Interviewee: Mr. Gustave Shubert:

Interviewer: Mr. Martin Collins

Location: National Air and Space Museum
Washington, D.C.

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TAPE 1, SIDE 1

COLLINS: This is a tape recorded oral history discussion with Gus Shubert. The interviewer is Martin Collins. The date is May 20, 1992, and we're conducting the interview here at the Air and Space Museum in Washington, D.C. This is tape one, side one.

We've done a number of interviews previously, and you've had an opportunity to go back and look at those transcripts. I wanted to give you the chance to reflect back on that and correct some points or give different emphasis to some of the issues that we discussed. I know that the question of interdisciplinary research was one thing you wanted to talk a little bit about.

SHUBERT: That's right. We spent a lot of time talking about how interdisciplinary research gets done at RAND, how we were organized, who the principal actors and characters were, and so on. It occurred to me, in rereading what we had gone over, that we had really given a distorted picture of RAND's interdisciplinary work by focusing almost exclusively on major projects--the SOFS [Strategic Offense Forces Study] projects, the Wohlstetter Basing Study, this big deal, that big deal, and the other big deal.

In fact, there's a whole other set of interdisciplinary work that was going on at the time in a much less formal way, a much less organized way, in accordance with the John Williams general theorem of how good work gets done, and that is that researcher A runs into researcher B rounding the corner in the building that John wanted to have designed for exactly that purpose so that people would have hallway encounters and they would meet each other and start to talk. Person A might be a mathematician, person B might be an economist, and they would start to discuss and then perhaps to argue and pretty soon begin generating ideas.

Out of that kind of informal interchange, which was really going on a lot, you got people, at the very minimum, who had their views affected by people from other disciplines, and in many cases you had the birth of new interdisciplinary projects, which may, in those more or less loose days, never even have gotten on the books as a project--just a couple of people with different backgrounds collaborating on a single issue.

So I did want to make clear that there is that distortion inherent in my previous interviews, and that John Williams' theory of interdisciplinary research is in no way discredited by what we said. I think it's valid. It was valid then, and I think it's valid now to the extent it can be in the hectic world in which we're all living. So I would like to remove any of that distortion that I can, or correct for it by making clear that there was a lot of talk of the following sort.

When I was in the Economics Department, Burt [Burton] Klein and I were working together, and we had some questions about space hardware. So we went over to some of our friends in the Engineering Department, just sort of wandered in, shuffled in, I guess, into somebody's office--I've even forgotten who it was--and sat down and started talking about hardware, hardware development, hardware costs, and where the real cost of the space program lie. Did the real cost lie in the purchase of hardware or did the real bulk of the cost lie in the operational area?

We had an argument. Right away we had an argument with Burt and I venturing the notion that the operational costs would dominate, and that therefore one ought to focus attention on improving those operations, with the engineers saying "That's impossible," that the major elements of cost lay in the hardware, and we were wasting our time. So we found a more or less sympathetic engineer, and the three of us set off on a little project trying to isolate the real cost of space boosters--were they, in fact, hardware or operation and test costs? In fact, we found that the operational costs did dominate the hardware costs, and we found a number of links between operations and reliability. That study went to become, in its own way, a rather influential one in the space community at that time, which was, I guess, in the early 1960s.

That's the kind of thing that went on, and very, very productively, as opposed to this large-scale organized effort, which falls under one or more of the approaches that I described in prior interviews. I just wanted to be clear about crediting the importance of the Williams paradigm.

COLLINS: So in your experience as a researcher and as a manager, this was something that you saw going on and, to some extent, encouraged whenever there was an opportunity to do so.

SHUBERT: Absolutely. Even today, I try to do that where I sit in the International Policy Department, by being contentious and cantankerous and asking people if they've ever thought of this instead of that. It's a stimulating thing to have somebody with a different background around, but experienced, who can raise this kind of issue and get people off in new directions.

COLLINS: One possible way to look at the contrast between larger, more highly organized types of studies and these kinds of--what you're suggesting--informally generated individual interactions is to suggest that on the one hand, the larger ones had some element of organizational inducement. You may have been asked to participate. There may have been some managerial incentive to get people involved. So there's a sense in which these things are organized and managed. On the other hand, what you're suggesting for these things are more spontaneous collaborations.

What did RAND people feel more comfortable doing in terms of entering into these collaborations? I don't want to use the word "pushed into it," but the sense that this activity was more managed versus something that seemed more informal and spontaneous. I guess the question is one of balance.

SHUBERT: Yes. I think you have to differentiate among people and levels of people. I think that the [Ed(ward)] Barlows and the [Albert] Wohlstetters and the [Herman] Kahns and others who were stars in their own right had no problem with participating as leaders and, dare I say, managers--I guess I do--of interdisciplinary efforts. But I think a large part of that is due to the fact that they were the leaders and that some of the people under them did, from time to time, feel unduly constrained by things like deadlines, which normally didn't really exist, and by the necessity to tailor one's work to dovetail with another person's work and so on.

I think that the typical research staff member, given his druthers, would rather be in an unmanaged state and be working on things that he or she thinks is an important area to be working on, with a relatively limited number of colleagues and without a lot of pressure to meet deadlines or without what has become, unfortunately, the way people operate now, without a guarantee that there will be a product. It's very hard to remain a risk-taker, and I think RAND ought to be a risk-taker, if you guarantee that every project will have a product and you furthermore undertake, as RAND, to say that that product will meet certain quality standards.

There are many, many projects that should not have products. They just go nowhere because the idea turns out to be bad or it doesn't work out right or whatever. Yet, in the 1990s' contracting world and the 1980s', as well, it's very often stipulated, not in the military side so much but on the domestic side, that this contract will have a product, and there is no allowance made for risk-taking. That, I think, is really antithetical to doing good, creative research.

So it's a long way of answering your question, but I think the majority of the staff would rather be involved in a low-key rather than a high-intensity way in interdisciplinary work.

COLLINS: I think probably it's time to let that issue lie for a little while and move on. Not having your *vitae* right in front of me, I'm not clear on the precise chronology and the precise titles that you were working under as you moved from activities and SOFS and the Research Council to assuming leadership roles in the Department of Economics.

SHUBERT: The chronology as ordered is follows. When the SOFS Study was finished, I continued to work with Barlow, who remained director of projects, and we went into the early phases of an air Defense Study, exploring what would make sense to do in that area by way of research. (We concluded, incidentally, we could do nothing.)

At that point, Charlie [Charles] Hitch invited me to join the Economics Department, of which he was the chairman, and I accepted and really picked up working with Klein on the R&D area. So sometime after that, when Charlie went off to the Department of Defense and became assistant secretary controller, Joe [Joseph] Kershaw took over the department, and he asked me to become his deputy or associate head or something. I've forgotten what the title was, but it meant sharing with him a lot of the administrative burden of running a department. Of course, in those days there were no divisions or other layers of bureaucracy, so the departments were pretty much a world unto themselves managerially.

Then Kershaw left and went to the Ford Foundation and then Williams College, where he was provost and professor of economics, and he was replaced by Burt Klein, who then asked me, and now this I'm quite sure of, to be his associate head. When Burt left RAND for a year in 1963 to take up the NATO Force Planning Exercise, phase one of that, which we'll deal with at another time, he asked me to take over the department and Frank Collbohm, who was then president, asked me to take over the department.

So I was the department head on an acting basis for about a year, maybe a little over, and at the end of that year, I left town. I went to Europe and took over the NATO Force Planning exercise and stayed there for a little over a year and came back to the Economics Department.

I had my research interests shifted for me, I guess it's fair to say, by Frank Collbohm, who was literally waiting for me in the lobby on the day I was due back, saying that he needed a leader, someone he could count on, someone who would pull things together and get things going a lot better than they were with our research on Southeast Asia, and that he had decided I was the right guy (for heaven knows what reasons!). So that's how I became involved with Southeast Asia, after offering a certain amount of resistance, saying that I had built up all this NATO/European capital and it would make sense to me to build on that, having trunks full of

purloined documents and stuff that I've never even opened because of this diversion. Frank finally persuaded me that it was in RAND's interest for me to take this project on, and when I saw the work that was being done, I agreed with him.

