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ORAL HISTORY PROGRAM

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Richard H. Truck
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Feb 17, 1988
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Frick, Richard H. Date: November 12, 1987. Interviewers: Joseph Tatarewicz and David DeVorkin. Auspices: RAND. Length: 3 hrs.; 43 pp. Use restriction: Open.

After reviewing his upbringing, undergraduate education in physics, work on ballistic problems for the National Defense Research Committee during WWII, and work for Bell Labs for a short period after the war, Frick (b. December 9, 1916) discusses his move to Project RAND in 1946. He then describes various aspects of his nearly forty-year career with RAND, including contributions in the celestial mechanics field to selected studies, freedom to engage in personal research, and relations with USAF and other military officials.

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Interviewer: Dr. Richard H. Frick

Interviewers: Dr. David DeVorkin and Dr. Joseph Tatarewicz

Date: January 12, 1987

Place: RAND Corporation, Santa Monica, California

TAPE 1, SIDE 1

Dr. Tatarewicz: I should mention before we start that any time you wish us to stop the tape or pause at any point whatsoever, just say so.

Dr. Frick: Sure.

Tatarewicz: We'll begin, then--what we like to do in all of our oral history interviews is to begin with some biographical information. The biographical sketch that you gave us doesn't say anything about your childhood, and I'd like to know where you were born and who your parents were and what they did.

Frick: Well, I was born here in Los Angeles, California, December 6, 1916, and my parents were Mr. William H. Frick and Mrs. Olive Oliver Frick. They were both from Illinois, and moved to California before I was born, in about 1914. Initially I lived in the Hollywood area, and went to school there. The grammar school was Grant Grammar School on Wilton Place and Harold Way. I went to LeConte Junior High School, which was just south of Sunset from us, and finally to Hollywood High School. After graduating there, I attended UCLA and received both my Bachelor's degree and my Doctor's degree. This was from 1933 to 1937 for my Bachelor's degree, and '37 to '42 for my Doctor's degree.

Tatarewicz: OK, if we could just back up a little bit. Do you have any brothers or sisters?

Frick: Yes, I have one brother who is six years older than I am. Up until about a month ago, he was a radiologist over in Pasadena. He's now retired from practice, and is adapting to retirement at the moment. I have a half-sister by my father's first marriage. She is living in Rochester, New York at present.

She was born around 1892. She had an older brother I never knew. He died at an early age, and so I really never think of him as part of the family, but I guess he really is.

Tatarewicz: What was your father's occupation?

Frick: When he was in Illinois, he ran a general store. When he came to California, he had sold his store, and went to work as a salesman in a hardware store. He worked for a hardware store downtown, Cass Murr, I think it was, and then later went to work for the Hollywood Damerals Hardware Store there in Hollywood this later became the Dresslar Hardware. He was very much interested in tools and working with his hands, and he was very good at it.

Tatarewicz: Did you do a lot of reading as a child?

Frick: Yes, quite a lot.

Tatarewicz: What kinds of things?

Frick: Oh, mostly novels. We--well, the family invested in a set of the Books of Knowledge. I must have been about four or five at the time we got them, and I worked them over pretty thoroughly. The family tended to have, well, current books and novels that both my brother and I read, and I have to admit that because of my eyes, I'm a very slow reader. I have a--I guess what they call a lazy left eye, and as I'm reading, I don't scan well. The left eye will click in once in a while, and that sort of puts a stop to it. I've never felt it as an impediment to me--after all, when you read a scientific article or something, you don't really do speed reading on it. That is, something mathematical, you've got to stop and think about it, and so from that point of view, I never felt this was a great difficulty. Now, if I had been in something like English or something of this sort, or law, for instance, where you're expected to read endless things and very rapidly and very accurately, I would have been in big trouble.

Dr. DeVorkin: Did your interests naturally take you to physics in science, or could it have been something to do with your eyesight?

Frick: No, I think my interest really took me in that direction. That is, something in mathematics or science was the sort of thing that appealed to me far more.

DeVorkin: Can you identify an influence in your life or an experience that caused that to happen?

Frick: Well, I think probably some of the teachers I had in high school were very influential in this. I had this one algebra teacher who had a very peculiar idea about mathematics, namely, if you're going to understand mathematics you were going to have to work on it, and she assigned homework as if her class was the only class that you were taking, and you were expected to have it done the next day. Well, it was rough, but I think I learned a lot of algebra from her.

Tatarewicz: Did algebra come easy to you, hard, or medium?

Frick: I think geometry came easier to me initially. I think I tend to think visually. Algebra initially--well, trying to think conceptually, I had to learn that, and like everybody, you learn to do the mechanics of it and then all of a sudden you're faced with word problems. And nobody likes word problems, and yet that's where the use of it is, and I think what really impressed me was not so much algebra, though, as when I began to get into trigonometry and into calculus. Then all of a sudden you were able to do things that I didn't think you could do before. That is, you could solve problems that seemed impossible. And that was a very intriguing discovery. I think I'd been very fortunate in having a series of very good math teachers, right through from high school and through college, and in college there was Dr. Sherwood--I took practically all of my college mathematics from him.

Tatarewicz: Sherwood?

Frick: Yes, G.E.F. Sherwood, I think it is. And this algebra teacher--Miss Ruth Welbourne, I think it was. She was an old maid, but awfully good.

