people around, and everybody wanted to run his own project. There was a period when RAND management began to have some concerns about the disappearance of the very big projects like Ed Paxson's or Wohlstetter's or Ed Barlow's, where a lot of people worked together under single leadership. Instead, there would be a project leader who had half of himself and a third of person X in it, and a quarter of person Y in it, and a tenth of person Z. The Strategic Air Power Project was one of the attempts by RAND management to get back the momentum of having a lot of people working in coordinated fashion. And you remember its leadership was Wohlstetter, myself and Herman Kahn. Herman and I thought it was fine that its acronym was SAP, but Wohlstetter was serious about it and he really never wanted it referred to as SAP.

Collins: Now, this was not in itself an attempt to do a study, as I understood you before.

Digby: SAP reviewed and tried to generate studies. We actively encouraged five or six strategic air power substudies under SAP.

Collins: Okay. Why do you refer to them as substudies? Substudies to what?

Digby: To SAP. Well we had a series of coordinated studies as opposed to each of the people who wanted to be a project leader having a study that person A might run for the next five years, beginning in 1958, person B might run the next three years after

1956, and person C might run, beginning ten years from the current date and running for ten years. So one obvious need was to have studies on things that really ought dovetail, that had some kind of coordinated timing and appreciation of the intelligence results that were mutually helpful. So that's one of the things that SAP did.

Collins: Okay. Were you, and Wohlstetter, and Kahn then going to integrate these studies in some fashion?

Digby: Yes.

Collins: Did that become an eventuality?

Digby: In a way it did because there was a succeeding study requested by the Air Force. I'd have to get some of my notes to find out all the exact nomenclature, but while SAP itself was for internal use, the things that it created were useful once the Air Force made a major request of RAND about 1958 or 1959 to do an integrated study.

Collins: Was this the Strategic Offensive Forces Study?

Digby: Yes, SOFS. It was run by Ed Barlow and resulted in the final break between Barlow and Wohlstetter.

Collins: Okay, that's something that I want to go into a little bit later in our discussion here. What then, in the Strategic Air Power project, was the relationship between that group and the Management Committee and department heads who also had a role in defining projects and looking at the long-term research product?

Digby: The Management Committee had appointed us, and in effect we reported to them. SAP would either report as a whole, or the individual studies would report at Management Committee meetings. So management was well aware of what was going on in SAP and stood behind it. But notably, Jim Lipp, Hitch and Barlow.

Collins: This was a specific device, then, on the part of the Management Committee to organize the research in this area in a more productive way.

Digby: Yes.

Collins: Okay. The other thing from our previous discussion that I wanted to clarify was the responsibilities or subject purview that you had as head of the Operations Department. We talked about the business of being a department head, but we didn't really talk about the substance of what that department was supposed to deal with.

Digby: Let's go back to the fifties. At the beginning of the fifties there was no such thing as a college course in operations research. And the name didn't really come into great currency until the formation of the Operations Research Society of America, which encouraged a few courses at MIT [Massachusetts Institute of Technology] and UCLA [University of California, Los Angeles] and a few other schools. So RAND had organized its department according to the skills that people had learned in college, like economics, mathematics and social science was the combination of political science and a few other disciplines that were related. The rest of the world would treat economics as a social science but not RAND. So, the operations department was a recognition that there really was a kind of separate skill being developed in RAND and beginning to be represented by people who were taking courses in colleges of operations research. It could have been called the Operations Research Department.

Collins: Okay. A couple of questions come out of that. One, what was your relationship with the universities that were beginning to establish programs? You seem to be suggesting that there was some kind of coordination between what RAND was doing and what the universities were considering. And the other is question is one of terminology. In other areas of RAND this kind of activity was referred to as systems analysis rather than operations research.

Digby: Okay. Operations research had a narrower meaning to most of us at RAND than systems analysis. In other words, system analysis included the economics political science aspects of a problem, as well as the mathematics and the engineering. Operations research was more like a--first of all it was a department under the Engineering Department, so it was a narrower look at that kind of thing than systems analysis.

Now, you asked about the relation with the outside world. RAND people were fairly active in the Operations Research Society of America. Charlie Hitch was one of its first presidents, and RAND people made talks at all of the early meetings. They were among the more notable talks. There was also a great intercourse between RAND and the university world, with professors coming to RAND for the summer. And also RAND people often would take sabbaticals and teach, although not quite as often as the university people coming to RAND.

Collins: I know this was a flourishing activity in other disciplinary areas, but you're saying this also applied to the fledgling field of operations research.

Digby: Oh yes, very much so.

Collins: What then were the notable projects or products that came out of your tenure as department head at that time that you would point to as contributions or accomplishments?

Digby: I'd have to look back at the notes. My mind has a little separation between what I did on SAP and what I did as a department head. First of all, the department that I was head of was under a division headed by Barlow, so it had a bit more of the personnel-type duties than of generating research. And Barlow was on the Management Committee and I wasn't as a department head. I would occasionally attend in his place. So what I did in the department was essentially hire people and read endless drafts of reports which would then have to be corrected.

Collins: Okay. Approach that from a slightly different angle. This as I recall from the organization of '55-'56 was a new entity within the corporation. The Operations Department hadn't existed before this reorganization. How was it staffed? Where did the members of the department come from?

Digby: Almost all were engineers--I'm trying to remember. I guess there were three engineering departments: missiles, aircraft, and electronics. They were lumped together into the Engineering Division Missiles Department, and an Aircraft Department. I'm trying to remember--was there an Electronics Department? Yes, I there was.

