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Hugh Miser
(Signature)

Dr. Hugh Miser
(Name, typed)

2/6/95
(Date)

Dr. Hugh Miser
July 28, 1994

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'Interviewee: Dr. Hugh Miser

Interviewer: Mr. Martin Collins

Date: July 28, 1994

Place: Dr. Miser's home
Farmington, Connecticut

TAPE 1, SIDE 1

MR. MARTIN COLLINS: To begin, if you could sketch out for the record where and when you were born, what your parents did occupationally, and some of your early educational interests.

DR. HUGH MISER: I was born in Fayetteville, Arkansas, on May 23, 1917. My father was a professor of mathematics at the University of Arkansas. He was a farm boy, born and reared in Pea Ridge, Arkansas, and discovered calculus when he was in his early teens and thought it was the most beautiful subject in the history of mankind, and for the next seventy years never changed his mind. He had gotten a doctorate in mathematics from the University of Chicago in its very early days and taught mathematics until he died at the age of eighty-eight.

My mother also was a mathematician. She had met him at the University of Chicago. In fact, she taught mathematics off and on during her lifetime. So it was not too surprising when my early interest was mathematics.

He moved from Fayetteville to Chicago, where he was on the faculty at Illinois Institute of Technology, then called Armour Institute, until 1925, when the family moved to Nashville, where he was on the faculty at Vanderbilt University, where I was then brought up and went to college, getting a bachelor's degree in mathematics.

I then went to Illinois Tech and got a master's degree in mathematics under Lester Ford, Sr. I mention that name because Lester Ford, Jr., his son, was later on the staff of the RAND Corporation. Then I moved to Ohio State University, where I started a doctorate degree, but a thing called World War II intervened in 1942. In the summer of '42, I moved to Chicago, not having been drafted yet, to help with some early wartime training courses for the army at Illinois Tech, and I stayed there for two and a half years, suspending work on the doctorate.

COLLINS: Let me just ask, when you were working on your graduate work, did you develop an area of interest within mathematics?

MISER: Yes, it was all in mathematics.

COLLINS: What aspect of mathematics?

MISER: Pure mathematics. Actually, my doctorate was in a union of analysis in topology theory, an area called "area theory" at the time. No arithmetic. The only time we ever added things up and counted them was when we added epsilons in the proof. [Laughter]

During that wartime period, when I was helping with these training programs at Illinois Tech, we were very much interested in the potential applications that the students would be involved in, and the biggest program had to do with training people for maintaining radar equipment. So the main focus of those programs was on the applied side, but they needed mathematics as a background, and that's what I helped with.

In March 1944, I moved to Appleton, Wisconsin, where I took up a post at a fairly tender age as acting head of the math department in Lawrence College, and I stayed there for nine months, until in December I got a call from a Mr. [Ralph W.] Anderson, who had a mysterious story about military applications of quantitative ideas. I met him then at the old Schrader Hotel in Milwaukee, and he discussed this, and he recruited me into operations analysis, as it was then known. So I resigned the position at Appleton, and my wife and I took the train to Washington, D.C., where I joined the staff of the operations analysis part of the U.S. Army Air Force.

Mr. Anderson was head of a group in Alaska somewhere, but by the time I got to Washington, there had been what I think of as a white slave trade. When he got to Washington, he found somebody eager to go, and he took him, and I was left behind and assigned to the rear headquarters of the Twentieth Air Force. Now, there's a little politics to explain that assignment. The Pacific was considered a naval lake, and when the air force began to get involved with preparations for bombing Japan, General [Henry] ["Hap"] Arnold, the chief of staff with the army air forces, knew that if he put his bases in the Pacific, which he would have to, they would be under the command of an admiral, which was unthinkable.

So to avoid domination of these things by naval thinking, he created what was called the global air force. Its headquarters nominally was in the Pentagon in Washington, D.C., but all of its operations were in the Pacific. As a symbol of this, the shoulder patch was, in fact, a globe of the world showing the latitude and longitude lines. So there was an operations analysis office in the headquarters in the Pentagon of the Twentieth Air Force, and it was very active, actually. It had in

it some of the people that later are in your history: Roy Brothers was one of the members of the staff, and a number of very, very able scientists, many of whom had worked in the European theater of operations, particularly in the Eighth Air Force, and even in one case in the China-Burma-India theater. So, as a young graduate student going into this very exciting environment, I was bug-eyed most of the time.

COLLINS: Was your status a civilian?

MISER: Yes, I was a civilian. In fact, there was a grizzled old army lieutenant colonel that was assigned the task of keeping me from being drafted so I could continue to work in this thing. Later, when I went overseas in uniform as a civilian, that was his job. He was very unhappy about this. He said, "Miser, I've been ordered to do this, and I'm going to do it, but in my view, it's not right." [Laughter]

During that era, I fell under the influence of probably one of the best scientists I've ever known, W. J. Youden, known to everyone as Jack. His specialty was bombing accuracy, and so I worked with him, and together we produced a number of standardized models of bombing accuracy processes that the last I knew were still embodied in air force manuals.

Also, with a meteorologist, I wrote a paper that had to do with whether or not daylight bombing over Japan would be successful, because in the summertime, Japan is extremely cloudy, and so at high altitude the probability on any given day that you could see a target to bomb it is relatively small. And so the issue was how to factor that into tactical decisions.

What made it more difficult was that the weather for Japan makes up to its northwest, which is in Manchuria. We didn't get much weather information from Manchuria in those times for two reasons. We didn't have the cooperative arrangements with the Soviet Union that would have made it possible, and the Soviet Union didn't have weather stations mostly in that area. So if you flew to Japan, the weather on arrival was likely to be a surprise at any time, and when there are only three or four days a month in which you can see any targets, it was clear that daylight high-level bombing, which was the tactic used in the ETO [European Theater of Operations] by the air force, would be a difficult proposition. We wanted General [Curtis] LeMay, who was the field commander in the Marianas, to understand this problem.

So that was my background. On the Fourth of July 1945, I went out to the island of Guam, where I did additional bombing accuracy work for the Twentieth Air Force until early August, when the first A-bomb was dropped and everything stopped. Fortunately, being a civilian, I was able to get back to the

United States before the first of September so as to renew my graduate work at Ohio State. I finished in a year, got my doctorate in the purest kind of pure mathematics, and then took a teaching position at Williams College in Williamstown, Massachusetts, for three years.

The second summer, I got a communication from Dr. Brothers, who had, in the meantime, established the postwar operations analysis office in the headquarters of the U.S. Army Air Forces, and he asked me to come down for the summer to work with his staff, and so I did that. That would have been the summer of 1948. Interestingly enough, I shared an office that summer with [Robert] Bob Dorfmann, a name that is widely known to RAND people.

That was the summer that George Dantzig, who was working in the basement of the Pentagon in air force intelligence, was in the process of inventing linear programming. I remember Bob Dorfmann coming up one day with a dittoed, triple-spaced batch of papers. They were not in the form of a memorandum. They were just in the form of worked-out linear programming problems, employing what George had already called the simplex method. Dorfmann assured me this was the most interesting thing he'd seen since he had been involved. So I read these and worked out a few problems of my own and stowed the papers away.

In the meantime, you should know that the reason Dorfmann was there was that he was so young when the war was over that, even though he'd spent the entire war in operations analysis as a civilian, he was susceptible to being drafted into the postwar army, and so they kept him employed there until that threat to his freedom, shall we say, had passed. He got old enough that he wasn't susceptible to be drafted. Then he went to the University of California at Berkeley, where he got a doctorate in economics, and his thesis employed linear programming, which is a fairly historic document because it was one of the first small books using linear programming that was published. So I had that early introduction to it.

I've since asked George Dantzig if he remembers those dittoed, triple-spaced papers, and he doesn't, which is interesting. I've lost them because I lent them to a fellow one time and never got them back. I'm pretty sure I could find them if he still has them.

COLLINS: Do you recall roughly the number of people during the wartime who worked with you on operations analysis problems?

MISER: You mean directly with me?

COLLINS: Directly as part of the group that you were with.

MISER: The group that I was with had about ten or a dozen people in it in Washington. There is a conscientious list that was published that I could show you of all the people in operations analysis during the war. In Guam, we must have had fifteen or twenty at these times, with quite a wide variety of interests and specialties. Their backgrounds were often quite inappropriate to what they did, but it didn't seem to matter very much.

The most striking contrast was, there was a Harvard professor, [William J.] Crozier, whose specialty was vision. He had done some of the early inquiries into vision that showed it was a digital process and not a continuous one. That was before the war. He had discovered why B-29 engines were unusually subject to engine fires. It seems pretty far-fetched, but he had done it. He later went out to Guam about the same time I did.

So their backgrounds were quite varied. There was a meteorologist, there was a sixty-mission fighter pilot, there was a statistician, there was a lawyer. In fact, the head of the section was a lawyer. And so on. It was quite a wide variety.

COLLINS: What was your sense of how the air force regarded your activities?

MISER: Oh, they were wide open to everything we did. In fact, let me mention the paper I mentioned earlier, that dealt with the possibility of bombing through cloud cover over Japan. My boss, who was on leave from being president of the University of Colorado at Boulder, had to sign it when we got done. In those days, you reproduced your report by putting it on stencils. This is a technology that maybe is too obsolete for you to even know about. So you had to get a signature on the stencil. So I went in, it turned out, the afternoon of President [Franklin Delano] Roosevelt's funeral. I took the papers in to get my boss to sign them, and he did. And he said, "Hey, let's just go down and get the signature from the headquarters staff."

There was a protocol in those days that when an operations analysis report was written, it was not sent outside the staff for whom it was written without the approval of somebody in the commander's office. So we went down to the commander's office to get his signature. Well, it turned out, by the time we got there, everyone had been allowed to leave if they wanted to watch the funeral procession down Pennsylvania Avenue, so there wasn't anybody there. But we made a little noise in the outer office, apparently, so this voice emerged from the interior office, "Who's there?"

My boss said, "This is Bob Stearns."

He said, "Come on in." And it was General Lauris Norstad, who was then a brigadier and the local commander in that headquarters. He and Bob were obviously very warmly associated, because General Norstad began to discuss all kinds of things that were on his mind to get Bob's reactions.

This went on for an hour or so, and during that time a buzzer in the Pentagon went off and that signaled the actual time of the interment of the president, as I recall, or something like that. The general kept talking, and then we got a little restless and got up to go, and the general said, "You must have had some business that brought you down here. What was it?"

Bob said, "Oh, we just wanted your executive officer to sign this report for release."

The general said, "What report is that?" He told him, and he said, "Oh, yes, that's a very nice piece of work. Let me have that. I'll sign it." And he did.

So we had no problem with acceptance, because the air force had problems, and they knew it had problems, and any help it got with these problems was very much appreciated. If you were a team member--in other words, if you shared the hardships and the deadlines and the exigencies of the situation--you were a member of the team, and nobody gave a damn what your uniform looked like. You were warmly accepted. Most civilians jumped right in and accepted the hardships of the time.

Out on Guam, we lived in leaky tents, you know, and ate at the mess and shared the inadequate sanitary facilities with everybody. [Laughter] So there was no grumbling about this, because everybody understood what the game was.

COLLINS: In the postwar period, I'm curious about how you began to think about your own professional life. You did go back and finish up your Ph.D. work, and it was in the area of pure mathematics.

MISER: Like most people, this nine months I spent with the Twentieth Air Force was the most fun I'd ever had, you know, for two reasons. One, the problems were important. The intellectual challenge was interesting, and you could see what happened when you finished. Nine times out of ten, if you made a recommendation, it was ruled on very quickly, and either something was done about it, or something wasn't.

Now, in the case of this memorandum which went to General LeMay on Guam, we weren't there, so I don't know anything about his considerations, but his tactics were not, in fact, to bomb Japan from high altitude. The principal tactics that were

adopted before the A-bomb were low-altitude night fire raids. The fire raid, I think it was in April of 1945 on Tokyo, I'm sure was far more traumatic to the city than the A-bomb drop on Hiroshima, for example. Now, whether our paper had a direct influence on that decision, only General LeMay could tell you, if he remembers, but it probably was influenced by a lot of factors. By then he knew the Japanese fighter defenses were almost nil. The Japanese had thrown their air force away, essentially, earlier in the war, by not rescuing their pilots. Their antiaircraft was rudimentary, and so there really was very little threat to fly in at low altitude.