COLLINS: That sketches out a period of years. Why don't we go back and examine your experience in the Department of Economics a little more closely.

When you began to assume management positions there, what did you see as the range of problems that were confronting the Economics Department at that time? What things were of concern, both in terms of the research activity and management questions?

SHUBERT: We always regarded the front office as a major problem. To quote one head of the Economics Department, "Let those guys tell you what to do, and you won't have any department left." So one of our jobs was to cope with the front office in such a way that we ran our own show, that we had control over the recruiting, that we had a satisfactory budget outcome for the department. So that, on the managerial side, was one of our main concerns.

Then we also had a very, very formal recruiting program that was run out of the front office of the department, but involved many, many staff members. In fact, we tried to have as many staff members involved as we could, and that tradition has essentially continued to this day with economists who are running departments in RAND's new organizational structure. So there was heavy peer involvement, as well as department management involvement.

We had the quality control process as a challenge to manage, and that ran all the way from the formal review of documents to the monitoring of people's careers to, again, the informal encounters which would lead me, as I did one day, to discover that one of our most senior researchers was getting data from an AT&T ad in the newspaper. [Laughter] He left RAND shortly after that--no causality implied.

The point I'm making is that relationships were really quite informal. I could go to that person and I could say, "What are you reading that for?"

He'd say, "This is a big input to my study."

I'd say, "You've got to be kidding!"

In fact, that event occurred on the day that John Kennedy was shot, so it sticks very much in my mind.

We were pretty autonomous with regard to hiring and firing as long as we maintained our budget limitations. I'll come to that in

a minute because that's a very interesting story. Then there were the ways in which we tried to structure things in the department to facilitate exchange of ideas and information. We had an Economics Research Council which was representative of the different schools of thought in the department. I think it met every week, and we kicked around ideas and so on and so forth. We had lots of seminars involving both our department and bringing in people from other departments. So it was a fairly lively time.

Of course, it was just one of eleven departments at that time, and one might very well raise the question, "Weren't you in competition in some sense with those other departments for scarce budgetary resources?" The answer is, of course, yes. But we still had at that time the luxury of having a central funding source, almost unitary, and the budget was really determined by Frank and his assistants. He would slice up the pie, and he would discuss with each department head, "What do you think about this for the coming year?" If the department head didn't like it, he would argue about it and maybe get more, maybe not get more, maybe some wanted less. Anyway, there was a goal set for each department and then a budget.

As an illustration of how uncomplicated it was in those days, I confess that I was in charge of the budget for the Economics Department. When we would get a budget allocation, I would sit down and go through the department staff and other expenses, and I would allocate the budget at the beginning of a budgetary year, with about a 20 to 25 percent overrun built in, because I felt that we could spend more than we had been allocated 20 to 25 percent productively. At the same time, or instantly after I finished doing the budget for the year, I would then do the budget report for the year so that I had unerring accuracy. At the end of every year, the budget report, which had been prepared the same day the budget was prepared and revealed a 20 to 25 percent "overspending," was duly submitted, and we were right on target because the budgets really, in the detailed sense, didn't really matter.

Now, the twenty-five percent business comes about as follows. In many ways in those days, the Economics Department was, in fact, if not the leading department and the brightest department at RAND, certainly one of the most aggressive. We would calculate that the engineers, for example, would get their budget, and they would be scared to death of overspending. We knew perfectly well that anybody who was scared to death of overspending underspends, and that was typical of several other departments, as well. So we got our twenty-five percent off the top without anybody ever saying anything. In fact, people were typically very grateful to us at the year's end because we helped RAND meet its minimum manpower pledges to the government because we had overbudgeted. We were not risk-averse, but we felt we were on pretty safe ground.

That gives you the notion for the informality of the whole process and, in a way, how once the big cuts had been made--department A, department B, department C--projects really didn't mean anything. It was all money coming from one pot. We had one project called Economics, which was a very big project. A lot of people charged to it. It was, as far as I can recall, never formally defined. It was, however, one of our most productive "projects."

COLLINS: When you say cuts, you meant parsing the corporation pie.

SHUBERT: Yes. Parsing the corporation pie is a nice way to put it. I haven't parsed a pie recently. But when you got below that level, I simply never took project budgeting seriously. I didn't have to, and if I didn't have to, why waste my time? That's no longer the case, of course.

Insofar as fending off or dealing with the front office, there were two ways the front office could make a department's life unpleasant. One was by front office bureaucrats wanting to stick their nose into what the department viewed as its affairs and, in so doing, causing perturbations, not little formless fears but maybe little formless irritations. So we tried the best we could to keep those people at their distance--the central budgeteers and so on.

Then the other thing was that from time to time, Frank would get it in his head to take what would be known in the State Department, I suppose, as a *démarche* and unload on the department. This didn't happen very often, but it happened often enough so that one had to be expectant, at least a little bit more than I was early on.

I remember...I don't think we've covered this before. My phone rang one day during the year that I was acting head of the department. I picked it up, and on the other end was Frank. He said, "Hi, Gus. This is Frank."

I said, "Hi, Frank. How are you?"

He said, "I'm fine. I've got something to tell you."

So I said, "I'm listening."

He said, "Some people learn fast, some people learn slow, and some people never learn at all. So-and-so in your department never learns at all. I want him out of here in a month."

So I said, "Frank, just hold the phone. I'll be right down."

He said, "You don't need to come."

I said, "Yes, we have to discuss this."

So I went down and went into his office and shut the door and said, "Frank, you have no right to tell me to fire So-and-so in my department, and I have no intention of firing So-and-so in my department. Now, if you want to get rid of So-and-so, therefore, you're going to have to fire me first and appoint somebody who will do want you want them to do. But as long as I am doing the job you asked me to do, I construe that job as not including following firing instructions from the front office, which doesn't know the good guys from the bad guys."

Frank was quite taken aback by this. He wasn't accustomed to this sort of thing. He just finally said, "Okay," and I walked out of his office and I never heard about it again. That person has enjoyed a long and productive career at RAND, not a noncontroversial career, but one highly productive, both institutionally and professionally, for himself and for his colleagues. So it was being prepared for that sort of random shot that might come over a department's bow at any time, and that was part of our ethos in the management of the Economics Department.

When I look at the departments today, I'm very sorry to say, I'm particularly sorry to say since most of the department heads I had something to do with selecting, some of the worst ones, as it turns out, I appointed, and there aren't enough guts among the bunch of them to stand up to Casper the Ghost. I just had a talk with one of the department heads the other day, in which I said to him, "What's going on here? This is your department. You're running this department. Why don't you run it?" Of course, he was moaning and complaining to me about the inadequate quality of someone's performance and this and that, and I said, "Who is in a better position to do something about that than you? In fact, you've got to do something about it."

"Well, maybe the vice presidents won't like it. Maybe the president won't."

I said, "That's all irrelevant. You have to do what you think is right."

A lot of that spirit is gone now, I'm sorry to say. I think it was very good to have that sort of independence from management that has disappeared. It had a downside, as well, but given the choice between doing everything management said and maintaining your independence, that's no choice. I think you just maintain your independence as best you can.

COLLINS: Frank had at least one other vehicle besides the occasional phone call to work with the departments, and that was the Management Committee Meetings, which I guess were held on a

periodic basis--weekly, I think. To what degree was that particular venue used to help direct the department in directions that Frank was interested in or give him an opportunity to make input about what the departments were doing and that kind of thing?

SHUBERT: I participated in many, many, many of those meetings, and I'm not sure that I recall Frank ever really...I want to say say anything, but surely that's not true. He must have said "good morning." But he would sit at the head of the table, and he would preside, and he wouldn't give instructions. He would ask people to talk, and he would go around the table, and he would listen to what everybody had to say. And then if he had some announcement to make about Air Force policy or new developments in the Air Force, he would do that, and that would be the end of the meeting. So the meetings tended to be excruciatingly boring and nonproductive, and people really didn't like to waste their time going to them.

COLLINS: So he didn't use them as a tool to help manage the activities of the department?