Collins: I should have an organization chart, but I don't have one with me.

Digby: There was also the Operations Department, and there were two things called staffs because they were somewhat smaller. One was headed by Bill Graham and the other by Will Kellogg. I forget what Bill's was called, but Kellogg's was basically this small group of meteorologists who were very good. There were about four of them.

Collins: Did most of your people come from electronics or aircraft or missiles?

Digby: Yes, they did.

Collins: All of those groups?

Digby: Yes. And then I hired people who were coming out of the schools with the new discipline of operations research. One notable person was Roger Levien, who later became head of the International Applied Systems Analysis Institute in Schloss-Laxenburg, outside Vienna. And another, who is still with RAND, was his good friend Tom Glennan. The first man I hired was named Firstman by the way. Sydney Firstman.

Collins: Was your group of people, then, seen as a resource for others who were doing studies?

Digby: Either that or they would lead studies. But they were interested more in systems analysis as a discipline than in turning out papers on electronic theory.

Collins: I would like to move ahead to the establishment of the

Research Council, unless there's any other particular area you want to add to what we've discussed.

Digby: No.

Collins: Before when we talked you alluded to one of the reasons for establishing the Research Council which was, as I think as you put it a concern on Frank's [Collbohm] part that too much power and discretion was getting lodged in the division heads and sort of stifling activity below and it creating an organizational problem. I wonder whether we might examine that a little bit more and list the reasons that motivated this fairly substantial organizational change that brought about the Research Council.

Digby: Well, I think you named the main one, which is that notably Williams and Hitch were becoming very powerful in the RAND establishment, and the method of making decisions at the Management Committee meetings, where Jim Lipp and Ed Barlow also participated made them more and more powerful. They were very smart people and could give excellent arguments. So Frank, at some point decided to change RAND into, eleven departments, as I recall. So in effect, he went one layer down to people who had not been division chiefs, and he made all the division chiefs into members of the Research Council. And he made Barlow the head of the Research Council's staff. No, he made Charlie Hitch the head of the Research Council, Barlow became Director of Projects, and Wohlstetter was made Associate Director of Projects, with the idea that they would look at cross-divisional or cross-departmental work. And I became one of about three people on what was called the Staff of the Research Council. So our job was essentially to help make those cross-divisional projects work.

Collins: Were these full-time responsibilities for the people who were involved with the Research Council?

Digby: Yes, except that we worked on projects too.
Collins: Okay. Something I'm not entirely clear on, and perhaps you can shed some light on it, is how some of these key people like Barlow and Hitch and Williams had this level of power by the late fifties. Just about all of these people had been active in the Management Committee for a number of years. What was different in that time than say in the early or mid-fifties?

Digby: Well, for one thing, they no longer were the line bosses of for the people in their departments. In effect, they were staff and not line managers.

Collins: You mean you were head of the Operations Department and they were not the line managers of the people of the department?

Digby: No, I stopped being head of the Operations Department in this reorganization. But Barlow no longer had anybody except his personal assistant, his secretary and me directly working for him.

Collins: That's after the re-organization.

Digby: After the reorganization. Before that, Barlow was the line boss over the five departments and two staffs. And if I had tried to give somebody who Barlow thought was a nut he had the right to veto it, although usually he didn't.

Collins: In the reorganization of 1956, Barlow was the one who made out best, in the sense that he had a greatly expanded number of employees under his supervision. I think mathematics and economics and social sciences stayed just about the same. There wasn't any real dramatic increase in terms of departments underneath those division heads.

Digby: Except economics. Economics had had a kind of nominal department already. We were trying to decide how the five division chiefs fared after the reorganization.

Collins: Why did the issue of their power become critical at this time in late '58 and early '59, as opposed to an earlier point in RAND's history in the fifties, when you pretty much had the same people in the same positions, functioning in the same ways.

Digby: Well, organizations mature like apples, and I think in part Collbohm was beginning to see patterns of management that he didn't fully approve of and didn't really control, or had difficulty controlling. So I think it was partly a matter of control. For example, I'm about to name an anachronism here with Herman Kahn, but the roots of this were already there. Albert did things that Collbohm didn't want him to do, and so he was too independent for Frank. Frank came from the aircraft industry, where bosses have quite absolute control over what their employees do. He didn't like the way Albert did his own thing, and he somehow couldn't control it entirely through Charlie Hitch. Similarly, he had a very strong prejudice against civil defense as a subject, and he didn't like the way Herman Kahn had made quite a big RAND project out of civil defense. But under the old system, Hitch and Kahn's boss, Ernie Plessett, sort of protected Herman. And Frank was disturbed by a few of these things. Somehow this affected his desire to change the way the organization worked.

Collins: Did Hitch maintain his chairmanship of the Economics Department after this?

Digby: No, he did not. It went to Joseph Kershaw.

Collins: So in a sense, Frank thought he could exert more influence over a new crop of department heads. Is that it?

Digby: Yes, and he could. In effect, he divided authority. The

Research Council had the main authority for creating projects and reviewing them, and the departments had the main authority for hiring people and reviewing written projects for professional accuracy.

Collins: So in a sense this also diminished the role of department heads?

Digby: Yes, it did. Except that those of us who had been under Barlow already had a somewhat diminished role.

Collins: Right. This sounds like a fairly major power struggle, in a sense, within the organization.