Tatarewicz: Did you start calculus in high school?

Frick: No, at that time, they hadn't initiated this business of introducing it in high school. As a matter of fact, at that time UCLA did not even offer it in the first year, which in retrospect was terrible, because you went into physics and they began to try to derive formulas, using calculus but not saying so. You began to get into these things where they were trying to let something approach a limit, and I'd look at it and I say, "Hey, but the denominator is going to zero here, what are you doing?" And it was a terrible disadvantage, in first year physics, because here you were taking college algebra and analytical geometry at a time when you really needed calculus. Well, they've changed that now.

DeVorkin: I'm curious as to how you chose UCLA. You were the first one in your family to go to college?

Frick: No, my brother--well, initially went to Caltech.

DeVorkin: Your brother did?

Frick: Yes. And again, in retrospect, that was a mistake, because he wasn't mathematically inclined, and he got into the first course in calculus there, and he just floundered, and finally flunked out. Well, he then went to LA Junior College and made up some credits, and then went to UCLA and he majored in psychology at that time. After he had graduated there, which I think was in --about '33. He taught in a private school back in Connecticut for a couple of years, and finally decided he wanted to go into medicine, and ended up going to Stanford Medical School, and graduated from there just before the war, in '41 or '42, and did his interneship at LA County Hospital, and then went into the Navy as a doctor for the duration of the war. After the war he came back and went into practice out in the Pasadena area, and has just recently retired from that.

Tatarewicz: Was there any counselling in high school to help you select a college? Your going to college--was that a foregone conclusion, or did that require some thought?

Frick: I was pretty sure I wanted to go to college, and I know --well, my dad had died by that time. He died in 1930, and I think my mother was very determined that we ought to go on and get further education, and it wasn't one of these things we were forced into. I think that's probably the worst thing on earth, is to force somebody to go to college that doesn't want to. She was working at the time, and was perfectly willing that we should go on and get our education. I know it was no small burden to her, particularly, medical school costs which weren't as high as they are now, but likewise incomes weren't either.

Tatarewicz: Caltech was more expensive than UCLA.

Frick: Yes, Caltech was, oh, I think it was already around four or five hundred dollars a semester at that time, whereas at UCLA, let's see, I think the incidental fee was $23.

DeVorkin: That's right.

Frick: And then you paid an extra four dollars for your athletic tickets for the entire year, and--

DeVorkin: Was there a chance, would you have wanted to go to Caltech if your brother hadn't gone there?

Frick: I think probably that deterred me from it somewhat, the fact that he had flunked out, although I always felt that UCLA did a very good job anyhow.

DeVorkin: That was the obvious school, as opposed to USC or any of the other schools?

Frick: I think so, yes. That is, I think a certain amount of the choice of course was financial. And at that time, I didn't feel that USC had the sort of courses I wanted or the sort of faculty. They had a good football team.

Tatarewicz: Had you of narrowed your notion of what kind of additional education you wanted to something in the sciences or technology?

Frick: Well, yes, I think I had, by that time. That is, in high school, they used to have these vocational guidance sessions once a year, and people would sign up on the basis of their interests, and they would have some representative of various professions come in and talk to the group that was interested in going into that. In the 10th grade, I thought I would like to be an architect, so I went to the session on architects, and they had an architect there that--well, if nothing else, he was very discouraging. It was along in the Depression, and he was pointing out, you'd probably have to work as a draftsman for ten or twenty years before you'd get to be able to do anything as an architect, and so on and so on. Well, that wasn't exactly encouraging. Then in my second year, I had taken chemistry, and I thought chemistry was pretty good, so I signed up for the session on chemists, and this was interesting. Finally, the third year, I had gotten into physics, and well, I don't think I signed up for the physics business, but it was more of a sort of general scientific treatment, and I think by that time, I was pretty well set that I wanted to go into physics. I'm afraid there were a lot of my classmates that hadn't made that definite a decision at that time. I think most people don't. But as it turned out, that's what I followed.

DeVorkin: We should move on to UCLA then?

Tatarewicz: Yes, unless you want to identify any particular teachers in high school or any other experiences that helped.

Frick: No. I think that pretty much covers it.

Tatarewicz: So you came to UCLA.

Frick: Around fall of 1933.

Tatarewicz: With an interest in physics.

Frick: Yes, that's right, and of course at that time, the physics department was fairly heavily weighted toward the acoustics area and optics. That is, Vern Knudsen was the chairman of the department at that time, and he was well known in the acoustical area. Of course, most of your first four years is pretty much a general physics course. That is, you don't really do that much specializing, and I think it's better that way. I went through the usual electronics courses and the--first of all, the two year undergraduate or lower division course, where you go through mechanics of solids, mechanics of liquids, electricity and magnetism and light, are the four general areas, and this gives you a pretty good background in--at least in physics as it was then. I don't know whether it comes close any more. But--

DeVorkin: Knudsen was there, and I know that Ellis and Delsasso and Kinsey were other names who were there.

Frick: Yes.

DeVorkin: I'd like to know, did you have courses directly with the professors?

Frick: Yes.

