Rumph, Ben. Date: July 23 and July 31, 1987. Interviewers: Martin Collins and Joseph Tatarewicz. Auspices: RAND. Length: 4.75 hrs.; 61 pp. Use restriction: Permission required for access.

After reviewing his upbringing, undergraduate education in aeronautical engineering, graduate education in aerodynamics, and work on various aerodynamic projects for Curtiss-Wright from 1940 to 1949, Rumph (b. 1912) describes how he was recruited by Gene Root to the Aircraft Department of RAND in 1949 and his initial impressions of RAND. He then discusses working on the refueling project, the interdisciplinary approach on projects, participating on the management committee, interaction between the social scientists and other scientists, and personnel evaluation and management policies.

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Interviewee: Ben Rumph

Interviewers: Joe Tatarewicz and Martin Collins

Location: RAND Corporation Offices
Santa Monica, California

Date: July 23, 1987

TAPE 1, SIDE 1

Mr. Collins: As a preliminary, let's just quickly sketch out a bit of biographical and educational information. If you could just tell us where and when you were born, and then outline your educational training.

Mr. Rumph: Okay. I was born in a town in central Georgia, Marshallville, in 1912. It was a small town and therefore the school was somewhat limited for what I ultimately wanted to do. This is why I determined for myself what I wanted to do at an early age, like the second year in high school, and this happened to be Georgia Tech that I wanted to go to to get my aeronautical education. I had to go to the school and interview them as to requirements so that I could prepare myself for entry. It turns out that I had to get my high school to offer me several private courses in math, in order to meet all the entrance requirements. I proceeded at 17 to Georgia Tech and got my bachelor's degree and my master's degree there.

One of the professors there was a Caltech graduate, and at the time I had contemplated going on to get a PhD. He recommended that I go to Caltech. I then toyed with an interesting thing. I applied to MIT and to Caltech and asked them about support, scholarships, fellowships, or what have you. That was simply because I didn't want my father to have to support me in any more graduate education. I considered them at the time sort of of equal quality, so the cost-effectiveness was simply who could offer me the best financial deal, and that's the one that won. MIT offered me just a plain scholarship which pays only the tuition, in those days, and that wasn't sufficient for me to live on. Caltech offered me a fellowship which not only covered the tuition, enough work to cover the tuition, but also to support me during my remaining college education work. So that's how I chose Caltech, strictly on a cost-effectiveness basis.

Dr. Tatarewicz: How did you choose aeronautical engineering when you were in high school? You seemed pretty firmly set on that.

Rumph: That was interesting because that was in the late twenties. Where I lived was a training field that was used for training for World War I. Towards the end of World War I, I was five or six years old. A lot of memories of that period have faded, but I do have memories of the Jennies which were flying over my house. I'd be in the yard, and Jennies as you know were flying low about 50 miles an hour. The pilots would always have their arms out this way, and they would see me there, and wave as they'd go by. And somehow even as a very small kid I got attracted to aviation. Now I can't state that that's what caused me to later want to go in that direction, but I'm giving you almost a prenatal reason.

Another thing was that my father was a farmer. He had planned to be a lawyer, but his father persuaded him to take over his farms, and he did. And he said, "Unless you want to, I don't want you to be a farmer." So he literally tried to turn me away from it, because he always regretted that he didn't get to be the lawyer that he wanted to be, and he gently says, "Do what you want to do. Don't necessarily think about farming." And I had an abhorrence for farming.

So those are two reasons why I did. I had no idea what the final capstone of my career would be, in this building and so on. I obviously had no knowledge of what that would be. Aviation was relatively new then, and I always have been attracted to things new, is another reason. I wanted to be in aeronautics at the time. This was long before the space age. So that's why I just made that firm decision early on, and I never veered from it until I got my education.

Now as I said I got my bachelor's and master's degree at Georgia Tech, and then I went to Caltech for a PhD. In working in my fellowship role I was ultimately, after the first year there, put in charge of running what they called the ten-foot wind tunnel, which was used not so much for research at the time but was used by all of the aviation companies out on the west coast, a number of them here, Boeing in Seattle, and even some of the midwest companies would do their testing at Caltech. There were not many of those wind tunnels other than at Langley Field in those days. The companies did not have their private wind tunnels as a number of them have later developed. So under contract sort of they would lease out, in effect, wind tunnel facilities, and I was in charge of running it.

I did complete all of my graduate work for a PhD, except my dissertation and German. I passed my French exam but I'd never taken any German, and I had gotten a little lackadaisical about it, and said, "I'll study for six weeks to pass that," and I couldn't do it. But anyway that's an aside. I was so engrossed in the work I was doing that I really only completed my course-work. I created a rationale for myself that said, "I will not have the ticket but so I have the knowledge, and that will be enough for me, and I'll go on my own at that point." And I never really regretted that I didn't finally finish it, but that's the occasion for it. I became engrossed--I worked almost entirely in running this wind tunnel for the last year there. It also was a tremendous entree into the industry I wanted to go into, because I became acquainted with all the people in the industry that were worth knowing from my point of view, say, in what I was going to do.

Tatarewicz: Did you have any sense of what specifically in aeronautical engineering you might want to do, or what types of things?

Rumph: Yes. Well, of course I wanted to be well-rounded in all aspects of aviation, building and design, not the airline operation, but the manufacturing companies. However, the most interesting phase of it to me personally was the technological subject of it more than others, not the management but the aerodynamics. See the wind tunnel is essentially aerodynamics, and I had great experience with that, and so I had qualifications that would put me in a better position in that particular part of aeronautics than any other, although Caltech had a very strong, what they called structures department, all of the structural aspects of it. But aerodynamics was my ambition at the time, at least to start with that.

And then I was now up to about the time that the Spanish War was going on, where you remember the German armies got a lot of training, just prior to World War II. So in late 1939 when World War II began in Europe, I decided to want to look around and go into industry somewhere, at least during the war, to build something. Okay. So I scouted around and the best offer I could get was in St. Louis, it turned out, at Curtiss-Wright. I went to St. Louis there. Hence I left Caltech in 1940.

Collins: Before we discuss Curtiss-Wright, I'd like to go back to Georgia Tech for just a second.

Rumph: Okay.

Collins: Were you interested in aerodynamics at that time?

Rumph: Not so urgently as--I tried to describe--later when I was out at Caltech, because of the wind tunnel aspect. I was very captivated by the design aspect, and the design aspect at that time was essentially biplanes and so on. So, no, I did not have that single-minded purpose of a specialty within aeronautics at Georgia Tech. That developed only at Caltech as an entree into a job.

Collins: Did you have to do a master's thesis at Georgia Tech?

Rumph: Yes, I did that.

Collins: Do you recall what the topic of your thesis was?

Rumph: Yes, it was on some experimental testing of a cyclogyro design. I got captivated with the cyclogyro because it was so different from other aviation devices. Do you know what a cyclogyro is?

Collins: Don't know what a cyclogyro is, no.

Rumph: Okay. It has a rectangular wing panel, and there are four or five of them around an axle, and they articulate. It's somewhat like a helicopter except that the cyclogyro blades operate this way. As they rotate, the blade advancing towards the airstream, increases its angle of attack. The retreating blade from the airstream reverses its angle of attack, thus providing lift in the same direction. The only cyclogyros that were ever built, to my knowledge, were built in Germany, and that's where I became intrigued with it, by looking through the German literature. It turns out it was a worthless thing but it was fun. And somewhere I've got a thesis report on the subject.

Collins: When you went to Caltech, can you recall who some of your instructors were?

Rumph: Robert Millikan was president at the time. Some of my instructors were: Clarke Millikan (Robert Millikan's son) was a professor in the aeronautics department there. Theodore von Karman from Germany, you've probably heard of that name. He was the head of the aeronautics department at the time. Ernie Sechler headed the structures department. Maj Kline. I can't even think of his given name. I don't know where he got the name Maj from either. He was a Caltech graduate and he was a consultant to Douglas, but he spent some time teaching design courses at Caltech. Now Arthur Raymond, whom you've interviewed here, was

one, but in a curious kind of way. He and a number of other Douglas employees came over to Caltech once or twice a week to teach design courses, application of design, from their experience with it. Arthur Raymond was one of those. That's where I first met Arthur Raymond. There were several other Douglas people, too. Now Bill Sears, too. The aeronautics department only had a graduate school. And there were about 30--not more than 40 anyway--students in the graduate school.

Collins: You must have taken courses from von Karman, then.

Rumph: Oh, yes. And he was a very interesting fellow. He twisted my arm so hard one time, I almost took a job down in Tellahoma, Tennessee. You know, there's a research organization of the Air Force there. Karman and I were very close for a while there, and I took a number of courses from him, and I have quite a few anecdotes I can tell about him. To me the most famous one is, he was so absent-minded, he got in his car one day and forgot to pull his arm in. He pulled the door closed and broke his arm.

Collins: That's absent-minded. [Laughter]

Rumph: That was his story anyway, and I know he had a broken arm, see. I didn't see him do it.

Collins: Well, was von Karman then instrumental in shifting your interest towards aerodynamics?

Rumph: Yes. I never thought of it that way, but really I instantly recognize it when you said that, that's right. Because while he was a specialist in a lot of things, you know, his greatest love I think was the aerodynamic field, and the strongest element of the Caltech professorship there. Clark Millikan's influence on me was very strong also. So that was another reason why I think I went in that direction. I gave a pragmatic reason earlier, that it gave me a better entree into a company where I wanted to go.

Tatarewicz: Aerodynamics, especially at that time, the theoretical and the practical aspects of it were separated by a fair gulf, in the sense that the theoretical side of it was so enormously complex mathematically and difficult to solve, people doing research in that end of it; and then you seemed to have a whole separate other community of people doing the approximations and the kinds of mathematics that one has to just live with, even though it's not perfect, in order to apply the principles to airfoil design.

Rumph: Well, yes.

Tatarewicz: Did you ever consider the theoretical or the practical sides?

Rumph: Oh, yes, and I took a variety of advanced math courses just to deal with that. Karman was doing a great deal of theoretical research during this time period. He was active in this in Germany before he came to this country. And the story goes that he did his best work on research at night, at home. He was never married, and he lived with his sister in Pasadena. The pharmacist near Caltech told a story, he says he knew "that Karman was working on an idea he was trying to get developed, because he would call sometimes at night, at midnight, and order a fifth of scotch and a ream of paper. And that meant that Karman was good for the rest of the night, working on something."

Collins: I'm not sure you've had a chance to fully respond to Joe's question.

Rumph: The theoretical, I haven't yet. I guess I didn't quite see this absolute difference that you're talking about between the theory of aerodynamics and the practice of it. Now, let's take the theory of aerodynamics, take what's called vortex theory and so on. Okay, it's very complicated mathematically to express, but the application of it is immediately seeable. You don't have to use complicated mathematics to use it; it is complicated only to explore the theory. Once you can establish the theory, then, the theory has some applications. It's not an unproven theory, if you like, if you've got enough theory behind it. Although we had this wind tunnel, we could do some experimental work. We had some small wind tunnels that could do all sorts of things, like trying to test theories of turbulence, the turbulence that goes within the boundary layers on bodies. Laminar airflow, turbulent flow, types of theories.

So I didn't make such a big chasm in my mind between the theory of aerodynamics and the application of it, as you inferred. Yes, I was never a theoretician like Karman. I could appreciate and understand what he could elucidate for you, but I could not ever be the originator of that kind of thinking. So I was never a theoretician in the sense of proving the theory. After it's proven, I can see the result. So I actually thought of my work as application of aerodynamics in aircraft. There were designs you could create that the pressure distribution would be such, you'd have more laminal layer than you'd have turbulent flow, and that would reduce the drag on the system, the resistance.

But on the other hand, it had to operate in a dirty world and get bees and bugs and dust and what have you, particles, all over these things, and so soon they found out that the notion of applying the theory of laminar flow to actual use in airplanes was a pretty remote thing. There were wings that looked like they ought to be laminar flow wings in a cross-section. But when you'd test the body as a whole, and especially when you'd get to the full-scale airplane, even though it's higher Reynolds number, as they say, the laminar flow would get disturbed by the dirt. The mechanics, you know, the grease monkeys or what have you, they'd walk around on the wings, and they can't afford in the airline business or in the military even to clean them off so well. In other words you're not living in a bubble, as this kid was who was born with no immunity and had to live in the bubble for his life.

Tatarewicz: You've answered my question very well. I could have rephrased it, as to whether you ever considered going into theory and working in that realm.

Rumph: I knew that was what you were just about to ask, and so, no, I didn't. I had the opportunity. I could have. [I had a roommate who stayed with this. Name is Homer Joe Stewart.]

Tatarewicz: Oh, Homer Joe Stewart was your roommate? How interesting.

Rumph: He was my roommate for a while, till he got married. You know him then? He did stay at Caltech. He became a professor there. I haven't even seen him in a long time. I don't know how he is now, but I think they're still living in Altadena. He did spend his time between JPL--Jet Propulsion Lab--and Caltech.

Collins: Did you begin work on a dissertation when you were there?

Rumph: No, I never even got to choose a dissertation subject. No. I didn't.

Collins: Did you have an advisor there nonetheless, someone who guided you through the selection of courses and your general program?

Rumph: No. There were no specific advisors I remember, assigned this way, except when you got to a dissertation point. There was some latitude, but essentially what it amounted to, every course was a course that could be a component leading to a PhD or a

master's degree.

Collins: Just very quickly, who were some of your other classmates? Can you recall any others?