Digby: Yes, particularly Williams often felt like challenging Frank.

Collins: What was the nature of the reactions of the department heads that you were the closest to, to Frank's maneuvers here?

Digby: Actually, I was on a honeymoon and I was not really following it in great detail until I got a letter from Gold [Dick Goldstein] telling me what had happened; I was at my hotel in France. And that's the way I learned about it.

Collins: Okay. What was your role, then, as a staff member of the Research Council? What kinds of things were you doing in that capacity?

Digby: We encouraged the right kind of projects. It was in two parts, really. My first set of tasks was to do what Barlow wanted done in terms of encouraging projects. we had worked together for years, and I was very comfortable with that. He was smart and steady, and the things that he wanted to do were usually very good things to do. So we tried to make sure that the projects that existed in RAND fit into a useful pattern, and if there were any lacunae, we tried to persuade somebody to start a project. We had been doing that for a short time when along came this Air Force request for the SOFS Study. So that became a somewhat consuming job for Barlow and for the Research Council staff.

Now let me mention that slightly later, it was right when John Kennedy came to office, Wohlstetter went to see Bill Bundy on Cape Cod. He was interested in having RAND work for the Assistant Secretary of Defense, International Security Affairs. We should look at some of the other records to make sure I have the right Bundy. But anyway, during the period that the Kennedy Administration was being formed, there was a contact with RAND about doing research for the Defense Department as opposed to just the Air Force. RAND had already been working for the Advanced Research Projects Agency (ARPA), but this added a new dimension of truly policy- related research. So Wohlstetter landed that contract for RAND. Meanwhile, Hitch became an

Assistant Secretary of Defense in the Kennedy administration, and Harry Rowen to be Deputy Assistant Secretary for International Security Affairs. So Harry Rowen was the person that the RAND contractors actually reported to. And that worked out in a very productive fashion because Harry was very smart and knew what RAND could produce. The RAND people worked very hard for him and for ISA.

But I'm about to lead up to my own role, which is that at about this time, Wohlstetter made his trip around the world, against Collbohm's instructions. And Collbohm fired him. There was also an incident in which somebody had left a RAND D Document relating to [Charles] DeGaulle in Harry Rowen's office, and it had been spotted by somebody from the Social Science department, which was in general the anti-Wohlstetter force at RAND. And Collbohm was a stickler, as was Hans Speier. Hans was no longer the head of Social Science, but he was very close to Joe Goldsen, who was. So they made quite an affair over finding this RAND document in Harry Rowen's office.

Collins: You mean when Rowen was in DOD.

Digby: Yes, when Rowen was in DOD and I don't know the full story but by Wohlstetter took the blame. It may have been Nathan Leites who left it there, it may have been Roberta [Wohlstetter], but Albert took the blame, and that was the second reason for his firing. And when he was fired, I then got a new job of being program manager for the ISA [International Secrutiy Affairs] studies, which was the first time RAND had had anybody called a program manager.

Collins: What was the reason for that designation?

Digby: It had been Albert's job, but Albert not being a very--you know, being Albert, does things in his own way. But it was my job to keep track of what it was the people in ISA wanted, and to make sure RAND delivered in a prompt way and followed all the necessary strictures about who they reported to. Also, I had to make sure that ISA came up with enough money for us.

The ISA contract included like about ten projects. And it was about a million dollars, so it was a matter of managing the million dollars so that ISA felt like it got what it was supposed to get, and so that RAND worked on what it thought were productive things to work on.

TAPE 1, SIDE 2

Collins: I'd like to return to the Research Council again. You were saying earlier that the Research Council tried to get an overview of the research activity at RAND, and that implies a couple of things. One is that they were attempting to develop a preferred map of research activity, and you also used the words "fill in the lacunae" to persuade people in the organization to take up particular research projects.

Digby: Or to change what they wanted to do to something a little bit different.

Collins: Right. I guess it's just a question about how this potential or desired universe of research was mapped out or considered. And about the tools that you had at your disposal to encourage, persuade, get people to alter their activities to fit in with this larger sense of desired research activity.

Digby: Well, it was a somewhat messy process as I recall. The Research Council had periodic meetings, about once a week. As staff to them, I was in the room with them, and it was rather hard for them to be decisive because they were all a bunch of very smart and very senior people. But I guess they eventually came to points of view. I think they mostly came to points of view by having somebody make an excellent criticism of some proposed course of action.

Collins: For example from the SOC activity came the "Next Ten Years," comprehensive document, a blueprint for things to be done. Did anything come out of the deliberations of the Research Council that would be comparable to that?

Digby: I don't believe so. I don't remember. If it had gone on in its early form long enough, this might have resulted. But as I recall nothing of that sort really happened. I think the first big event that happened was the SOS study, and that took over as being the most important. And along came the new administration. I've forgotten just when the Research Council was disbanded, but several people left. Barlow left to take a new job at Varian Associates, and Hitch left to become an Assistant Secretary of Defense. Hans Speier was a less accepted chairman than Charlie had been, although he had Collbohm's ear. But his point of view was much more that of the essay writer than of the quantitative engineering person.

Collins: Just to return to that there doesn't seem to be a basic incompatibility between the essay writer and somebody who is interested in quantification. The empirical can lay at the bottom of both styles of presentation, so I guess I'm not entirely clear.