COLLINS: During the war, did you ever come in contact with Edward Bowles and his operation?

MISER: No.

COLLINS: To get back to this question of how you were thinking about your career in the period after the war, what I'm seeing is two different directions here. One is the academic direction of interest in mathematics in a collegial setting, and mathematics in the real world, as evidenced by working with the air force. How were you sorting these things out? Did you envision yourself as a professor training new generations of mathematicians or teaching undergraduates, or did you want something different?

MISER: I don't know that I thought an awful lot about it at the time. I was influenced largely from time to time by the novelty and challenge of a new job. In other words, I didn't try to preplan my career. I don't know that the word "career" would have entered my mind to associate with myself at the time. I was just interested in tackling interesting and challenging problems.

COLLINS: That didn't seem to present itself in your position at Lawrence?

MISER: Lawrence was not a graduate school. We had undergraduate programs there, and so it was all teaching. The same with Williams. That was an undergraduate program. The graduate program at Ohio State, which was not as famous as, let's say, the one at Michigan or Chicago, was nonetheless extremely good. They had some very good mathematicians there. I think they trained their students better than some of these more prestigious institutions, because they got them really involved with mathematics as a functioning subject. I realized when I talked to students from these other institutions that we were much more involved in the subject on a live-wire basis than a lot of these other students were at a lot of these other places. So it was a good place to come from, but it was different. These were varied experiences.

COLLINS: By "live-wire experiences," you mean application to physical problems?

MISER: No, what I mean is that we talked mathematics. We gave seminars. The policy of the faculty there was to give oral examinations rather than written examinations, for example. For an oral examination, a professor would ask you to prove a significant theorem, and you were up there at the blackboard with nothing but your head and your bare hands and a piece of chalk, and you were expected to prove a theorem.

Now, to do that all off the cuff, orally, in front of a critical professor and your peers, means you've got a far better grasp of the material than would be the case if you were struggling to write it out on a piece of paper, and a lack of grasp becomes much more apparent very quickly, too, because you stumble around. And so we were trained really to talk about difficult mathematics *viva voce*, as well as on paper, and that meant you got a real grasp of these difficult concepts early.

COLLINS: Describe, then, the circumstances in which you transferred from the college environment to working for the air force.

MISER: Brothers offered me a job, and I had by then a wife and number-one boy, and college salaries for assistant professors were pretty close to the subsistence level. I think that my annual salary was $3,500, and the civil service salary was more than double that. I hate to be callow, but I suspect that doubling one's salary overnight played a role in the decision. So I went back in March 1949, March or April 1949, to Roy Brothers in the operation analysis office in Washington, where I stayed for the next ten years.

COLLINS: Perhaps a way of getting into that experience is to compare the function of the office when you came there in '49 with the kinds of things that were going on during the war. How had that operation matured over that time?

MISER: I don't know that the word "mature" would be relevant. The cast of characters, except for Roy, was completely different. There was a complete break. The air force operation analysis organization consisted of a headquarters office during the war which simply recruited and dispatched analysts. It did not do any work in the headquarters during the war, scientific work. It recruited and dispatched analysts who did their work at the commands.

When the war was over, that headquarters simply folded. Then the air force decided to recreate in its headquarters an analysis group, and Roy Brothers was asked to head that analysis

group up. So he started essentially from scratch and built a new group, and he gathered up people that were interested, but they were mainly new people. There wasn't much overlap. I'm an exception, in other words, from the wartime people, because most of them were mature scientists who had professorships or good jobs other places, and they went back to these things that they had been doing. They were married, they had children, families, and so on, and they weren't as mobile as I was at that point. So there was a break, essentially, between the end of the war and the beginning of this postwar operations analysis office.

COLLINS: What was your sense of the aspirations, the responsibilities, the direction of this group when you came back to work there in '49?

MISER: It was very simple. We proposed to help the air force with its problems, and as they came up, we did. Perhaps the most striking problem that arose very early was in the fall of 1949. President [Harry S] Truman announced, if you recall, that we had evidence that the Soviets had exploded a nuclear device, and apparently, the president asked the secretary--I'm making this up now, because I don't know what the succession of steps was--but apparently, the president asked the secretary of defense what the implications of this were for the U.S. and its defense, and he asked the secretary of the air force, and the secretary of the air force asked the chief of staff, and the chief of staff asked us.

And so Dale Oyster, whose name you recognize, and another colleague, David DeBeau, and I undertook to write a paper that would predict, as crudely as we had to, what would happen to the United States if the Soviet Union had nuclear weapons in, I remember, 1952 and 1954, and if they used their TU-4, which was their copy of our B-29 long-legged airplane, and staged an attack on the United States.

We wrote this paper. At that time, the U.S. had no air defense system. Literally, it had none. There was a thing called the Air Defense Command, but it was a sign on a desk at Mitchell Field in Long Island, and there was nobody sitting behind the desk. So to fight a paper air battle, we had to invent an air defense system and then assume its characteristic properties and apply those characteristic properties to the sort of an attack that a TU-4 might make, and calculate the results. So we did this.

We invented an air defense system roughly like the one that the U.K. [United Kingdom] had had during the war, whose properties were, in fact, thoroughly documented. We had the papers from the U.K., the operations research reports, and I had been to the U.K. and looked at that system in that summer and had

gathered up a lot of the reports about its wartime operations. So we did this report on the possible effects on the United States of a nuclear attack from the Soviet Union in 1952 and 1954.

Dave DeBeau did the part on antiaircraft, and I forget what Dale Oyster did, I think the part on nuclear weapons, and I did the bombing accuracy, and I also did most of the writing. As I remember, six copies were typed. In those days, we didn't put things that were super top secret on mimeograph. There was an original and five carbons.

That report was sent forward, and what it said was that, if a determined attack came, we would not be able to repulse more than a modest fraction of it. Some significant fraction of the attack would get to its targets and drop its bombs. So therefore, from a policy point of view, if you had to assume there was a nuclear attack on the United States, the United States would be very seriously damaged. If you knew anything about nuclear weapons at the time, that meant maybe half a dozen or ten of your major cities would be treated like Hiroshima. So that meant that whatever your policy was, you had better assume that a two-sided war was not something that you wanted to tempt anybody into.

I have no idea whatever happened to that paper. My guess is that it has some very distinguished fingerprints on the original copy, but I don't know where that is. The only evidence I now have of the existence of that paper is a footnote in the book about RAND that was written by Bruce Smith. In the chapter on the base study by Albert Wohlstetter, there is a footnote that refers to this paper. So I know that it was seen at RAND later on. There may even be a copy at RAND, for all I know.

Then, of course, we were asked to brief this all around, so we did that, too. I don't remember how many briefings we gave.

COLLINS: But those briefings would have been principally within the air force, or outside?

MISER: We didn't give them outside the air force. Whatever went outside the air force, somebody else took. But as you can see, that was a fairly important issue. That was not a trivial little staff problem. That was a high-policy issue. I think that paper established--I'm guessing now--but I think it established the idea which still exists, that whatever our policy is, it ought to be that a two-sided nuclear exchange is not a reasonable future to contemplate. It's just too damaging. You can't do enough about it to stop it, so what you have to do is to avoid it.

At one time later, there was a secretary of defense, his name was [Charles] Wilson. He'd been the president of General Motors. To be distinguished from the other Wilson who'd been president of General Electric, he was called "Engine Charlie." When Engine Charlie came in, he announced, obviously without having been briefed by his staff, that while he was secretary of defense, any attack on the United States would be stopped at the borders. Everybody who knew anything knew that Engine Charlie was blowing smoke, because it was not something that was possible to do.

The other thing, of course, the air force had to do was somehow or other be sure that we got that understanding to the other side, which I think we did, somehow or other. Either they thought it up on their own, or we did. But that was probably the most important paper I ever had anything to do with. It wasn't very long, twenty-five or thirty pages, with tables and charts and so on.

COLLINS: During this period that you were at the operations analysis office, about how many people worked in that office?

MISER: The most we ever had was about twenty-five, so it ran between twenty and twenty-five during that decade.

COLLINS: In the early period when the office was gearing up, did it reach that level fairly soon?

MISER: Fairly quickly, yes. You see, there was a very empathetic view of civilian scientists in the air force generally. General [Carl] Spaatz, for example, who was a very influential man, had written some very glowing things about the value of civilian scientists helping the air force in its problems. In fact, it was that feeling that led to the creation of RAND in--I believe it was--1948, wasn't it?

COLLINS: The project was started at Douglas in '46, and became the nonprofit corporation in '48, yes.

MISER: I have an interesting story about that that I should put in now before I forget it that's not in these other interviews. Sometime after I left the air force, I forget now where I was, I was asked to go to Syracuse, where the university was trying to set up a sponsored research institute, which was a popular thing for universities to do in those days. The man who was trying to do this was a retired air force colonel, and this colonel, during his air force career, had been in supply and logistics, so his main base had been Wright Field. In fact, he was at Wright Field in the early postwar years.

When the contract with Douglas was being written, he was involved. He was the principal staff officer on that contract. Or rather, he was maybe at one level above it. So when the contract hit his desk, he could see that none of the traditional constraints that were built into procurement contracts were in this one, and so he wrote a blistering memorandum saying this contract was improper and illegal because it didn't obey the rules that Wright Field had for contracts.

There was a meeting then, which was chaired by General LeMay, believe it or not, and he briefed all of his objections to this contract, which were straight out of his training--you know, everything was traditional--because this thing essentially gave this new organization complete free rein without any oversight beyond a very general one and no control over the subjects treated or anything like that. General LeMay listened to him very politely, and then he said to the colonel, "Colonel, you just don't understand the problem," and sat him down. The colonel had had his say, so he sat down.

The interesting thing was that his story was that the main influence in getting the contract and the relationship was dominated by General LeMay, who was at that time a member of the air staff. I find that very interesting. Now, I can't remember that man's name anymore. I don't think I have a record of it, unless I have some old correspondence with him. But that would have been one of the very early contracts.

TAPE 1, SIDE 2

COLLINS: You were describing the approximate size of the office. What was the mix of professional skills in the group?

MISER: We had mathematicians, physicists, statisticians. We had a medical doctor. It's kind of hard to remember, because we didn't think of people in terms of their technical specialties as strongly as you might think.

We did one other thing, however. We tended to get into particular slots with respect to air force specialties. That is, somebody would be the one you'd naturally turn to for tactical air warfare, another person for air defense, another one for strategic, and so on, although these were not fixed. So we thought of ourselves, in making assignments, we thought of two dimensions. One was the technical content in one dimension, and the air force operational content in the other dimension. And so that gave us a grid. If a problem, say, involved physics and strategic air warfare, we might pick a physicist who had had some acquaintance with SAC [Strategic Air Command], for example, as a team member or maybe it's the team leader. And, of course, we had mathematicians, which was my case.

COLLINS: You were there for about ten years. Did the other professionals in the office stay there similar lengths of time?

MISER: It varied. People came and went. They were civil servants, of course, so they could stay as long as they wanted to. But generally, people would be there a while, and then, like most upwardly mobile professionals, there would be a limit to what we could offer them. There was no way of advancement, and when they found a better job, they'd move on. But most of them were there--without being able to do any statistics--I'd guess the average tenure was five years. The administrative files for that office exist in a barn in Virginia. I could even tell you the name of the guy who has them, but I'm not sure it would be worth exploring. [Laughter]

COLLINS: We may pursue that a little later. How did problems for your consideration come to your attention? Did you generate studies within your group, or was it principally things that were requested of you by different members of the air staff?

MISER: It was principally things that were requested, but, of course, when you're working on things that are requested, you see other things, and so you tend to generate problems that way.

There was a celebrated time in the very early days, about 1950, when the air force succeeded in getting the defense budget unbalanced. Right in the early postwar era, the defense dollar went thirty-three cents to the army, thirty-three cents to the navy, and thirty-three cents to the air force. But Mr. [Thomas] Finletter, when he was the secretary of the air force, succeeded in getting more than the air force's share at the expense of the army and the navy.