DeVorkin: So there were no TAs or--

Frick: No, TAs were a very minor part of the department then. Right from the start, as a freshman, the lecture part of the course was taught by oftener than not a full professor and at least an associate professor. Now, the lab sections, well, at that time, even there they tended to have professors in the lab sections, because they didn't have many teaching assistants. In my first year as a graduate student, I was a teaching assistant, and there were only three of us. Nowadays, the place is flooded with them.

Tatarewicz: Three teaching assistants out of how many graduate students?

Frick: Oh, probably we had--wait a minute, now. Yes, Dick Bolt, the (Richard) Bolt of Bolt, Beranic and Newmann in New York.

Waldo Lyon, who is down at the Naval Electronics Lab in San Diego. He was one of the senior scientists on the NAUTILUS when it transited the Pole the first time. Then there was Bob Leonard, who later became a member of the department as professor. There was an Ed Fricke, which caused no end of confusion. The last I knew of him, he was working for Republic Aviation in the East. Also there was Bob Krueger, who later was at RAND and started Planning Research Corporation. But I believe that was about the extent of the graduate students at that time. It made it very nice since you knew all the graduate students, and you knew all the professors, and they were accessible, which I thought was good.

DeVorkin: Was Joseph Kaplan there at that time?

Frick: Oh yes.

DeVorkin: What was his area, was he more in chemistry or was he in physics?

Frick: No, he was interested in the nitrogen afterglow in the night sky, the "knight of the light sky." There was, let's see--Kaplan, Kinsey, Warner, Edwards, Dodd, of course Knudsen and Delsasso, Norm Watson, I think he was probably an assistant professor at that time. Ellis, of course.

DeVorkin: Yes, Ellis. I'm curious about Delsasso. Did he go on later on to the East Coast or did he stay in this area?

Frick: No. He has a brother. The one at UCLA is Leo Delsasso, and his brother Louis Delsasso worked at the Aberdeen Proving Ground, and he was there at UCLA before I was there. I did not know him at UCLA. I met him once when I was on a trip, and stopped at Aberdeen.

DeVorkin: Yes, there's an interesting connection with rocketry, because he certainly got into rocketry, the other Delsasso did.

Frick: Yes, that's right.

DeVorkin: This was his brother.

Frick: Yes.

DeVorkin: What were your courses like? As you said, you had lectures and labs. To what degree did you feel you had a--was it a major laboratory experience, hands-on type department? Was there a heavy theoretical component?

Frick: It was pretty well balanced that way. I think I tended toward the theoretical end of it. I could find my way around in a lab, and without short circuiting too many things. Well, I don't think I've done any experimental work since I finished my thesis, and I've hunted for enough leaks in vacuum systems to satisfy me for the rest of my life.

DeVorkin: While you were there, at least I have a recollection from Daniel Popper, who came there as an astronomer a little later but I think also was there early on, and others that Samuel Herrick as early as 1940 gave courses in celestial mechanics.

Frick: Oh yes.

DeVorkin: And rocket flight, that sort of thing. I'd be very interested, I think we both would, to know what your contact was, if any with Samuel Herrick at that time.

Frick: Well, my only contact with him was after I became a teaching assistant, and this would have been in probably fall of '37, I was teaching assistant for three years. The teaching assistants were assigned the duty of handling registration over in Royce Hall each semester, and passing out the cards for people to enroll and so on. But the physics department shared a room with the astronomy department, and Herrick being the junior member of the astronomy department, and since they didn't have any teaching assistants, he was the one that was handling registration. I got to know him somewhat in those sessions. I didn't actually take any courses from him, because my interest in celestial mechanics and this kind of thing didn't start until I was here at RAND, but I knew him to speak to. I guess after his death his opus on celestial mechanics was published, and at the typical exorbitant price of hard backed books these days.

DeVorkin: I would like to know if you had any confirming recollection that he did teach celestial mechanics pre-war. From a standpoint of ballistics and rocketry. Would you have any way of knowing that, or any idea how we could find out?

Frick: Gosh, it's hard to put a date on that. No, I'm afraid I don't. Well, I left there in '42.

DeVorkin: Right.

Frick: And I don't remember hearing anything about it, but he could well have been working in this area.

Tatarewicz: You had had some exposure to at least rudimentary celestial mechanics in the context of physics and mathematics at some point?

Frick: Yes. In the course in classical mechanics that Kinsey taught, they got into a certain amount of Keplerian type motion and this sort of thing, although it wasn't really emphasized there. That is, it was just what was available in Ames and Murnaghan, the textbook that we used. Ames and Murnaghan. The green book there, just about the center of that shelf, that one.

Tatarewicz: THEORETICAL MECHANICS, Ames and Murnaghan.

DeVorkin: That's Ginn and Company.

Frick: Yes.

DeVorkin: OK.

Tatarewicz: At that time, did it catch your intellectual fancy at all?

Frick: Somewhat, yes. Although I have to admit, when I first came to work at RAND, and found out that they were working on the study of the world circling space ship, I thought" They're mad. This can't be."

Tatarewicz: Why is that?

Frick: Well, I think probably more from an engineering point of view of, could you build it? After all, the planets have been working pretty well for quite a while, so I guess that a space ship could, too.

DeVorkin: We should go back and identify how you chose your thesis topic, whether there was any decision to continue on in graduate school after you finished your undergraduate work. Why don't we start with that question?