Digby: The social sciences have become much more quantitative since 1950. You have to remember the people that we had were trained pre-1950, by and large. That is, the people who were important in these events, and they would tend to write--their doctoral dissertations might be 500 pages, with maybe 150 or 200 footnotes, including some obeisance to their major professor. The economist's doctoral dissertations would be fifteen pages for some of the very best, and have five footnotes. So it was a difference in style. And the economists naturally thought of themselves as being somewhat superior to these fellows who should say, on the one hand, so and so, on the other hand, the opposite.

Collins: Part of the question, then, is what form the empirical basis of your argument comes in--whether it comes through some kind of social data gathering that is not quantified, or whether it is something that can be structured in such a way that quantification makes sense. For example, I'm not familiar with that many of the social science studies, but one I am familiar with is the so-called Warbo Study of the early fifties. That is very strongly empirical in its way, and in the way it gathered data and tried to extrapolate conclusions about the responses of populations to the threat of war. So, there is this strongly empirical basis to such studies, but it's not in terms of quantification.

Digby: Yes, but Warbo was not representative of the average political science study at RAND. In fact, Hans Speier was a more quantitative social scientist than a lot of the people he brought into his department. Because their specialties might be politics in the Soviet Union, or politics in Germany or politics in England or the Scandinavian countries, and so forth. By and large these senior people absorbed a kind of gestalt view and wrote essays and did not use numbers.

Collins: When you indicated earlier that the social scientists were a large part of the contingent of the anti-Wohlstetter forces, as you put it, was it basically a question of methodology in the way you approach and define problems and pursue answers, or was it something else?

Digby: Well there were probably two factors. The first and underlying one was a different way of pursuing answers, and the term "belles-lettres" was Wohlstetter's as a critique of the Social Science Department--the feeling that belles-lettres had their place in life, but somehow they are not quite as confidene-inspiring as guides to correct answers as quantitative methods. The second thing is that the social scientists often thought somewhat different things were important than the economists thought.

Collins: Can you give me an example of what you're thinking of there?

Digby: The most notable one is not from an economist but from John Williams who--well, John Williams and Ed Paxson had ideas about--it was Williams who thought that we ought to begin World War III. His D on "Hunting the Tiger" had that as its theme, and the social science people were horrified by that thought and Brodie in particular. Brodie is the one who wrote the counters against it. But that's probably the best example.

Collins: Okay. Within the Research Council you had all of these strong personalities, people who had had important positions in RAND, and by any generous interpretation they were stripped of a substantial amount of their power by being put into the Research Council. Did they take this activity of the Research Council seriously, given that context within which it was formed?

Digby: Yes they did. I think they all took RAND fairly seriously, and being put into a new arrangement, they did their best with it. Also, it was probably intellectual fun because the people that they worked with were very smart people and the subjects that they worked on were important subjects. And part of the business of being a department head at RAND was pretty boring, which is reading all the written output and reviewing it. Very important but it gets kind of deadly. I think I had forty people in my department and they would turn out things I had to review. Every day and a half or so there would be another forty or fifty page paper. So that gets to be quite a burden.

Collins: I'd like to talk a little about SOFS now. To the best of your knowledge, what was it that brought about the formulation of the study and how it initially evolved at RAND?

Digby: Well, it was an idea in the Air Force. I'm not sure, I may have known at the time why they were thinking of it, but it was one of those turning points in the Air Force. In some ways it may have been a reaction to Wohlstetter's briefings, and in other ways a reaction to the coming of ICBMs [intercontinental ballistic missiles]. Probably the latter as much as anything else. And being that it was to be quite forward looking, they thought they would rely on RAND because RAND was their instrument for looking into the future. RAND had excellent relations with General Tommy White, who was Chief of Staff of the Air Force at about that time. One of the famous RAND documents which I've not been able to find was called "The Letter to General White." Actually, I found a draft of it, and it's filed with Vivian's [Arterbery] stuff.

Collins: I have it right here. I've looked for it in there but I haven't found it.

Digby: I have a draft of it in there.

Collins: Okay, I'll look again. This is all the material, and I haven't come across it.

Digby: I think this was prior to the SOFs Study. I think it was '59.

Collins: As I recall, I think it's referred to in here as early 1960.

Digby: Okay.

Collins: What was the substance of that letter? Do you recall?

Digby: Oh, it had lots of different things, but in part it said, pay attention to what Wohlstetter said about the vulnerability of SAC [Strategic Air Command]. It probably told them to put new engines on the B-47, something that the Air Force resisted every time RAND proposed it. It probably had some stuff about the air defense of the United States in it.

Collins: These were all issues that were dealt with under SOFS to a certain degree, so was it an offshoot of the SOFS activity?

Digby: What I can't remember is the sequence of events. I think the Tommy White letter came first and then SOFS later on. I'm not absolutely sure. They were both in the '59, '60, '61 period. And in fact, it may have been the fact that the White letter was received with some interest that resulted in somebody deciding that RAND ought to do SOFS. But I would have to check the files to make sure about sequence.

Collins: Yes. To the extent that I've been able to locate documentation so far, SOFS for me, at this point, is like the elephant where I manage to feel part of it , but I really don't quit have a grip on the overall concept of it and what was really being attempted through pursuing this study. Can you give me an encapsulation of what you thought it was trying to achieve?

Digby: Okay. It had some strategic aspects and some mechanical aspect. The strategic aspects were that it proposed a serious consideration of counter-Soviet Air Force attacks. In other words, counterforce. And secondly, it proposed serious consideration of protective construction and was specific in some of the protective construction things that had to be done in the era of ballistic missiles. It may have had some things to do with ballistic missile defense, but I don't recall that specifically.