Rumph: Yes. Bob Schairer--and the name Schairer is in aviation quite a bit, because there are two brothers, George Schairer and Robert Schairer. I first met George Schairer, Bob's older brother, when he was working down in San Diego at what was called Convair at the time. It's General Dynamics down there now. And they would do tunnel testing at Caltech. He later went to Boeing and he stayed in Boeing till he retired. He'd come down often testing for Boeing, you see, and so I knew him well. But I also was in the class with his brother, Bob Schairer, who later went to work for Douglas and then worked at RAND briefly, and then left to go to Lockheed. Another classmate named George Mellinger who later became the director of flight test work at what was North American then and now Rockwell. Another, I can't think of his given name, a man named Malina.

Tatarewicz: Frank?

Rumph: Frank Malina. He was a rocket man, it turns out. And he and Karman helped put together the Aerojet Company out here east of Pasadena. Bill Sears I remember, but he had finished most of his work and was sort of an assistant to Millikan and Karman. Oh, there's another German fellow whose name I can't recall right now. And there was a Chinese fellow named Chen, who got into all sorts of clearance questions. He went back to China, you know.

Tatarewicz: Chu Chin or something like that.

Rumph: Was it Chen or Chin? I don't remember.

Tatarewicz: I think his first name was Chin Su or something like that, but he's the very famous theoretician.

Rumph: Yes, mathematician, theoretician, had to leave. He was there at the time in the class. There was a Japanese fellow there. I can't remember his name. I don't know whatever happened to him. Of course the Japanese were not involved in the war at that time. See, I was there in the period 1936 to 1940. So, and as I said, in 1940 the war was heating up in Europe and that is when I left.

Collins: Did you indicate your position was supervisor of the wind tunnel; is that how you phrased it?

Rumph: I didn't use the word "supervisor." I said I ran the tunnel. I was head of running the tunnel, in effect. There were two or three technicians who were not students, that were there to maintain the facilities. But I was in charge of negotiating with the companies for the job they wanted to do, planning the work to be done, arranging for the other graduate students, who were also on fellowships, to be on hand for work in reducing the data that was taken, to put it in a form that finally Clark Millikan could write the report.

Collins: Who did you report to?

Rumph: I reported to Clark Millikan. Karman was not in that operation at all. I guess he didn't really want to. That was a commercial operation. He wasn't particularly interested in it. And some people criticized it because it was owned by Caltech, but it was used when I was there almost 100 percent by these companies. Some research project briefly might go into it when there was an idle time, but the companies took prime time. This was just prewar and there was very furious activity going on in the military field. Oh, things like the old twin-boom P-38, if you can remember any of that from World War II. Kelly Johnson was over developing that at the time, I remember, at Caltech, is where he did it. So it was sort of a commercial operation even though it was criticized somewhat at Caltech for that, and it earned quite a lot for the college. Now, it was a Guggenheim school, but the Guggenheim did not give continuing endowments. They gave the facility. There were five of those Guggenheim schools all over the country. Georgia Tech, where I first was, was one.

Tatarewicz: How did you learn the techniques of managing an operation like that? I mean you've got companies coming at you, and I'm sure each one of them wanted first priority.

Rumph: Well, that is partly on-the-job training. Partly Bill Sears. Now I mentioned Bill Sears. Before I came and the first year I was there--I was one of those that worked with the tunnel crew but was not supervising any of it--this was Bill Sears. But he wanted out from under that, so that's why about the second year I was there--I had observed its operation for a year, and of course Clark Millikan was in and out a couple of times a day and would write the reports and so on. He would look over what was happening. So, he would give indirect supervision. And it was by osmosis. There was no formal training for it. Now I left, I said, in late summer or August, 1940, to go back to Curtiss-Wright.

Collins: The companies that came in to use it, were they charged on an hourly basis?

Rumph: Yes. That's right. All the staff to do whatever was needed for the testing and analysis was paid hourly (Graduate Students) including the fact that there was a shop down on the first floor. The tunnel covered three floors. The shop would get their models ready. The companies would bring them in, in a truck, and they would have to have fittings put in to adapt to the tunnel. And those were costs, just the cost of operating that shop, doing that work, was charged for by the hour. The wind tunnel time was charged to them, whether it was running or not. If it was tied up, it was charged to them, if the model was in there and nothing else could be done.

Collins: And your contacts with these various industries were primarily through the designers?

Rumph: That's right, and not so much the designers because--again it had to do with my aerodynamics bent--most of the people that came with those models as representatives of the companies were from the aerodynamics department, because the aerodynamic phenomena produced the airplane behavior characteristics, things that had to do with the interaction of air and the model. Of course the models, they were static models. They didn't simulate any dynamics.

So the people were usually aerodynamicists that came, and by and large they were usually the heads of the aerodynamics departments in the various companies. George Schairer was one. Another one whose name may be familiar was Gene Root. Gene Root, I didn't mention. He had been at Caltech but he had left I think about two years before I got there, but I pretty soon got acquainted. That's where I first got acquainted with him. He'd come over with his models. He was at the Santa Monica plant for a while, and then he went over to the El Segundo factory. Kelly Johnson, while he was a designer of the whole aircraft at Lockheed, he was also an expert aerodynamicist, too, so he served both roles somewhat there. But he was the man that usually came to represent the company. A man named William Hawkins was his replacement and later became a vice president of Lockheed, but he was a junior man then but took over some of the testing. So they were either designer-aerodynamicists or aerodynamicists, but there were some features of aerodynamics in all of those who came.

Collins: Just one final question on the wind tunnel. Can you recall which corporations were the principal users of this serv-

ice?

Rumph: Well, let's see. There was Douglas--Douglas El Segundo and Douglas Santa Monica. I say that because the El Segundo factory mostly was a Navy operation. Most of these are all military aircraft I'm talking about. So that's two of them, even though both are Douglas. There was a Vultee Company at that time. It's long since disappeared. There was Convair; it was called Consolidated I think at the time. Anyway its General Dynamics now, down at the San Diego branch.

Tatarewicz: Convair came from Consolidated Vultee Air, when they merged everything together.

Rumph: Yes, "V" is the only thing that survived. They had started the Fort Worth plant at that time, I believe, and so there was some Fort Worth, that was from Texas, here. There was Howard Hughes. One of his racers was in there. Now that was only because the man who was the chief aerodynamicist and it turned out was also a meteorologist, named Rockefeller, was also consulting with and helping Howard Hughes design one of the racers he had, and that was the only reason. Howard Hughes never showed up in the place at all. It was just this fellow Rockefeller, who had been a student at Caltech also earlier than I was there. Then there was Boeing in Seattle. There was occasionally Curtiss from St. Louis, not Curtiss in Buffalo though, because the Buffalo factory had a wind tunnel of its own. Even one time Grumman from Long Island came out for some test work that I remember. But you see, by and large, most of them were located generally on the West Coast.

Collins: Why don't we move on then to your change to Curtiss-Wright.

TAPE 1, SIDE 2

Tatarewicz: We're very interested in documenting the networks of relationship and communication among all these various individuals and these companies. This is very useful.

Rumph: Well, that's mostly what I've been talking about but it's pre-RAND.

Tatarewicz: Oh, yes, but this forms part of the context that's important to understanding RAND.

Rumph: Well, okay, now. To illustrate why I was interested

oftentimes in very curious things, I already told you about my master's thesis. One of the attractions that took me to St. Louis, and I think I realized that St. Louis wasn't going to be a long-time employment actually, but they came out with a design that the military had bought as an experimental project, that was a tailless airplane. Now there's another unconventional thing that intrigued me. It was controlled by a swept-back wing which had rudders, vertical fins, at the tips. It's not a flying wing. It had a regular fusillage body, a pusher engine in the back, and the horizontal control was a free-floating device that we called an elevon, not an elevator. We had ailerons out on the wing tips as normal airplanes do.

The free-floating elevon had its axis of rotation near its center of pressure, so there was not much load for the pilot to control. The center of pressure was back just enough to make it stable and give a little load for the pilot to control, but otherwise it was free-floating. If you took your hand off the stick, there was tremendous stability in the airplane. You put your hand on the stick and give it some rigidity, a lot of stability disappeared, but maneuverability was enhanced. You were maneuvering when you had your hand on the stick, in effect. So it was pronounced highly maneuverable because you transferred from stability to less stability, hence it was easier to maneuver if you had less stability. I just cite that because that's just another one of the nonconventional ideas that struck me so much in life.

Collins: Was this the project that the chief designer asked you to come to Curtiss to work on?

Rumph: No, he asked me to come and be the chief of the aerodynamics department. They had a number of other projects going on, but the thing that attracted me most was this design. I just sort of followed it to St. Louis. But what he offered was a job as head of the aerodynamics department. So I never worked in the industry except in a supervisory position. I didn't climb up through the organizational structure.

Tatarewicz: That's highly unusual.

Rumph: Now I do not remember who it was prior to my coming there, and I'm a little dim now on this. I did not displace anyone that I know of, because everyone that I worked with had been in the department. And I can't remember whether someone had left and disappeared, and I happened to come up on the scene at the moment. But I realized I had a problem of making friends and influencing people, when I could just come from the outside and

step into this place. So that's the first big experience I had in developing managerial skills.

They were short on people. They needed several. So what does a manager do? He brings in some people that he knows somewhere else. So I brought either two or three students, graduate students from Caltech. After I got there, I brought them. I didn't know the situation at first. It wasn't pre-arranged. But I then brought them to bolster the department. Well, that gave me a little more confidence, because it was someone I knew and someone that I knew how they'd been trained, etc.

Collins: Who did you bring, by the way?

Rumph: Well, there was a man named Fredrick, one of them, J.L. Fredrick. The other one was named Tangren. I can't remember his given name right now. There was another one I toyed with, but he was so erratic in his behavior about technical matters that I didn't want him there really. But I thought about him, so I won't mention that at all.

There were several airplanes that had already been designed, and were being built that needed aerodynamic attention and modifications every once in a while, so that I had to get involved, but not a great deal. They were developing a training airplane which was a conventional training airplane, which was designed without even any wind tunnel testing. It never was wind tunnel tested, and it worked out as a trainer pretty successfully.

The tailless airplane had to have very extensive wind tunnel testing because it was so different from the customary. We decided that California was too far away, and we didn't have a tunnel available to us, so we went to MIT. So finally I got to MIT, but not as a student. And I went there to supervise the model tests.

The project did develop into a full scale flying model. It was a steel tube, a doped cloth covering type construction, a cheap kind of full scale, flying model. So next I had to come out to what is now Edwards Air Force Base, but then was called Muroc Air Force Base. Edwards was named for a Major [Glen W.] Edwards, who was killed in another flying wing accident, the Northrop flying wing, you may remember. We went out there to the desert, where there is a 20-mile runway in many directions; you can land most anywhere. Further development was done in flight on this craft.

Tatarewicz: Was this a full scale?

Rumph: It was full-scale. It was intended to be ultimately a fighter plane. So it was not a big bomber or something like that, like the flying wing was. Oh, it was a thing that had a wingspan of about 25 to 40 feet and the same length fuselage, and one pilot, one person. We got further developments on it out there, until we thought we had enough under control that they could begin building an experimental craft, aluminum regular monocoque structure type of airplane, and we did. We built either two or three experimental planes. I'm trying to think. It was tested not at Edwards. It was tested at a field across the Mississippi River from the the Curtiss plant in St. Louis. I can't remember the name of the airfield. It may have been a military airfield where the testing was done. A lot of testing went on there.

And finally the ultimate happened, that people worried about, because we had enough wind tunnel tests to know that it could, but we didn't know whether at full-scale it would be better. The pilot was doing some acrobatics with it, and it was at a high enough altitude you couldn't see visually what was going on, but he reported this later, that he somehow got it into a very bad stall and he could not get it out of the stall. So what happened, it turns out that it went into a stall and it went over on its back. And it was so stable on its back, that's when he was trying to get it out, he couldn't pull it out. That elevon, that thing up front, was not powerful enough to pull it, because there was no forward speed. It was standing still, sinking. He couldn't get it out, so he finally, when he got close enough to the ground, he released his canopy. Being upside down he let go of his seat belt and he just fell out, parachuted down, and the airplane came down and literally hit the earth at absolutely 90 degrees flat. It was not broken up. It was all there, except it had no thickness anymore. Just squashed flat that way.

Tatarewicz: Still upside down.

Rumph: Still upside down.

Tatarewicz: Still completely stable through the whole jump.

Rumph: Completely stable throughout.

Collins: Let me just interject here. You mentioned this evaluation of designs. Did you work with NACA during this period? I mean that's the kind of activity ....

Rumph: Only by consultation with them, not by testing. That was the beginning of the downfall of that particular idea. The military flew it and tested it some, but it didn't go anywhere. The one thing that really killed it, and I even have trepidation today with all the jet airplanes, and that is, a swept-back wing. We learned, inherently when it begins to stall, it stalls at the wing tips first and progresses inward. Now imagine what happens to the stability, if the wing is giving stability and it loses lift at the tip. That makes the center of pressure move way forward, gets in front of the CG [center of gravity], and it's unstable now, see. And every swept-back wing jet that's flying in the airlines today has that same problem. The reason it's saved is, it's got a stabilizing tail behind it. This craft didn't have.

Tatarewicz: A horizontal stabilizing tail.

Rumph: Horizontal stabilizing tail behind it saves the jets today. The flying wing's the same way. The Northrop flying wing. Now I'm going to tell the story of Edwards. He was a major and flying one of those, I forget which one it was, out at Edwards. I was out there on another project at this time and had come back from some place, had breakfast off the base, and as we were driving back to the base, we saw an intermittent highlight in the sky. Just an intermittent highlight. All we could see. We couldn't tell what it was. And finally we could see. We could see that it was this flying wing and it was falling like a leaf. It just was turning over and over and over, like a leaf, and it never stopped. It went all the way to the ground, and we watched it, and we saw the smoke and explosion come from it. We were about eight or nine miles away, but we reached there as fast as we could. We got there almost as soon as the fire trucks did, from the base. And Major Edwards was killed in that accident, and that's why it's named Edwards Air Force Base.