Frick: Well, after I finished my undergraduate work, my feeling at that time was, I'd like to go on and get a Master's degree, and this was agreeable to the department, so I enrolled in graduate school. Well, this was '37, and as time went on, the international situation was getting kind of uncertain. I talked to some of the members of the department and said, "Look, how about my skipping a Master's degree and starting in directly for a Doctor's degree? And in the event that the war starts, or what have you, would it be possible for me to revert back and at least

get a Master's degree?" That is, I don't want to lose everything that I've done thus far. Well, that was fine with them, so I started out directly working toward the doctorate. I was still working on it when war was declared; I finished up about a year later, and with dispensation from the draft board and a few things like that too. It worked out very well, because I finished up and almost immediately went to work for some of the defense research organizations, and I know of a number of people who were there at the same time who didn't finish up their degrees. By the time the war was over, they had been out, they had a job, they were married and had a family, and there are very few of them that ever went back and finished up.

Tatarewicz: The urgency of completing the degree, the uncertainty of the U.S. entrance into the war and that effect on your career --did that influence your choice of topics? Were you led to choose something more doable?

Frick: Well, of course, that's always a problem in selecting a topic. You don't want to pick something that's a life's work, and you don't want to pick something that you can do tonight--but no, it worked out rather well. At the time I was trying to decide on a topic, I was sharing an office with Bob Leonard, and he had done his thesis on measuring the sound absorption in gases, carbon dioxide in particular, and he had his equipment there. At that time, the university had an exhibit up at the Fair on Treasure Island, and he was going up to run that exhibit.

DeVorkin: This is San Francisco?

Frick: Yes. So his equipment was just going to be sitting there, so he and Dr. Knudsen suggested, why don't you use this equipment to measure some absorptions of other gases and this type of thing? So I sort of inherited it from Leonard.

DeVorkin: This is acoustical absorption and dispersion.

Frick: Yes. That's right. It's in the range from--

DeVorkin: You have 20 to 100 KiloHertz, yes.

Frick: Yes. And this was on the system that I learned how to hunt for leaks in vacuum systems. It consisted of a tank about so big around.

DeVorkin: About two feet around.

Frick: Yes, and a magneto striction rod flush with the bottom

face, and a microphone which was mounted on a micrometer type motion, and you would simply start the sound and slowly move the microphone away, and see how the intensity dropped off relative to an inverse square field.

Tatarewicz: Then effectively the path through the gas from the oscillating face of the--

Frick: Yes, and the gas was sufficiently absorptive that you didn't worry about things that went over and reflected off the walls. If you had a gas with very low absorption, the system wouldn't work at all. You'd just get a horrible standing wave pattern, and it wouldn't tell you anything. So, in that sense, I'm not really sure whether I chose the topic, or whether it was chosen for me. I don't have any regrets about it. It was an interesting study, and there again, I tended to emphasize more the theoretical end of the absorption and dispersion problem, but with experimental results to back it up.

DeVorkin: It's interesting that that kind of absorption and dispersion work carried over in some of your NDRC work. You indicated that while you were still a graduate student, you did work part time on an NDRC project. Who was the Principal Investigator for that? Was it your thesis advisor?

Frick: Well, yes, while I was there at UCLA, we had a project under Dr. Knudsen, who was the chairman of my thesis committee. He at the time was spending most of his time down at what is now the Naval Electronics Lab. It was the U.S. Navy Radio and Sound Lab then. But there was this project at UCLA in which they wanted to investigate high intensity sound propagation in the atmosphere. Now this was at low frequency, that is, 500 Hertz up to a maximum of 4000 Hertz. They provided us with this bullhorn which is one which is used on aircraft carriers to talk to the pilots while they're in their planes and the engines are running. It's run by a one kilowatt amplifier, and the speaker is 50 percent efficient, so you have 500 watts of acoustical power. As a matter of fact, we cranked it up one evening, out on the athletic field near where Pauley Pavilion is, and we wanted to see how propagation was across a grass field. There was a large grass field there, and this was fine. We were checking the various frequencies. And about 11 o'clock in the evening, a very irate resident of Bel Air arrived on the scene, and it turned out that we had been propogating across the field, and the combination of the direct wave and the reflected wave was giving us a maximum that was probably focussed right on Bel Air.

Tatarewicz: How many miles?

Frick: Oh, about a mile or so. And this guy said, "I've been calling the police all night, and all they'll tell me is that they're on university property and we can't touch them. " Which I think means, "We don't want to do anything. "

DeVorkin: So what did you tell him you were doing, or did he ask, or just what--ask you to desist?

Frick: Well, about that time we were packing up to go home. Dr. Knudsen told us the next day, "I think you'd better work some place else." No, we ended up doing most of our work out on some of the dry lakes, up around Lancaster, Palmdale, or some of the lakes that Edwards Air Force Base uses now.

Tatarewicz: Did you have a large gas generator you took with you?

Frick: Yes. We had this truck with the generator, the amplifier, and the speaker in it, and there was a ramp in the truck that we could either aim the speaker straight up, out of the top of the truck, or we could bring it down and turn it and aim it horizontally, and for the vertical cases, we had to have balloons to pull the microphones up above us.