Collins: Okay. In order to stimulate our discussion a little bit, let's refer to this document that you prepared, dated February 11, 1959. This is in what I've put together as a SOFS folder from your papers, which seems to be at least an initial attempt at sketching out the subject areas and projects that one might have to grapple with in SOFS. And it's truly a daunting compilation.

Digby: Yes, it was a very ambitious study. Yes I see I have aircraft design and operation here, missile operations and missile design, penetration. Do you know what penetration means in this sense? It means whether an airplane can get through the Soviet air defenses and get to its target. Anti-ICBM. "Air defense" means of the United States. Posture and program costs, by Burt Klein, who was one of Charile Hitch's successors as head of the Economics Department. And then smaller projects on wespons, the Soviet team, base rights. That is a useful reminder.

Collins: That basically, I covers a fairly substantial fraction of RAND expertise.

Digby: Oh yes, it did.

Collins: And I think I noted at the conclusion of that document that you indicated it would take up the time of approximately fifty engineers, with no indication of the time of people from other departments. But clearly it would also be a substantial contribution from them.

Digby: There would be quite a few from Economics and Social Science and Mathematics involved, too. It was a big undertaking.

Collins: Now as I understand it, your role in this came to be one of a deputy or associate to Barlow in carrying this out. What were the organizational problems that you faced in trying to put together a project of this dimension? And what I also seem to get from some of the documentation was that there was a fairly limited time in which to pull this together and make recommendations or draw conclusions, or at least present a picture.

Digby: Actually, it seemed to come with such a strong Air Force blessing. Probably from the Chief of Staff calling Collbohm and Collbohm talking to the Research Council and department heads and so forth. So there was not much bureaucratic resistance to it. And I think also the fact that it was to be for a fairly brief period of time helped us to get it all going. But I notice the people that I named there under the individual projects were all quite senior people, in some cases department heads. So it was a thoroughly authorized and legitimate kind of project. People were rather eager to get RAND's expertise on all these things before the Air Force. And people trusted Barlow to be able to put together a thing that was that complicated. People liked Barlow. He was kind of old shoe and had a fantastic memory.

Collins: When you say old shoe, what are you referring to? Just that he had been in the organization and people were comfortable with him?

Digby: He had a very different style from Wohlstetter, say. Wohlstetter was imposing and would cite very esoteric references from mathematical logic or opera or literature. And Barlow would tend to be much more low key and say, "Oh, I remember in engineering school I learned that if you're operating a pile driver and it goes down only 10 percent of the amount it went down before, it's time to stop." You know, things like that. Very practical kinds of knowledge. And he had gone to a very practical engineering school in Jersey City.

Collins: The most insight that I have into the kind of character of the SOFS enterprise is the things that Ed Lindblom produced during that summertime when he was at RAND in 1959. He apparently watched the SOFS activity fairly closely. It seemed to be a critical period in SOFS, in which studies were beginning to come together and RAND was planning for briefings and attempting to distill some product out of all of this activity. And as you note from this letter from Lindblom to Barlow, dated August 14, 1959, there's a real sense of tension and disorganization. Well, not disorganization, but of forces that are essentially pulling the project apart. What were your perceptions of how successfully this thing was going along, whether this very large task that had been set was really able to be done or not?

Digby: I think it operated reasonably well, given the very large scope that it was supposed to have. And critiques like Lindbolm's were in the RAND tradition. You try to get a smart person to tell you what's wrong and then pay attention to it. They're not always practical. In other words, you can't necessarily change Jack Lind, who's going to brief about missiles, just because Lindblom, who is from a very different culture, doesn't like what he says. But Barlow was quite capable of understanding Lindblom and trying to fix it. There was, of course, another tension there. This was the main period that Wohlstetter was being resistant to Barlow. He was nominally Barlow's assistant, and Albert didn't like that designation. But Harry Rowen worked--of course, Harry has always worked well with Albert, and Harry was a useful project leader on the SOS Study for Barlow.

Collins: Yes. My sense is that Lindblom's observations about the project were not so much on the quality of the substance or making comment about the substance--he didn't feel he was qualified to do that--but just looking at the procedures under which the studies were being done and posing the question of whether or not they had a proper venting or review. I thought he made an interesting observation ties back to some of our earlier discussion, on page three. This was the question about communicating results to the Air Force, and he says the following:

"What should be communicated also depends upon the Air Force's readiness to hear, on their present capacities to use the information effectively through the implementation of recommendations accepted on SOFS confidence in its conclusions, etc. What to try to communicate is an extremely serious and difficult question for anyone who wants SOFS not simply to display its results, but to be effective in influencing Air Force decisions."

This raises a larger question. The RAND studies are presented, for the most part done in the vein of studies by professionals or scientists in the university laboratories. You do a study, you produce results, you send it out to your community of peers for review. What this suggests is that the audience for RAND studies is very different from a typical scientific community which receives the input of researchers.

Digby: Yes, it does suggest that you're talking to decision makers, and you undertook this study because decision makers asked you to do it so they can make additional decisions. They had two smart colonels and one who wasn't so smart--he may have been smart, but he didn't seem to be. He went out partying all the time and was not always in very good shape. One of them was named Sherm Wilkins. A very smart colonel. Sherm was the vehicle by which Barlow understood what kind of messages could be transmitted to the Air Force and how to form them up to be most useful. So he was a very good member of the team. He wasn't really a team member, he was the Chief of Staff's representative on the study. But he was very cooperative and very useful in making the study hit on the important questions.