This Dr. DeBeau that I've mentioned worked directly with Mr. Finletter on his briefings to the Department of Defense and Congress, for example, and the interesting thing was that nobody in the air staff seemed to mind or object to the fact that this fellow who was down in the civilian woodwork there would trot up to the secretary's office and help him with his staff work. Dave went away very early. He had an offer he couldn't refuse.

One day when Mr. Finletter--this was very early in my career--was preparing a second or third round of briefings, the annual budget cycle, I got a frantic call from the secretary's office, could I come up immediately. So I fell out, got up there, and without a word being said, I was ushered into what for me was a huge office. I looked around, and all I could see was the backs of a group of charts at the far end of the room and some feet under a table. So I went down there, and it was Secretary Finletter with his executive officer, and he was looking at all these charts. He motioned me to a chair, so I sat

down. These were obviously the charts that he'd prepared the previous year with Dr. DeBeau, but Dr. DeBeau wasn't there anymore. And so presently he stuck out a bony finger, and he pointed to a number on the chart, and he said, "Dr. Miser, where did that number come from?"

Of course, I didn't have a clue. So I said, "I don't know."

But then he said, "Oh, I remember. Thank you very much," and I left. [Laughter]

So the problem initiation was very informal. We almost never got a memorandum. In fact, we would have discouraged that, because if somebody has enough of an idea of exactly what his problem is, he's probably wrong. If he's written it down, then you've got to overcome the misstatement of the problem and get it properly stated.

COLLINS: How did this informal interchange work, then? Where were the venues in which you interacted with members of the air staff or commands?

MISER: It ran all the way from the secretary down to the lowliest second lieutenant in the staff. They would come in and ask questions, and if we could help them, we would. Now, if we helped, we helped the guy who asked. We didn't report that study, let's say, to the commanding general. We reported it to the guy who asked the question.

COLLINS: The operations analysis office was part of the Deputy Chief of Staff for Operations?

MISER: It had various attachments, depending on what era you're talking about. It floated up and down, depending on personnel and attitudes.

COLLINS: What other ones besides operations do you recall?

MISER: Well, we were nearly always with some part of operations, but sometimes it was with the directorate of operations or sometimes with the deputy for operations. These were two different levels. These were the main places. We fought shy of being much higher than the deputy for operations, because we felt that we'd be sufficiently marooned in the eyes of the guys with problems, you see.

The average air staff man doesn't want to present his commander with a problem. He wants to present his commander with a solution to a problem, and he doesn't want that problem communicated at a high level until he's got his arms around it, you see, and so there's always a worry that you'll leak. So we

felt that sort of a median position in the staff, high enough for our budget to be defended, but low enough that people would feel like walking in the door, was preferred, and I think that was about right.

COLLINS: Did you have some kind of obligation when you did a study to circulate it more broadly than just the office or person who requested it?

MISER: Oh, yes. We always wrote them up, or at least almost always wrote them up. Sometimes we did things so fast that an oral briefing was all that was called for. We generally wrote them up, but then we'd have to get permission from the guy who asked us the question to circulate it, which we did.

We were regarded as a general consultant service, not the captive of the office that we were attached to administratively. In fact, one time a very amusing thing happened. It was not uncommon every couple of three years for the air force to decide that its air staff structure in the Pentagon should be reviewed. A review panel would be set up, and the various principal offices would be asked to contribute somebody to this review panel. They obviously dealt these people off the bottom of the deck. They didn't pick their best people for this because they were busy on important problems.

And so, one time they had one of these review panels, and they looked at us, along with everything else, and they decided that, as in the case of the colonel from Wright Field, that we were a loose cannon. Nobody was controlling what we did or who we reported to or who we worked for. At that time, we were attached to the directorate of operations, which was a lower level in the staff, and they recommended to this general that he bring us under proper control, and since we were doing most of our work outside of operations anyhow, cut us in half, so it would be appropriate, you see.

As fate would have it, Roy Brothers was out of town when this all went forward to the general for review, and so I attended the meeting. For the director of operations, we had a man that, if I could judge from that day, had the saltiest vocabulary of any air officer I have ever heard. He had profanities that were so inventive that I had never heard them. The colonel presented this view of our operation, and the general stopped him essentially in mid-sentence and proceeded to describe the function that we had. Up to that point, we didn't know he even understood us. He explained that we were a service to the air staff as a whole, that his job was to support us administratively, not to control our program, and so on, and he did this with the saltiest discussion I think I've ever heard.

Then with a seraphic smile he turned to me, "Now, isn't that what you would have said, Doctor?" he said.

And of course I said, "Yes, sir." [Laughter]

The poor colonel who'd done the review was white and shaking when he got done. So in effect, this fellow was an operations guy. He'd been involved in Pearl Harbor, for example. He was a flyer. He wasn't a policy man. But he understood what we were up to, which was nice. We couldn't have defended ourselves anywhere near as effectively as he did, certainly not with his vocabulary. [Laughter] So there seemed a wide appreciation of the operation.

COLLINS: I'm interested in the process of how you did a study. Once you were presented with a problem or focused on a problem, how would you go about preparing an answer?

MISER: That depended entirely on the circumstances. Sometimes we would join a team that had air staff officers on it because of the problem's nature. At other times we would take the problem entirely into our own activity. But if we were going to sell it in the end, it was very important to keep the most interested air staff officer or office in touch with what we were doing as we went along. This was never formalized in some standard way, because if we had done it, then nothing that ever came in would have fitted the standard and vice versa.

The main thing you wanted to do was to be of service. Now, usually, if a staff officer had a problem, he had a deadline. If you didn't help him before his deadline, you might as well not bother. Sometimes you could solve a problem with a long discussion. Sometimes it took a five- or six-man team six months, depending on what was to be done. We did this all pretty informally.

COLLINS: Can you give me an example of the latter, more intensive kind of activity.

MISER: This example I described earlier, of answering the question of what impact the possession of nuclear weapons by the Soviet Union would have on U.S. defense, the three of us worked pretty intensively, I imagine, for six weeks on that study, and we wrote a very carefully written paper before we exposed the results, because we knew this would probably be controversial and needed to be very carefully thought through and documented. Also, it would probably go to the president, certainly to the president's advisers, so it had to be pretty straight.

Now we were sometimes called on for advice. One of the things that happens if you get a position like this, people begin

to respect you as a person. They will ask you for advice off the cuff. My philosophy always was, if they ask you, you should give the best answer you can. Now, you can qualify it, but do the best you can. Some people I know object to that attitude, but I think they're wrong.

I take my cue from a story that I should get into your record because I think it bears on this. One of my heroes was Vannevar Bush. I never met him but one time, and that was in a line at the officers' restaurant in the Pentagon. I had made a date to go to lunch with a friend of mine in the defense research and engineering office. We arrived at the peak time, so we were in the queue, and the next person to join the queue was Vannevar Bush and a friend. My friend knew Dr. Bush, so there were mutual introductions. I was somewhat tongue-tied to be in the presence of the great man, so Vannevar Bush, being a very perceptive and gentle soul, led the conversation, and he started telling stories.

The story I remember was that when he was first involved with the Defense Research Committee, he would report to President Roosevelt at periodic intervals on progress. One of the early times he went in to report to the president, he had a fairly short report because not much had happened. He got up to leave, and the president motioned to him to sit down, and he began to ask him all sorts of questions about the economy, the morale of the people, all sorts of things. Dr. Bush stumbled around, and eventually protested. He said, "Mr. President, all these things you've been asking me are not within my domain of competence or expertise. Wouldn't it be better for you to ask these questions of people that are more knowledgeable than I am?"

He said the president took his pince-nez glasses off, he put them down on the desk, and he leaned forward and said, "Dr. Bush, when I ask you a question, I want an answer." [Laughter]

The conclusion that I drew was that Roosevelt's method was to ask everybody that walked in his office, whose judgment he respected, the string of questions about the things he was concerned with, and that he would perform a mental analysis of the varying answers he got in the process of forming his own views.

So with that example, you see, when I was asked a question, I gave somebody an answer. If it was good enough for President Roosevelt and Dr. Bush, it was good enough for me.

COLLINS: In terms of the allocation of your time, how would you characterize it in terms of these activities of informal advice, sort of over the counter, versus time spent on more formal analytic studies?

MISER: Almost all of our time was spent on the more formal work. These informal things came up as incidents and were done in a matter of a very short period of time. Sometimes it would be a quick study. I remember during the Korean War, after the Chinese had come into the war, all of their air power was based at Shenyang, which is a city with an air base just north of the Korean border. A colonel walked in one day around noon and had a scheme for bombing that base and putting it out of commission. I forget what ordnance he was going to use, but he asked us to do a force requirements calculation for that tactic. He wasn't concerned about the international political implications of our bombing something on Communist Chinese territory, but they were using this base, you see, to support their operations in North Korea.

And so a lady analyst and I set to work, and we discovered in the course of the afternoon that not only was the probability that this would be effective very, very low, but we didn't have enough airplanes or ordnance in the theater to do this. In fact, we didn't have enough airplanes in the whole U.S. to do it all at once. So we suggested to the colonel the next morning that this was not a very good idea to propose, and he accepted it and went away. But I think we wrote a one-page memorandum, and I think that was useful because that meant the air force didn't try to do something that wouldn't work, and which would have had terrible international repercussions.

COLLINS: This story brings to mind a question. What kind of information did you have immediately at your disposal about the air force force structure, about the history of operations, about how tactical units functioned?

MISER: We knew everything there was to know that we'd bother to gather up. We had general access across the board to everything. We were not restricted. Within the framework of classification, people wandered around pretty freely, and so we had a lot of stuff simply because of prior work we'd done, and what we needed, we went and got. What else we didn't have, we'd go and get, one way or another, by visiting air bases, by gathering up classified papers, by the various devices you use in that kind of a setting.

COLLINS: I guess another way to ask this question is, did you attempt to organize some kind of intellectual framework of information you wanted at hand, that said this is important kinds of information for us to answer questions that are likely to come up?

MISER: We didn't have time for that kind of overhead. We generally fled from one problem to the next. But there was coherence within these streams, you see. I worked on air defense most of the ten years, for example, so I had a tremendous body of

information about air defense matters either in my files or in my head, or I knew where I could get it. Other analysts had the same thing. We didn't organize this in some formal way.

COLLINS: I wonder if we might, looking over the ten years you were there, consider potential milestones that may have affected the organization, whether it was the Korean War, the change of presidency to [Dwight D.] Eisenhower in '52, Sputnik in '57. Were there singularities that stand out that affected anything about your operation?

MISER: During the ten years I was there, it was very stable. Now, there were a lot of perturbations that we fended off. For example, space in the Pentagon was always at a premium, and so one time they tried to move us out of the Pentagon, and we felt that this would destroy our method of operation. If we were half a mile away, nobody would walk in. We wouldn't get any problems. But our boss at the time, who was the deputy for operations, felt that we should go on balance with other things that he had to balance.

So my boss, who was a very gentle man, Dr. Brothers, went up to have a final interview with him and plead for our continued existence in the Pentagon, and the general said, no, he couldn't change his mind.

So my boss said to him, "Boss, do you mind if I go over your head?"

The deputy for operations was so amused by this question he threw back his head, and he roared with laughter, and he said, "Well, Roy, you and I can go around again," and he changed his mind. [Laughter]

So we had little things like this, but nothing that effected a perturbation, until Roy left. Then the question of who was to succeed him came up. They brought in the man that had worked for General LeMay in Omaha, and he was a disaster, and the outfit went down very quickly after that.

COLLINS: What date, approximately, did Brothers--

MISER: He came in just after I left, so it would have been mid-1959 when he came in.

COLLINS: You're referring to [Carroll] Zimmerman?

MISER: Yes. I was called back a couple of times after I left the air force to do quick studies, and on one occasion, when I was asked by the secretary of the air force and the chief of staff to do something (it had to do with the Vietnam War), the

chief of staff, who was then General [ ] McConnell, kind of gave a little thumbnail history of ops analysis in the recent years, and he made an offhand remark which I found satisfying, because it confirmed what would have been my prediction. He said, "Zim was here for a couple of years, but he didn't do anything." After that, they didn't have very good people at the head.