SHUBERT: I think he used them indirectly. That is to say, he learned at those meetings what I was doing, what [Hans] Speier was doing, what this one was doing, what that one was doing. Then he would go back into his office and sit and ponder that, and that might or might not result in some sort of *démarche* or other direct communication with the relevant person. Frank was not a very public person, as I'm sure you know, so he strongly preferred to deal one on one and in private than to do things in a public forum. So I would say it was an indirect way for Frank to learn a certain version of what was going on.

Of all the Management Committee Meetings I attended, I can think of only one that was really worth it, and that was one where the head of the Social Science Department and I swung at each other across the table over the research quality of a project that was in contention. That was fairly exciting.

COLLINS: Was this a joint project?

SHUBERT: It was a project of which I was in charge, but it was run out of the Social Science Department. My belief and that of my Economics colleagues, none of whom ever saw fit to back me up in anything but the most surreptitious and devious ways, as sometimes happens in academe, all agreed with me. In fact, many of the social scientists agreed with me, as well. But I could not persuade Speier or the project leader to stop it.

I finally got it down to a minimal level. I brought a quantitative political scientist in from outside as a consultant, to "evaluate" the research project. He concluded it was not, whatever it was, a <u>research</u> project. I got some other RAND people

involved in using the material that was generated by the project, who used it and came out with conclusions diametrically opposed to those that were coming through the project leader. Then when [Harry] Rowen came, he was one of those who from outside, from OMB [Office of Management and Budget] or the Bureau of the Budget, where he was, was watching the study. One of the first things he did was sit down with me and say, "That's got to stop and So-and-so's got to go." And I said, "I'm with you. Let's do it," and it was done. But returning to the subject of the management committee, that's the only meeting that I can recall where there was something to get excited about.

COLLINS: I don't know whether it's worth going into the goal and content of the particular study to illuminate this in any way, but did Frank intervene in any way in the course of this meeting? Was he a mediator or a judge, or served in some capacity to help resolve the difference there?

SHUBERT: Frank sat at the head of the table and got whiter and whiter and whiter, and his lips got tighter and tighter and tighter, and he did not intervene in any way. So it was left to us boys to settle things down at the middle of the table.

The table--I suppose it's fortunate in a way, we were seated at the broadest part of the table and therefore couldn't reach each other. So we were standing there flailing away, without making any contact, except, of course, to our egos. There was a lot of general shouting, as I remember, but there was no resolution of the issue.

COLLINS: Why wasn't there a mechanism to resolve this issue? Given the importance of interdepartmental work, when problems like this arose, why wasn't there a mechanism to resolve them?

SHUBERT: There were mechanisms. There were review mechanisms; there were the mechanisms of having me being the boss of all this work; there was the mechanism of my being able to appeal to Frank. They all failed in this particular case. I went out and reviewed the project in the field. It was a field-oriented project. From the very minute I set foot in the office and on through going out into the field and seeing how interviews were conducted and how interviewees were selected, how the data were recorded, what interpretations were made of the data, the whole thing struck me as a joke. Only it wasn't a joke; it seemed to me dead serious. I determined right then and there that it had to stop, ten minutes into the game. I saw it as a serious threat to RAND's integrity and to RAND's quality and RAND's reputation. For all this, I couldn't get it stopped until I managed to get it stopped when Rowen arrived, although I managed to get some really good work out of this by bringing in other people. But that was a case that was so extraordinary, that all of the conventional mechanisms we had

available just failed.

COLLINS: In a sense, I guess it speaks to Speier's strength as a department head that he could, in essence, support his person.

SHUBERT: The meeting ended with Speier saying--I remember his words--"And I say hooray for So-and-so," who was the project leader. Those were the last words of the meeting. On that note, the meeting broke up. But Speier wasn't succeeding in defending him. It was the man who was dead white at the head of the table who was succeeding in defending him because unless I could move Frank...and the irony of this is that Frank knew there was a problem. He put me in to fix the problem, and then he didn't seem to want me to fix the problem.

So he and I had many long talks about this, and I just couldn't persuade him that there was no research paradigm, there was no research design, that their methods were bound to result in misleading databases and to give one all the scope in the world to draw any conclusions the project leader wanted to.

It was a project which has become...you've probably heard about it from Wohlstetter and others. It was referred to by the cognoscente as the "How I Learned To Love To Be Bombed Study," the study done in Vietnam, one of its central conclusions being that "our" South Vietnamese would go through a night and a day and a night and a day of bombing and incoming artillery, and they would come out of it and blame it all on the VC [Viet Cong], which seemed to me ridiculous on the face of it. There were no underpinnings for it in the material that we had collected that couldn't be refuted instantly by using the same material.

COLLINS: This example, then, is drawn from a slightly later period than when you were acting chair of the Economics Department.

SHUBERT: This was in 1966 or '67. I was on the Research Council, I guess, at that point.

COLLINS: Two questions out of this example that you sketched. Does this in some way indicate a difference in methodological approaches between economists and social scientists? Was that one of the dividing lines that was evident in this situation?

SHUBERT: There were certainly differences in approach. Maybe I can best illustrate the kind of difference by referring to another study that was done in New York by a sociologist who was working for us up there, who had a propensity to want to draw curves using only single points, and that was the quality of research that was going on in this project. It was just bad work. To call it scientific is to ridicule the word.

I viewed my job, as I say, as to get it fixed, to make it rigorous, and so on and so forth, and I had some success in that some of the really important papers were written not by the project leader but in spite of the project leader, and they were received and taken very seriously by the secretary of state, by the secretary of defense, and by other high-level decision-makers. So some excellent work came from a very bad situation.

TAPE 1, SIDE 2

COLLINS: Maybe I can frame my questions in a slightly different way. Hans Speier was, in his way, a consummate professional. He had a very high standard of scholarship. I guess I'm asking you to think about what's going through his mind. Was he, in essence, saying, "This person was doing good work and you're not understanding it"? Or, "I'm going to support my staff member because he's my staff member"?

SHUBERT: I think it's even a little more complicated than that. If you will recall the videotape that we did, at one occasion in that videotape, Hans gave a rather eloquent and, I thought, almost heartrending speech on how unhappy he had been as a department head and how he viewed his job as a department head, which was to keep Frank happy. I believe that in this case, Hans was defending his person because he was in his department and because he knew that this person had Frank's full support.

Frank was in the paradoxical position of giving me his full support and giving this social scientist his full support, and he just couldn't do it. It was simply impossible. Something had to give, and finally the project went. It was wrapped up and put away, and not without good work.

So Hans, I think, was meticulous in applying those standards of scholarship to himself and to any co-workers that might be working immediately with him, but he got very myopic when he got into areas that he didn't know a lot about, and certainly Southeast Asia was one of them. I'm not even sure to this day that he even read the stuff that was being ground out. That may be unfair to him. But it wasn't as important to him as it was, obviously, to me and to the person who was doing it.

COLLINS: Is it worth identifying the individual involved here? You have been careful not to specify the person. I'm wondering whether it would be useful for the record to indicate who we're talking about and the study that was involved.

SHUBERT: The study was called Viet Cong Motivation and Morale, and the principal investigator was Leon Gouré.

COLLINS: My other question was wondering about your background in

relation to this. You had a master's degree in economics.

SHUBERT: No, in literature.

COLLINS: In literature, that's right. But you had essentially self-taught yourself in economics. In this kind of case where you were asked to render a judgment about the professional work of somebody who was in the fraternity of social scientists, I'm wondering whether you ran into the problem of there being a sense that you weren't the person to make this kind of judgment, based on your background.

SHUBERT: No, I don't think it was individualized so much. I think it was more that no one from the Economics Department or associated with the Economics Department should be in that position. So I don't think it was personalized, although I may be wrong about that.

Another fascinating thing about it is that I took care to make sure that there wasn't something there that I was missing, and so I set up review boards. I mentioned that I brought in one person from the outside to review the project. He is a social scientist, not an economist. I got people in the Social Science Department, as it was then known, to look at the project and its output, and of course, I went back to my buddies in the Economics Department and had them take a look at it. So I had three different perspectives, and from any way you looked at it, it was baloney, according to these people, as well as according to me. So I wasn't exactly going into this alone.