But I'm not sure that the SOFs study had as much real influence as some of the other smaller RAND studies when you put it all together. It came at a crucial time in the history of the Air Force because ballistic missiles were just coming in. All the people who heard about it learned a lot of things about the way you have to protect against ballistic missile attack and how important the missiles were. And yet, about that time, the Air Force was proposing to put Atlas missiles above ground. The first Atlas missiles were installed like sky rockets, above ground, the way we would do a sounding rocket today. And it took some persuasion, mostly by Harry Rowen, to get them to put the missiles into concrete hardened silos. That's the kind of thing that was reiterated in this study, but I think of the crucial time on that being the study about missile hardening, not the SOFS Study. The SOFS study collected a lot of RAND wisdom and put it out all in one piece, but most of it had to be developed individually earlier on. I don't remember as much effect from the SOFS study as from individual studies on specific things.

Collins: This poses a question, I think, of what RAND was better suited to do as an organization: to grapple with more limited problems like the question of hardening a missile silo and its importance for a broad pattern of security; or to take on something like the SOFS study which involved the integration of a number of studies, and come out with a comprehensive picture.

Digby: My own feeling is that one should alternate such activities. In other words, about every four years an outfit like RAND needs to have a big overall putting together of things so that the ideas have the right respective weights. But then, in between, you have to develop smaller projects that you can talk about with greater certainty.

The Air Force itself has tried to have two or three major future-oriented studies. One of them was called Project Forecast. And then there was a Forecast II. I think the main benefits of those studies were in generating certain things as subparts, rather than the correctness of the overall view.

Collins: This may be a bit of a digression, but Project Forecast was situated at Aerospace and not at RAND. Why do you think that was?

Digby: Most of the people on Project Forecast were military people or government employees, and I guess, for one thing, RAND didn't have the space to do it. I'm not sure RAND would have wanted to do it either. But for Aerospace Corporation, its role in life was to do things the Air Force wanted it to do.

Collins: Okay. In discussing this with Ed Barlow, one of the things that was troubling and problematic for him about the SOFS exercise, and what he thought was problematic for RAND as an organization, was that there was a much stronger Air Force presence during the course of the study, involved in shaping it and in being present while it was on going. How did you see that? Was that a positive or negative element, in your mind, for doing the study?

Digby: I have a feeling that that needed to be done every now and then. In other words, like my alternating theory about the big studies versus the more detailed ones. It was good for RAND to be exposed to how smart colonels like Sherm Wilkins felt about its studies every four years or so, or for RAND people to go and join in something like Project Forecast.

By the way, I now recall I said something wrong about Barlow. He left not to go to Varian Associates, but to go to Aerospace Corporation.

Collins: Okay, that's what I thought.

Digby: And he went from Aerospace to Varian Associates.

Collins: One of the things that Lindblom points to in his series of memos to Charlie Hitch, after he had a chance to reflect on his time here during the summer of 1959, was his sense of how, with respect to issues of strategy, RAND seemed to fall into fairly well-defined and contending camps. And in his memo, I believe he used the phrase "biases along party lines" as a way of describing it. The question comes up at this point, what the character of the different points of view were at RAND in terms of strategy and the tensions in the organization that Lindblom seemed to suggest because of these different groups having their adherence and engaging in a fair amount of back and forth.

Digby: I would have said that's a fairly healthy kind of thing to have been the case, as opposed to having an official strategist who decides what's the proper strategy--instead, to have two or three approaches and have smart people argue about it. In other words, I don't see a unitary truth to be found by voting on it.

Collins: I don't think Lindblom was implying that. I think what he was perhaps suggesting, at least in the case of the SOFS study, was how RAND could bring these contending points of view into the composition of a study that was meant to recommend particular things to the Air Force. How do you build in these different points of view to aid the Air Force in their decision making?

Digby: Doing a big study like SOFS is one way of getting people to think about other people's points of view on strategy. So it was useful in that respect, I think. By the way, I should note that Lindblom had been away from this kind of work for about five years and was a professor of economics, I think at MIT.

Collins: At Yale.

Digby: So he was brought in just to survey it, and one has to remember that his comprehension of what was being said may not have been always completely full or correct.

Collins: Yes, I appreciate that. As I mentioned, I am operating from the perspective that he has, at this point, the best or the most accessible documentation on this.

In your judgment, then, what were the strengths and weaknesses of the SOFS experience? Did it do anything for the organization?

Digby: You know, I should say that once we got the reports and briefings out the door, we moved on to other things in a very urgent way. I don't recall SOFS as doing much organizationally, other than giving Barlow an important task to do after the creation of the Research Council and pointing up the fact that Albert wouldn't work under Barlow very well. We learned that during SOFS organizationally, and it was good for getting people to work together on a common purpose for a while. But it was more or less a blip, in my view, not a major turning point or change. It did not bring about any great changes in RAND. I would say the changes were really brought about with the incoming [John F.] Kennedy Administration and the fact that Hitch, Rowen and about four other RAND people moved into central jobs in that administration and we began relating to them. That was a much more important sea change at RAND than SOFS.

Collins: Did SOFS, to your knowledge, serve the purpose of getting the RAND people closer to the Air Force points of view and the Air Force closer to an understanding of RAND activities? Did it serve that kind of mixing process?