DeVorkin: It sounds like a rather sizeable operation. How many people were there?

TAPE 1, SIDE 2

Frick: Well, there were three of us working on the project, this Bob Leonard, who was in charge of the operation, and myself and a fellow named Dave Evans who was an undergraduate student, and he knew how to cook, so that was one of the reasons for his coming. He did a lot of the dog work on collecting stuff and seeing that the tires on the truck were inflated, all that sort of thing. But Bob Leonard and I were the two main investigators, and of course we were working under Dr. Knudsen.

DeVorkin: So as far as the balloons were concerned, these were tethered balloons that had microphones on them?

Frick: Yes. Also in the truck we had the cylinders of hydrogen to inflate them, and we never had any explosions, so that was good.

DeVorkin: Well, you had your thesis and the war was on. What were your plans, or were they determined for you?

Frick: Well, originally I was expecting to go down to the San Diego Navy Radio and Sound Lab. About a month before I finished up my thesis, Dr. Knudsen came around and said, "Look, would you be interested in going to a project that we're starting up back at Duke University? " It turned out, it had to do with sound ranging of artillery, so as far as I was concerned, this was fine with me. I felt that I knew more about that sort of thing than I did about underwater sound, which is what I would have been working on at San Diego.

Tatarewicz: You'd been working with gases, there'd be more application there than in the extremely dense medium of water.

Frick: That's right.

Tatarewicz: What was the relationship with Duke? You said Dr. Knudsen said "We-- "

Frick: Well, at that time they were setting up these National Defense Research Committee projects at the various universities. The one that we had there at UCLA, on sound transmission was an NDRC project. The main reason for setting one up this one at Duke was that this was a convenient location relative to Fort Bragg. The Field Artillery Board is located there, and they have acres and acres of space that you can use for experimental purposes, and so, Duke being only about 50 miles from there. This was a convenient and comfortable place to have your offices. When I first went East, I didn't go directly to Duke. I was up at Brown for a couple of months, because Dr. R.B. Lindsay was going to be head of our group, and he couldn't get away from Brown for a couple of months. So Izzy (Isadore) Rudnik, who's over at UCLA, the two of us went East and went to Providence and stayed there for a couple of months, and then went down to Duke after that.

Tatarewicz: Was Knudsen in some sort of a supervisory relationship? Was he helping to set up various labs in this general area of acoustics for NDRC?

Frick: Knudsen was, yes. Now, Lindsay was acting as head of one of the groups on this project, down at Duke. The supervisor of the project was J. P. Maxfield, who came from Bell Labs, and so we had an interesting combination of academia and the practical-engineers, and it was good fun, really. I hate to say this about the war, but I really enjoyed that job.

DeVorkin: It would be interesting to know what you enjoyed about it.

Frick: Well, the military had really done very little in this area, and so anything you did was a contribution. That is, there was no problem that you were going to spend six months working on something and suddenly found out, hey, that's been done ten years ago.

Tatarewicz: What did you think of that aspect, that the military hadn't really done much on this? Did you find that surprising? What kind of a picture of the military did you get?

Frick: Well, not really, because going back a little bit, when I was in my sophomore year at UCLA, I got to wondering "what am I going to do when I get done here?" So I went around to my advisor, who was Dr. Dodd, and I said, "Look, suppose I go through four years and graduate in physics, what will I be qualified to do?" I said, "I like physics, but from the point of view of supporting myself, what kind of a job could I look for?" Well, he said, "First of all, there are a few industrial laboratories like Bell Labs, General Electric," I'm not sure if he mentioned General Motors at that time or not, but he said, "Now, they're only going to take the top 1 percent, and then there are a few military laboratories--the Signal Corps and the Navy labs. They also don't take very many. And then you can always teach." And that was the end of the message. Well, at that time, I seriously considered transferring over into engineering. It didn't really make that much difference, because the courses I was taking would apply equally well to pre-engineering as compared with physics. And after about a year, I thought, well, I'd rather do something that I like to do, rather than switch over, and as it turned out, about the time I got out, there was the war research, and after the war, there was a much bigger demand. So I'm of the opinion that you ought to study what you like to and hope it will work out.

DeVorkin: The good experience you had, constructive, positive experience you had when you were at Duke, you were working primarily with civilian scientists and engineers?

Frick: On our project. Now, when we would go down to Fort Bragg, we had to deal with the military.

DeVorkin: And what was dealing with the military like at that point? Did they know what you were doing or why you were doing it? What kind of communication was there?

Frick: We had very good communication. That is, they were very helpful to us, and anything we wanted to do, that was fine with them, because anything was a step forward. Now, the thing is, in

peacetime, what we were doing would presumably be done by the Signal Corps. The Field Artillery and the Signal Corps and the Ordnance Department don't speak to each other. That is, they just don't get along. On the other hand, the Field Artillery got along fine with us. And so it was a very good relationship. As a matter of fact, we were down there one day on a field trip, and here we were out in the field with all this equipment, trucks, military personnel, jeeps and everything, and one of the scientists from the Signal Corps happened to stop by, and he looked around. He said, "My God, when we come down here, we're lucky if we get a jeep to get around the base with!" They were just leaning over backward to provide us with anything we needed.