Time moved on. There was another fighter plane we were developing that was a larger thing, a conventional airplane. Actually I was out at Edwards for the test on this one. It started into production, but by now we were in 1944 or so and it was getting late in the European war. It was completed but it never went into production. The war came to an end by the time it would have gotten into production. So that was about the time I could see, while I had not been in St. Louis all the time.... As the war began to grind down Curtiss-Wright had three plants. It had a St. Louis plant and a Buffalo plant and a Columbus, Ohio, plant, and I worked in all three of them, and I moved to all three cities.

Tatarewicz: In this short few years?

Rumph: Ten years really with the Curtiss Corporation. I went there in 1940 and by 1945, before V-J Day but after V-E Day in World War II, I was moved to Buffalo. Now up until this time I've spoken of "I" and so on, and I was single up until this time, but in 1944 I found the girl in St. Louis that I married. We had our wedding all set, but before we were married I had to go to Buffalo. V-J Day came while I was in Buffalo and pretty soon thereafter I went down back to St. Louis. But before I returned to St. Louis I bought a house for us near Buffalo. And then I quickly got in touch with her, "Would you come up now to see this, because you're going to have to live in it." So her mother sent her up and she looked it over, and it was, thank goodness, very good for her, and later in August we got married.

By December of that year, the war being all over then, they moved us from Buffalo to Columbus, Ohio. We had to leave there after there'd been 75 inches of snow accumulated in Buffalo on the ground, and you can imagine what the roads were when the plows throw all that stuff up. It's almost half tunnel. And a friend of ours and his family and we left Buffalo together. We sold this house and we had to leave with a blizzard going on and get out to get to Columbus. The two families followed each other until we got out of it. As soon as you move 50 miles south out of Buffalo, you're out of this bad snow period. It's only what falls from Lake Erie when the weather moves from the lake over onto the city.

Anyway we went to Columbus, and I worked at Curtiss Columbus until 1949. This was '45, so there's four years at Curtiss in Columbus. And they were working on some postwar things then. They were working on some plans for commercial airplanes. But I could see Curtiss was going to fade out of the picture, as far as aviation was concerned. Now Curtiss Corporation had engines. They made Wright engines. They made propellers and they made aircraft. But the aircraft business was going to fade out, I could see.

Tatarewicz: Why? Was that specifically for Curtiss or is this the general postwar slump?

Rumph: No. Well, no, I'm speaking specifically of Curtiss, but there were others, too, that phased out, but not all. Curtiss had acquired a great deal of assets, and I often wondered, out of these military contracts they'd had, they put the profits aside, and they had a lot of capital, none in facilities. They'd had facilities even in Buffalo, and the government bought those

facilities during the war. Curtiss put the assets aside, and they ran the facilities while the government owned them. The same thing was true in Columbus. It was a government facility. The same thing was true in St. Louis. It was sold to the government and then the government built an expanded facility during the war. Curtiss amassed 175 million dollars and they were going out of business. They did keep the Wright engine company for a while, and I don't know where it stands now. I'm not sure whether Curtiss has any engines or not. I don't know. I never worked with the engine people. Aviation does not need many propellers anymore anyway. Therefore that was when I began to think about moving. So I put out my feelers again, and this time it was very interesting because it changed my career direction.

Collins: This would be in 1949.

Rumph: Well, I did this in late '48 and earlier '49, till I'd get settled.

Tatarewicz: I'm curious about how your managerial responsibilities with Curtiss evolved over this period. I mean you came to a department that had aerodynamicists in it and brought some Caltech students in. If you could just tell us a little bit about how that evolved.

Rumph: How that evolved is--I'll go back to St. Louis. I don't remember exactly, but during the war period there, I built the aerodynamics department up to a group of about 15, 20 people.

Collins: Let me just ask quickly, was this the number you started with say in 1940 or did this increase during the period of the war?

Rumph: No, it was only about five to eight or something like that, so it a little more than doubled. Now this included some people who were engineering flight-test people. With all of this development going on, I also had aerodynamics and engineering flight tests, not the piloting, not the mechanics that would work on it. They were from the factory, but what we called engineering, the engineers who plan and organize the flight-test work to be done. So this 20 involved not only aerodynamicists but engineers to plan and manage the flight-test work. It was all in my same department.

And along the way, backing up a little, one airplane we were going to do then was when we thought we were going to be short on metals during the war. Howard Hughes built the big Spruce Goose, you know. Curtiss built a wooden airplane also during the war,

but smaller. It was a cargo airplane with a box side, box fuselage, but essentially all wood except the engines, I think. what all the structural parts were--wood and plywood and so on. The only comment I want to make about it, because I flew in it during some of the test flying: you'd have it flying fine; it was all trim and stable and steady and so on, and you'd go into a cloud. Humidity changed, whoops. It all went out of trim. In other words you had to quickly turn all the cranks to regain control again. Just warping of the wood. I don't mean it's so out of control that you couldn't fly it, but I mean it wasn't in trim anymore. It would either start to dive or climb or roll or yaw or do something, with the warping that was taking place.

Now there was not enough experience with these wooden machines during the war to know whether anybody ever would find a way to solve that wood warping problem. I don't know. The Spruce Goose never knew it had it, because it only got off the water about two feet and back down again. But this flew enough and I flew in it, and this was sort of a sideline of that, the wood. We didn't find out how to do it. And incidentally most of the wood, after we designed the frames and all, who made the wood for us? A piano company made it.

All right. So the organization--I had this group. And now when I was sent up to Buffalo, the St. Louis engineering was being disbanded. Now some people left at that point and some went on to Buffalo, see, with me. Now when I reached Buffalo there was another man in charge there, and I was number two man in Buffalo when I went there. I didn't stay in Buffalo but six months, you remember, I said earlier. I got there somehow in the summer and then by the winter we'd left.

Collins: What would your title have been there in Buffalo then?

Rumph: Associate head of aerodynamics or something like that and flight test. They had them combined, too.

Tatarewicz: Was it a larger group than you had had in St. Louis?

Rumph: Yes, the Buffalo plant was a much larger plant during the war than it was in St. Louis, yes. But then they closed the Buffalo plant down in December of '45, and then not many of the people from Buffalo migrated to Columbus. I don't know where they went. I lost track of them. But some did. I did.

And then I had a difficult problem. There was a man there who was the chief of aerodynamics and flight tests, but the man who came down from Buffalo to be the director of engineering

wanted me to be that head, and so then there was a problem. What do you do? Well, it turned out that they managed to do it by taking the chief that was there and making him an assistant to the chief engineer or something like that. And I was then the head of aerodynamics and flight tests for the next about four years, from '45 to '49. It was a group then, about the 20 that I had originally. So when I was in command, I only dealt with a group of about that size throughout this career of ten years in the aviation industry. You said evolved. It sort of evolved; it evaporated; exploded and evolved again, in a sense.

Collins: Were your activities essentially the same at each of these plants?

Rumph: Generally.

Collins: Is it just that each of the plants was manufacturing a different type of airplane?

Rumph: Well, that makes a difference, but I don't call that a basic difference. It was the same kind of activity, but with different airplanes is all.

Collins: One final question about organization. You mentioned that the aerodynamics department came under a chief engineer.

Rumph: Yes.

Collins: What other departments fell under the auspices of the chief engineer?

Rumph: Well, there was another group that was called the power plant group. Now the power plant group mainly meant only installation of power plants into airplanes. In other words they didn't build any engines. It wasn't a power plant group in the terms of designing power plants. It was adapting power plants to airplanes and selecting power plants from either the Curtiss-Wright Company or Pratt and Whitney or whatever, see. There was an electronics department which had to do with, in those days, radio, navigation gear, things of that nature that were equipment in the airplane. There was a design department which did the physical design of parts and layout of airplane, where their information came from all these others, because they had to get the engines fitted in. The design department was by far the largest department, because it's very intricate, lots of things to be done there. And we didn't have computer design at that time. So we had pieces of paper on a drafting board, etc., including the fact that they usually made these things called

full-scale mockups. They wouldn't trust the things; they have three-dimensional layouts on paper. They had to have three-dimensional mockups to make sure things would fit and so on. Then there was a structures department that is a stress analysis operation. There may have been another department, may have called in when there was a large operation going on, a liaison department, sort of the engineering liaison with the shop, building stuff, and so on. They were the principal other departments that made up an engineering department, and then there was a chief engineer.

Collins: Among those departments probably the one you had the most interaction with was the design group.

Rumph: Yes. Design and power plant. Not the electronics because, in other words, they could take care of the navigation and radio control without the need of aerodynamics. The only time it ever got in was if they wanted to install some kind of awkward antenna or something on the outside, they'd have to consult with us. But largely with the design and the power plant, only in terms of the selection of power plants, in order to match the power plants to fit the aerodynamics of the airplane. It's going to take so much thrust. Therefore we were the experts also on the propellers, even though we didn't make propellers, because we had to select the propellers from some propeller company, and the aerodynamics people did that, whereas the power plant did the engine alone, see, generally. And that organization was pretty typical in the whole aviation industry countrywide. There were some differences when they were building seaplanes. There would be some hydrodynamics for instance. Douglas was building some seaplanes then, too, and I think Grumman built some seaplanes, too.

Collins: What was the interaction between the various aerodynamics departments in the different plants? Was there interaction or were they relatively independent?

Rumph: They were sort of perfunctory. They were independent as far as action was concerned. The chief engineers of the three places caused us to have joint meetings to coordinate what we were doing, but not with action. They had no power to act for the others.

Collins: But there was some kind of mechanism so you'd be aware of....

Rumph: Of the other places, that's right. In other words you could learn some things from them, but they could not dictate.

And you'd be well-advised sometimes to take some of the ideas that you learned from them. In that sense it was an indirect dictation.

Collins: I think we're ready to move on to how you came to RAND.

Rumph: Well, all right. So then here I was in Columbus, and I then put out my feelers in three directions. One was General Dynamics in Fort Worth. I knew very well a lot of the engineering people that were in the Fort Worth General Dynamics plant. Another was Canada Air in Montreal. Now the reason for that was very strange. There was a director of engineering over all three of the plants, when they had the three plants going, a director of engineering who operated in the corporate office. When Buffalo and St. Louis were closed down, he became the director of engineering in Columbus. I got to know him quite well in Columbus, the four years I was there. He had come from Curtiss. His origins were in the Martin Company in Baltimore. He had been the chief engineer or director of engineering, I don't know which, in the Martin Company in Baltimore prior to about 1940 or some such thing like that. His name was Ken Ebel. I don't know whether you've heard of him. I doubt if he's still living, because he was perhaps 15 years older than I.

But anyway he tried his best to get me to go to Canada Air. He was going to Canada Air as the director of engineering up there. He would keep adding the salary offer, you know, just so you could not turn it down. And he asked both my wife and myself to come up there and look over the place. And he turned his wife loose on my wife to convince her. She was a Baltimore type, and, I remember my wife said, she tried to impress her by the fine linens and so on that were in Montreal, having come from the Baltimore area. Not quite an FFV type but close to it.

Collins: What's FFV?

Rumph: First Families of Virginia. And we had an interesting time in Montreal. They wined us and dined us, every officer in the Canada Air outfit and took us around to places we could live, etc., etc.

Tatarewicz: You mean in Canada?

Rumph: This is in Canada, in Montreal. So we talked it over at night. We were there about three days. My wife and I decided we didn't want to do that. Number One, for some reason, and I can't justify this. Are either of you British, by any chance? No. We said we just didn't think we wanted our kids to be raised under

the British flag. Now that's a crazy kind of patriotic thing. But anyway that was one reason given. The other was we found that in spite of these phenomenal salaries, there was a law in Canada that didn't allow you to move your assets out of Canada into the United States except at a very slow rate. I'd spend the rest of my life getting all my resources out of Canada, if I wanted to come back to the States. So put those two together and we said, thanks but no thanks, see. And we did not go to Canada Air.

Then the third was, Gene Root. Now I mentioned him before. I'd been in touch with him many times at various meetings and this, that, and the other during the war. There's a kind of community develops out of these people. This was now '49, but it turns out I had received some of the reports from RAND. It was still in the Douglas Company facilities. Now, you know enough about the history to know that they were over in the Douglas Company originally. And Gene Root had gone into RAND. He was in RAND at this time. It wasn't the RAND Corporation then. It was just called Project RAND, which was run under the roof of the Douglas Company, Santa Monica.

Now we'd been in touch, and he called me on the phone one day and he said, "I'm in Washington and I want to come by to see you." Okay. So he came by. We were living in Columbus then, Ohio. So he came and he stayed there a couple of days actually. We put him up at the house and so on. And he started to give me the story about the RAND Corporation and what it was going to do. And I don't know whether you know Gene Root or not, but he is a very good engineer, but he is a better salesman. I could kid him about it. He would give me everything. He says, "Now,"--he looked at our windows--he said, "What are those things?" I said, "They're storm windows." It was about October. It hadn't gotten that cold yet but I'd put up the storm windows. "Oh, we don't have that sort of stuff in California!"

Anyway he stayed there and he convinced me that I would have a better life and a better enjoyment of my career through what RAND would have to offer. So Gene Root was the man that sold me on coming, even though I had read some of the RAND reports and I knew what RAND was all about, more or less, not completely. So it wasn't a new phenomenon to me. But here was a man in it, who gave me the entree.

Collins: Do you recall how you came to see RAND reports at that early stage since the project was considered very secret at that time?

Rumph: Well, I had clearances, having been in a lot of military work in the industry, and it was circulated among industry people.