I did a survey once about three or four years after the outfit had changed hands, and I went around. I couldn't find they were helping anybody. I couldn't figure out why the air force hadn't abolished them, if they weren't helping anybody. What happened was, they were folded into an outfit that now exists called, I think, Studies and Analysis. In fact, one of the members of our staff is still active in that studies and analysis group. This is a man by the name of Clayton Thomas. You might want to put that down, because he has a terrible voice through some problem or other, but his career spans the period mid-fifties to now in analysis in the air force. He occupies a very much respected position in that Office of Studies and Analysis.

COLLINS: Over this time during the fifties, did you attempt to cultivate or extend your mathematical training? How did you keep up to date with developments?

MISER: I didn't. I didn't do anything with mathematics during that period. I was absorbed in the creation of a new profession, namely, operations research.

COLLINS: Let's talk a little bit, then, about the building up of that as a profession, and what the role of the operations analysis group was. As I recall, the call to establish the Operations Research Society [of America] was '51, '52, something like that. Is that correct, or is it a little later than that?

MISER: No, it was earlier. Dr. Philip M. Morse at M.I.T. [Massachusetts Institute of Technology] was appointed the chairman of a committee, I think, with the National Research Council. That would have been in the late forties. My recollection here is kind of hazy. They sort of sprinkled holy water on the concept, and about 1950 or '51, a little group of people decided to create a professional society called the Operations Research Society of America [ORSA]. He convened a group of people at Arden House, which was the old Harriman estate up the Hudson, which belonged to Columbia University. The reason for that choice was that one of the principal people that was involved was George Kimball, who had been a partner of Phil Morse's during the war. Kimball was a professor at Columbia, so he knew how to arrange it. About sixty people gathered up there and founded this society.

COLLINS: Were you a part of that?

MISER: I went to that meeting, yes. In fact, I could show you the list of people that went to the meeting. It's a matter of public record.

COLLINS: When you talk about forming a new profession, what sorts of things were you and the people who attended this gathering thinking of? What was the goal?

MISER: What we had discovered during the war was that, to oversimplify it--when you look back on these things, you always attribute thoughts to the early times that you probably didn't have--but looking back, what we had discovered was that complicated systems of men and machines, that we've always thought of as being totally inchoate, nonetheless exhibited aspects of regularity.

You think of air warfare as being the most tangled and disorganized human activity you can think of, and yet our analyses showed that there were aspects of this activity that were astonishingly regular. You could predict in advance what the numbers were going to be. Our idea was that if you could do that for warfare, for heaven's sake, surely there were a lot of civilian activities in which generically the same kinds of phenomena would occur. And so our idea was that we should extend this mode of inquiry and thought to non-military areas of operation.

The key word here is operations. It's unfortunate that we named ourselves operations research, I think, because I think that's scared a lot of people off, particularly possible clients. There were a lot of professors in the group, so research was an okay word, you see, and so we adopted it.

COLLINS: Did the air force encourage your involvement in this activity?

MISER: Oh, yes. In fact, the air force, the navy, and by then the army, all were very positive toward all this, yes. I don't know that we asked them specifically, but they certainly were kindly disposed toward all these professional activities.

COLLINS: How did you see what you were doing in the operations analysis office at the air force contributing to this notion of carving out a profession that had certain kinds of practices and standards?

MISER: I don't know that as the corporate office we thought much about it. As individuals, we did. For example, Dr. Brothers was the second president of the society, as I remember, second or

fourth, one of the early presidents. No, he was not the second president. The second president was the navy's man, Dr. [Jacinto] Steinhardt. So I think Roy Brothers was the fourth president of the society, and I was the second secretary of the society. Most of our people had some involvement with it, some more than others. Some people are more oriented toward this kind of activity than others.

COLLINS: I guess what I'm trying to probe a little bit around is how, following a format of professionalization that other disciplines and areas had undertaken, would further the kind of thing you wanted to promote. Certainly there were not very many university programs oriented toward operations research.

MISER: There weren't any until the early fifties. I take that back. M.I.T. had a little activity, I think as early as '49. But the university programs began to emerge in the fifties.

COLLINS: Maybe we can come back to this and touch it in other ways. You've described the operation of the group in the context of the air force, but what was the nature of your contacts with the other military operations research groups: the navy, the army, and then later the--

Miser: They were direct and informal. We knew each other, and when we wanted to find something out, we'd go ask them. We, to some extent, shared reports, depending on what restrictions our masters put on their distribution.

One of my minor specialties was as the navy expert, so I knew a lot about naval operations. So I knew a lot of the people in the navy group, and I knew a lot of the people in the army group, which in those days had its headquarters out in Chevy Chase [Maryland], not very far from where I lived, so I could get to their office in ten minutes, where it took me half an hour to get to the Pentagon.

Key people knew each other on a first-name basis throughout this period. It was a relatively small community, a couple of hundred people. What you discover when you have a common interest and it's a small community, you're more anxious to interact than if it's a great big outfit. For example, I think about 1950, a group organized a little seminar which met in the Pentagon, and there would be a speaker on a rotational basis from each of these groups that would describe some study that could be reported with minimal classification problems, and then there would be a discussion of it. I was on the program committee, as I remember, for that. It didn't last very long because we found other venues. But that was the four military groups, because that was all there was in Washington at the time. The fourth one

was WSEG [Weapons Systems Evaluation Group], the joint chiefs' group.

COLLINS: Was your relationship with the WSEG group any different than with the navy or the army?

MISER: No. In fact, a colleague and I essentially wrote the major portion of WSEG's first study. It was on strategic air problems. We knew more about it than anybody, and they co-opted us, and we wrote it. You might say that independence was lost at that point. Independence of the WSEG study was lost, but they got the report out, and we got what we considered reasonable analysis included.

COLLINS: During the first years of your employment at the operations analysis office, the Research and Development Board [RDB] was still in operation. Do you recall having any contacts or involvement with the activities of the RDB?

MISER: I had a very nice relationship with a man who was an assistant to the director for a long period, as it changed its name and its functions. His name was Sam Clements. I don't know where Sam is anymore or if, in fact, he exists. But I don't recall that that had much substantive content. I think it was an accident of mutual acquaintance. We had mutual friends that introduced us, and we seemed to enjoy each other. This is the man that I was with the day I met Vannevar Bush, for example.

TAPE 2, SIDE 1

COLLINS: What was the nature, if any, of your interaction as part of your work with the academic community? Did you have university consultants who worked with you on a periodic basis, or was it pretty much the office itself which handled the studies that came up?

MISER: No, the air force group, perhaps thinking of the old-fashioned concept of a sudden war blowing up in which we might need a greatly expanded staff, established a kind of reserve unit at the University of North Carolina, in which a lot of people that either had had experience in our group or were interested in this experience, would become members, and they would often come up and work for a summer. We would have them at general briefings and whatnot. I forget what that was called, but we did have an extension into the academic community through that. Not everybody in that group was at the university, but they all had university connections. I can't remember what we called it, but that was our principal connection to the academic community, and it was formalized to that extent.

COLLINS: Let's go back to ORSA for just a moment. Here you had, I assume, a mix of people who were employed by government and the military services who were concerned with operations research, and at least a sprinkling of university people.

MISER: And also a sprinkling of industry people.

COLLINS: And industry people. But most of the work that had been done in operations research had been done in the military environment and was done under classified auspices. Did this affect the ability to talk freely and be able to organize this professional society?

MISER: No, it didn't seem to. One of the little hobbies that some of the early men had was finding civilian activities that had gone on earlier that, if the terminology had existed, would be recognized as operations research. You'll find in the early issues of the *Operation Research Journal* a little scattering of short papers discussing these discoveries that they had made. One, I remember, had to do with a mail-order operation in Newark, New Jersey, where a fellow had studied the issue of the relationship between the mail-order returns and how promptly they got the order out and things like that. Of course, there had been quite a lot of work that could be called operation research in telephone circles, where "queuing theory" had been invented around the turn of the century, as a matter of fact, by a Danish electrical engineer.

COLLINS: One of the principal audiences for operations research in the fifties, if one goes back and looks at some of the literature, was the corporate world, an attempt to get corporations to adopt this form of analysis as a management device. As part of your work in the operations analysis office, what kind of interchange did you have at all with people from the corporate world?

MISER: Not much, because our worlds were too different. When I left there, I went to the Research Triangle Institute in North Carolina for a year, which was an attempt to sell that kind of service to the corporate world down there. But we didn't get very far in that first year, and I got an offer of a job at M.I.T. at the end of it, so I never finished the experiment, you might say.

COLLINS: Let's talk a little bit about the interaction of RAND and the operations analysis office. Judging from the papers that I found in the archives, one of the things that the office would do, at least on occasion, would be to take a RAND report and assess it and provide an opinion or an evaluation to members of the air staff.

MISER: I don't know that we ever formally assessed it, because the RAND work that they let out of the house was generally so competent that we wouldn't have found it anything but first rate. But we frequently were on the front end of interpreting it, because a staff officer would come in and ask us about it. In fact, I logged a good many hours helping Harry Rowen, who was the front man on the bases study. Albert [Wohlstetter] came and went, but Harry came to Washington and stayed a long time, and I worked with him quite a lot on interpreting what that meant to the air staff.

We had splendid relations with RAND. Our people came and went. Their people came and went. On one notable occasion, I had got involved in a foolish enterprise, from some points of view. The air force decided, once it got its air defense system built, that it should have a continent-wide surprise air defense exercise, and I opened my yap and said, "Hey, if you're going to do that, you should analyze the results, shouldn't you?"

They said, "Yes, Doctor. Why don't you do it?"

Since it putatively was going to cover the entire North American continent north of the Rio Grande River, this was going to take a bit of doing, and in the end, I had to assemble a short-term staff of about a hundred people to do it. Obviously, we didn't have that kind of staff in the Pentagon, and I couldn't use most of our people anyhow, so I had to beg, borrow, and steal these people. I wrote a letter to the RAND Corporation and asked them how many they would ante up, and by telephone I got a call back, "How many do you want?"

In the end, I think six people joined the exercise. That involved going to a school to learn what the phenomena were, and how they needed to be analyzed, and then going to the field and analyzing it. But you see, that had a sex appeal for the RAND people, because the one thing they were most separated from was actual operations, and so they always felt a need for a firsthand acquaintance with actual operations. What really goes on is not what you read in the manuals. It's always a bit different. And if you didn't know what the differences were, you could make some terrible mistakes.

COLLINS: How did you gain that knowledge as someone who sat in the Pentagon?

MISER: I didn't sit in the Pentagon. I went out into the field.

COLLINS: But you were based at the Pentagon.

MISER: Oh, yes.

COLLINS: So an important element of your job was to go out and gather information.

MISER: If we had a problem, we'd have to go to where the phenomena were going on. For example, in connection with this analysis, we were going to have to sort out the evidence and the way it was treated at all these air defense centers in the North American continent. There were seventeen of them, and we had to send a team to each of those seventeen places. Now, I knew enough about air defense to know that if you thought that what went on at those seventeen places was standardized, so that each of them would be a mirror of the others, you were naive. So I had a staff man who spent full-time marshaling evidence on the variant practices at these seventeen places. He didn't go to all seventeen. He went to a sampling. But he gave a two-hour briefing on the characteristic variations that you might run into. This was just a summary of what he had learned.

Also, things go on in a situation that don't get recorded that may be decisive. For example, we had an air defense sector one time that was doing an absolutely splendid job up to a point, and then all of a sudden it went to hell in a handbasket. There wasn't anything in the record to say what happened, but we had a man on the spot, and he knew that at this point the commander had had a nervous breakdown. Now, they didn't write that down in the records. You don't do that, for some reason or other, but it had a decisive effect on operations. But if you were studying that operation, and you attributed this failure to something that you had been able to put your hands on, you would have made a fundamental mistake.

We understood that to really do good analysis, we had to have reasonably intimate knowledge of actuality, and so we went to a lot of trouble to get it. I rode around in a lot of tactical airplanes and logged a lot of time in base BOQs [bachelor officers' quarters] that were fitted out with army cots and things like that.

COLLINS: In this respect, would you say that you had a greater familiarity with the experiential side of the air force than RAND did?

MISER: Oh, I'm sure we did, because we did more of it. But they understood this equity, and so, as I pointed out, if you gave them a chance to gain a direct personal experience, they grabbed it. In fact, if you'd ask a guy, "Would you like to spend a week looking at this air defense," boy, they'd pack their bags so fast it would make your head swim. They were anxious to get it. But their physical administrative location gave them a barrier that was a little harder for them to cross than it was for us.