DeVorkin: I'm beginning to wonder if this is one of the reasons for the success of the OSRD and the NDRC people, because they were coming in in a non-competitive way, working with each of the services independently, whereas the services couldn't work with themselves.

Frick: Yes. I think there's a lot to that.

Tatarewicz: Did you have any contact with the Signal Corps on this?

Frick: A little. I met a couple of their representatives there at Fort Bragg who were specializing in this sound ranging problem. I've been up to the laboratory at Red Bank. I wouldn't say we had a lot of contact with them, though.

DeVorkin: The project as it was stated to you, to get some sort of sense of how this was stated to you actually, was, if I understand it correctly, to determine the point of origin of an artillery shell from the acoustic noise created by the path of the projectile, but not necessarily an audible cannon in itself.

Frick: Well, that was what I was involved in. Now, the conventional sound ranging relies on the actual sound of the gun. You lay out a set of microphones in a line, and you have them all tied in to a recorder, and you get the arrival of the gun wave at each of these microphones. Well, this is enough to give you a reasonable triangulation. The trouble is, with long range guns, quite often the refraction of the atmosphere is such that you never hear the gun. It's refracted up, either because of wind or temperature gradients or what have you, and on the other hand, as the shell goes over, at least the longer range ones where you're getting supersonic velocities, you get a tremendous shock wave that is coming from points along the trajectory. The question was, well, can we make any use of that, since that's easy to

hear.

DeVorkin: That's right.

Frick: We developed a technique that you could get a reasonably good line on the gun, and then we made up a bunch of templates which, if you had an idea of what gun it was, that is, what kind of a gun, you could set these templates and say something about range from the impact point, to the gun. You could usually locate the impact point where the shell went off, and use the shock wave to determine the line toward the gun, and then if you could use these templates to get the range, it wasn't the sort of data that you would use to fire back at that point because your uncertainties were too much, but it was good enough that you could send photo reconnaissance out and look in a specific area.

DeVorkin: This is certainly true with a big gun that wouldn't be too portable. Yes, that makes sense.

Frick: Yes.

DeVorkin: So you were actually at that point, if I can be a little presumptuous here and assume that this was a contact with a ballistic trajectory, only from an acoustic sampling, you had to reconstruct a ballistic trajectory out of that.

Frick: That's right.

DeVorkin: And you created a graphical method for doing so.

Frick: That's right.

DeVorkin: Now, what was your actual role in designing this whole procedure? Where did you sit in the team?

Frick: I was the only one on it. I formulated the idea, and got the trajectories for these various guns, plotted them up, and went through the graphical procedure of determining these templates of the shape of the trace of the shock wave on the ground, as a function of what range the gun is firing at. It was a pretty cumbersome thing, that is, we had a stack of templates about this high--

DeVorkin: An inch and a half, two inches high--

Frick: Something like that.

DeVorkin: That's very interesting, so did you have to convince

someone, or did somebody assign this problem to you, or did you look for this problem and find it and say, I'd like to try this kind of solution using the trajectory of the projectile rather than some other form?

Frick: Well, I'm not sure I remember just how it originated, but it's an easy thing to see that the question would come up, because you look at these tapes out of the recorder, and here you've got these beautiful blips. Well, it turns out those are the shock waves. And way down here, you've got something that goes like this. That's the gun. And sometimes you're not really sure whether that's the gun or whether it's just an instability in the recorder, and so the question would always come up, couldn't we have used these nice neat shock wave arrivals to say something about this?

Tatarewicz: Shock waves are very precise. Their magnitude is large. I shouldn't say precise, they're short duration.

Frick: Yes. If you are out on the range and you're listening, and you hear the gun wave as sort of a dull boom, or kind of a thunder in the distance. But if that shell goes anywhere near over you, bang, it is loud and very sharp, and so the recorder gets a nice precise break on it, as to when the arrival was.

DeVorkin: I have to ask you at this point, how did you feel sitting down range when these projectiles were coming over?

Frick: Well, I never was in any particular danger there, but some of our group were out there on the range--late one afternoon, and they were doing some sort of measurements, and it turned out that one of the gun crews which had been firing into one of the other target areas knew that they were going to be in the same location the next day, but they were going to be firing into this target area where my colleagues were working. The gun commander decided that, just to be a little ahead of the game, he'd fire a few registering shots into the new target area that afternoon, so he'd be ready in the morning. Well, this one friend of mine who was there said he certainly learned the utility of a fox hole about then. But I was never anywhere close to it. I've been where I could see the shells landing, but as a matter of fact, it was very late in the project before I actually saw a gun fired.. We finally made a special request of one of the military. "You know, we've been around here for three years, and we'd kind of like to see them fire a gun." So they took us down to the gun position.

Tatarewicz: So you were doing the analysis, and you presumably

had other people gathering data for you, for the construction of these templates?

Frick: Yes. This was one of the reasons I had to go up to Aberdeen, was to get some of the trajectory data for our own guns and likewise for the German guns--there's the 175 millimeter, it's the equivalent of our 155 millimeter gun, and then there was a 210 millimeter howitzer--these were the two main guns that the Germans had. We made up templates for those, and then we had some templates for the 155 mm gun so that we could try the system out in this country. I had no desire to go overseas and test it out myself. But most of the data gathering and experimental business was up to me. Of course, the military would run the sound ranging equipment. Just present me with endless rolls of film.