Digby: To some extent, because we had about three officers out here, including the one I remember best, Sherm Wilkins. He was very smart, and he conveyed the Air Force point of view on these things. So I think it was good in that respect. And the RAND visitors who went back to the Air Force, in the course of SOFS, of course absorbed broad gauge points of view because it was a broad gauge study.

Collins: Do you know what area of the Air Force Wilkins came from?

Digby: Plans.

Collins: As I understand it, one of the recommendations that came out of the SOFS work was not to proceed with the B-70, which was the Air Force's next generation of bomber. Do you recall anything about that particular part of the SOFS activity and the question of manned bombers versus missiles that were very much at the heart of the SOFS activity?

Digby: I don't recall too much about it, but it would be typical that the RAND people would feel that a new bomber like the B-70 was a very expensive way to do the work of strategic air command. That's why, for example, we were proposing reengining the B-47, and to use B-47s in refueling, and things like that. B-70 was never a very popular airplane at RAND. But I don't recall many of the details of it.

Collins: But during this period, was most of your activity taken up with SOFS, or was it divided with other responsibilities as well?

Digby: I think a lot of it was taken up with SOFS. I actually was on a small team--I led a small team that tried to calculate outcomes with very simple minded calculations. In other words, we did Soviet attacks against United States facilities, and we tried it with one hour of warning, and with ten hours of warning, and with two days of warning, and things like that. And we tried it with different effectivenesses of the air defenses, and it was a blackboard/pen and pencil simulation of what would happen. I was very busy with that, and I thought the results were quite fascinating. But I don't recall too much of what those results were. And I'm not sure of the extent to which they were briefed in--we would try alternatives for the United States forces, and then Olaf Helmer, Dave McGarvey, and I would sit in my office with calculators and slide rules--we were still using slide rules at that time--and the blackboard and scratch pads and decide how much of the United States Air Force was left to make a retaliatory blow or what would happen under different circumstances. So that was one of the things that kept me quite busy.

TAPE 2, SIDE 1

Collins: Were you involved at all in the series of briefings that came out of SOFS?

Digby: To tell you the truth, I've forgotten. I might say, in general, that our giving this much attention to SOFS gives it more importance than it had, in my general feeling of the important things that were going on in those years. I think the interesting thing about it was that it involved a lot of RAND people and it involved a lot more interaction with the Air Force. But it was an intense short thing, and if you asked me what the five most important things were that RAND did between '55 and '65 I probably wouldn't even remember SOFS.

Collins: But yet you pointed out that Frank gave his imprimatur to this kind of activity to get an institution-wide commitment to it, and that strikes me as a bit unusual in RAND's history.

Digby: Yes, it was unusual in that respect. I don't recall it as generating too many new ideas.

Collins: Okay, What did you move on to after the SOFS activity? Was this what you were describing earlier?

Digby: I became the program manager for ISA. That was a rather demanding kind of activity because it meant going back and dealing with the people in the Pentagon in order to get (a) enough money each year and (b) a sense of how that money would be split up among about ten projects a, and then overseeing the fact that the projects got done and product was delivered that was reasonable.

Collins: This time period in 1960, early '61, saw a fair number of changes in the organization apart from the Research Council activity. A number of key people did leave the organization, either going into the [Kenndey] Administration or to other places. Did the tenor of the place change in any noticeable way, at that point, with the departure of these people who had been very key and very powerful figures in the RAND organization in the fifties?

Digby: No, it really didn't. I would say the main thing that changed, from my point of view, was that we were working with people in the Pentagon very directly, in a much more intimate way than we had worked with the Air Force during the last half of the fifties. We saw Harry Rowen or others in ISA quite often and knew what they were worried about, and there were a number of crises afoot such as the Cuban Missile Crisis. France was undergoing some real changes about that time.

Collins: How did this much closer relationship with DOD [Department of Defense] affect RAND's relationship with the Air Force?

Digby: Negatively. First of all, the Air Force people saw some of their proposals being judged by people in Systems Analysis under [Robert] McNamara. Alain Enthoven was never known as a person of great diplomatic nicety, and he was head of Systems Analysis and an ex-RAND guy. So the Air Force would see all these people, whose education they had supported, sitting in judgment on Air Force projects and often, in a very knowledgeable way, ruling against what the Air Force wanted to do, including things like the B-70.

There was a project called Skybolt, which was not regarded very highly by the RAND people but was important to the Air Force, and it got cancelled during this early McNamara period.

Collins: Frank's view, throughout his tenure as director and president of RAND, was to do the best he could to maintain a close, cordial relationship with the Air Force. How did he respond to these changes?

Digby: I think he resented the role of Enthoven, for example. And he resented the fact that RAND people could occasionally be--I know he resented that a certain RAND person would be in the room with Enthoven, when Enthoven would hear out the colonels making a certain argument, and that person would snicker at some of the Air Force arguments. I'm not going to mention the name of the person, but the snicker is one of his characteristics. He just snickers sometimes, and not that he means anything insulting by it. But this event was reported back by the Air Force as "Here we've paid you guys for all these years, and what you do is send in people to the Pentagon who sit in judgment on this and don't take our points of view seriously." So they put Frank on the griddle about the RAND people who were in OSD [Office of the Secretary of Defense] and working for OSD. I naturally had to worry a lot about running an ISA contract which was kind of--

Collins: What was the nature of your interaction with Frank while you were heading up this program?