As part of a military staff, when we made a trip, somebody wrote orders. In fact, as an executive in the group, I could write orders and sign them. In fact, from time to time, we had military officers as members of our staff, and I would write orders for the military officers, believe that or not. This would never happen in the navy, but it happened in the air force. I've written orders for military officers. In fact, I've filled out fitness reports for officers. The thing was that the air force in that era regarded us as members of the team. It was a very nice relationship.

COLLINS: Do you recall the approximate date of this continent-wide air defense exercise?

MISER: December 1955. How could I forget? It involved me for a year to get ready and a year to write the report.

COLLINS: Up to that point, RAND had been very active in looking at questions of air defense. Ed Barlow and Jim Digby were some of the principals. Did you have a working relationship with them during this period?

MISER: Oh, yes. I had known Jim Digby, I think, since 1950; Ed I didn't know so well.

There is an interesting postscript to this air defense exercise. By 1956, when I finished the report on the exercise, I had probably sat through a hundred RAND briefings. Every time a RAND briefing came to the Pentagon in the air force, I usually went to listen, and so I had logged a lot of sitting time at RAND briefings. So Digby or somebody invited me out to RAND to give a briefing on the report of this exercise, so I said, "Here's my chance for revenge, sweet revenge." And so I gave a four-hour briefing at RAND, with an intermission; I hasten to add nobody left. I had the material in hand, I had good slides, and it was intrinsically an extremely interesting story, you see, because it was real. There wasn't anything artificial about it. It was real. The pile of data that had accumulated, you see, was tremendous, and it was also real data, but you also had to know the background. So I got my revenge, a four-hour briefing at RAND.

About a year or two later, the head of the cost analysis group, David Novick, sidled up to me. Dave never offered anybody a job. That wasn't his way. But in effect, he told me he was under pressure, that his people were all so academic that none of them could give an adequate briefing, and I think he was offering me a job to join his group so I would give their briefings, but I didn't fall for it.

We had good relations, as, in fact, I do now with a small subset of people that remember that era, like Gene Fisher and Warren Walker.

COLLINS: One of the things that RAND gained a certain notoriety for was these briefings.

MISER: They were superb. They really were.

COLLINS: How would you characterize them, and what was distinctive about their approach to presenting their studies?

MISER: They knew how to make charts that had the right information on them, but not too much, and they used them with great skill. You see, the equity here is quite different from an academic one. You don't have a blackboard and a piece of chalk and infinite time. If you're going to give a briefing to General LeMay, he may allot you twenty minutes. You've got to get your whole story into twenty minutes, or else don't bother. What you can do is, if you have a mixture of visuals and talk, you can pack a tremendous amount of information into twenty minutes. They learned how to do it, and they did it extremely well. I imagine they still do, but I haven't heard a RAND briefing in twenty-five years, so I don't know.

COLLINS: Were they better at this than other groups, say, if the WSEG came out with a study?

MISER: Oh, yes. Well, they set the standard. In fact, what you could see happening in the air staff in the early fifties, the air staff began to imitate the RAND style, and the air commanders began to expect that style--crisp, short, and punchy, and a lot of information.

To give you an example, the most literate general I ever worked for in the air force was a man by the name of [ ] Partridge. When he was the deputy for operations, we were attached to him. General Partridge had listened to thousands of briefings all his life, and he had formed very strong prejudices. One of them was that if you brought a piece of paper in that you proposed to read from, it was curtains. You were out the door.

When I was doing this air defense exercise, obviously, the plans for the analysis had an awful lot to do with NORAD [North American Defense Command], and General Partridge was the commander of NORAD, and so I had to give him a briefing on our preparations for the analysis. His executive officer came around to see me several days beforehand, and he said, "Now, Doctor, you understand what General Partridge's requirements are for briefings?"

I said, "Oh, yes. I've worked for him before. I know what he wants."

He said, "You understand you can't read anything."

I said, "Oh, yes. I know what the general wants. Don't worry about it."

Well, he did worry about it. But in the event, I gave this briefing, which was how we were going to analyze NORAD, so naturally every senior officer in the NORAD staff was in the room, which consisted of a three-star army general, a two-star navy admiral, a bunch of brigadiers, and every colonel in the place, and I had my charts all nicely prepared. They didn't have to be artistically done. You could do them by hand. That was all right. There was no requirement for artistic presentation, but they had to be unambiguous.

Now, you could cheat on General Partridge in one respect. You could take your pencil and very lightly write your outline on the edge of the chart, which would not be visible back there, but you could see it.

I gave this forty-five-minute briefing, and then there were questions. And then the general said, loudly, so his staff could hear him, "Now, Doctor, this is splendid. Is there anything else that my staff can do to help, or is there anything that they haven't done that you need to have done to help?" That's what you call getting the imprimatur.

Most of these air generals, even LeMay, had sat through thousands of briefings, and another one was not a big event. It was not something they approached with joy. So you had to get through it pretty concisely and precisely. If they wanted more information, they'd ask questions. RAND had set the pattern for it. People all over the air staff were imitating, and if the outsiders weren't smart enough to imitate, they soon learned.

COLLINS: One interesting feature about operations research as a profession and a discipline is that in mathematics, if you did some research, and you were going to go deliver a paper, you would probably deliver it to your peers, and they would have a certain similar kit of theoretical tools to evaluate it. When you do an operations analysis study, a systems analysis study, the people who are doing the study have a different set of tools than the people who are listening to it and are going to use it to make decisions.

MISER: That's right. That's a good point.

COLLINS: I want to probe this a little bit, how you approached this question of presenting technical results to a largely non-technical audience to get them to consider possible decision choices. Maybe using the example of the air defense exercise that you went through, we could examine that role.

MISER: You talked about what they were interested in--in other words, their operations. You suppressed all sorts of technical material. For example, suppose you were going to use a chi-square test to test significance. Well, they'd never heard of this before. You didn't mention it. In fact, I never knew an air audience that wanted to know what your technical tools were, or how you'd used them. They were perfectly confident that you could use your technical tools competently.

What they did want to know was what assumptions you had made, and what results you got, and they came to know, largely, I think, as a result of RAND's work, that the results were a technical interpretation of the assumptions. So if you didn't like the results, there must be something wrong with the assumptions. In other words, if you didn't agree with the assumptions, then you couldn't agree with the results, necessarily. But I never heard an air audience try to explore that bridge between the two.

As they got more sophisticated about systems analysis and this work, they would go after the assumptions at a terrific clip. If they had to agree with all the assumptions, then they were stuck with the findings. Sometimes there would be a grumpy officer who would say, "I wouldn't believe it," but it was pretty much you just suppressed the technical bridge. You didn't present that. Now, if you were addressing, let's say, another group of OR people, they would want to know about the bridge, of course, but that was a different thing.

COLLINS: It seems, from looking at some of the documents that I found in the archives here, the range of ways you could attack the assumptions or question the assumptions was pretty broad. As you were pointing out, the practice of conducting operations is very messy in trying to even define an assumption sometimes.

MISER: It was hard sometimes, yes.

COLLINS: What I'm wondering is what the inclination was to question the assumptions, and was that a function of a concern about the validity of operations or systems analytic types of studies?

MISER: No. In the beginning, right after the war, operations analysis, in the air force at least, had such a good name that it was tough to live up to it, in a sense. In other words, the guys

who had done the work during the war had established such a firm place for this kind of inquiry that after the war it was never fundamentally questioned in the early days. In fact, it was not uncommon for an officer who was fighting a rear guard action against some course that somebody was proposing to say, "We ought to get that one operations analyzed." See, that was a holding action, to get us to do an analysis. In other words, it was in vogue for a while.

By the end of the fifties, I think it wasn't in vogue any more. The audience had smartened up considerably. It knew how to separate the good from the bad much more skillfully than it had in the early fifties.

COLLINS: I'm not quite sure how to frame this question, but let me try to put it out there. Much of other scientific activity, as you attempt to professionalize, is in many ways an attempt to depoliticize or get rid of subjectivity, however defined in your area of activity. In the case of systems analysis, operations analysis, you were essentially embedded in a political process, where subjectivity and qualification are important.

MISER: That was certainly the case. In fact, you were continually reminded of that all the time. Another problem, of course, is that when you do a study, you really don't comprehend everything that the commander has to consider. You may have picked out the key things, some of the key things, but there are always aspects of it that you haven't put in your study. Morale, for example, probably isn't in there. Political relationships between key people aren't in your study, probably. All sorts of things probably aren't in there that have to be. So a study is an abstraction of a portion of a reality that seems relevant to the issue. The commander is going to have to make a decision in which he factors in a lot of other things that you didn't consider, either because you didn't have time or you didn't know how or you didn't know they were there. So you took a piece of it that you thought was important.

The academics have formulated a picture of the situation in which they talk about decision analysis as though the analyst could grind the analytic crank and then he could come up with something and say to the client, "Do this," and it will comprehend all his concerns. All he has to do is salute and do it. That's a false picture to any situation I've ever been in. [Laughter] The executive or the commander always has a lot of things to consider that you haven't put in your study.

COLLINS: A related issue that I'm interested in understanding a little bit better is how the air force made decisions. For example, in the case of the Wohlstetter study, one had the sense that this was a real communal process, in the sense that everyone

had a crack at evaluating the study, making comments about it. But what's not clear to me is how that's all put together and then a decision is made. Do you have any insight into that process?

MISER: Let's start with the formalities of air force staff work. If someone in a given portion of the staff decides that some action should be taken, he has to write a staff paper in which he proposes this action. As part of that exercise, he has to sit down and figure out everybody else whose interests are affected by this action.

The air force actually has a front-page form that went on the staff paper that had a series of lines on it for coordination, and everyone whose interest is affected by this action, potentially, has to be listed on this form.

The staff officer, before he can present his paper to the next highest echelon, his boss, has got to go around and get every one of those other interests who were affected to sign off. If they choose not to sign off, they have to write a paper which will then go into the package. If they object, they have to state their objections formally, and that objection paper then is attached to the original proposal. And so when it goes to the next echelon for the next step, what the commander gets is the proposal, plus all the people who have either coordinated or non-coordinated, and their objections, if they have any, to look at. If the whole thing is within his purview, he can decide. If it's not, then it has to go to the next highest echelon, where there is somebody that has everything that's involved within his purview.

Well, the existence of that formality meant that obviously, if you're going to propose something fairly revolutionary, you don't just spring it on all these people over here that are going to have to coordinate. You go around and you discuss it with them first, informally. This promotes a lot of informal interaction that never becomes a part of the record, and so when you do propose it, you probably know who's going to coordinate and who's going to object, and why, but you go through the motion because you want it settled.

We were often asked to coordinate on these things. We couldn't coordinate on the ultimate, but we could say that as far as we knew from an analysis point of view, this was an okay argument. In fact, for a long time, I guess most of the time I was there, it was considered a feather in your hat if you could get operations analysis to coordinate.

COLLINS: You mean if you were a staff person trying to get an action?

MISER: Yes. So we were often asked to coordinate. Now, within that framework, of course, I think it would be foolish to argue that that formality made decisions in the air force. [Laughter] I have to believe that the top echelons did a lot of informal discussing at lunch and at cocktails and all that sort of thing. But what those discussions have consisted of, in any given case, depended on the case, I would imagine, because as a rule we weren't involved, so I couldn't give you a direct personal experience.

There's one exception to that where I was involved. It's fairly notable. During this air defense exercise, SAC had a new electronic countermeasures [ECM] system in their airplanes, and they used that very extensively in one sector. We observed in the exercise that in that sector the air defense was one of the most vigorous and successful of any of the sectors, in spite of the ECM.

Now, ECM is supposed to shut down the defense activity. In this case, it seemed to have done the opposite. And so it was proposed that WSEG--I don't know who proposed this--I don't think it was the air force--run an electronic countermeasures test. That's a long story, which I won't go into, but I got involved with that. There was a radar up in Pennsylvania north of Altoona that was dedicated to this, and they ran various electronic countermeasures missions in the direction of this radar.

One of my colleagues and I gathered up all the evidence from this thing, and we did an analysis. There was a staff officer whom General LeMay had brought back to active duty. He was a general contractor, and he'd brought him back to active duty to honcho this electronic countermeasures program. This colonel was very interested in this experiment and in our analysis. He used to come by every day and say, "How are you doing, Doc?" We'd show him the results.