Tatarewicz: Film? Does that mean that these were meters that were read by camera? Or did you have Brown reporter output?

Frick: It was a photographic recorder with a tape about maybe an inch and a half wide, and it had up to eight channels on it, which were tied into the various microphones, and this thing would grind through putting marks on every hundredth of a second, that is, just a mark across the tape. Then through the various channels you could determine what the time was at which the break occurred, and it ran through, recorded, went through the developer, came out, sort of a soggy line of stuff which dried off fairly rapidly. They'd wipe them down and put them in the sun, and it was fairly usable.

Tatarewicz: So some light source was being used to put traces on the unexposed film?

Frick: Yes.

Tatarewicz: And then it ran through an automatic developer.

Frick: Yes.

DeVorkin: It sounds like a very fast recorder, responsive to the time, much faster than a Brown recorder.

Frick: Yes. This--we used to tell ourselves we could read milliseconds in the time. I think we were kidding ourselves. That is, a hundredth of a second was about, I'd say maybe one and a half millimeters on the tape, so--but when you've got something that's curving up in the break, it's a little hard to really say, when did it really get there?

DeVorkin: Yes. But that's an interesting recorder. Was that a standard recorder?

Frick: Yes.

DeVorkin: That you easily acquired, or the military had available?

Frick: Yes, this was their standard M-1 recorder or whatever they say. That is, I'm not sure that a lot of development had gone on between the two wars. Or at least it was very small. And these things were extremely cumbersome. They had a plotting table there that it took six men and a boy to move it around. It was heavy metal with big graduated scales on it here, and an arm that would swing around, and you could move the pivot from one microphone position to the next, and so you'd say--not a microphone position, but they'd have two microphones and the azimuth was from the center point between them, so they'd put the pivot of this arm at that center point, and then they would swing it around to the appropriate azimuth, and draw a line.

Tatarewicz: So this was a custom plotting table specifically for this kind of acoustic direction?

Frick: Yes. And it required that you had exactly the right microphone spacing and so on. Now, in this day and time I'm sure they would computerize this to the extent that you could just plug in any microphone position and get your solutions.

DeVorkin: It sounds like a set experimental setup that an Army detail could be trained to use, that did not have any variation.

Frick: That's right.

DeVorkin: And so you could trust that they would follow those rules, and you design your experiment to fit that data, so to speak.

Frick: Right. Well, of course, like an awful lot of the military business, they had these nice forms, where you fill in such and such a number in this column, such and such a number in the next column, subtract column 2 from column 1 and put the result here, and these guys obviously didn't have the slightest idea what they were doing. One of them in particular was the chart for measuring winds. They'd observe these balloons going up, and they'd record the azimuths and time and so on. And they'd compute the ballistic wind from that. Well, it was very interesting sometimes to discover that at 5000 feet altitude, the wind velocity was zero

and its azimuth was due north. That is, they had to put something in the azimuth column.

Tatarewicz: There was no choice for unknown azimuth.

Frick: I think what they were probably told was, "If you don't have a wind velocity, put in North."

DeVorkin: Well, what was your reporting structure? You would write reports, and then submit them to who?

Frick: Our reports went, I guess--well, of course they went to the NDRC headquarters. I think that they also went to the Field Artillery Board, which was our immediate contact in the Army, and probably to the Field Artillery School in Fort Sill. I don't know whether they were sent to the Signal Corps or not. They probably were.

DeVorkin: When these were written up eventually, did you have anything to do with writing up the final reports for your division? Do you remember the NDRC division you were in, the numerical division ?

Frick: Oh gosh. I used to know. I was just wondering if I have anything around--I don't think I have anything around that would show it.

DeVorkin: Knudsen was one of the project leaders in the same division.

Frick: Yes, he and Harvey Fletcher. That is, yes, Fletcher would probably be the place you would find our division.

DeVorkin: I can reconstruct it from that.

Tatarewicz: How was your division organized internally? That is, the group you were with at Duke. What else were other people doing? Were there other people working in parallel with you, on different problems?

Frick: Yes, we had a physics group which was initially under Lindsay, and included myself, a Dr. F.E. White, who's back at Boston College. Well, he's retired now, and Izzy Rudnick who's at UCLA. In that division we were working on this shock wave detection. Rudnick was working on sound transformation from the point of view of how much variation do you get in a wave as it's being propagated, because if you're picking one point out here, and the wave goes by, how much uncertainty is there just in the wave

form? I think the British called it "cockleyness." And Rudnick was working on that. White was working on a nomogram type of construction which would replace this monstrous plotting board that they had, and it still was tied to the uniform spacing of microphones and all. Now, there was another group under Mike Burger who came from Bell Labs who were working on a thing called a "Dodar." This was for the Marine Corps. DODAR. And what it did was, you had a rather small box which contained all the electronics, and this was tied to a set of two microphones out here, spaced maybe a hundred yards apart, and two microphones out here, also a hundred yards apart. When the gun went off, you would get a reading as to what was the time interval between the arrival at this microphone and this microphone. On the left hand set. This would give you either a plus or a minus reading of so many milliseconds. I guess they had two meters on it, is what it amounted to, so they would get a time interval for one set of microphones and a time interval for this one, and then you'd do a very crude plot of this on a grid, where you had plotted these microphones, and you could make a quick triangulation of the gun position. Well, now, this I believe was taken into Guadalcanal, but my impression was that it didn't work out in that kind of fighting. That is, after all, getting a first order survey out in the battlefield is not an easy thing to do.