But the interesting phenomenon was that at this management meeting, and on other occasions when it would come up, it was such a hot issue and people knew how Frank felt. That's the danger of concentration of power in one person and of having deferential staff. The minute I raised it and I turned to people who I knew agreed with me, who fifteen minutes before had been grousing about the absurdness of this study and how it's got to be stopped and so on and so forth, when brought into a room where Frank was seated and I would turn to them and I would say, "If you don't believe me, ask him," and pointing out someone both competent and knowledgeable that person would say, "Oh, don't ask me. I don't know anything." [Laughter] So all my support would just drop away.

COLLINS: I've had that experience before.

SHUBERT: So I was out on the end of a very long limb, apparently all by myself, although I wasn't all by myself and I knew perfectly well I wasn't all by myself. It's just that people didn't care enough to take it on.

COLLINS: Your sketching this out a little further raises an

additional question. I wonder whether the issue of Frank's own ideology comes into play here. I haven't looked at this particular case, but looking at Frank's activities in the fifties, one might see a relationship between the objective of the study and Frank's own ideological tendencies with respect to the Air Force and Air Force activities. Was there some way in which Gouré's study reinforced or fit in with, in some sense, Frank's own predispositions about what ought to be done in Vietnam?

SHUBERT: It's very, very hard to read people's minds and to know what's actually going on. Let me just say that the study started under totally different leadership, and although they used essentially the same interview methodology, the conclusions that were reached and published were essentially what one might expect--that is, if you drop a bomb on me, you do not make me hate some third party; I hate you.

The study advocated a cessation or diminution of bombing, artillery, and so on, as being essentially counterproductive. Not only were we killing thousands and thousands of people, but we were alienating the survivors as well.

This began when I was in Europe, so it was really remote from me and my activities and my interests. In fact, I didn't even know about it. Somewhere in there, before I came back from Europe, a change was made in leadership, and Gouré was sent out to take over the project. The minute he took over the project, all this "How I Learned to Love to Be Bombed" flavor began to permeate it.

I guess there was some kind of rebellion or something that got Frank upset, and that's why he was waiting for me in the hall when I came back. Why me, I don't know. Maybe just because I'm the one who wouldn't fire the other guy, or he knew I was somebody who would stand up for what I think is right. But I don't think he ever thought that we'd end up with such diametrically opposed views, but we sure did.

So whether subconsciously or consciously Frank made that leadership change because he didn't like the message of the study, because he knew what Gouré would say, I don't have the foggiest idea. But if one believed in conspiracy theories, one would not only have John F. Kennedy riddled by thirty-two bullets fired by seventy-four people and an orangutan in a tree, one would have Frank plotting with the Air Force to fire these guys out there, replace them with Gouré, and then get the right message out which would support Air Force policy. But you really have to be a conspiracy theorist, it seems to me. I mean, it's easy to build that, but I don't believe that it would even be supported.

I first ran across this project, incidentally, when I came back from Paris, where NATO was at that time, to visit the Pentagon to

help write the secretary of defense's annual statement for the NATO Council. While I was there, I dropped around and I saw all my friends, and they all were smiling and so on and so forth. Finally, I said to one of them, a general friend of mine, "Johnny, what's going on around here? Why is everybody sitting around smiling? It seems to me everything's going very badly, indeed."

He said, "Oh, that's simple. Leon's just been through. It's just like a breath of fresh air to hear how well we're doing in Vietnam. It really raises everybody's spirits."

I didn't think anything more of it. I just went about my posture statement, or whatever it was that we were writing, and went back to Europe and forgot about it. But when I came home, the whole thing really turned out to be quite a can of worms, and in my opinion, people were being fed what they wanted to hear, or they were being given a message that was very, very welcome. As this general put it to me at the time, "Leon is the only hopeful sign on the horizon." I think I said something about, "Gee, I hope he's right," although even at that point I had a penchant to doubt.

COLLINS: We started off on this tale looking at the relationship between Frank and the departments. As you began this sketch of department life and its place in the organization, how did Frank make initial determinations about what was appropriate for each department to get of the corporation pie? Were there sets of discussions beforehand that helped to set broad priorities in the sense of what each department needed? How were budget discussions handled?

SHUBERT: Typically, the number-two person in the department would go down and talk with Frank's budgeteer--I think there was only one in those days--and tell him what the department was doing. There was no zero-based budgeting. We knew how much we'd had last year, we knew how much was coming in this year, we had a strong supposition of what would be coming in the year after, and so we would just generally describe the sorts of things we had been doing, the sorts of things we intended to start that were new, what all this would cost, and we'd ask for that. I never had any trouble getting it.

There were some bad years, of course, when we had reductions in force and that sort of thing. That's a whole different issue. But on the positive side of budgeting, the elegant simplicity of it was such that one pretty well got what he wanted, unless there was something in there, some idea, some project, that Frank would like to know more about before he gave money for it.

COLLINS: I'm not getting the sense that Frank used the budgeting process as an opportunity to assess what the departments were doing and perhaps provide some input about direction.

SHUBERT: Not in my presence, no.

COLLINS: You had talked about when you got your budget and factored in a twenty-fiver percent overrun. Did that mean that you would then have to go back to Frank and say, "We need X amount of dollars to complete all these studies we've got under way"?

SHUBERT: No. I would just go ahead and spend what I'd been given, plus twenty-five percent, counting on others to underspend, and we were right every time. So it was an informal process.

COLLINS: So the thing that was important was not that individual department budgets came out at the right figure. It was just that the overall corporation budget hit the right target.

SHUBERT: That's right. As I mentioned earlier, this informal process that we went through of adding this little kicker in there on a number of occasions was responsible for the corporation meeting its minimum manpower requirements. Our Project Air Force contract, Project RAND contract then, specified a manpower level and associated dollars with that level. It had a minimum and it had a maximum, and if you didn't meet the minimum or if you exceeded the maximum, you were out of the contract. I didn't see any sense, if we had good, productive things to do, in busting a contract on the downside when we always had more exciting things to do than we had money, anyway. So we didn't play it very conservatively. Other people did, and there was a good fit.

COLLINS: At that particular time, did you have to give much effort, once you had a department budget, to carefully deciding which projects were going to get support and which ones you just couldn't support? Was the problem of developing a research agenda something that took a good deal of effort and guidance?

SHUBERT: I would say the answer to that is no. I'm not sure that you would even say that the department had a research agenda in any formal sense. What it had was a bunch of good people participating in a bunch of projects that were generally deemed sequentially, rather than at any one confrontational meeting or one big session, to be judged worthy of support. There were areas, for example, the Soviet area, and we had a feeling about how much that ought to be of our total effort, and we had the intelligence area and we had a feeling about how much that ought to be of our total effort, and we had some work on methodology and so on. We simply didn't have to have any big formal to-do about that kind of stuff. It just all worked out.

COLLINS: But there was a sense in your mind that there had to be some balance between areas, or at least some allotment for research in different areas?

SHUBERT: Yes. Sure. And we were clearly trying to maximize our comparative advantage and where we were the strongest, and we were trying to phase out areas where we were weak, or else recruit people into them if we thought they were very important, and then we were always looking forward for new kinds of things, new ideas, to fund.

At the very beginning, when I came back from NATO, I remember talking with one of my colleagues. (I came back to the department. I guess I didn't go directly to the Research Council.) In talking to one of my colleagues about a research project--and I think we had just begun the bombing of the North--I asked him to take a look at the vulnerability of the North Vietnamese economy and other less-developed countries' economies to force and intervention from the outside, and I had a devil of a time talking him into doing anything at all about it. He came back in a week and he said, "I've done the project."

I said, "That's great. What's the conclusion?"

He said, "There's nothing there. All we're doing is making holes in the ground. There's no economy, in the conventional sense, for us to go up there and destroy." So that was his conclusion. It was never written up. It was maybe the world's shortest project, and I think it probably had the world's "rightest" answer.