What our results were showing was, unambiguously, that the electronic countermeasures system was helping the defense tremendously. They would turn on their electronic countermeasures, say, 250 miles away from the radar, and the radar would immediately get a strobe that pointed at them. [Laughter] You had to be a very stupid radar operator not to realize that this strobe was coming from an attacking airplane. The ECM didn't blank out the radar until it got within about forty or fifty miles. So the ECM gave these guys a couple of hundred miles of help in sortieing airplanes, guiding them, and all that sort of thing. So the results were pretty unambiguous.

At this time, General LeMay had come into the headquarters as the vice chief of staff. One morning, Colonel [Robert] Borders came in the office about nine o'clock and said, "Doc,

we've got to brief the general on these results. I've got a date this afternoon at 4:20 to brief him on these findings."

I said, "Bob, we're still doing the analysis. We haven't got the findings anywhere near done."

"Sorry, Doc," he said, "We've got to tell the story. You'll have to prepare the charts, and I'm going to coordinate this action up through the staff," because he was three or four echelons below there. So he spent all day coordinating the fact that we were going to tell this story, and I prepared charts with a felt pen. We were going to share this twenty-minute briefing, so I prepared his charts and mine both. He came back about fifteen minutes before we were to go up there and took a look at his charts, and we sortied up to General LeMay's office, and we gave him this twenty-minute briefing on how his ECM program, which was scheduled to cost $500 million, was helping more than it was hindering.

Colonel Borders was a good briefing officer. He gave his part, then I gave my part, and then Colonel Borders thought the general, whose expression had not changed--in fact, he hadn't even taken his cigar out of his mouth--thought the general hadn't been listening maybe, so he decided to extend his remarks, whereupon General LeMay took his cigar out and said, "I think we have enough on that subject," and we departed, not knowing whether we had had any effect at all.

The next morning I got the courtesy copy of a memorandum from the general, canceling the program. In that case, he had all the people in the room there that were directly affected, who heard the same briefing, so I attribute that to the fact that my friend Borders had been talking about these results for a month or so before we went up there.

COLLINS: Was your goal to recommend the cancellation of the project?

MISER: No, we didn't recommend anything. We just presented the results. We hadn't finished the study. [Laughter] I think we would have recommended that, but we didn't have to.

COLLINS: I'm a little confused. This was testing out presumed enemy countermeasures coming in?

MISER: No. More importantly, our countermeasures in an attack on an enemy. In other words, SAC's countermeasures weren't working, and the analysis we did was absolutely unambiguous. There was as little ambiguity about it as anything I ever did. In that case, we got a quick decision, but, again, what counsel

General LeMay took from his other officers up there before he wrote that memorandum later in the afternoon, I have no idea.

The air force was very conscious of the concept of coordination, though, much more conscious than anything I ever saw in civilian life. In big corporations or whatnot, say like the Travelers Insurance Company down here, it's much more like the military than the military is. If the president catches cold, everybody else comes down with pneumonia. The air force, in that sense, in the fifties--I can't speak for any later era--was very well-managed, in the sense that they consulted everybody, and they gathered information. They made mistakes, but they didn't make them because they didn't consult. That was the worst thing you could commit, was to make a decision without proper consultation.

COLLINS: We've talked at kind of a general level about analysis, but let's take this air defense exercise as an example. What kinds of technical tools did you use? After data was gathered, how did you structure and analyze?

MISER: Very simple. There wasn't anything involved or anything very sophisticated here. We used the logical tools of sorting and simple statistical tools, and in one case a very simple probability model, but nothing that would have been beyond the reach of somebody who had had an introductory course in probability and statistics.

COLLINS: Did it require any kind of special effort on your part to familiarize yourselves with the various technologies that go along with air defense and their potentialities and limitations?

MISER: I had this general background by then, having worked in the field for six years. I knew pretty well how radars behaved, how fighters behaved, how bombers behaved, and so on.

COLLINS: Did you have much interaction with the Lincoln group at M.I.T. and the Lincoln Summer Study that was done in the early 1950s on air defense?

MISER: I attended the briefings on that summer study. I wasn't a part of it, but I attended the briefings at M.I.T., I think it was.

COLLINS: Yes.

MISER: I remember the briefing that Edwin Land gave, the Polaroid man, and others. That was the summer of '49, I think, wasn't it?

COLLINS: I thought it was a little bit later than that.

MISER: It may have been. It was a summer that I remember because I drove my little car from Washington to Cambridge, and that was more of a drill in those days than it is now. [Laughter] You didn't have any interstate highways, and you had to ferry across the river near Wilmington, Delaware, and so on. It was a twelve-hour drive, no air-conditioning in the car.

TAPE 2, SIDE 2

COLLINS: One of the central themes of understanding the military in the postwar period is interservice rivalry. How did that enter into, if at all, how the operations analysis group functioned?

MISER: As far as I could tell, it didn't enter very strongly, because the OR groups didn't consider themselves rivals, and they traded information and knowledge back and forth. Once in a great while, we were asked to do a study that bore on some rivalry issue, but not directly because it was rivalry.

I have one anecdote that is rather amusing that bears on the question. The navy was developing, early in the fifties, an airplane called the P-6M. It was going to be a heavy flying boat that could carry strategic bombs. Martin [later Martin-Marietta] in Baltimore was working on this. One day, the navy decided to go public with a lot of claims about how with this airplane, this flying boat, they could take over the strategic mission. I think the secretary of the navy or an under secretary made a speech at a banquet.

The top brass of the air force got very upset by this because they had just come out, a year or two earlier, of what were known as the Key West Agreements, which split up the missions. So one of the staff officers from plans came in to see me, and he said, "Doc, we've got to do an analysis of the P-6M."

I said, "Why?"

He said, "Well, there are these navy claims. If it's going to contribute to one of our missions, we ought to see what it can do."

I said, "What do we know about the P-6M?"

He said, "Well, practically nothing."

I said, "If we don't know anything, it's going to be awfully hard to do analysis. Let's look at the problem a little more broadly." See, I was flying blind here, trying to keep up my end a little bit. I said, "The Key West Agreements stipulate that the strategic mission is an air force mission."

He said, "Right."

I said, "The P-6M, according to the allegations of the navy assistant secretary last night, are that it will contribute to this mission."

"Right."

"We don't know what it can do." I said, "Under those circumstances, it would seem to me what we ought to do is to borrow one from the navy and test it, see what it can do."

He said, "Wow, Doc, that's great." He rushed out. Before the day was over, he'd written a staff paper which he coordinated through the air staff with the speed of light, in which he pointed all of these things out and requested the navy to lend us a P-6M for tests.

Thirty-six hours later, I read in the paper that the same assistant secretary of the navy had made a speech in which he was complaining bitterly that the air force was trying to horn in on navy equipment and missions. A few weeks later, the first P-6M in a test flight crashed, and the development was suspended, and we heard nothing more about it. But that's the only direct analysis of anything like that I ever did, and that just a short conversation with the officer in which we analyzed the strategic situation. [Laughter]

COLLINS: Did your office have any involvement when the question of the use of ballistic missiles came up and a role of the air force in ballistic missile activity and navy with the Polaris? Did this become a subject of staff concern?

MISER: We did some analyses of ballistic missile matters. That was not my area, and so I don't remember what studies we did. But I don't think any of them dealt directly with any controversy that might have existed.

The history of the office during its successful decade was that we stayed on the front of the new problems. In the early fifties, it was nuclear weapons, offense and defense. In the middle fifties, most people in the air staff had learned so much about these, they didn't need much help, but the new stuff was missiles. So we began to do studies about missilery and all of its implications.

But at the end of the decade, the new technology that everybody was wrestling with was computer technology. We actually borrowed a man from RAND to head up some studies of the relation of computer technology to air force interests. So that the moral of that, I think, is that a consulting organization of

a freewheeling sort can't decide what it ought to do and keep doing it. It's got to stay on the front of the new problems that the organization has, because once it becomes standardized, then there will be a staff office that takes care of it. If you're competing with a staff office with regular responsibilities, you're dead. And we did. We kept moving out to the new areas.

COLLINS: I know that at the SAC command they established an operations analysis group. What were your relations with that organization?

MISER: They were cordial, but they weren't as interactive as they should have been, I thought. In fact, our operation analysis organization consisted of not only the headquarters group, but offices in a dozen other commands, and usually about once a year we would have a convention in which people from all these commands came together and gave papers on what they'd been doing lately. Sometimes this meeting would be in Omaha, sometimes down at the Tactical Air Command, sometimes in Washington, sometimes other places. So there was a free flow. In fact, there was some rotation of personnel on an unsystematic basis from one place to another.

COLLINS: Was it your sense that they were staffed with the same kinds of people that were at headquarters?

MISER: More or less, yes.

COLLINS: People with academic grounding in mathematics and physics?

MISER: Yes.

COLLINS: Let's explore a little more your connections and perceptions about RAND work. RAND was attempting, from the late forties onward, to distinguish what it was doing by calling it systems analysis.

MISER: They didn't call everything they did systems analysis.

COLLINS: No, but in terms of what they regarded as their distinctive product and one of their principal contributions, they dubbed this practice that they did "systems analysis." What was the difference in a name there, and did it really convey anything different between what they did and what you did?

MISER: The only difference, I would say, was in scale and scope. They could afford to put a hundred people on a systems analysis study for a year, and did once in a while, whereas this was completely beyond our capability. But generically, the nature of the work was just the same. They just could go into a great deal

more detail over a much larger scale and scope of effort. So I regard systems analysis as just a large-scale operations research, and, indeed, I've just written a couple of encyclopedia articles in which I've said that.

COLLINS: During this period, the use of the word "systems" increased in its common usage. It applied to a lot of different kinds of situations and practices, and certainly a sense that talking about systems and thinking in systems terms enabled one to grapple with problems that one couldn't deal with before, see certain kinds of relationships that one didn't see before. I'm offering that up more as a kind of observation from looking at the record, but I wonder whether you have any comment on this phenomenon in the period.

MISER: What RAND was observing, and what we were observing, too, on a smaller, less sophisticated scale, was that everything's connected to everything else, and if you take a single object or a single discipline or a single-cut view of things, you're going to miss a lot of the important interconnections, so you'd better study the interconnections while you're at it.

I think of a very early time when the air force was having a budget cut, and we had a particularly unsophisticated three-star general in the headquarters. I think he was in logistics, or whatever they called it then. So he got his staff--I remember the meeting--to take the budget line items and put them down with the dollar figure, and when that total equaled to what the budget was, he cut off everything below it. I remember a staff man getting up and saying, "But General, one of the things you've cut off down here is the guidance system for the bombsight that will go into the bomber up here, and the bomber won't be any good without the bombsight, which is down here in the program you've just cut off."

The general said, "Fffffffbbbbbbbbb. Follow orders." Well, I don't think they followed orders.

At RAND, they developed the concept of the slice. That is, if you have a strategic program, the strategic program gets support in the top headquarters in the Pentagon because it's devoting its time to that, and a lot of the logistic things out here are being supported, so that if you're going to cost that program and understand the total program, you have to see not only what's going on at Omaha and its bases, but also all of the things that support it, both on the side and below and from above. This was a very seminal idea which they exploited throughout the decade of the fifties, and they just carried it more a disciplined and systematic way than most other people ever had. We understood that, too. We didn't have the resources to deal with it in such a systematic or comprehensive way. So we

appreciated what they did and used it, but couldn't compete with them in that.

COLLINS: At the same time that RAND is talking about systems analysis, and your group is called operations analysis, the Operations Research Society is getting started. How much discussion was there among the people interested in this subject about the terminology?

MISER: None. We understood it was all part of the same flow. I don't recall any discussions in which we worked up any emotion over the differences in terminology. We all understood we were sort of in the same boat, heading in common directions, and went on from there. The differentiations have come later, if there are any worth noting.

COLLINS: The reason I bring that up is, just before we took a break, you showed me a book on professions. One of the issues there is this standardization of how you perceive your work and present it, in an attempt to gain legitimacy.