TAPE 2, SIDE 1

Rumph: The reports, I don't know. I found them in the library, in the classified library. I don't think they were secret. I think they were confidential or something like that. They weren't highly classified. It was classified anyway. And later, I'll jump ahead, I found out that RAND did deliberately send its reports that were related to aviation matters to all of the industry people. And I'll later come to the fact that there was a reason for that. That was the one way we developed relationships with industry. But I saw some when I was at Curtiss in Columbus and was familiar with what they were doing. I'll later talk more about it. They were these generalized aircraft design studies, and I'll describe that in more detail when we get to it in RAND.

Collins: You had an idea of the type of studies RAND was doing through these reports.

Rumph: Yes.

Collins: I'm curious if you can recall how Gene Root presented the RAND concept to you and the kind of potential and possibilities there were at RAND.

Rumph: Oh, yes. Let me then elaborate more on my recollections of his interview of me, right in my home there in Columbus, Ohio. I gave some of the jovial stuff and so on, the selling business. But he says, "Okay, now, RAND has as its purpose to advise the Air Force." (At this time it was only Air Force. We were not so broad as it is now.) "To advise the Air Force on any and all things that the Air Force has to do." Now the Air Force didn't have to take that advice, neither did RAND have to take a dictation from the Air Force, like a study that is a sales document, that is selling an item that the Air Force wants to sell to the Congress or to somebody else. So that was the first light that lighted up in my mind, about how to do work for the government or for anybody for that matter. Because it certainly wasn't true in industry, where you could be free of the so-called notion of vested interest, and he gave a lecture on that subject, too.

Maybe now I really can merge what I'm saying here with sort of what I learned early on at RAND. General [Hap] Arnold, had

set up a fund. He had been so interested in RAND--now I may be telling something that you already know from Arthur Raymond--that he wanted to find a way to perpetuate the fact that the Air Force could make use of the tremendous resource of innovative brains, that can do things to advise the Air Force, that they had done during the war, but as individuals and a lot of them at that, consultants and so on.

General Arnold wanted to institutionalize that idea. How to do it? He said, "Well, we've got an institution. We've got civil service." But they looked at civil service and said, "We don't think there's enough innovation that can be exhibited in the civil service system." Well, what about universities? And universities at that time did not have so many research institutes. They were more academicians, and nothing was going to get in the way of their academics and so on. And he said, "It's applied thinking we want to consider, not the doing of the applied work, but thinking about things that can be applied." Which again, in those days, the universities didn't quite have either. And so Arnold said, "I can't use universities. I'm trying to think of where to put an institution within institutions that exist." Well, what about industry? Well, then he said, "What industry? The Air Force has got every industry in the country practically involved in this, that, and the other."

So it finally emerged--and I wasn't there--Charlie, with General Arnold talking about all of this, that he called his friend Donald Douglas Sr., who he thought could be an unbiased representative of industry, that could be understanding of what he was talking about, what he was trying to grasp--had in his mind but couldn't quite articulate exactly. And he called on Donald Douglas and said, "Look, here's this idea of institutionalizing some of the best brains for innovative and mutually reinforcing use, and it's a myriad of things. It's not just the engineers." He visualized almost all of what RAND's got now. He visualized--it's not just engineers to build things and do things of that nature. But it's not politicians particularly, but this political science, the State Department kind of things. The Air Force or the military is an arm of diplomacy, that sort of notion. So hence he had ideas of having that. And certainly the budgetary matters of the Air Force, and the country related to the Air Force, involve economists. Now he's just about picked up the core disciplines, as they call it, that make up this sort of organization.

He said, how can I make that thing all work and pull together? So he says, "Doug, I want you to help me. Will you take off your Douglas hat and provide for me housing, a home for

hatching this egg? And we'll probably start with technicians and engineers, because that's essentially what you've got, but even be willing to give up some of your best people to work on this idea? And I have confidence enough in you, I could ask you to place it in your plant and isolate it from the rest of the company. That is, not give Douglas any privileged status as to what is done over any other part of the industry."

Gene Root gave me all of this lecture. I've given you a lot of background on myself. It says I like innovative stuff, and that's generally what he sold me. And I personally knew Gene Root and I had confidence in him. I had known him for a long time, and he wasn't giving me just a sales bill, although Gene's life didn't last here that long. He left after about, I don't know, six or seven years after the beginning of RAND. But anyway he sold me on it, and then he came back and checked it out--with Frank Collbohm who was president then. Okay.

RAND was building then. I was one, not the first, but I was among the early people that came in from outside of Douglas. RAND did get a core of about, I don't know, 30 to 50 people or something--you may have that data somewhere--that were transferred from Douglas to this. Now this was still called Project RAND at this time. It was never the RAND Corporation inside of Douglas. It was Project RAND, but it was isolated from the Douglas Corporation to work directly for the Air Force in this sense, with that independence: independence of the Air Force not having to do what RAND says, and independence of RAND not doing what the Air Force says, but communication between each other to understand each other.

And Hap Arnold says, "Okay, I'd like to begin the experiment by giving you ten million dollars,"--I think it was--"and nobody's going to interfere with you. You're supposed to have the responsibility to figure out how to do all of this, and I'll give you four years." In other words, ten million dollars for four years. "Then come back and let's see whether we have hatched a live chicken or not."

Now that's the speech that Gene Root gave me. It's also what I would have said when I was talking about the founding of RAND. So out of all of this he says, "Lest you don't believe all of this fully"--I didn't know General Arnold--but he says, "Call Don Putt." General Putt was the director of development. He was the closest general officer in the Pentagon at the time who would have cognizance of what RAND was doing. I'd known Don Putt ever since Caltech. Don Putt was assigned to Caltech as a student when he was a major in the thirties, when I was at Caltech, and

we were students in the same class. We knew each other well. Again, this contact system. So I called Don Putt on the phone in the Pentagon and talked to him. I told him a little bit about what my problem was, and what Gene Root had told me, and I said, "What would you advise, seeing it from your Air Force side?" He said without a doubt, "Take it."

So now we're here. That was it.

Collins: That would have been October of '49?

Rumph: October '49. And I drove the car from Columbus out to here and started to work, after I spent four or five days getting out here. I started to work within a day or two after I arrived in California. None of this building existed at that time.

Collins: I'm wondering, a couple parts of Gene Root's sales pitch seem fairly novel, and I wonder if you can recall how they struck you at the time. One, you mentioned the sense of equal communication between RAND and the Air Force. That wasn't a typical kind of relationship that you found between industry and the Air Force.

Rumph: No. That's right. That's what was so intriguing about it.

Collins: As well as this question of involving other disciplines in this question of looking at the applied aspects.

Rumph: Yes, what amplification are you asking?

Collins: Those are two clearly novel elements, and I'm wondering how did you respond to those?

Rumph: Well, that's what convinced me. In other words those novel elements are what convinced me that I could have a much happier and maybe more productive life in that environment, to do that. That's how I reacted to it. That's why I was sold on the idea. Even though there were many frustrations. I'm not trying to say everything was peaches and cream. And those two novel ideas no longer seem novel to me, but have persisted by and large I think throughout RAND until--I must confess.

See I retired in 1977, so it's ten years this year, ten years the 30th of July next week. And I didn't sever completely then. I was a consultant. I did a little bit of work, but I finally said, "I'm not being productive enough. If you're only down here for two hours every ten days or something, you can't do

too well at this sort of thing." So I was being paid a fee for doing nothing but learning what RAND was doing all the time. So I gave up the consultancy, at least the paid consultancy. I refused to accept any more money for it. But I would come down for the first couple of years and just take a few minutes' time from people to find out what was happening and so on, different, and see how the change was taking place. But after two years, the last eight say, I've lost contact except occasional ties. I'll be with Gus Shubert on a social occasion or some such thing like that and ask him some questions. But I feel like I'm eight years behind. You'll be much more up to date when you get through with all of this than I.

Collins: Our emphasis is really primarily on the first ten to fifteen years.

Rumph: I see. You don't care exactly what it is today.

Collins: Yes. We're interested, but the real focus of our effort is the earlier period. I thought we might just take a five-minute break here before we get into the details of RAND.

Collins: Okay, you've just described to us Gene Root's coming to you and requesting you to come out to RAND, and your family moving out here. Once you began to work, what were your responsibilities and duties and what did you find?

Rumph: They were quite different from the industry. That's the first thing I observed, which I expected. This was 1949. RAND Corporation existed then, so I had no experience with the work in the Douglas plant. And those that started, started in 1946 at the outset, so you see I was about three years behind in that sense. They were in a building over here in Santa Monica when I came, and at that time it was the RAND Corporation. Gene Root was the head of what was called the aeronautics department at the time. There was another man whose name was Jimmy Lipp, who was the head of the missile department at the time. There was another man whose name slips me right now who was head of an electronics department. You've probably got that somewhere in your notes.

Collins: Yes. We do have some organization charts that have that information.

Rumph: Electronics. What I'm trying to say is that some of these interdisciplinary things, at least in terms of organization, had been already begun to jell at the time I arrived here, because I've named three. Now these three I've named so far are

what are called more or less the hardware types. There was an economics department. Charlie Hitch was here. There was a mathematics department. John Williams was here. There was a social science department. Hans Speier was here. And that's all. That was it.

Now you can see they were all organized in these departments. The question is, how do you work within that mix? I was in the aeronautics department, and I think the total staff size of RAND--the total research personnel of RAND--was about 200 when I came. The departments were about equal size as I remember. Now all of the seven department heads were reporting to Frank Collbohm, the president. He wasn't called the president. He was called the director of Project RAND I think at the time. No, RAND Corporation it must have been at that time.

Tatarewicz: That's an organization chart from 1950, which has the departments.

Rumph: Yes, well, okay. Here are Dick Goldstein, and Larry Henderson in the Washington office, that's right. Now did I get all the departments? No, I forgot the physics department. It was called nuclear energy apparently but later they called it the physics department. I guess it's called aircraft, not aeronautics, apparently here at the beginning. And I forgot that at the moment. But I got the remainder of them right.

Tatarewicz: The question that I have about your first impressions of RAND when you arrived was, these departments look pretty traditional to me. What did they look like to you?

Rumph: First of all, until I was here for maybe a year, something like that, I was trying to get myself well-seated in the particular department I was in and become acquainted with people in the other departments, not necessarily to work intimately with them, but to get acquainted. That took about a year, I'd say. Now, why the year, was that RAND is an organization that oftentimes engineers, particularly.... A lot of these people--missiles, electronics, aircraft--those three, were all kind of engineer types, most of them. Many, many engineers are not happy at all unless they are doing something in engineering that they can finally see, a physical object, make something. RAND had nothing like that. There was only one element, I might say, as a sideline, and that is, they built computers. That's all, see. And there's nothing like a computer--I don't know where--here numerical analysis was down under the mathematics department. Now RAND then did not build things, and pretty soon <u>The Wall Street Journal</u> dubbed it a "Think Tank." Now what is a Think Tank? How

do you do something in a Think Tank? And in spite of the fact that I can hear all of these high-sounding phrases that I heard from Gene Root that attracted me here, I had, not a traumatic experience, but I had a learning experience, to sort of get adjusted to the fact that I was no longer doing anything except on paper, being an engineer.

And I might say, throughout the history--my knowledge of the history of RAND--a lot of engineers had trouble ever getting adjusted to what RAND was supposed to be up to, in spite of the fact they could mouth the things they were up to. It was because they just could not get out of their system that they didn't do anything physical (hardware), to see the result of it. That's been a problem for engineers, more so than I think for the social sciences. Math and social sciences don't quite have the problem of having to see an article, a physical object, as the fruits of their work. And I didn't realize that I was having that problem, but I was intrigued with the idea that the objectives of social science was more important than seeing a piece of hardware built.

I immediately got into the business of working with Bob Schairer, who was one of them, and a man named Schamberg, who long since has left RAND and went to Northrop, actually. And Bob Schairer, I mentioned earlier, went to Lockheed after he stayed a while at RAND. But those two and myself, I remember, we were working directly on what was called generalized aircraft studies. Now what does that mean? That means, as I like to think of it, trying to design families of aircraft mathematically. In other words, we would set up, sometimes, empirical equations, particularly on matters like weights. We would get data from aviation industry people. Now this is also touching on the relationship to industry questions, too. We'd get data from them on statistical weights as a function of size of aircraft, see, and by statistical analysis we would produce mathematically families of aircraft of varying sizes, of varying performance, and varying load carrying ability, and so on. And that incidentally was some of these reports I had seen back when I was at Curtiss. They were on these generalized families. The purpose was never to design an aircraft. Purpose was to see what the state of every art that had to go into aircraft--what that state of the art was, and then make perturbations around that state of the art for the different purposes that might be wanted, in Air Force terms: in bombers, in fighters, in air defense work with fighters, bombers, strategic bombers, and so on. So it took me almost a year to get well-adjusted to sort of designing aircraft mathematically. Now we did have some crude computers, so we could develop these equations and our numerical analysis people could produce numerical results for us so we could in effect chart trends.

Now we also had in the economics department a subgroup called cost. They also had gotten busy and one reason we--let me sidetrack just a second, a parenthetical remark--had so much data on industry's aircraft, etc. That actually was before I came to RAND. Gene Root had been given a project to handle within the aircraft department of developing a little book for people like Secretary of Defense [James] Forrestal. Wasn't he the first Secretary of Defense? Something like that. He couldn't understand all this stuff the aviation people would give him, and he didn't have to at that level, but he wanted some understanding of how things compared. Well, the way to compare things is to do these generalized kinds of things, see, and then you've got a basis for comparison. But in the doing of that, Forrestal set up with the whole aviation industry an edict that they wanted certain performance data, which said data to be prescribed by RAND, what they wanted to be given to RAND. And they set up a local momentary RAND office in Dayton, where most of the engineering work of the Air Force goes on, and all the aviation companies would come in, just like I'm in today, and they'd give testimony on their stuff. They'd hand their stuff over. They had a group, Root and four or five of his people, would look it over, and they developed this format that Forrestal wanted. But on the other hand, they had data that statistically could be used for these other purposes.