COLLINS: I'm wondering whether you can characterize whether there is anything interesting about the transition from the Charlie Hitch/Joe Kershaw team to Burton Klein and then yourself for the Economics Department. Charlie was essentially the first head of that department and was there for a long time. Kershaw worked closely with him. I'm sure they had a certain similarity of viewpoint. I wonder if the change in leadership in the department had any interesting consequences.

SHUBERT: I think the principal characteristics of the change--and I would put Burt and me in one pot and Charlie and Joe in another for this purpose--I would say that with Burt and me, you certainly got a much greater willingness to take chances than had gone on earlier--chances on people, chances on projects, chances on doing this NATO thing, the NATO Force Planning Exercise. It would make some people recoil in horror to think of Burt Klein, and then Gus Shubert, sitting at a great big table with the American flag in front of them, and a placard saying "United States of America" and arguing with all these guys from all these other countries who were all diplomats. And who were we? We were the boys from RAND. (Some of us used to call it "the Marx Brothers at NATO.") But we were willing to take that kind of risk. I'm not so sure Charlie would have been willing or that Joe would have been willing. So I think that was probably the biggest change.

In personnel, I hired someone who had been tossed out of M.I.T.'s [Massachusetts Institute of Technology] graduate school two or three times for general dereliction of duty and malfeasance, and finally went back and finished up and did extremely well when he got his mind made up. I thought he was a good bet, and I hired him after running him through the department, and everybody was either lukewarm or ice cold. I said, "Well, I think he's worth a risk," so I hired him. I had a line outside my door running all the way down the hall--people wanting to complain and yell at me and why did I do this and do that.

At the very head of the line was Jim [James] Schlesinger. I can remember this very, very well. He came in and said, "This guy is not what we need. He's not qualified. Don't you know he was irresponsible as a graduate student?"

I said, "Yes, but he's not a graduate student anymore."

It was less than six months later that Jim came to see me, and he said, "I want to apologize to you. I'm working very closely with this person now. I think he's one of the best people that ever came to RAND. He's working, and we just see eye to eye. We're working together very productively, and that was a very wise decision you made," which obviously made me feel very good (making up for some of my blunders!). I don't think that Charlie or Joe would have taken that risk.

There was also a change--which maybe reflected the times as much as the personalities--in the willingness to accept female and minority professionals. I had been told by one of the former heads of the department, when I asked him what we were going to do with this woman who was an RA [Research Assistant] and was about to receive her PhD. in economics, when were we going to promote her and give her a full professional rank, and I got the answer, "Never. The thing to do with that kind of person is to get rid of her."

It was, I guess, a year later that I brought the first, you might say, mustang up from the ranks and made her a full professional, and that was cause for another long line outside my door. "You're bringing this woman up and making her our equal and she's not even qualified and she isn't this and she isn't that and she isn't the other thing." It's hard to believe how things were in those days. So there was much more openness of that sort.

Certainly, with Burt--I don't mean to omit myself from this, but certainly with Burt there was more eccentricity than had ever appeared in those august chairs of power in the Economics Department before, because not only do I have my own quirks, Burt was, is, and always will be the world's greatest eccentric, or the most eccentric person I have met during my lifetime. Also, in many

ways the smartest and perceptive and creative in his thinking. But he is eccentric. So that added a different flavor.

I saw Charlie recently. Last summer I went to an outdoor picnic, and Charlie showed up wearing a business suit and necktie. [Laughter] That's just Charlie.

COLLINS: When Hitch went into the Kennedy administration, was there any substantial number of personnel from the department who also left at that time for whatever reason? Was the staff pretty much the same under you and Burt as it had been under Charlie?

SHUBERT: I think it remained pretty much the same. Obviously, Charlie didn't want to appear to be loading down the government with RAND people, although RAND people did go. I'm just trying to think who they were. I'm sure some people from the department went, but it was a fairly large department and reasonably stable in its composition. Nobody, when Charlie left, said, "I'm getting out of here. The place is going to the dogs." I didn't see any of that or feel it, nor did I see it demonstrated by people just voting with their feet and leaving. I saw some of that when Harry came. Some of the economists voted with their feet and left, but not many, and I saw it probably because I knew them so well. I knew that that's what they were doing, they were voting with their feet, because they told me. So I would say those were the principal differences.

Probably professionally there was also...I ought to touch on that. I was not, am not, and never will be a full-fledged economist. Burt is an economist. He's got the credentials. But he is probably more scornful of the economics profession than he is of any other single thing in the world. So he is a real critic of conventional economic thinking and the kind of training that kids are now getting in graduate school in economics. It makes him sick. So there was that difference.

Charlie was very, very much a conventional economist, and so was Joe because Joe said to me one day when I was talking to him about poor people down in Venice, "Don't tell me about them. All I'm interested in are numbers, and the numbers say that everything's okay." Now, that's truly an economist's statement. Burt would never say a thing like that, nor would I. So that was another difference.

COLLINS: What were the kinds of contacts that RAND had with the university community? The Economics Department was very strong in a professional sense. They were certainly people who had great respect in the general professional community. What was the nature of the relationships that the department tried to keep up with the university community when you were there?

SHUBERT: First of all, we had, of course, a very broad array of consultants on our rolls who were at universities--[Paul] Samuelson, [Robert] Dorfman, [Robert] Solow, [Kenneth] Arrow. Those are all Nobel laureates, I think. Then we had a lot of working contact with people like Schelling, people at the University of Chicago, people at Stanford University, people at Yale University. Joe Peck at Yale University. Merton Peck, known as Joe, did a lot of his work on R&D, costs of R&D and R&D strategy, in affiliation with RAND. So there were the ghosts of the past, even at that time, in the form of the people who were in at the very beginning--Solow, Samuelson, Dorfman, Leon Tiev. I mean, the whole crowd. They were all in there.

Then you had the active cadre, which was the people like Schelling, Bob Summers, who was at Yale. He's Paul Samuelson's brother. We've done a lot of work very informally with Anita Summers, his wife, who's just retired as the head of the Public Policy School at the University of Pennsylvania. So there have been all kinds of working-level contacts.

Then there have been very good relationships with people like those who had the misfortune to be made department chairmen, and we would be in constant touch with them, say, in six or eight of the best schools, looking for their best students. They knew RAND. They knew the kind of people we wanted. So every year at recruiting time, we'd be on the telephone or we'd be visiting them and that sort of thing. Then also, the department members were very, very active in publishing in professional journals and going to professional meetings, very active in the American Economics Association. So the department, I think, was the most visible in academe of any department in RAND. It was really the department that I think was the demonstrator that, if one relied on high-quality professionals and their motivation as professionals to do good work, that's the strongest thing you can have going for you. I don't think other departments have had that much success or seen things that way as strongly as the Economics Department did.

I know from dealing with the Engineering Department, for example, that many of the engineers miss this, but they don't find the opportunities. When we had only "military work"--that is to say work sponsored by the Air Force--in a sense, people were more free to exercise their professional credentials and to engage them in meetings and disciplinary intercourse, both written and verbal, than when we became fragmented as we are now with 5,000 different contracts to do this and do that and do the other thing.

The reason I put quotes around "the military" is that, as you know, the Project Air Force/Project RAND work statement is a very simple work statement which says, "Do things associated with the future of the United States of America and the public interest." It is about a sentence long. And so there were a lot of things

that fit under that rubric. Kenneth Arrow began his work at RAND, with the Air Force sponsoring it, on the theory of democratic choice. It's the work that ended up leading him to the great prize in the sky. We had one guy who developed ulcers. He happened to be an engineer, but he got very interested in ulcers, and so he did a research project on ulcers using RAND fee money. So there was a latitude there which comes from simply being very, very flexible within broad limits, and I would say a lot of that has disappeared now, which I miss, and I know many of the professionals miss it.

On the other hand, the upside of the current environment is that people tend to be more productive. There were people at RAND in the bad old days who disappeared into the woodwork and never did anything and showed up ten years later, and someone would say, "Gee, I forgot about him."

I suppose the total productivity of the place was around 10 percent, and under the current arrangement, at least in some of the domestic areas, people are just going flat out all the time with lots more product and lots more visibility and that sort of thing. Maybe we're two or three times as productive--quality aside--than we were, and when you figure that the literature on research generally characterizes 10 percent as average or better, well now, we're really cooking with gas!