MISER: That's happened to operations research, but in recent decades, not back in the fifties. Now that operations research has become a profession, I think it can fairly be said that it's almost dominated by its academic wing; not quite, but almost. Now you have this problem, because in systems analysis, as RAND practiced it in the fifties, OR would be one of the contributing specialties, you see, whereas there would be a lot of other interdisciplinary activity. In the fifties, we didn't think about that at all. We just thought about the total schmier as being part of it.

COLLINS: In just skimming the publications you gave me, one interesting distinction that stood out and that I've tried to grapple with as I do some of this research is the distinction between a focus on problem-solving and creating a specific disciplinary milieu. It sounds like you're suggesting that in this period the focus was more on the question of problem-solving, as opposed to defining the discipline.

MISER: That's right. We did understand, however, and I understood very early, that a person's problem-solving ability, if it's not supported with a sound theoretical base, is pretty fragile and likely to fade away. In other words, if I were going to solve strategic problems, you might say I should hire an officer from Strategic Air Command. But in our business, he would have been useful as long as he could tell us about the details of those operations. But whenever a theoretical scientific foundation was needed for analysis, he wouldn't be worth a tinker's damn, because he wouldn't have that basis.

We found that persons with a good solid broad scientific foundation could turn their hand to any new field they wanted to, provided they were willing to invest the time and effort into becoming acquainted with that field, and that's a very important proviso, because if you don't know what's going on in the field with considerable intimacy, you're not equipped to conduct appropriate analysis in that field. So that's why, mostly, our people would have one or two or three, sometimes a third operational specialty that they were thoroughly familiar with, and then, of course, they'd have their professional tools they brought in with them, and you'd seek a union of these in a problem situation.

COLLINS: What did you consider were your strengths as far as your professional tools were concerned?

MISER: Mathematics teaches you logical thought, to start with, and it teaches you a form of intellectual discipline in analysis, but I didn't use any of the tools that I had learned in pure mathematics, with a few exceptions, like calculus and trigonometry and a few elementary tools.

COLLINS: But did you find yourself personally getting engaged with issues of game theory and dynamic and linear programming?

MISER: It turned out, I was probably the third person in the world to learn the simplex method, but I never had a problem that involved linear programming, so I never used it again. In fact, I never used it. Indeed, our whole office never used linear programming, except one time one of my fellows said, "Hugh, we ought to use linear programming."

I said, "Yes, except we've never had a problem that fits that model."

He said, "I'm going to take one, and I'm going to hammer on it until it's so out of shape it's a linear program."

I said, "Okay, as long as you don't take it seriously."

He said, "No, no, I'll just do it for fun." And he did. He didn't have enough to do, you see. So he did and he wrote a little memo on it and put it in the file, and he was happy and went away singing.

The models you have to use really grow out of the problems you try to tackle. See, that's different. In academia, you're studying a theory, and you're working outward from the core, center, whereas in practice, you're out there, and you've got to find tools that will address the problems that turn up in your inquiries.

A great disappointment, I think, at RAND was that game theory, which had a terrific run in the fifties, never really in practice provided a useful model. Now, conceptually, I think it influenced a lot of thought in a very useful way, but when it came down, let's say, to a practical problem and calculations, I don't think game theory ever was terribly useful.

COLLINS: Where it seemed to have its greatest implications was in staging what RAND called war games, these political scenarios where it was a back and forth. Did your group or did you personally ever get involved in that kind of analysis?

MISER: No. We did simple paper war games, but not the sort of thing I think you're describing. I knew about those, but I never was personally involved.

COLLINS: I found a quote from Ed Quade. The document I'm looking at here is a RAND document designated D-1991, and it was written in December 1953 by Ed Quade, the title of which is "The Proposed RAND Course in Systems Analysis." He's talking about the reasons to go ahead and do a course, and he says the following: "Possibly RAND does not really know how to do systems study. If true, then it is a strong argument for a course. Others know less. The air force is oversold on the system approach. A surfeit of low-quality studies may cause not only a complete loss of faith in the product, but also in the salesman. RAND should do what it can to protect its own position, whatever that is, with respect to systems studies."

MISER: I think that's a reasonable statement at the time. I wouldn't have any quarrel with it, especially since I attended that first course once he had got it organized and designed.

COLLINS: You did attend the course?

MISER: Yes, I did, in 1955.

COLLINS: Do you have any impressions of that experience?

MISER: It was a wonderful experience. It was beautifully done, and it was real. In other words, these guys were wrestling with the issues. There was nothing academic about it. I still have the text for that course around here somewhere.

COLLINS: You mean from the time?

MISER: Yes.

COLLINS: I'd like to see that.

MISER: If you're willing to hang around for a week or two, I might be able to find it. [Laughter] But I'm sure you could get it from RAND more easily and maybe not so dog-eared and mildewed. But, no, it was a wonderful experience. Herman Kahn gave a four-hour nonstop lecture, in which I don't think he finished a single sentence, but everybody understood what he was saying. He kept saying, "We really should have an intermission so people could stretch their legs," but he never did. This was a *tour de force* if you ever saw one. This was the higher intelligence at work.

COLLINS: What was your sense of it as a statement of where the practice in the field was?

MISER: Of course, that was probably in advance of practice at the time, because most other people hadn't thought about things anywhere nearly as deeply as that team had. That first book by Quade--I forget who his co-editor was--that appeared shortly thereafter, is essentially an offshoot of the content of that course. It's not the same, but it's an offshoot of it. I treasured that experience. Still do, as a matter of fact. It was a wonderful experience.

The thing about the RAND staff in those days was that it was talented, it was hard working, and it was incredibly sincerely devoted to the task in hand, which was to understand the issues the air force should be addressing and help them address them. It was palpable, it really was.

COLLINS: What was your sense of the kinds of air force people who attended that course? Where did they fit into the air force staff?

MISER: It was a variety. I don't recall we had any general officers there, but we certainly had senior staff officers. A civilian like me was definitely a rarity. They gave the course then again several times. I don't know how I managed it the first time around. That was the first time I really got acquainted with Ed Quade. That's when our acquaintance started.

COLLINS: One of the distinguishing elements, I think, of the RAND activity was the close working interaction between mathematicians and physicists with economists and social scientists. What were your impressions of the incorporation of economic disciplinary points of view into these kinds of problems, and especially questions of trying to assign costs and considering those kinds of issues?

MISER: I think the main contribution of economists to the studies was the insistence on the macro view. See, the average scientist is trained to look at things in the small and pile them up. In fact, there's a branch of economics that does that, too.

But Charlie Hitch and his colleagues there insisted on looking at the total picture, as well as the details, and I think this was their most significant contribution. Obviously, the cost work was part of that, because the total picture has to consider costs, because resources are never more than restricted.

There's not an infinite resource availability for anything that you want to do, something that I think it's hard for a lot of people to understand today in connection with the health care debate. That no matter what you want to do, or how virtuous you may feel about doing it, the resources are, in fact, limited, and they will always be limited. The only state that has recognized that to date is Oregon, I think.

COLLINS: In the context of the work of your office, did that encourage you to say, "We ought to have an economist on staff, or somebody who does costing?"

MISER: We didn't do much cost work, because our problems were much shorter term than that, as a rule. If we had costing to think about, we'd borrow something from RAND. Our time horizons generally were shorter than RAND's time horizons by the nature of the questions we were asked. That may or may not have been right, but that's what it was.

COLLINS: One of the arguments that lay behind RAND was the notion of it being separated from the air staff and hence giving it purportedly a freer, more open voice in examining air force practices and policies and recommending changes. Did you feel that they had more of an opportunity to question institutional activities than you did, sitting where you were?

MISER: I don't know whether I ever took that matter up. We were fairly free, too. But, of course, when we were busy, we were very busy, and you tend not to be prowling around looking for new things to do when you've already got more to do than you can. But I thought they were a splendid influence all the way across the board on matters of that kind.

There were times when we brought up new subjects. For example, I was told off one time about 1950 to go to England and study the vulnerability of our bases over there. Somebody had raised the question about it. I wrote a report, which again is in a footnote in the chapter in Smith's book on RAND, which deals with the air base study. I, in effect, provided the homely detail of the vulnerability of those bases to a wide variety of threats. This was rather unwelcome information.

In fact, the commanding general over there, the same General McConnell that I spoke to many years later when he was chief of staff, had just fought, bled, and died to get chain-link fences

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put around all his bases, and I came along and told him that that wouldn't have much to do with sabotage, because the first lesson you teach the saboteurs is how to go quickly and easily through a chain-link fence. [Laughter] Now, what it would do would be to deter the casual Brit who's had a pint too many at the pub and is taking a shortcut home and walks across the air base on the way home and maybe gets into trouble on the way. But if you're worried about sabotage, forget the fence. That's not the issue.

That study was part of the intellectual background of Albert's work on the base study, not a very large part, but part. That was a real part, you see, because I'd been there and I'd looked at bases. I looked in detail. I could speak with some authority about what I saw. I had a team of people, and we worked together.

COLLINS: A related subject, but perhaps somewhat of a tangent, one phrase I run across as I look at documents from the period, in their analyzing and talking about studies, is the phrase "planning factors." Essentially, as I interpret that, it means variables in your study that you have to assign values to. One document I came across, as an example, was a planning for a study on the subject of peripheral wars, and someone had put together a list of what they dubbed the planning factors that had to be considered. It's a document that's five, six, seven pages long with a planning factor maybe twenty-five, thirty on a page.

MISER: Actually, I'm not surprised. They put out a manual that thick [demonstrates] one time that dealt with planning factors for this war. That document you saw might have been either an input to that or a summary of it, because we made good use of that on occasion.

COLLINS: I guess what I'm asking is for you to describe your sense of what planning factors are, and where they fit into the process of putting together a study.

MISER: Lots of parameters in a study, you don't need to consider them in all their variability; you can consider them as a single parameter that's an average, let's say, of the total, and that will serve an immediate purpose. In fact, one of the main preoccupations we had in our office was developing and using such planning factors, and I imagine a lot of the RAND planning factors had originally come from us through our work, simply because that was an essential ingredient if you're going to do a study. For example, if you're going to take a bombing tactic of some kind, you really need to know what the typical average error is of that tactic. Another factor that you need to know is, if you send a hundred bombers out to do a job, how many of them are likely to get to the target in the first place. It's a lot fewer than you might think. Given, let's say, a certain level of air

defense, how many of them will be shot down or wounded sufficiently that they won't get to the target for that reason, and so on.

The typical planning factor for the fraction of the bombers that will get to their target, even if there's no defense at all, is eighty-five percent. In other words, fifteen percent will get lost, go the wrong place, seek the wrong target, do something wrong. That may seem high, but experience taught us that was about fifteen percent.

COLLINS: That kind of statistic came out of analysis, say, of World War II?

MISER: Yes. You'd have a lot of evidence. You could sight on your thumb for these figures, but that wasn't considered very good form. In World War II, it was about fifteen percent that, for one reason or another, had trouble. The typical attrition factor for enemy action varied a good deal depending on what kind of defense you had, but it was on the order of, say, five or ten percent, something like that. It was less than the number of people that didn't get there because of some internal cause, interesting fact of warfare, and so on, all sorts of things. So that what you've seen there was a typical bit of the lore and background of that kind of analysis.

COLLINS: When you say you had these at your disposal in your office, how were these maintained? Did you have a notebook that you kept with these things? How did that work?

MISER: The individual analysts had records, and then, of course, we had a file. If you'd been around a while, you knew what work had been done. You could put your hands on them. We didn't have a systematic compilation. The air intelligence people started such a systematic compilation one time, but I don't know how far they got with it, because they picked up a lot of our stuff at the time, because clearly this would affect planning.

At the end of World War II, one of the World War II analysts got a doctoral degree from the University of Illinois in civil engineering by putting together a report, which was really a manual, of the planning factors that related to strategic bombardment. This dealt with vulnerability of targets, accuracy, and all that sort of thing. It later was made into an air force manual. The old number was 200-5. I don't know whether that's still the number or not. But in that case, somehow or other, although this was a classified document, he was able to get the University of Illinois to give him a doctorate for putting this all together. So that existed.

In fact, when the Korean War broke out, and they had to have information like this, there was one copy of this report in Japan. The analyst we had over there became a full-time reader of this report, because the staff officers didn't know how to read it very readily. They didn't know where to go to look up stuff. So we quickly sent some other copies and sent some other analysts, too. So we were into the planning factor business fairly heavily, because it was an essential foundation for our work.