That's the aside, see: where did that data come from? That's where it came from. And a lot of it was statistical. A lot of the aerodynamic data was not; it was sort of pure math. I mean after all everybody knows, you know, that a Frenchman named Brege' developed this equation for range of an aircraft, and that's going to be long lasting as far as aircraft is concerned, see. That's a known formulation of the problem, and so it expresses all the components of an aircraft; namely, it expresses the power plant system. That is in terms of the specific fuel consumption that the engine generates. That's the fuel it's going to burn. The structure determines the weight, the empty weight of the thing, see, generalized structure, same as the empty weight. The aerodynamics determines the lift and the drag of the thing, and that's all the elements there are in the Brege' equation, so you put those three together and you get the logarithm of them, and you can express this, see. So that's why I said that the aerodynamics part of it was a coalescing of the power plant and the structures into the design, in effect, mathematical design.

Tatarewicz: But to get that data, you have to have individual competing companies turning over what could be considered propri-

etary information.

Rumph: That's right. But now wait a minute. We started because we were just beginning--now I'll tell you, a little of this took place before I came, remember I said, this thing of getting it from Forrestal. The very beginning, and that was before my day at RAND. But that's what Root managed to get. I don't know whether he convinced Forrestal of this or Forrestal wanted it and that's what Root figured he'd get. Forrestal in effect ordered it. So in other words, thou shalt. And so that gave RAND a problem once we were loose from the arms of Forrestal, see. How do we keep relations with the industry, in order that we can keep a flow of information?

And we do that. How do we do it? We do it by being able to develop confidence in the minds of the industry that they're not going to have something that they think is a trade secret given to somebody else competitive, you see. So that was the task of developing a relationship with industry, and that had to be developed. A lot of it was going on in the beginning right when I came out. We did it simply by--and it wasn't easy. I mean it's like pulling teeth. There were a number of people in different industries, not all of them, that knew this core of people. You remember they came mostly from aviation. They didn't know the economists or the social scientists, but in terms of what they could give was going to be hardware data; they knew the people who were going to do it. And I can only say that it began--because there had not been a demonstration yet--it began by their hearing what we had to say, and by knowing us, considered us honorable to keep our word as to what we were saying. I think that's the only way it got cracked in the first place. We were not strangers to that industry, see, as far as the people were concerned, so we had that. But we were very meticulous for years, in that, to be very sure that there could be nothing like Congressmen always get blamed for, leaks. There cannot be leaks in this sense. There could be nothing that industry could see whereby specific information that they had given us was ever gotten to any other competitors hands in the industry. And it was only by witnessing it, and standing under the bright lights of that thing all the time, being conscious of it, that allowed it to happen generally.

Now you might say, well, okay, but you had these generalized studies, and these reports of the generalized studies went to all of the aviation companies. Yes, it did but that didn't go in any form where anything that could be called a design secret of anything was shown. It was only statistics of the state of the art, of how light can you design a structure of a given size. That's

state of the art. How low can you make the drag, how low can you make the specific fuel consumption of the power plants. And this brought in of course the engine companies, Pratt and Whitney, Westinghouse, GE [General Electric], and some of these came in with the jet age. So we haven't quite gotten into the jet age yet. These first studies were all propeller-driven machines. And there was no way, except being super-conscious that we would not be able to do our work if we didn't maintain this separation of companies with each other.

Tatarewicz: Was this information tightly controlled within RAND, or even within the aircraft department?

Rumph: Well, the raw data was mostly classified. If it wasn't classified in a military sense, it was classified as far as the companies were concerned. It was called company confidential kind of stuff. So that gave some protection to it by not allowing the raw data out of the aircraft department of RAND. The social scientists had no need nor reason for it anyway. The economists had no reason. The cost people might have, because they had to develop statistical cost relationships, you know, because also part of the information was what these airplanes were selling for, too, the costs. So there were statistics involved in the costs originally, because we didn't want just the generalized aviation without it being attached to the cost-effectiveness notion. And it was protected as being only within the aircraft division.

Tatarewicz: Were there logging procedures?

Rumph: I'm not sure that it went so far as a logging procedure. It wasn't quite like top secret stuff in the military. No, I don't think quite that much. But every person who was working with these missiles, electronics, and aircraft, were not newcomers to the field of those disciplines but had been in industry, and they all were perfectly aware of that sort of thing.

As an aside, I'll cite one where a mathematician, say, was wanting to do something, and they were in the Douglas plant. And somewhere the purchasing department in the Douglas plant got a request from John Williams, who was head of the mathematics department, for, say, 15 pounds of paper clips. The purchasing department at RAND just couldn't conceivably figure out what in the world anybody wanted 15 pounds of paperclips for. So he questioned that. What he wanted it for at that time was to test some of the probability laws by dropping the clips from the ceiling and measuring their density distribution on the floor.

Collins: I'm still a little unclear. The Air Force or the Office of the Secretary of Defense said to the aircraft industry, "We'd like RAND to work on this; we'd like you to provide them with the data." Is that correct?

Rumph: Let me separate two things. I've got two things mixed together there. One is, what Forrestal wanted was a book, and you'll find in the archives some place a copy of the book around here: a book where each aircraft that the Air Force had, had a page, with certain specific data that described its performance. Maybe a graph of some such thing, but on that page. The next aircraft was on another page. But all of that data had been reduced to common terms, so if I look at one number for that aircraft, and the same number for another aircraft, one could compare them. That was an Air Force book, although we have copies of it here because we produced it. But that was mainly for the Secretary of Defense and Air Force people to use.

Collins: Was the original data received from the aircraft industry, or was this something that the Air Force already had on hand?

Rumph: I was not at RAND at the time. Hence, some of what I have said on this subject is hearsay. It would be better for me to drop this subject and let someone else that you interview fill you in on this.

Collins: In terms of working out the relationship between RAND and the aircraft industry, what was the initial force that brought them together? Was it the Air Force saying, please cooperate with RAND, or was it the long familiarity that members of the aircraft division had with the aircraft industry?

Rumph: Well, I think it was some of both. Now I think we would not want the Air Force to order it to be done. In other words, since they were still contractors to the Air Force they could practically order it to be done, because they were buying these aircraft. Never would they do that. I think the Air Force did suggest that, but it was not a thing that was incumbent upon the companies to do by edict or by order. It was a more gentle thing than that, because that would hurt relations more than it would benefit, if the Air Force ordered it to be done. That would sound like you were suspicious then suddenly, if you were going to do it that way. So I guess I have to say that while we were amateur psychologists, we were applying some psychology of what it takes.

TAPE 2, SIDE 2

Rumph: I'm not sure there's any prescription you can write down, that by reading it you'll know exactly what the relationship is. I think it's more subtle than that. And the only test is whether in time you succeeded or you failed. And so, I do not know how to articulate actions you take that will insure that this will happen. That's why I have to leave it somewhat fuzzy.

Collins: I'm wondering what you saw and the aircraft people saw as the use of these generalized data studies?

Rumph: All right. After I had been engulfed in these things for about two years, see, I began to feel like I wanted to be a project leader. What does that mean? Okay, I'd heard about all this interdisciplinary research even from Gene Root way back, but I hadn't seen it really. I'd gotten my feet on the ground for two years, and then I barged forth. And I managed to join forces with an economist, Steven Enke. I don't know whether that name has cropped up anywhere. Steven Enke and I were called project leaders, on a project that had to do with taking an Air Force strategic mission--what's a strategic mission? See the nuclear weapons had been not fully developed, but we were in the nuclear era by now. And we had a physics department with all of the nuclear stuff; therefore a lot of us had to go through the Q clearance system and so on. But what the project consisted of: studying what would be the most effective or cost-effective way for Strategic Air Command, for instance, to be able to accomplish its mission, and what can we say about the policies concerning what kind of aircraft development they should have. Not yet to the ballistic missile era, see. What they should have, and what do you need to know in order to do that? That's the objective of the study. So we entered into this project.

Tatarewicz: If we could back up just a little bit, what I'd like to know is how the idea of this study arose, and how you got hooked up with the economist and whether you tried other possibilities.

Rumph: You forced me. All right, I wasn't going to do it this early, but you forced me into telling about that thing on the wall out there. Now this was started before we moved into this building, and we were scattered in three buildings. There was one building and one across the street and one over there. This started--it may have been it was finished in this building. See this building was finished I think in 1953. Where am I? Two years, '51? Maybe so. The project may have been finished in

this building. I guess the point I want to illustrate by this is that we got these people in this corporation, and it doesn't do any good if they all sit in their offices or sit in their conference rooms and just talk with each other and do work together with each other. You'll have no interdisciplinary action. So you want some interaction between people of different disciplines. How did we do that?

Well, it seems like a simple-minded way but I think John Williams actually invented the idea, propounded the idea. He said, "We promote the idea that we want everybody in RAND to get acquainted. We want you to walk around the halls and talk to John Doe and Richard Roe and everybody, the sociologists, the economists, and you don't know what to talk about but you've got to learn what to talk about, see. You haven't done it before but you got to learn. You've got to talk and commingle this way, see." So as you now can begin to see, we had terrible trouble. The economists were in one building, and I was in another building, and we could cross the street, and that was difficult. But in planning this building, John Williams says, "Look," and he persuaded RAND and the architects, "Let's make this building such that you have hallways that maximize communication between people that are in the halls, passing if nothing more." If everybody's got to go around this way, everybody's got to meet, so here's another cell over here, and everybody goes around that way, and finally you can make a figure 8. This building, with patios in the middle and all those little checkerboard things, was deliberately designed. And even the architect may not know it. RAND wanted it that way. John Williams I think had the idea. It's to maximize communications among disciplines. Maybe somebody is set in his ways and he goes down one path and back, but this is the only way you can make a building do that.

So all right, I had gotten acquainted with many of the economists. I knew Charlie Hitch. I knew all the heads of the departments immediately and talked with them a lot, and I knew a number of the economists, and Steve Enke and I were talking. It turned out he was yearning to want to do something that he could do with economic expertise that was related to the mission of the Strategic Air Command. And so did I. So we got to talking. How can we do this? The idea emerged. I said, "Okay, I can figure out how to present an array of aircraft capabilities, not aircraft but the capabilities of aircraft, but all I can do is present the array. Here they are. So what? I know that cost is going to bear on it. So now, Steve, what do you say?" Ah. The light begins to dawn. "You know I can take economics and find out where you can localize, how can you optimize in a sense, between physical array of things." And by physical in this

sense--it's on a piece of paper, a graph, say, "physical array and the interesting places in that array that you want to study." You don't study them all.

So we met and talked about costs. Then we introduced the costs. We had this cost data. They were working on that sort of background data, and by and large not all but most of the cost people were economists. They were in the economics department originally. That's how we got together. That's how it got started. Then he drew in some more economists, a little help, although he went on to other things bigger than that pretty soon. You may have heard of Albert Wohlstetter. There he was, see. He was a strategic thinker. But he was an advisor, if you like. Steve Enke and I were the project leaders. There were some economists, mostly cost people, he had working on this, and also I had some other engineers on it. Together we put together the things that had to be processed in the computer to do it.

Now I'll go back and say, immediately after World War II, we had nuclear weapons, and SAC was terribly overburdened with the fact that how in the world are we going to deal with this. And we don't have missiles now. And so what did they have? They had things like the B-36, you remember, things like that. How can we have intercontinental bombing? We can't. We can't fly from here to there. So now we have to go into the NATO arena, and we get all sorts of overseas bases, and we find ourself there; there's SAC with overseas bases with their vulnerability. It was in Europe, not here. The B-36 wasn't particularly useful for that, but shorter range things.

Okay. Out of these studies came the fact that convinced SAC and the Air Force that there was a way to skin this cat that was not the overseas bases, their vulnerability, to get rid of them. That invented a thing called aerial refueling. Right out of those studies. See there was no aerial refueling before, and you know the idea seems simple these days. It's very simple for engineers to find out how much you can extend a range if you can increase their fuel. See the refueler has to go out and it has to come back, but it's got to have some excess fuel it can transfer and all that stuff. So the state of the art of all that tanker stuff can be built into this thing, and was. And I wanted to, you know, show--you've got another section in here where you talk about what effects did you have on the outcome of working for the Air Force and so on. I like to think--I don't know whether everybody does--that that was the equivalent of, not technologically, but the equivalent of the technical invention of refueling. And what about it? We don't have any such strategic overseas bases any more. They are gone, and that was an enormous

boost to the Air Force.

Tatarewicz: We're going to have to bring this to a close in a little bit.

Rumph: Okay.

Tatarewicz: And I know for sure that we are not going to be able to finish talking about this study.

Rumph: I had just finished this study. I was getting ready to go on to something else.

Tatarewicz: No, I'm afraid I have a few questions about this. I don't think that we're going to be able to really do it justice.

Collins: Let's just quickly ask, on this project--Joe's right, I think there are several questions we might want to bring up here. What was Albert Wohlstetter's role in this early venture?

Rumph: He was mostly a drop-in, look over your shoulder, consultant, kind of.

Collins: But you and Enke were the...

Rumph: He's not listed on the report as an author. He may have been given credit in the preface about consultation and advice or something, but I think it's just, as I recall, Steve Enke's and my name on the report.

Collins: Okay. I want to go back just a little bit to the early generalized studies that you did. What value did the aircraft industry find in these reports that you prepared?