COLLINS: In the fifties and I think into the sixties, the Air Force periodically raised concerns about RAND's staff members publishing the results of research in professional, open journals. This was an issue that came up periodically. Did that ever impact on the Economics Department, which maintained this kind of vital involvement in its professional community?

SHUBERT: As far as I know, not directly, no. In fact, one example comes to mind that I think is the work that Summers did on the accuracy of cost estimates over time, where he did a case study of the development of the Minuteman missile. I don't know whether that was first published as a RAND report and then as a journal article or vice versa, but I know it appeared as both and had four times the influence it would have had, had it been only a RAND publication.

I suppose it could be construed as not too flattering to Air Force cost estimators because it says what everyone knows to be the truth, but it says it empirically, that when you're at the beginning of the project, you don't have the foggiest idea in the world of what it's going to cost, and as you work your way down the line, you begin to narrow the uncertainties, and pretty soon you can begin to develop reasonable estimates. Summers was able to calculate what the standard errors were likely to be at any given stage in an R&D project, an extremely useful thing to do, and it got a lot of academic acceptance, as well as creating some stirs in

the military, on the Hill, and elsewhere. It was a very, very good piece of work.

TAPE 2, SIDE 1

COLLINS: Is there anything else we want to go over with respect to your activities in the Economics Department? Our discussion ranged over a wider span of time, but our starting point was your role in the department management in the '63, '64 time frame. Is there anything else?

SHUBERT: I think the only other thing is to point out that in parallel with the management, of course, I was putting at least half my time in on research in the R&D management area, in the space systems area. Then I went off to NATO, and that essentially ended that R&D research experience. I think I came back as associate head but then quickly went to the Research Council when RAND was reorganized after Rowen came.

COLLINS: Let's talk a little bit about the NATO experience and how you came to be asked to take on that role, precisely what that role was, and what RAND was doing over there.

SHUBERT: In 1963, there was a major battle raging within NATO as to what appropriate force levels ought to be, and there was a major split between the civilian part of the American defense establishment and the military part of the defense establishment about NATO strategy and about what course was likely to be most successful in persuading our allies to "level their forces up," as the saying went in those days.

So [Secretary of Defense Robert S.] McNamara, being a rather energetic fellow, as everyone noticed, asked the question, "What do we have now? What are we leveling up to?"[4] It turns out that virtually none of the countries was clear on the composition or the number of the forces that they had--land forces, air forces, sea forces--that were committed to NATO, and, indeed, some of the countries, such as Greece and Turkey, weren't even clear on what kind of forces they had altogether.

So McNamara used this as an opportunity to get what he thought were a bunch of smart analysts from RAND over there as part of the American delegation to NATO and to participate in something of his design, but he persuaded the secretary general to institute it, calling it the NATO Force Planning Exercise, the ostensible purpose of which was to establish a baseline of forces in existence and

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[4]Robert S. McNamara (b. 1916). U.S. government official, banker business executive. U.S. secretary of defense, 1961-68, caused controversdy by applying modern managerial concepts.

committed to NATO. Now, you would think that would be a fairly simple question to answer, but I can assure you it was not.

Then there was also a hidden agenda, and the hidden agenda, not very well hidden, was to decrease reliance on the American nuclear deterrent and to increase reliance on conventional forces in Europe. That was McNamara's goal. Of course, he was bitterly opposed by the French in that goal, so much so that the French finally...ultimately this issue played a major role in their leaving NATO. He thought through this Force Planning Exercise that he could show how far below strength the countries were, and that that would give them motivation to commit more money to improving the quantity and the quality of their forces so that they could avoid a nuclear war in Europe.

So that was the hidden agenda, and that, of course, was calculated to get us--"us" being the NATO Force Planning people, Americans--into difficulty with the American military because we were going to produce numbers which would show how bad things are, and their view was, if I understood it correctly at the time, that such a distressing set of numbers would only discourage our allies, and they would say, "We could never make it, so we're not even going to try."

General [Lyman] Lemnitzer,[5] for example, seemed to believe that there should be constant exhortation of our allies: "More money, more money, more troops, more troops, more money, more troops. Level up or we'll level down," that kind of dialogue. Our point of view was that that was counterproductive. So the biggest problem we had--"we," again, being the RAND group--was not really any of the countries seated around the table, including France, although that was a problem, but it was with the American military, who regarded us as the archenemy, and we regarded them at the time as the people who were standing in the way of progress. This became a very public and open rift although somehow it never rubbed off on RAND.

Our status there was very peculiar. Burt was the first leader. I took over from him as the second leader. The American team was composed exclusively of RANDites at the beginning. Along the way, we picked up a Navy captain. He was in uniform at the time. He liked what we were doing so much that he resigned from the Navy and joined the State Department as an FSR, and he became a member of the team.

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[5]Lyman Lemnitzer (1899-1989). U.S. Army general. Engaged in World War II and Korean War; chief of staff, U.S. Army, 1959-60; chairman, Joint Chiefs of Staff, 1960-62; commander, U.S. forces in Europe, 1962; Allied Commander, 1963.

Indeed, we began digging out data about the quantity and the quality of troops, and we found that they weren't even close to what the Americans thought that they were, and our belief that their reaction to more pressure would be to simply throw up their hands was confirmed.

They also seemed to share the general impression that they would rather have a nuclear war in Europe, a tactical nuclear war, than to spend more money on conventional forces. So as a subsidiary part of our activities over there, we put together a scenario of limited nuclear war in Western Europe and briefed that to all the assembled multitudes so that they could see what we thought such a war would look like. That also was extremely divisive. It resulted in even worse relations between us and the military.

COLLINS: Was that part of the planning exercise?

SHUBERT We made it part of the planning exercise, although it was clearly RAND work. We brought Harvey Averch over from Santa Monica to do this briefing although we had had a big role in preparing it, and he gave the briefing. There were so many stars and broad stripes and so on in that room from every NATO country, everybody was there but Lemnitzer himself. His chief deputy, Ted Parker, who was, I think, also a four-star general, was there. I got up and introduced the speaker. He painted this scenario which was very grim, indeed. The message was that "You may not have liked World War II, but if you didn't like World War II, you're going to like this a lot less because there's nothing left by the time this 'tactical' exercise is finished." That was the message of the briefing.

When the briefing was finished, I went up to the front of the room. There was this absolutely dead silence--dead silence. You could have heard a pin drop. Everyone realized how counterdoctrinal this heresy was. So I went up and got to the podium again and I said, "We'd be glad to take any questions."

At this point, Ted Parker walked up to the front of the room and he said, "I have a question, Mr. Shubert. In fact, whenever I hear a briefing or a presentation that is so impressive, as impressive as that one was, which is a rare thing indeed, I have three questions that I have to ask. My first question is, how did you get so smart? My second question is, where did you learn all that? And my third question, Mr. Shubert, is, so what?"

There was an even thicker silence in the room, and the meeting broke up. So that gives you an idea of the sort of pressure being generated.

I think that the exercise was very constructive. I think it

did have a positive influence on getting NATO strategy changed and decreasing reliance on nuclear weapons in doing baseline planning--that is to say, recognizing what you have. There's no use planning an augmentation of twenty-five percent when you don't know what it's twenty-five percent of. So we were able to do that. In fact, when we left, the activity was institutionalized. A civil servant was put in charge. I guess after I left, Jim [James] Digby came over, and then after Jim left...no, I guess Jim Digby didn't take over. He was number two, and a civil servant took over, I think. But anyway, there was some period of transition. Then it became institutionalized.

I myself left, I remember very well, on the same day that the U.S. ambassador left, Tom [Thomas] Finletter. He and I became very good friends and collaborators during this exercise.

I suppose if you draw back and look at it, it's at least interesting that McNamara called on RAND and RAND people to do this. It's also interesting that RAND said okay, without, I think, giving it a lot of thought, and it's interesting that there was never any attempt to recruit us, except for me. Before I left, I was offered a ministerial post, and I said, "No thanks." But there was never any attempt to regularize our status so that we were really unknown quantities, loose cannons on the deck, anything you want to call us. Nobody knew what to make of us, which is a great advantage when you're in a bargaining situation like that.