COLLINS: Would the validity of these factors be revisited over time as technology changed?

MISER: You had to revisit them every time you did a study, because you had to ask, "Where did this one come from? Is it relevant to my purpose? If it's not relevant to my purpose, how am I going to adjust it?" That sort of thing. There is a kind of a movement today in the air force--I've run across it because of my correspondence with Clayton Thomas--to standardize models and planning factors.

To me, that's nonsense. That's to say that the future will all be like the past, and nothing could be farther from the truth. So, true, you got these things, but you've got to be awfully careful how you use them. In other words, don't just go in the book and look up a number and plug it in. You've got to ask where the number came from, whether it's relevant to this new situation. If so, if you have to use it, what kind of apology are you going to have to make in your final summary for having used it rather than better information, and so on. I don't think there's any such thing as a standard planning factor or a standard model. There shouldn't be, let's put it that way.

COLLINS: Reflecting back on our discussion about your air defense exercise, did that experience get distilled into developing planning factors for air defense problems in any way? Did that enter into what you were doing then?

MISER: I didn't do anything like that. I think in Colorado Springs they did some of that. But the little bit I saw they had done, I didn't think was done very well, so it's hard to say. I don't really know the answer to the question because I wasn't involved in something like that, because I moved on to other stuff.

COLLINS: I guess what I'm getting at is, it's that kind of experiential activity that would generate the possibility of a planning factor.

MISER: Sure. The difficulty with that exercise was that there were seventeen sectors, and most of the sectors were relatively

idle. There were a few active sectors, but each of those sectors was so different, both in terms of the threat it faced and the way it responded, that you got a spread of consequences that you could look at, but you'd have a hard time summarizing into a planning factor. You could say roughly, for example, that, as I remember, between forty and fifty percent of the bombers probably got to their targets unimpeded by any defense activity, which is not bad. It's a lot different from five percent or ninety percent, you see.

Given the equities of a situation, it's a little hard for traditional scientists to understand, but a planning factor that's as fuzzy as that is still a very useful factor. You know that Engine Charlie was wrong when he said, "We're going to stop them at the border," and that's very important information. I submit that from a policy point of view, whether twenty percent of the American cities are destroyed or fifty percent is not too different. In practice, if you lived in one of the thirty percent different cities, it would make a big difference, but from a policy point of view, you can't afford either one, so it's the same answer. So it depends on the use to which the thing is put, whether accuracy matters.

This is a difficulty that scientists that are advising the federal government in recent years on other matters, say, ecological or whatnot, have had a lot of difficulty dealing with. For example, if you're talking about the ozone layer or something like that, and the scientists will make these very positive statements with numbers to three decimal places, and they don't know it to a factor of three, really. The next scientist will give a different number that differs from it, and now the congressman wants to know who the hell is right. The answer is, neither one of them is right in the strict sense. But to communicate those differences so that the layman will not be confused is a problem that most academic scientists have not addressed. But we understood it back in the early days of OR, and we were pretty careful not to confuse our clients about this. And they understood, too, so we were not addressing an amateur audience.

COLLINS: Part of the reason I bring this up is that I noticed that one method of critiquing a systems analysis or a study is to say you used the wrong planning factors. We have different factors, updated factors, or something now.

MISER: That's right. In that case, you're a little bit in the situation of the Count Orlofsky [phonetic] in the Die Fledermaus. Count Orlofsky announces at his ball that it's everyone to their own taste. As far as planning factors are concerned, it's a question of where it comes from and what it applies to. That's an issue of the evidence that supports the planning factor. In

all honesty, I suspect if there's a quarrel over a planning factor, both sides would have to admit they don't know anywhere near as much about it as they ought to. [Laughter]

TAPE 3, SIDE 1

COLLINS: Did you have any contact or involvement with the study that took place at RAND in the latter 1950s called the Strategic Offensive Forces Study, known as SOFS?

MISER: That doesn't ring any bells with me.

COLLINS: It was one that Ed Barlow headed up at RAND.

MISER: I had some involvement with a study that Barlow did, but that name doesn't ring any bells, so it may be just my problem with the nomenclature. I knew about Ed Barlow's study. It was not one of their more successful studies, I think, somehow or other.

COLLINS: This was an attempt, really, after RAND had gained a decade of experience in working on air force problems, to synthesize a lot of both the strategic and defensive problems that they'd worked on and to come up with a statement about what the air force force structure requirements would be.

MISER: I identify that study. I didn't have much to do with that one. It didn't really come to as sharp and well-focused a conclusion as the base study did. I don't recall that it had anywhere near as much influence as that one did, but my recollections aren't very clear on that.

COLLINS: Toward the end of the fifties, with Sputnik and the strengthening of the Defense Department, particularly its role in administering research and development, the air force lost some of its power in dealing with these issues, especially when ARPA [ ] was formed, and decisions about research and development were moved up the institutional hierarchy. Did that affect at all the activities in operations analysis? Do you recall that period?

MISER: I recall that period, but it didn't have any effect on us particularly, with one very minor exception, that the DDR&E [Directorate of Defense Research and Engineering] conceived the notion that they should get a copy of all operations analysis reports that were ever published on routine distribution. That caused a problem, because within the air force, we didn't distribute things generally. We distributed them to the client, and if the client wanted them further distributed, then he would authorize it, and we would do it. This directive from DDR&E sort

of overrode that relationship. I don't remember how it was resolved.

We were afraid that this would deter people coming to us to help them with problems. If they didn't want the problem exposed to DDR&E, they wouldn't ask us to work on it, you see. I don't recall how it was resolved, or whether it ever was resolved, but I remember the issue being debated. I think the guy that drafted that was my friend Sam Clements, and when I dined with him a time or two, I expostulated over the issue on the grounds that we've just discussed. Otherwise, I can't recall, and that may be irrelevant, I don't know, but this other thing didn't bother us one way or the other. I'm sure it bothered the air force, but it didn't bother us.

COLLINS: In conclusion, let's talk a little bit about your decision to leave the air force and continue your career elsewhere. What sort of things happened at that point?

MISER: I needed to make a career decision of some kind or other. The man that was going to come in, Zimmerman, whom you've mentioned earlier, was not a man I particularly cared to work for. I was the acting head of the office for a year, before he came, and I worked with him. So when he took over, he had a shop that was all busy at good problems. But I left largely because it was time for me to go.

The other thing that bothered me a little bit: the air force was changing at that time. They weren't quite as open to problems. People were beginning to have special interests and guard them in political ways, so that it wasn't quite the open, "Hey, we've got a problem, Doc. Let's work on it together" situation.

In fact, I think the last year, when I was the acting head, I was invited to a meeting on a routine distribution, and then I got a call from an executive officer disinviting me to the same meeting. Of course, if there's anything that will pique your curiosity, it's that sort of a thing. So I snarfed around, and it wasn't hard to do. If you've survived three rotations of officers, you have lots of friends.

I discovered that this was a meeting in which they were going to discuss how to abolish RAND, and they knew perfectly well that I was virtually a spy for RAND, certainly a strong partisan, and they didn't want me to be there. So I called Larry Henderson over in Washington, and I told him what I knew, and that was the last I knew of that. But that's what I mean by the atmosphere was changing a little bit.

COLLINS: Do you attribute this to anything you can identify?

MISER: No, not particularly. I do know that there were a lot of officers that felt that RAND was a little too bossy, in the sense of trying to tell the air force what to do. They were all independent flyers, and they kind of liked to fly their own airplane. General [ ] White, for example, I think was in that category. I think it depended on the cocktail conversation at a high level, and what forces emerged dominant in cocktail conversation at those levels, more than anything else. It wasn't serious enough that anything happened, but life wasn't going to be quite as easy to live. And I felt that I'd put as much time into military matters as I wanted to, and this opportunity I was offered at North Carolina was going to be all civilian.

COLLINS: How did you assess that? One of the things that interests me about the period and the work of people who have academic backgrounds is their interest or ability to move between classified work and unclassified civilian work. The reason that question I think is of interest is because certainly the training of academia is one which encourages a certain kind of openness and professional collegiality, that isn't quite consistent with the classified work environment.

MISER: But you'd be surprised, within classified circles, how much openness and collegiality there was, and it was enough to satisfy that craving, at least except for a margin. I told you about these seminars we ran in the Pentagon with the four military groups. These acted just like any professional meeting--freewheeling discussion, everybody was interested in the problems, and so on. For example, I didn't hesitate to go over to see one of my colleagues in the naval operations evaluation group and discuss classified matters. I knew I was clear this was a matter of mutual interest, and we'd talk about it.

In fact, one of my principal exploits as a pure staff man was that everything in the joint chiefs of staff that's nailed down under the regulations every three years has to be pried up and reconsidered--regulations, staffing, all that sort of stuff. And so the charter for WSEG came around under this three-year rule to be reconsidered, and I had always felt that the charter of WSEG was too restrictive. They were restricted then to only consider studies that had been brought to them by the joint staff. I felt they should have some freedom to initiate studies.

So I went around to the staff man in the navy. This was not the OEG [Operations Evaluation Group]. This was a military man, a lieutenant commander. I had this piece of paper from the joint staff and explained my position. He said, "Well, Doc, we're not interested in this paper. If you're interested in it, we'll coordinate on anything you want to do." So I went around to see the army staff man, and he took the same attitude.

So I went back to my office, and I rewrote the charter for WSEG. I think I also went up to see the director of WSEG, but I don't remember that specifically. So I rewrote that charter and got the two guys in each of their services to take my draft and staff it up through their commands. There's always a meeting of, not the chiefs, but the deputy chiefs, that prepares the agenda for the chiefs of staff and the joint chiefs, and if anything gets to that group and it's unanimous, it doesn't go any further. It's just approved straightaway. And so when my paper got to that level, it was unanimous. They all agreed with my position.

In the meantime, of course, the air force didn't give a damn, either, and so I got my way. Whether they ever made use of that provision, I never knew, but at least it was there. Now, this wasn't a classified issue, but it involved classified work eventually.

So what you learned to do, even if you're a civilian consultant in a scientific way, in a bureaucracy you learn how to work the bureaucracy, you see. If you don't fight the bureaucracy, if you learn how to pull the strings, you can get a heck of a lot done. The other thing is that you develop a set of acquaintances, the "can do" fellows, the ones that take a positive attitude toward getting something done, and you work with them, and you avoid the other guys, you see. Pretty soon you can move a lot of paper.

COLLINS: That's all the questions I had. Do you want to make any summary reflections about what we've talked about, anything that may have been left out?

MISER: I reread the interview I had with Brownlee Haydon [phonetic]. I had some other stories in there that I think are relevant to this one, that I would repeat except I don't need to bother because you've got them. It was a fairly exciting time, and I think anybody that lived through it was sort of marked for life, never took quite as academic an attitude again as he might have if he hadn't had the experience.

You know, don't you, that the influence of that period got clear up to President [Lyndon B.] Johnson, when he was the president, and he said to one of his Cabinet members one day, "If we can solve broad-scale national problems like the air force, the Defense Department, and the military do, why can't we do that internationally?"

And so he gave McGeorge Bundy, who was on his staff at the time, the job of trying to negotiate an international institute that would do the kind of thing, what you and I would say RAND did, although I don't think President Johnson knew about RAND specifically. It took McGeorge Bundy ten years to negotiate

this, which meant that it was the latter part of Nixon's presidency before it came to pass, and this was the International Institute for Applied Systems Analysis at Laxenburg, Austria, where both Ed Quade and I went later. It still exists. It's not doing exactly what I think it ought to do, but it's done good work, it's still doing good work, and in a small way--their staff is quite small now, about forty people--it's trying to apply to international, regional, and world problems the sort of systems thinking that RAND began in the fifties.

In fact, my current project is to finish the third volume of a handbook of systems analysis. This one will be devoted entirely to cases. Ed and I worked on the project when we were both there. When we left, the institute didn't have anybody to work on it, so we took it home with us and worked on it on our own. That's why I have that word processor over there. So we're carrying on. I have a Polish friend who now works for the World Bank in Washington who had told me recently that everywhere he goes he takes the first volume of the handbook with him.

COLLINS: Maybe that's a good point to end our discussion. Thank you very much.

MISER: You're welcome.