Digby: I made at least weekly reports to him, because I was afraid that his--basically he was a very honest guy, but he had the hearing problem and he was getting a little bit tired as RAND president by then. I just worried that he would misinterpret something that I said and not realize that he had told me to do it. So I made very extensive notes of all of my reports to Frank as project manager. I have yellow tablets full of what I said and what Frank said. The yellow tablets at first were just lists of things I wanted to report to him. But by and large we got along okay.

However, I was rather happy in 1964 when an interesting thing came up over Memorial Day. McNamara called Collbohm. He had been sort of set up for this by Rowen and Hitch--this was Memorial Day of 1964--and he said, "I would like RAND to send a team over to Paris to participate in the study of NATO [North Atlantic Treat Organization] force planning." He even suggested that Burt klein be that person. Burt, by then, was head of the Economics Department, a very unique kind of character, and Collbohm agreed to this. You know, when the Secretary of Defense calls you on a holiday, you are impressed with the importance of the message. And so very soon it was my task as ISA program manager to organize a force. I shared with Burt Klein the responsibility of getting about eight or ten people to go to Paris. They included Andy Marshall, Ed Paxson, Fred Hoffman, Joe Large, people from different disciplines but very good people. Their job was to help NATO set up a force planning exercise. It was going to take a RAND-like view of the forces that NATO would require, and this would replace what had been a military statement of need which was always too ambitious. The military statement of need would also cost twice as much as what the countries were politically willing to put into NATO. So RAND was to bridge the gap between the economic realities and the military requirements. It was a big, sort of what might be called a requirement study, except that we distinguished what we did from the military version of a requirement study by saying it was an analysis based on the resources available.

Collins: Were there, to your knowledge, in this study and other things you were doing under this program, any recommendations that suited the overall needs of the Defense Department or of NATO but ran against the grain of Air Force interests that you had to worry about specifically?

Digby: Not too much, in this case. Most of the things that were under consideration were questions of man power and NATO strategy. I'm sure there were a few things that ran against the Air Force point of view, but that was not a major stumbling block. The real stumbling block was that we were thrust into this NATO environment, where each of the nations had a radically different point of view of what it was trying to accomplish by being a member of NATO. The Greeks and Turks were members of NATO because it was their key to getting substantial military assistance from the United States. Germany was in it because it legitimized German military efforts. Britain was in it for roughly the reason the United States was. I think we always felt a kinship with the Brits and the Nordic countries and the Low Countries, of being in it for the true, overt reason that NATO was founded, where as the other countries sometimes had their individual purposes.

Collins: To follow up a somewhat earlier point when you were commenting about the Air Force concerns of RAND's larger participation in DOD and perhaps taking positions that were not favorable to the Air Force. It reflects a really difficult professional position for RAND in that, on the one level, were people who were committed to the standards of professional or disciplinary activity, making judgements to the best of their ability based on professional standards, versus the institutional interests of the Air Force. As you pointed out, Frank was very concerned about being a responsive individual and a responsive organization to the Air Force. How did these tensions play out in this period when the Air Force was very much concerned about its standing?

Digby: I would say the tensions didn't really play out, they continued, and many of the senior RAND people continued to turn out proposals and things that the Air Force didn't really like very much. But by the time this NATO exercise came along--I want to mention a couple of personal things about that. By the time that came along the contract work for ARPA and for ISA was big enough that it kind of had a life of its own, and the Air Force took it for granted that when RAND worked for a different boss, it might have some results the Air Force didn't like. Most of the things, though, that ISA and ARPA wanted us to do did not lead to direct head on clashes with the Air Force.

When we talk about Vietnam, I will mention one of those, but for now let me just mention the two personal things that happened as safety valves for me. One was that in early 1961 I was newly married, my wife was pregnant for the first time, and we had bought a new house to hold our growing family. An opportunity came up to be part of a presidential commission on air traffic control. Frank relayed this to me and never thought that with a baby coming along, I would accept it, but I did. In part it was a safety valve to get away from the tensions of running the RAND international security program, so that was an interesting thing for about five or six months in Washington.

The second thing was an even more needed safety valve. After having worked on getting this NATO force planning thing going, after about a year of it I decided to volunteer to be the head of it, replacing Gus Shubert. Shubert had replaced Klein and it was time for Shubert to come home. I volunteered to replace him and that got me out of the great pressure of being around Frank when things were going on that he didn't really like in the ISA contract.

Collins: In referring to these as personal safety valves, does that reflect a high level of tension within the organization at this time over these issues about the different contracts and potentialities for conflict?

Digby: It was more the tension in the organization. In other words, I was in charge of a program that Frank regarded with some skepticism and as a source of trouble, and this was resolved for me, finally, at the end of my NATO tour of duty when I heard that Frank was resigning and being replaced by Harry Rowen.

Collins: Were you, during this period when you were running the ISA program, giving briefings at all on that activity to the Board of Trustees?

Digby: Yes, I did.

Collins: Did you have some sense of how the Board of Trustees was dealing with this issue of RAND's expanded contract base and questions about how to deal with these sensitivities?

Digby: Not too much. That's about the time that the Board had

Newton Minow as its chairman, and I think we all felt that he was a person who wanted RAND to speak out with whatever it found, as opposed to what was good for the Air Force. But the Board behaves in a very private way. While I would brief the Board, I was not present for their Executive Committee meetings, of course.

Collins: I thought it might of perhaps have come out in discussion of your programs.

Digby: Not too much, no.

Collins: This might be a good place to wrap it up for today.

Digby: Okay.