Rumph: I'm not sure that I could say they found a great deal of value in them, because they are oriented more to designing, see, around specific points. And to have the generalized studies--I'm not sure that they got a great deal of value. Now there may have been some technological value in the fact that in here are formula. Out of all this developed formulas that allow you to produce a study, and maybe aircraft industry people were searching for the best thing to propose for the Air Force or even for commercial aircraft. I don't know.

Collins: Well, if there's an ambiguity about their value to the aircraft industry, what did you think their value was?

Rumph: That's why I tried to illustrate--the value was to the

Air Force. And it didn't have to do with specific airplanes. It facilitated the making of this other study, this Enke-Rumph study. It involved refueling, but it took that background of all that generalized study to run up and down the scale, to find what mixes could be put together, to minimize the cost if you like, to generate certain missions.

Collins: I'm still a little bit confused about what goes into a generalized study and what's covered and perhaps that would be a better way to start off our next session, because I'm not exactly clear on that. One thing comes to mind that perhaps you could think about. These studies which you characterize as state of the art studies. I'm wondering. You mentioned the refueling. The refueling was essentially something that was an innovation, that was not part of the state of the art.

Rumph: Well, it became that, though, once this was introduced. What is state of the art? In other words state of the art really means, what is the best that mankind has invented and how can you describe it? You describe it in certain technical terms like I gave you this Bregé equation thing. In other words that's a matter of commingling the parts. You take these state of the art elements, which are the best we've invented for engines, or for design weights, or for aerodynamics, the best minds have invented, and you put it together in this generalized study. That's what makes it state of the art. It's the elements that have gone in it that are state of the art. Refueling changed the state of the art. But I guess the mental view of what we were looking at here was enhanced to the point that allowed the notion of refueling to get invented. I wouldn't say, but theoretically I could hypothesize it would never have been invented were it not for that, but I don't believe it. In other words some other thing might have happened that you would invent it. I don't know whether before that time or not, we've had all these long endurance flights, you know, records you try to set. I've been up there seven days or something, and airplanes going up and down and refueling them. There's the concept right there, see. They're not going anywhere; they're just trying to make a record. This was with a purpose. The purpose was, we know the geography of the earth (that) needed traversing, to get to Russia, for instance, so we've got to have that range, whatever it is. And we can't cause one airplane--the state of the art is not good enough to allow one airplane to be made to fly that far. How can I stretch that? So, now it's ripe to invent something, and that's what comes forward.

Collins: So just to characterize it and tell me if this is wrong, the state of the art study lays the foundation for forward

looking to see what needs to be done.

Rumph: That's right. It is. You know state of the art only tells you a benchmark of where you are. You've already invented all that stuff, see. But it shows up where you're deficient. I just wished I could do this but I can't do it with this study. That makes it propitious to put the mind to work to try to invent some way to skin the cat. You can't meet the mission, see. I'm just trying to say that was sort of the one or prime example I could think of in the whole history of the RAND aircraft business that to me is most outstanding.

Collins: Why don't we call it a day, then. Thank you very much.

Rumph: Okay.

Tatarewicz: Thank you.

Interviewee: Ben Rumph

Interviewers: Martin Collins, Joe Tatarewicz

Location: RAND Corporation
Santa Monica, California

Date: July 31, 1987

TAPE 1, SIDE 1

Mr. Collins: Last time, we concluded our interview with a discussion of some of your very early studies at RAND in the early fifties, investigations into the state of the art of aircraft technology and capabilities. And you indicated this was primarily work that went on within the aircraft department, but soon after that you began to take these studies and apply them to interdisciplinary work. If you could describe your initial efforts in that direction.

Mr. Rumph: All right. I think I described pretty well before the first one, namely the Enke-Rumph study, and described how it was in the early stages interdisciplinary, and what its consequences were in terms of benefits to the Air Force, namely the refueling concept for intercontinental bombing purposes. The studies of this nature were briefed to the headquarters of the Air Force and also to Strategic Air Command, which brought on a little bit stronger demand for more of that kind of thing.

Now the "more of that" veered somewhat, I think, in follow-on kinds of studies, where the ones I'm describing were still related to aviation matters, but they had to do with, not the concept of intercontinental bombing, but what sort of mechanisms and means were appropriate for the Air Force to use to accomplish that policy. That led to studies on the size of aircraft that would be appropriate with refueling in order to accomplish our mission and yet be minimizing the cost aspect, and that meant that the cost was really a more important element of the system. Hence it was more of the economic disciplines brought into this than had been on the earlier ones and less of the technical ones, because so much of the state of the art studies had been done.

Now following that there were beginning to develop studies whose central leadership were led by, really economists by profession more than any other one of the disciplines within

RAND. And they were those that perceived the general notion of strategic purposes, broad strategic purposes of what the Air Force is all about, and how can it serve the strategic purpose of the defense of the United States. And this got into State Department questions. This brought in social science questions. It got into more generalized research than just the use of hardware studies that had been used, and so this was the wane of those hardware studies. They more or less faded from the scene at that point, because the kinds of studies that were being done were on strategic policy altogether for the United States. The first name that comes to mind as a leader of that kind of project was Albert Wohlstetter. Now, strange as it seems, it was another one that was a physicist that came forward there, but he was a bright fellow and knew how to deal with this kind of problem. It wasn't just physics. That was Herman Kahn. Now Herman Kahn may be a name that has cropped up somewhere here.

Collins: Certainly.

Rumph: It turns out Herman Kahn was such a salesman. Also, that he began to have trouble with Frank [Collbohm] as to whether he was being totally objective with everything he had to say, and it was not long thereafter that Herman finally left RAND. And Hudson Institute was founded thereafter.

Now right at that point, I'd like to pause to say that RAND has been useful, I think, to the nation in a role that it probably never really anticipated at first, and that is, it has been the spawning ground for a variety of things, not only the supply of people in the Defense Department, whose training at RAND in thinking that way was beneficial to apply that way rather than in formal studies by RAND, but also in contributing to the growth of think tanks in the country, the Hudson Institute maybe being one example, because Herman Kahn was the spark plug of that particular one and came from RAND.

There was one; I can't remember its name now. It did work for the Army. Another organization, it was based in Washington, I think. I can't think of its name at the moment, but on many occasions the president of that organization and other members of it would come out to visit RAND to sort of pick RAND's brains on how to do so and so and so and so. I got a feeling that while we didn't have anything to do with the creation of it, we did sort of render advice. They looked to us to get advice on their operation vis-a-vis the Army, a totally different corporation.

Dr. Tatarewicz: Just as a sideline of interest, do you remember any names from people in that organization in particular?

Rumph: I was trying to think of that while I was talking, and I couldn't draw those out.

Collins: I think I know the organization you're talking about, and I think in fact Arthur Raymond was helpful in getting that going. I think he was one of the people.

Rumph: He may have but I don't know.

Tatarewicz: I have a question about the move towards more influence on the studies by the economics and policy people, and as you put it, the waning of the technical studies. Now state of the art is not something that one does once.

Rumph: No, that's right.

Tatarewicz: Although state of the art is not as fluid and rapid as to require the kinds of detailed assessments that the technical people were doing.

Rumph: It no longer became a kind of study to assess the technical state of the art that could be oriented to design of aircraft. But it was an easier job. Let's say that it waned in intensity, but it didn't evaporate. Now by that I mean the state of the art studies thereafter in aircraft, anyway, involved such things as the advent of supersonics. Lots of studies in the early days about the pros and cons of turboprops vs turbojets. RAND made many technical studies, that indicated, when put in the context of range missions like bombers and so on, that turboprops were much more effective than turbojets. That idea was never sold to the Air Force. And I have to confess that it may not have been a good idea. Sometimes you have to alter the technical work, to obtain some other strategic objective. And supersonic aircraft could never have been with turboprops. The Air Force saw the subsonic turbojets as a forerunner to the supersonic jets. And maybe RAND wasn't broad enough to see that. I'll even confess that maybe the Air Force was right. But the turboprops--there were many local technical studies done on that, but they went for naught and when I look in hindsight, maybe it was a right thing for RAND to have been squashed on that particular thing, see, the turboprop versus the turbojet engines.

Tatarewicz: In any case the engineers doing those sorts of studies don't seem to have been aware of, shall we say, non-technical factors that would incline the Air Force towards a particular technology.

Rumph: That's right. After these early studies I went through, those engineers who were perceptive of the broader policy things and wanted to be participants in that, went to that. Those that didn't want to do that but wanted to stay doing their thing lost ground in RAND, because it didn't need as much. You know to create these state of the art studies in the first place took a lot more ingenuity and effort to do than to move it from a subsonic era to a supersonic era, see. So it didn't take as much of that piece of engineering work, that wanted to do that and not broader things, and therefore it did in fact lead to the diminution of that other class of engineers at RAND. Some people were not needed anymore.

Now I looked at it this way, really, and tried to convince those that had to go this way that they were not interested in what RAND was moving to, and therefore they were inappropriate for RAND. That is many engineers, when they became educated as engineers, somehow had a notion that they would never be satisfied until they could take the fruits of their education and design something and look at it--in the vernacular, kick the wheel or touch the airplane, put your hands on the physical object that's my work, see. RAND was not that kind of organization. That generalization idea was hard to see. They wanted specific designs. And many engineers were so imbued in their education with designing hardware; they were the ones then that didn't want to do all of this mushy generalization kind of work. Therefore they lost out at RAND. And there are a number of those I think that still have a little bit of an unkindly feeling toward me, because I was in the role of having to help cut some of those out.

Collins: In other words, leave the organization.

Rumph: Yes.

Tatarewicz: If they wanted to do traditional kinds of engineering, did you encourage them to look into the corporations and companies in the industry around here?

Rumph: The people who were interested in structures work were encouraged--because it was something that was sort of being generated right at that time--to do generalized research in the same kind of state of art thing. It's a state of art in structures, related to what I call ceramic and fiber structures. You know aluminum was one thing, but it was about that time that embedded fibers in metals and ceramic materials being generated that would look very promising in a state of the art. Now there were a few, a little core of structures people that I did encourage that way,

but that was only three or four people, see. Engineers came in a flood at first because they were just transferred from Douglas to RAND. The first engineers were just that, see. And I was one of those in engineering who had been in the hardware business, see, as I said earlier, but I had an ambition to want to be broader than an engineer. I didn't want to be an engineer all my life. But you see a lot of engineers don't.

Collins: One of the questions that intrigue us is the impact or influence of this initial corps of technical people, engineers, from Douglas and the other aerospace companies that came to RAND and really formed the nucleus of the early group. What you seem to be telling us here is that there was an ethos of sorts, if you will, by which these people considered their work.

Rumph: Yes, if I understand what you mean by the ethos there, yes. But I'm saying that that ethos was not permanent at RAND, if you mean by that--there was a niche for them to do their thing vis-a-vis hardware. Even though you didn't build the hardware, you were working on capabilities of hardware. But I don't mean it vanished. There's still--I don't know exactly, whatever department it's in--there's still some there. I mean there's very little activity right now that I know of in RAND that's related to aircraft at all, for the Air Force. Now we're in the ballistic missile era. The engineers have been doing a lot of work vis-a-vis that state of the art, but that's more electronics than anything else.

I'll cite a prominent example of the missile era and the communications era that's come with it and that's Cullen Crane. Now does that name come to you somewhere here? He's still in RAND. He has not retired yet. He did not come in the early stages. My recollection is he came in the late fifties, first of all. But he was an electronics person, and he has been enriched so by living in the environment of RAND and its broader studies, and even as a teacher in this graduate school, too. He's a product that's been generated by RAND and been influenced by RAND, and yet--he's still a RAND employee--he's on the go, travels all the time. He personally consults with the whole communication world and the whole, you know, missile guidance world and so on. And he can look at it from a broad perspective. Now that's just another example of where a few hardware types or engineer types rise up to do things that are different from the early things that I expounded on, see. And it doesn't mean engineering has vanished from RAND, but the kind of engineering more or less has.

Tatarewicz: Now organizations--there has to be some mechanism that operates from people to people that implements or enforces,

or even decides first and then implements and enforces, the direction of the organization. That says we're not going to be a traditional engineering company, and you're either going to have to adapt or.... So what I'm curious about is, how does that work on a day-to-day basis, and who decides?

Rumph: It has been done through a thing that has been called over the years a management committee. [Now this is not the research council. That's a different thing.] The management committee has been the leaders in the various subparts of RAND. But this management committee would come together, and the various members, having a little different perspective, would have these sort of policy questions boil up in the management committee, for Frank to make a decision--the president--or Harry Rowan later and I guess now even with Don Rice. Is this good or bad relative to RAND's long-term objectives? And we always keep in mind it's a long-term objective--is of being a resource for the nation that can be advisory in some sense. It's not going to be running the nation but advisory in this sense on matters of broad strategic policy.

Collins: Early on you were just a member of the aircraft division.

Rumph: Yes.

Collins: Did you have a sense of the kinds of deliberations that the management committee was doing in the early fifties?

Rumph: Well, not really, let's say for the first couple of years. If anybody in the research staff wants to talk about a problem of his own, or they think there's something going wrong at RAND or something, they'd get an audience with the president. So you can short-circuit in that sense, the management committee. I mean everything is not an in-line system. And I said long ago that no one wanted RAND to be a rigid in-line system, that everybody had to take a command from here to here to here, see.

Collins: How did your department head Gene Root or later [Ed] Barlow communicate policy?

Rumph: They in turn, about once a week, had what they called staff meetings including communication of management trends.

Collins: I wonder, in these earlier staff meetings and later on when you were a participant in the management committee, how the broadened role of the engineer was conveyed to the staff?