I think, on the whole, it was probably worth it. It was probably worth the energy and the time and the money that went into it, in the sense that it did introduce some element of rationality and--reality, I guess, is a better term--into the NATO planning process. It had its moments. There were lots of arguments and lots of quarrels. Burt was sometimes at his most eccentric, and you can imagine in a diplomatic setting how that goes over.

Particularly, one scene comes to mind. I was with Burt, and we were with the German delegation. Burt was a pipe smoker, and he would smoke anything that would burn in that pipe. He'd crumple up cigarettes, cigars, pipe tobacco, anything. In this meeting, we all came in. The Germans got up. We sat down. The Germans sat down. Burt started groping around in his pockets and had to stand up to find his pipe. He stood up, and all the Germans stood up. He found his pipe and he sat down and all the Germans sat down. Then he started looking for his tobacco. He had to stand up because he couldn't find his tobacco, and they all stood up until he sat down again. And then he looked around, as only he could do, and said, "Anybody got a cigarette?" Cigarettes were proffered, and he then sat there and crumpled it up and put it in his pipe to their utter amazement and bewilderment. [Laughter] They didn't know what the State Department had come up with here. Nobody could outdo Burt in situations like that. This up again, down again, up

again, down again went on for an hour.

COLLINS: It sounds like Three Stooges or The Marx Brothers.

SHUBERT: That's right, the Marx Brothers at NATO. I'm telling you, it was hilariously funny. It had to happen with the Germans. That's just the right place to have it happen.

But for all his eccentricities, Burt saw right to the heart of the matter, what the really big issues were. Nobody had to tell him. So he figured all that out right away, and then we transitioned in. So when I took over from him, there was no problem in understanding what the name of the game was, and there was good continuity, and some of the team stayed on anyway.

There were some pretty powerful RAND people over there. For example, Andy [Andrew] Marshall, who's still in the DOD [Department of Defense], was a member of that team. Fred Hoffman, who was one of RAND's leading interdisciplinary researchers and economists and who is now working with Albert Wohlstetter. Oleg Hoeffding, who was a Soviet specialist, was there. It's so long ago, I guess I've forgotten the others, but they were all good people and highly motivated and, I think, really did a good job.

COLLINS: Just to clarify the organizational setup. This was something that was done under NATO auspices. Was your little group designated as the lead for this study and people worked with and contributed to your organization of it? Or was somebody else in NATO at least the titular head of what was going on?

SHUBERT: There was a titular head who was a NATO bureaucrat of some sort. How important it is is suggested by the fact that I don't even remember him because he didn't do anything. The United States team was composed of RANDites. The United States team was, *de facto*, the leader, but never was it so designated. It was partly the leader by default because nobody else wanted to do it. Then we got the Germans, who were interested in doing it for their own reasons, because they had large forces and large reserves and they wanted to show that they were more than doing their part, to the embarrassment of some of these other countries.

The French wanted to participate, in my opinion, because it was bound to be a divisive exercise and maybe help them get themselves out of this mess that they thought they were in--the mess being NATO. The Dutch were very interested in the exercise. So we gradually built up a team of interested people from different countries who more or less accepted answering the questions that we thought ought to be answered, or trying to. So that's what I mean by *de facto* leadership.

We weren't really wallflowers, either. We didn't hesitate to

raise the questions and ask the questions and raise argumentative issues and so on because we weren't diplomats. We were not supposed to be diplomats. We were experts in force planning, it says here. And so we would be pretty gauche on occasions on purpose. (Sometimes we didn't have to try, but sometimes it was on purpose.)

So the overall exercise was a NATO exercise. Each member country was invited to constitute a team, and the head of that team was to be its delegate to the Defense Planning Working Group, which was a working group officially recognized by NATO and established by NATO, and it reported to the Defense Planning Committee. That's, I guess, who the mysterious...there wasn't a somebody; there was a committee. This reported to the Defense Planning Committee, and the Defense Planning Committee consisted of the defense ministers or secretaries of defense of all the member countries wearing NATO hats. So, once again, you're dealing directly with the McNamaras, the Dennis Healys, the this ones, the that ones, and so on and so forth. So while we were unofficial, the exercise was official.

COLLINS: Can you give me some indication of what the problems were in determining this baseline of saying, "Here are the forces that we have that are devoted to NATO, and this is part of our cooperative force structure"?

SHUBERT: Starting at the lowest level, as I said before, there were some countries who didn't know how many people they had in their armies, how many divisions they had, how many weapons they had. It's just not something that they had bothered to count. They knew they had an army and they knew they had a navy and they knew they had an air force, but they didn't know how many men, ships, or planes or rifles or broomsticks or whatever it was that they had. That was typical of the Greeks and the Turks, who were much more concerned with fighting each other, of course, than they were with this exercise.

Then when you came to some of the more advanced nations--for example, the French--they felt under no obligation to tell. Why should they tell? That was their business, and they were going to do what they were going to do. They firmly believed what [Charles] de Gaulle believed about international organizations, which is that they're generally soggy and unfit for use, so they weren't publishing anything. As I say, the Germans were very cooperative because they had done their thing and they wanted the world to know it. The Scandinavian countries again didn't have data in a form where it matched anybody else's.

That's another problem. Nobody's reporting form matched anybody else's reporting form, so that just getting a consistent format was a major problem. What's a division? A division is a

mental construct, and it may be 5,000 men in one case and 500 men in another case. So to say that you've got ten divisions doesn't tell you anything. So we had to do all that and put all that together even when they were talking.

So gradually, as the thing progressed, more and more people talked. The French began giving us information, and we did the best we could with the Greeks and the Turks, sort of piecing things together and trying to keep them from garroting each other, which was not easy. They collaborated, when they began to collaborate, because they thought that not to do so would jeopardize the aid that they were getting bilaterally from the United States, as well as through NATO.

There were all sorts of amusing incidents that happened. There was a Turkish general who never came into our offices without kissing the team leader, which caused great embarrassment to everyone. Then one day a Turkish general came in and said, "There's just been a shipment made to the Greeks, and we didn't get any. We can see it. Our agents tell us it's all sitting there on the pier, these big boxes."

I thought, "What is it? What is it?"

He said, "They've got something like fifty crates of something called 'This End Up' and we don't have our share of that." So we got him calmed down.

The international rivalries were really quite intense. That's sort of a comic opera version. The French were much more subtle. Perfidious Albion (the UK) was always on the scene trying to do everybody. It was a rollicking time in which it was good to have your wits about you.

COLLINS: I'm sure this has been explored in the historical literature on NATO and force planning and that sort of thing for this period, but I'd be interested in your assessment of the reason why there was agreement among all the militaries, including the U.S., that a preferred way to go was to rely more on nuclear and less on conventional. What was undergirding that belief for these different participants in NATO?

SHUBERT: I think they shared a common vision of the Soviet horde running across the Central German front in insurmountable numbers and simply overrunning Western Europe in a conventional mode and that they would never be able, with any realistic defense budgets, to develop conventional forces that could contain them. "Therefore, why try? Let's just let go with the nuclears essentially as soon as they break through, which they're bound to do." There was a terrible pessimism there--"We can never take them on." Well, America had it, too, this simplistic vision of the

Russians poised there, ready to leap at a moment's notice, which I just think was very naive and untrue.

For example, there was never really any serious discussion on the difference between the Soviet forces and the Warsaw Pact forces--qualitative differences, organizational differences. What made them think that the Soviets and the Warsaw Pact forces were so powerful? That was all just assumed. Of course, we tried to undermine that with evidence, too. But I think the real reason is that they thought since you can't get there from here--that is, to a conventional capability that will do the job--why try? Use the nuclears instead.

Our answer to that was, "You use the nuclears, and you're not going to have any Western Europe left," and I think, at the margin, that had an impact on their thinking.

COLLINS: What difference in deployment of weapons, roughly, would we be talking about under the different scenarios? Under the one in which one would rely more on conventional forces versus one that may have fit the sentiment in NATO more closely, would it be 20, 30, 40, 50 percent more?