Rumph: How it was conveyed? Well, first of all, its discussions, within the management committee, were more or less as I made the discussions right here. Now how do you convey that to the staff? I guess in the sense that this happened in a slow, gentle enough way, almost osmosis, that any of the staff that was observant could begin to detect alone. You know it's almost like an institutional body language going on, not a verbal body language going on, see. Now when there were actual direct actions, like we had to curtail some people, you couldn't leave it to that sort of action then. It had to be more direct. But a number of the people among the engineers themselves, that didn't want to go into this broadening aspect, as I said, saw the signs in front. And they decided that RAND wasn't for them any more, and they, of their own will, would leave. Now let me see.

Tatarewicz: Well, the question was--there's actually two parts. First of all, what group of people or individuals came up and expressed the goals of what RAND should become and what it should avoid becoming?

Rumph: Every research man had an opportunity to express that if he wished it, as I said, directly to the president. However, that's not sufficient, to just leave nature to take care of itself. It takes more guidance than that. And the thing that finally makes policy, and you can get it done, was through this management mechanism, which you see actually wasn't only the people and the members of the management committee. They had lots of inputs from staff in turn, which had come through their own organizations that flowed this way, to get an organized way of doing it. In other words a staff man may have an idea that he'd like for us to work for some organization we'd never heard of before. But he couldn't just go out and propose to do something like that. He'd have to do it through the president or through the management committee or through some body along the line. So there was control. It wasn't left exactly to chaos.

I don't know whether you're going to talk to Dave Novik or not. Did that name occur to you? He came from government to RAND. He used to say that the best way he knows how to describe the organization of the RAND Corporation and the way it operates are two words--and he called it that for years--"organized chaos." And that fits what I'm trying to describe at the moment, see.

Tatarewicz: The other part of my question was whether Frank seemed to have in his mind a firm, clear idea of what RAND should be and what RAND shouldn't be.

Rumph: Frank, I don't think, had a perception of RAND ever going to the breadth of the state that it's in today. Frank was himself, in his own mind, limiting what he thought RAND--the ultimate of RAND would be oriented to essentially all aspects of the foreign policy of this nation that had to do with the Department of Defense. Or maybe the State Department and the Department of Defense. In fact a number of us raised the notion that maybe the time has got to come when we need to be in relation with some of these domestic organizations. Frank did not [[think?]] that it would be a good idea.

Tatarewicz: How early would you have raised this with him?

Rumph: Well, probably in '64 or '65.

Collins: Two questions. The first is, your perception of the relationship between the hard and soft sciences in the very early period, say pre-1955 or '56. And secondly, you indicated that the engineering segment of RAND continued to grow and was a fairly large division, even after the soft sciences became....

Rumph: Well, it did grow, but I don't think it grew in proportion to the other. I meant the totality of RAND staff was growing.

Collins: Okay, so in relationship they were....

Rumph: I did make the remark that the engineering division was bigger than any one of the others. That was merely because at first they coalesced three equal parts into one over here, see. That was not an absolute expansion, but then there was this growth. That is, the contract effort that we were working on began to support more and more. We were in effect expanding the staff throughout, not necessarily in engineering, but actually the staff grew in many areas, whereas engineering itself, hiring of engineers, new ones, got to be less than these other people. So what was engineering, it's got a different name now, but Gene Gritten is the head of it, I think, and if you look at the number in there, I think that is smaller than the old engineering division by quite a bit, see. Now I'm trying--you sensed there was a conflict in something I was saying.

Collins: Well, not necessarily a conflict, but perhaps a kind of a paradox, if you will.

Rumph: I should have said that relatively speaking the engineering was larger initially, when it was combined with other things, you know, aircraft, missiles, electronics, all of those put

together. They were all engineers. And that was true in the early stages--even the first day it wasn't all engineering. John Williams, a mathematician, came to RAND I think the day it opened its doors in the Douglas plant over there. He was the head of the mathematics department. Charlie Hitch, an economist--I think he was the first one they hired for the economics department. I don't think he came aboard until they had moved over into Santa Monica, which was only two years after its inception. So the engineers started at 100 percent. I don't know what the percentage is now, but I'd be willing to say I wouldn't be surprised to find it to be less by quite a bit than 50 percent of the total.

You asked this question I didn't finish, I guess, about did Frank perceive RAND today, or did he perceive everything I'm trying to talk to. You should ask Frank.

Tatarewicz: Did he have any other seemingly firmly held convictions, clear ideas, of the boundaries of RAND? Of what RAND shouldn't do, especially in the early days, or what it should do? I'm curious about to what degree the notion of bringing in social science and economics and policy, to what degree Frank held that goal firmly in his mind.

Rumph: I think that he had that in mind from the outset. Now I don't want to confuse this with the other disciplines I'm talking about that are now being used to service these domestic projects. I'm not talking about that. You mentioned--social science was really political science at that time. Economics--it's obviously needed for the type of breadth that we need, even in Frank's concept. So Frank at the outset was fully aware that we had to have some mix. He didn't know what mix at the outset. Engineers, physicists, electronics--that's still engineering, economics, political science, mathematics--because that's sort of got to service a lot of the quantitative aspects of what you're trying to do, and I guess that's about it. That was in his mind initially. And when I arrived here three years after its inception, there were already organized departments of these, see. Not so big but they were organized. So yes, he had that.

TAPE 1, SIDE 2

Rumph: I'd just like to cite, stepping back a little bit, another man I think you are aware of who's at one time one of the leaders in this interdisciplinary generalized project work, was Dan Ellsberg. Now, other problems developed with Dan Ellsberg, as you know, but he was here for a period. I think it was in this same period we're talking about, maybe the first five years

of the sixties or some such time.

Tatarewicz: '67 to '72, yes.

Rumph: Even before I left RAND the domestic division, if I measure it by the number of research people involved in various projects--which may be a bad measure but that's the quickest thing I can think of--there were about the same number of people working on problems in the domestic division as there were in the national security division.

Tatarewicz: Since you mentioned Ellsberg and his interdisciplinary work, we've seen him acknowledged and referred to by a number of people as someone who was good on interdisciplinary work and on exploring ideas.

Rumph: Yes.

Tatarewicz: And he's not known for much else, other than the papers. Could you expand a little bit on this other side, this aspect of Ellsberg as an interdisciplinary?

Rumph: Other than his interdisciplinary?

Tatarewicz: No, as an interdisciplinary.

Rumph: Well, I guess the single most important thing I could say about it is that his mind--he was so bright--that his mind comprehended from talking with various people, engineers, social scientists, psychologists--he was an economist by profession--that his mind assimilated interdisciplinary research to the point that he could perform it as a one-man project. If that means anything to you. What I'm trying to say is that I thought of him as being bright enough to say that he didn't need an engineer or an economist or a political scientist or a mathematician or what have you. He could learn enough about the way the discipline had taught them to think that he could assimilate that in his mind. He was a one-man interdisciplinary project officer, policy research type. He was bright enough to do that I thought. Now is that the kind of thing you meant?

Collins: That raises an interesting question about how you define interdisciplinary research. One of the things we've noticed in our discussions and some of the things we've read, that you have, I guess as a starting point, perhaps two different kinds of models. You have an Ellsberg-type model, where you have an individual who essentially is able to coordinate all these interdisciplinary aspects and is really the key individual. Then

you've got these other much larger team projects, where you've got various individuals providing the inputs. How does that strike you as a characterization? How would you modify it?

Rumph: The two kinds of projects you mention do occur. It's a matter of talents and style employed for accomplishment.

Collins: How are leaders "selected"?

Rumph: Well, first of all, I thought you were going to say, how are the leaders made.

Collins: We can put it that way as well.

Rumph: Well, no, I frankly think they're born. I'm not sure I can understand a way to educate a man to be a leader or to have a policy course you go through to become a leader. I'm not convinced but what it's the mentality that one is born with principally. Now that's how they're made, but now how do you recognize one is another question, see. And I don't know any way to recognize, except as RAND has done it. You can certainly recognize the man who's smart. Now once you recognize that, you give it a trial. In other words can he do it or not do it? See. And who's going to judge whether he did it or not do it is another question.

Tatarewicz: Well, whatever the nature of the person, how in RAND did a person come to occupy the position of leader of a project?

Rumph: By creating in the minds of the leaders of RAND--the president or some division head or what have you--that they would like to gamble on him. And so, he's allowed to do it, you see. I mean he's aspiring already, see.

Tatarewicz: So he comes up with an idea, and says I'd like to do this project which would draw on this kind of expertise and this kind of expertise. Does he write a formal proposal up?

Rumph: Sometimes it has been that way, particularly when it may be a project that is going to be paid for from what we call RAND research funds. Anybody who had what he thought was a good idea and wanted to do just what you initiated, he would go to Frank, in effect. Frank had an evaluation committee. The staffer would have to make a proposal of what he wanted to do, how he was going to do it, and what he expected to accomplish out of it, and that would be reviewed by Frank's proposal committee. And then Frank, the president--or Harry Rowan or Don Rice--would make the decision, yes or no, to allocate it.

Collins: Was this evaluation committee different from the management committee?

Rumph: Yes, I think. I'm not sure. I think in general it was always an ad hoc thing. The proposal would come to the management committee, say, or Frank. Frank would pick a group of people that he thought would be the most relevant to evaluate this thing. It may be some management committee people. It may not be some management committee people. So it's not formally just management committee. No. Now that's one.

Now the next kind of thing is, if we're going to introduce--somebody wants to do a project related to Air Force. Now we're not expecting RAND's research money to pay for it. Usually that comes about by the person who's going to be the leader proposing, by having visited various places in the Air Force to talk over what he has in mind, to find out what kind of proposal he wants to write. Now this is not to say he's going to write something that's just going to be subservient to what the Air Force says, but this is to give him some reason to believe that it would be, in his own mind, useful to the Air Force. Not Air Force telling him to do anything. And that would go through then the same kind of process. In that case for instance, when Ed Barlow was the director of strategic projects, that proposal would come to Ed Barlow to be evaluated as to whether or not it fit into the scheme of things that were ongoing already in RAND vis-a-vis the Air Force, if it was going to be for the Air Force. Or was it going to be some new idea, new thing, but worthwhile. So decisions were made kind of on that basis if it was a proposal.

Now other things are done the other way around. The management committee or department head or anyone has got an idea, a dim idea of what ought to be done somewhere, but doesn't want to do it, see. Now that sort of thing can be taken through the system of the management committee and Frank, or Frank may have the idea in the first place, see, of something that ought to be done. He puts the finger on somebody that he thinks is interested enough and capable enough to want to do it. That's not initiative from the man but initiative from the top of RAND down.

Collins: But I would assume that the leaders actually represented, if you want to call them that, a relatively small fraction of RAND.

Rumph: That has always been the problem of RAND. Harry Rowan was a good leader this way. Ellsberg was one, in a different way. Ed Barlow was one.

In a more limited way, in the very early days, was a man named Ed Paxson. That name may have cropped up someway. He's dead now. He was an integrator of some of the best of the studies back in the early fifties, and he was bright enough, and he was one of the most convincing men that I ever saw give a briefing to General Power or whoever it was that was chief of the Strategic Air Command. He could be more convincing in the way he'd converse in a briefing than anyone I've seen in a long time at RAND. He was just excellent in communicating the results of something. So he was one but in a more limited way. I don't remember; he died about ten or fifteen years ago now, I think.

Collins: I think it would be helpful if you would relate for us how this informal structure of how projects, and individuals associated with projects, came into being, with the general concept of the freedom of the researcher to do the kind of work they want to do.

Rumph: Well, the first word I'd use about it is persuasion, by persuasion. If there's something that corporately wanted to be done, then the corporation always tried, they first--you'd even make a list of the potential people that might be good at doing this. And then you would go to them, by persuasion, so you'd have a selling job to do, see. That's what I meant by freedom, in a sense. You could have the right of refusal. I don't know. Frank--sometimes he could be so persuasive, you could tell he was twisting your arm. But the principle was to want to be persuasive about it and not order someone to do a thing, see. That incidentally is a quality that didn't have to be in the president only. But these outstanding project leaders had to have a quality of that type.

Tatarewicz: So you can institutionalize this and give it a name, call it matrix management, which is what happened later on.

Rumph: We've used the word matrix many times in RAND, trying to describe our way through the maze of this sort of thing, you know, on a particular project. In fact at one time we drew some organization charts that called a matrix organization, which was simply saying that here were department heads who held the human resources of RAND, here are the strategic problems, and here are the project leaders of that, or whatever you want to call them. And now you've come to any one of those boxes, and that's when you have the problem. Conceptually you can draw that instantly, see. But then you come down to--I'm using the word persuasion as the key word when you get to a box down here. If the leader doesn't have some of that ability, it's not likely that he's

going to get his boxes very well-filled down there with work to be done, see.

Tatarewicz: Is this the sort of thing that you'd talk about in the management committee?

Rumph: Well, maybe not quite in such stark terms as I did right then, but yes.

Tatarewicz: In a specific case, somebody would say that they've got a project going and they're having trouble getting help somewhere.

Rumph: Well, if you've got a project being organized, and you go down and you try to get some help, and you're having trouble with that, then that project leader will go to the director, like Ed Barlow. He was director of projects, with a number of project leaders around here. He would go to Ed Barlow, as an example, to lend his persuasive powers to getting some of this help, see. He might even go to departments that would be relevant here for getting some of this help and describe this problem and let the department head contribute to, how can we get this solved? See what I mean? Maybe he's got an alternative to the particular man that this project leader went to, another one who would be very cooperative or something like that, and solve the problem.

Those are parts outside of the matrix. It's a way of working your way into the intersection of two legs of the matrix, see. But there was no command rules about this. And sometimes it may have been that if there are too many people resisting, it may have been that it was an ill-conceived project in the first place, see what I mean, so it throws back to the project leader then. Could be. I don't know any particular ones that come to mind, but I think with considerable thought I might think of some that were that way. It just was a good idea that we don't start that project.

Collins: So it was kind of a checks and balances quality.

Rumph: Yes, that's what a matrix system is anyway. I think of it as.

Tatarewicz: Would Frank ever, in the course of a management committee meeting or something like that, say to a department head, "We've got this project starting up and they're going to need some help in your area. Please do whatever you can for them."

Rumph: Oh, yes. Yes. Yes. I say there's a resistance to this

command authority, but don't let me wipe it out altogether. You know, just the psychology of Frank, the president, saying that to a department head makes him want to be cooperative, and that passed on to the relevant people there, makes them want to be cooperative. But nobody literally ordered it.

Collins: This question of either inclination or disinclination to contribute to interdisciplinary research--did people in the social science side of things tend to be more amenable to this kind of activity than people on the engineering side?

Rumph: What I have to say there, I want to preface by saying, what I'm going to say is maybe not really balanced, because I'm originally from the physical science side. But here is what I observed. The political scientists have very rarely turned out to produce a project leader. They like to write papers all their own, individually. Now they will try to argue with pros and cons about it, you know, give a balance to the thing. That's their professionalism. But when you go to social science or political science to try to get some help, rather than agreeing to be a member of the team, they would say, "Come ask us our question after you've done something, and we'll give you political inputs about that." They want to be advisors only. That's a little bit different from being a member. Now you can understand why I said that.

Tatarewicz: By social scientists, you mean demography?

Rumph: Well, wait, if we're back in the early regime, if we're back in the first two-thirds of RAND, not the last third now, so far. It really was not called social science. It was called political science. The typical person was a State Department type of person, see, political science. Now we called it the social science department, but what they were doing, what their expertise was, they were organized by country. They had Russian specialists. They had German specialists. They had Far East specialists. They had Latin American specialists and so on. That's the way they were organized, just as the State Department is. In fact Hans Speier worked in the State Department, who was the head of this social science department out here first. But they did political science activity.

Collins: Let me back up a little bit. From what we've heard so far about the mathematics department and John Williams, I got the impression, given the fact that he participated in the design of this building, that he had a very strong interest in inter-disciplinary area.

Rumph: That's right. He did push it. But I really was talking about the way his math department functioned. He was a very strong interdisciplinarian personally on the management committee. He was tremendous at critiquing interdisciplinary studies. But the mathematicians he hired and had in the mathematics department, not all of them but most of them by and large, were those, "Bring me your problem and I'll solve it and I'll give it back to you." But I had a great deal of respect for John.

Collins: You've done a marvelous job of laying it out for us.

Rumph: Now how do you judge that?

Collins: In the sense that we're learning something we didn't know before. As Joe was indicating earlier, we know the organization charts are really very crude schematics of what actually happened. What you're providing us with is a sense of how it actually worked.

Rumph: Yes, that's what I'm trying to do. Yes.

Tatarewicz: As soon as we figure out whether history is a humanities or a social science, we'll let you know where we are.

Collins: Just to round off our last discussion, I'd earlier asked you a question we never quite got around to, and that was your perception of the interaction between the hard sciences and the soft sciences in the very early period. Say up to '55 or '56, if you can characterize that.

Rumph: Well, let me see. I think I'd first say it was varied. There were people in the hard sciences and people in the soft sciences who didn't seem to want to commingle much with each other. The soft sciences were less that way, because after all they were the newcomers into RAND, and they must have had some urge to want to be in some kind of interdisciplinary relationship or they would never come to RAND in the first place, see. But the engineers, I must admit, by and large most of them didn't have a very sharp perception of what I thought RAND ought to be or was going to be. They saw it as just another project for the Air Force or something like that. They would do their thing and that's that. Now that's one end of it.

Now, out of that grew what may be called the project leaders that could do what I've tried to describe of this interdisciplinary work. Not necessarily alone but by persuasive attitudes towards commingling, and they were the ones that rose up

from the various departments, that became the leaders. And I can cite those that came from engineering, those that came from economics. Not many from social science, though, because as I said, they had this other advisory role only.

Now in that early time it literally was, you'd be told to walk around and get acquainted. There were only 250 people there. Get acquainted with everybody, see. Talk about something. Well, when you talk about something, you're first going to start talking about things that you know about. And somehow you're talking at cross-purposes, but he says, "Keep talking." And what are you going to do? You're going to find some common ground when you do that, see. So there was a deliberate desire to bring them into discussion in the halls even. And I cited this building concept. This thing was not--not this wing of it but the other part of the building--was finished as I remember in 1953. You see, so it was pretty early on.

So early on there were some that.... Well, I'd go around and when I'd be in some engineer's office he may say, "Those damned political scientists" or economists, "they can't understand what I'm doing," or some such thing. But they didn't care much, some of them, to want to communicate with them. But enough did, and enough did in all of these different areas, so that RAND I think has survived. It might have gone away if it had been too much that other way. That original four-year notion I spoke of, that Hap Arnold says "Here's ten million dollars. Come back in four years and show me what you've been able to do with it." RAND was totally experimental at that time, whether it could survive under the concept or not, see. And I doubt if General Arnold had in detail the concept of what I have tried to speak of here, the concepts of what RAND is about. And I didn't either at that stage, see what I mean, and he just intuitively saw there was something there that ought to be done. And so it was pretty halting in the early stages. It didn't start off with a bang by any means.

Collins: Why don't we pause here and just take a brief break.

TAPE 2, SIDE 1

Collins: It's been implied in your discussion, or you get a sense of what the reward system was in RAND during this period. I wonder if you might just talk about that explicitly. How did people get ahead in RAND, in terms of either in prestige or increase in salary or position?

Rumph: I think I can say that most people finally saw, maybe dimly, what the thrust of RAND was. Now that's such an indefinite thing. I don't know whether many people did or not. If they wanted to be getting ahead, they would either be people who wanted to go in that direction, that thrust, and they could bring out all the talents they had in those directions and be judged on it. At least that's the way you attempt to want to get ahead with it. Those who wanted to be only technicians and advisors, unless you were very, very good at that, you would have a difficult time. In other words a mediocre social scientist or a mediocre engineer or a mediocre mathematician that wanted only to deliver their services, of their disciplines. If they were very good, they could get ahead and some did. Not as an employee but as a consultant. The greatest example of that is John von Neumann. He was only a consultant. He never was an employee. But you know, he was sort of a giant in his field, and no one would want to argue with him about whether he'd give a tinker's dam about interdisciplinary research or not. But by and large the signals, very subtly, would come out from this thrust I've been talking about. Now that's what people strived for.

Now the next thing, about salaries, is maybe a little bit different question. I guess I have to describe it, not quite so far back as the beginning, because I wasn't a part of it then. But it was just in the late stages, I believe, of the engineering division headship that I had. Later I became a member of what Frank had--and Harry Rowan and Don Rice later--was the salary review committee. All in-line organizational heads that had people they were responsible for would make proposals. There's a review of salary basis twice a year. They would make proposals to do something, not do something, quantitative specifics, and this committee would review it. Pros and cons of the proposal by the Department Head would be discussed and a decision reached.

Occasionally RAND would give sort of what they call a cost-of-living increases, and sometimes they would just be a percentage thing across the board, but mostly the salary review system was one on merit.

Collins: You indicated that it was difficult to evaluate merit, but you obviously had some rough criteria about whether somebody was doing a good job. Was it publications? Was it participation in an interdisciplinary project?

Rumph: Well, it's a variety of those things. For any one person, productivity; part of the merit of productivity was, "what have you done for me lately?" It's like the baseball people say. And then you'd look at--sort of display of what had been

accomplished and had it been accomplished through sponsored research or sort of individual research or was it his contribution seen in a publication related to a project that was interdisciplinary? And so on. And I suppose it could be said that, at least in my mind, that if there were two equal quality pieces of research here, but one was done in an interdisciplinary environment and the other one wasn't, I gave some more plus to that one, if technically they were equal.

Collins: Were there any other possible measures besides publication or participation in interdisciplinary projects?

Rumph: Yes. Another is very successful briefmanship with a client as a measure of high quality. You might say, okay, but a client would think so; who's measuring that successful briefing? If the client was measuring it, he might be measuring only in terms of the study or the briefing of it agreeing with what he already thought. Never mind what the quality was. But if the briefing happened to be against what he thought and he condemned the study, now you have the problem of facing, what was that study? But in general, it was done not by a client trying to interfere in any way.

I guess the other thing is that it wasn't always just publications. That's too much then like a university system, if you have it always that. That is a tangible way to think of it, and you can look at that quality and the review system that goes with it. But another was if people were growing, into the interdisciplinary project leader kind of system. When they relied upon a lot of the inputs, from other people, but <u>their</u> integration of it.... See, it wasn't original research necessarily, but their integration of it. The integration may be original; you had to carefully look to see that it was what he was producing as a project leader, not as a researcher inputting to it.

Collins: Yes, that makes it clear. Perhaps it would be best to move on to discussion of your activities as a division head, when you moved up to that.

Rumph: Okay. Now that was at first a difficult period, because in effect as a division head, I had to, not demand but command kind of respect for the job I was holding. Because here I was put in that position, and as I said, there was Bill Graham and I sort of--we weren't vying for it but the two were being considered. I tried my best. In other words I did everything in my power to try to work with Bill Graham as if he was a coequal even though I knew I had the final responsibility for something. That was my technique of wanting to be in a good working relationship

with him. But then you see, these other departments. And there were all sorts of little jealousies and so on that you could sense a little bit. Even Bob Buckheim was another case in point. But we got along pretty well. Yet he was an interesting case in that division. See, he had come up through I guess what was one time the missile division. I don't remember. It may have been. I don't remember where he'd been just before. He hadn't been there too long. He came in a little later. But he was very well-equipped to do just what Frank asked him to do.

Somewhere along the line when the so-called space age had come upon the country, Frank and others and some people even in NASA [National Aeronautics and Space Administration] thought there was a need for a useful book, paper, report, or something that would lay out the fundamentals of what's happening in space. Not very technical but at least technical enough that it could be kind of understood by the public, not completely but, you know, as much as you could. It really was a book that was called--I forget the title of the thing, but it was equivalent to the state of the art in space, where we are in space, see.

Collins: I think that was just called The Space Handbook.

Rumph: All right. Space Handbook, that's what it was, yes. Now Bob Buckheim was the most eminently qualified person to lead that, and Frank knew it and he picked him without my advice, but I didn't disagree with it. He just told me, see. But I would not have done anything different. And there were so many other people around who had been working on this, were very interested in contributing to this, and Bob Buckheim handled that pretty well. There was no jealousy, as far as I could tell, with other people thinking they wanted the principal's job. But they were so imbued with contributing, they just wanted to have a part of it, a piece of the pie. So with that attraction, he got--I don't know--maybe two dozen people who contributed parts of that thing. And I think it was one of the most successful things that RAND had done, for its purpose. Now it was not what I'd call an interdisciplinary kind of research project. Yet Bob Buckheim was eminently qualified for that. In that Handbook he didn't want to dabble with what the space policies ought to be for the nation. I don't think he did anything about that, see. So here was a case and here was a one-time project--and that incidentally was a RAND-sponsored project--that RAND paid for and all. It didn't get a contract with NASA or anyone. And I think it was eminently successful, and that was the greatest success that Bob Buckheim had at RAND.

Bob was at the time head of the aerospace department within

the engineering division. So I think he had some kind of a jealousy notion because of my job there, but he got unhappy after a while and actually he left. He left RAND a year or so after that Handbook was published. He went into academia. He went to Vanderbilt University I think as a dean of engineering or some such thing and later to some other college, I believe. I don't know even where he is now. I think he's in Arizona in some kind of research activity but I don't know what.

Now the engineering division as I had it--I don't remember exactly when I came into it; it must have been about 1960, I think. What I'm trying to lead to is, the engineering division was short-lived in that sense. The research council came into being about '62. I'm not sure.

Collins: That was late 1959, '60, I believe.

Rumph: The research council?

Collins: Yes.

Rumph: Well, okay, I don't know. But anyway my memory is that my time in the engineering division was not very long, and therefore I essentially agreed with what Frank did. He then wanted to emphasize, as I said earlier, the strategic program side of things. He took all these departments and made 12 of them and they were the resource suppliers in the matrix side of the organization. The engineering division as such disappeared.

Tatarewicz: You also had a mix in the engineering division of engineers and scientists. These were people who you wouldn't think of--I'm thinking of Kellogg's division.

Rumph: Yes, they were other physical scientists. The extreme of that was Harry Vestine who was an eminent geophysicist, and he had a worldwide reputation in geophysics, and he never was involved in anything but that when he was at RAND. And he was in Kellogg's department. I knew Harry Vestine very well. We even went on vacations together, he and his wife. He was in an international meeting in Moscow when some eminent scientist in the Soviet Union got up during this symposium, when he was introducing Harry Vestine and introduced Harry Vestine to the Soviet group there, to the whole group, including a lot of Americans and British and others: "Dr. Vestine is to geophysics what Newton was to gravity." So that was one of the highest accolade he could get. And yes, that's true. But they were physical scientists. Engineering is a physical science, too. And I'm not sure where that was, before the engineering division. It was associated

with something. Maybe all by itself even. I don't remember.

Tatarewicz: Then it was separate from the physics division.

Rumph: Oh, yes. The physics division. When the Latter brothers were here, I think both of them had been head of the department, Dick Latter for a while, and then his brother Al Latter was head of the department. They were strictly nuclear physicists. That department really survived on contracts with the Atomic Energy Commission, mostly, in the early days. Until Al Latter and Dick Latter left RAND. They were present when nuclear physics had not fully developed the state of the art yet, into the weapons.

Collins: I think we've about reached the end of that side of the tape and maybe we should stop here.

Tatarewicz: You've been very helpful.

Collins: Thank you very much.

Rumph: Good.

Collins: Thank you so much.