SHUBERT: Of course, when we started out, since nobody knew what anybody was doing, so we didn't know where we were starting from, we couldn't have answered that question. There was tremendous variance from one country to another. As I say, Germany was, after all, right on the line there, and they had an army. (One of my German expert friends said that if anything had happened, they never would have fought, but that's another issue.)

But for the others, it would have meant major increments to their ongoing defense budgets, say between 25 and 50 percent, in order to bring themselves up to what the United States thought was an appropriate level. Of course, it was a contradiction in the United States' position because the United States not only wanted them to spend all that money on conventional forces but then it also wanted to have nuclear weapons in Europe in case even those conventional forces wouldn't do the job. So there's a little bit of a problem with the logic of this whole construct that was being sold to them over there.

COLLINS: So the variable here was not so much nuclear weapons, which would either go up or down depending on which way the policy went, it was conventional forces. Is that a fair way to summarize it?

SHUBERT: I would say so, yes, although you can see how the nuclear is brought into play by saying, "If we have the nuclear, we don't need the conventional," which is what many of the Europeans were saying. As Burt Klein put it once, posing as a European, he said,

"I've been through World War II. I know what that's like. I never want to go through that again. I'll take my chances with a nuclear war." But they didn't know what those chances were, so we tried to help enlighten them.

But the real issue was conventional forces, and that's what, more than any other single thing, drove the French out, besides the fact that they don't believe in multinational commands and that sort of thing. They didn't think that it was realistic to count on any large increments of the sorts that we were concluding were necessary to meet "the requirements."

COLLINS: I'm not sure whether this is true, but thinking over the studies that RAND has been involved with, with the Air Force and the military more generally, when differences have arisen, it was over an end result, a policy choice, whether you go with missiles of this type or bombers of this type. It had a concrete technological end, in a sense. In this case, I think you both agreed on a final goal--a stronger defense for Europe. It seems that the difference was in the strategy and approach for bringing all of the military partners together to make it happen.

SHUBERT: Right.

COLLINS: That strikes me as somewhat different from other cases of differences between RAND studies and the military.

SHUBERT: Yes, that and our MO [*modus operandi*], so to speak. We were there but behaving at least three-quarters of the time as diplomats as opposed to analysts, or at least the team head was. That meant official visits here and official visits there and negotiations on this and that and the other thing, which is rather unusual for RAND analysts to be doing.

COLLINS: Did the NATO experience open up for RAND, in the longer term, new opportunities for research or studies or contacts? How would you characterize the potential impact of the NATO activity? You've highlighted a number of RAND's good people who participated. If one could assess it, what was the longer-term benefit or fallout for RAND?

SHUBERT: I think that the longer-term benefit for RAND was increased confidence on the part of the secretary of defense, and probably succeeding secretaries of defense, in RAND's capabilities to deliver. This didn't reveal itself again in a specifically NATO context although RAND always has some NATO study going on, lo unto the very day we are sitting here, but manifested itself in other ways by calling us in for special tasks, bringing us into the inner circle as a dissenting voice sometimes on the Vietnam War, just built up an aura of trust that I think redounded to RAND's benefit and, I think, probably the country's. But no direct continuation

of the NATO work because when it was institutionalized, then everybody was a government GS-something or FSO-something, and there was really no room for us anymore.

COLLINS: Is there anything else we want to cover in that experience, or shall we move on to your next responsibility after that?

SHUBERT: I think we might as well move on. I don't see at this point any particular additional lesson to be drawn from it, except that RAND was asked to be very flexible, very unorthodox, to deliver high-quality people on relatively short notice, move them abroad, have them function in this quasi-diplomatic status, and RAND was up to that job, which is kind of surprising.

COLLINS: I thought of one additional question. As you're doing an exercise like this, which you indicated was something of a departure in many respects, was the team over there responsible to some part of RAND back in Santa Monica? Were you still responsible to the Economics Department in some way or to Frank? What was the management link between RAND and the group over in Europe?

SHUBERT: The team was responsible to its leader, whether Burt or I, and through its leader to RAND. But as a practical matter, the leader was responsible to McNamara and Finletter. We were right in the chain of command. Now, if we'd done something awful, we would have found out about it, I assume, from Frank or the department. But since you had the head of the department and then the acting head of the department both running the thing, we weren't likely to get into too much trouble with the Economics Department. First, it was Burt running it with me running the department, and then me running it with Burt running the department. We certainly understood each other. There was no problem with that. So, again, the *de facto* bosses were in the government.

COLLINS: Did Frank have any special interest in this activity? It seems like it was certainly an important responsibility for RAND at that time.

SHUBET: Yes, but he really showed no special appreciation of, or interest in it, at least to me. We tried to brief him every once in a while to let him know what we were doing, and he accepted that and encouraged us to continue.

COLLINS: What was your next set of activities, then, when you left the NATO responsibility?

SHUBERT That's when I became involved in Southeast Asia. As I mentioned, Frank was waiting for me to come back from Europe, and dragooned me into taking on responsibility for all of RAND's work in Southeast Asia, which I then tried to expand into a programmatic

concept.

Now remember, RAND did not have programs in those days, and my concept was to put together all the work we were doing in Southeast Asia, plus all the work we were doing on the same substance--revolution, counterrevolution, terrorism, and so on--into a single program. I even tried, but not with great success, to pool all the government money that we were getting to do this work in one single pool. There was ARPA [Advanced Research Projects Agency] money, there was ARPA Agile money, there was Air Force money, and probably some other DOD money, but I don't remember where it came from. I tried to develop the concept of a program where you take all your money, put it in a pot, the program director allocates the resources to what ought to be done, obviously in consultation with the staff and advisors, and you don't worry about these silly little projects. I would say it worked out about halfway.

I think one fundamental problem with the concept is that it's probably illegal. [Laughter] And sooner or later there was concern that the auditors would catch up with it and I'd be ridden out of town on a rail or something like that. So that partly accounts for the constraints on my ability to do that.

So I became involved with that and, as I say, the first thing was the Viet Cong Motivation and Morale Study.

COLLINS: Just one point that came to mind as you were relating that story before. If Frank had a basic kind of confidence in Gouré, why did he come to perceive it as a problem?

SHUBERT: He never explained it to me. I think he must have known, at least intuitively, that the work was very bad, and I think that he wanted it to look better, and he may even have wanted it to look better than it was. So he may have been calling me in as somebody who would make it cosmetically more acceptable but not change the message. Unfortunately...well, fortunately for RAND, I think, he got the right man, or the wrong man, the wrong man for the cosmetic job because there was no way I was going to let that continue.

It was a battle, I tell you, that was waged over a couple of years. I started this in '65, and it wasn't until '67 that we were able to shut the whole thing down. I spent a lot of time in Vietnam. I wanted to be sure that I wasn't being unfair, so I actually went out into the field and went along on interviews, saw how the interviewees were selected. It was worse than you could even dream, so I knew I wasn't wrong. Something had to be done. You were getting a totally biased input just from the very beginning, and then that wasn't all that was wrong. Speeches were made which said, "I have drawn the following random excerpts from interviews." How do you draw a random excerpt, and how come they all come out saying the same thing? It's obvious they're not

random or excerpts, and I don't think Leon understood. Not to impugn his honesty, I'm not sure he understood what "random" means. But I tell you, he got a lot of plaudits from the Air Force and from the DOD when he started emitting those positive signals during the darkest days of the war.

But the best work that was done was done, as I say, by someone who was not with the project at the beginning, joined the project as an analyst, never went to Vietnam, and wrote a paper which took the inputs and the data and simply interpreted them in exactly the opposite way from the way that Leon had been doing, and said, "What these data show is that you have an enemy who, no matter what you do to him, is not going to quit." That case was then documented using the same materials, which were used to show the opposite by some of the other people on the project.

So there was some really quite good work done by people like Konrad Kellen and others that caught a lot of attention, got a lot of people's attention in Washington.

COLLINS: I have a couple of other questions along this line, but I'm wondering whether we might save them until we get together in about a week or so and pursue this a little bit more and then follow through on some of the other activities.

SHUBERT: Fine.