DeVorkin: Yes, that's right.

Tatarewicz: You would have to place these pairs of microphones pretty precisely in order to--

Frick: Yes, when you're talking about milliseconds, why--that's a foot.

Tatarewicz: Whose idea were these projects? That is, how were the problems given to your group and then distributed and defined?

Frick: My impression was that as far as the NDRC organization was concerned, we were simply assigned to assist the Field Artillery in the area of sound ranging. Now, I think most of the projects were probably self-generated within our group there at Duke. Needless to say, a certain number of them would result from discussions with the military, as to "Gee, I wish I had this," or "Can you do this?"

Tatarewicz: What I'm getting at is that I detect a similarity to RAND, in the sense that, the way you've described things, your group, the group of which you were a part, had a rather open ended responsibility to assist in coming up with various methods and techniques to assist the military.

Frick: Yes. I think that's true.

Tatarewicz: Within your own groups yourselves you would try to come up with some specific assignments, call for a device to do this.

Frick: Yes. Plus the fact we weren't hidebound by a lot of the preconceived ideas that some of the military had. That is, well, for instance, conventionally the military laid these bases out in equal spacings, and the old line officers, that's the way you do it. Well, about the beginning of the war, as a matter of fact, the Signal Corps came up with the idea that you would get better directional properties if you put a set of microphones in an equilateral triangle here, and an equilateral triangle here. Because with a straight line base, as you come around and begin looking down the length of the base, you don't have any precision at all to do anything.

Whereas with two sets of triangles, each one would give you a fairly uniform precision in azimuth, and so you take two of those and triangulate that way. Well, first of all, it came from the Signal Corps. Therefore, the Field Artillery would have nothing to do with it. Plus the fact that within the Field Artillery, there were the experts on sound ranging, and some of them were making a career of making this a mysterious procedure that nobody was intended to understand. We had no such compunctions, and as a matter of fact, we even talked some of the Field Artillery types into setting up some of these triangle bases, just to try them out. Well, they would do that, for us, they wouldn't do that for the Signal Corps. I'm sure there's a lot of this small empire building that goes on there. I do remember being down at Fort Bragg the day after the first atomic bomb was dropped, and there was one colonel in there in the Field Artillery Board, and his reaction was, "By God, I've gotta hand it to the scientists this time. " He says, "I don't think there's a man in Ordnance that could have done that!" And I think he's right!

DeVorkin: This experience with the military, how did it sit with you? Did you find them good patrons? People you liked to work for as a civilian in the future?

Frick: Some of them, yes. I'm afraid I don't think like a military man, though. But I remember when we were first going down to Fort Bragg. Our boss, Mr. Maxfield, got us all together, and he said, "Now, look--you're going to be going down to Fort Bragg, and you're going to be working with the military. There are two things that you want to remember. If you are down there and

you're out in the field, and you want something done, that is, you need some equipment or you need something moved, go to the highest ranking officer and ask him. Because he can then delegate this down the line. If you want some information, go to the lowest ranking officer, because if he doesn't know, it's no embarrassment for him to ask the next man above him, and if it gets clear up to the top man and nobody knows it, well, that's all right. But don't ask the highest ranking officer first, because if he doesn't know, he doesn't dare ask his junior officers." They have a lot of pretty hidebound protocol that I wouldn't like myself, but I got along with them fine.

Tatarewicz: Did you keep in touch with friends and colleagues who were doing other projects for other branches, other services, at the time?

Frick: Well, for the most part, the ones that I kept in touch with were--well, a few down at San Diego Navy Lab, this Waldo Lyon and there were one or two others there. But that's about the extent of it. That is, of course, Waldo has been there at San Diego longer than I've been here, so--

DeVorkin: Toward the end of your NDRC period, and especially after you heard that the bomb had dropped, and knew things were winding up, did you start thinking about what you'd do after the war?

Frick: Yes, that was one of the favorite occupations along about that time, back there, and--well, as it turned out, Ray Wegel, who succeeded Lindsay as head of our group, he was from Bell Labs at Murray Hill and he was going to be going back there. He was going to be in charge of a small group. It ended up that I went up to Bell Labs for an interview. Initially this didn't come to anything. Then when Wegel found out that he was going to be heading up this group, he arranged to make me an offer, and so I ended up going there. I finished--let's see, I think I finished at Duke along in September, and came out here for a month's vacation, and then went back to Bell Labs to work there.

DeVorkin: Before we go to Bell Labs, I want to know if you had any other options in mind or any desire to return here or any other job offers, or did you go to any other interviews?

Frick: No, as a matter of fact, I didn't. Given my druthers, I would have rather come back here. But this looked like a good opportunity.

TAPE 2, SIDE 1

Frick: Well, all the time I had been in school, Bell Labs was the Great White Hope for a research man, so I was delighted to get the chance to go there. It's the only job that I have ever gone into where I wasn't going into a new organization. That is, at this point, I was going into something that was very well established and as a very minor cog in the machinery. Well, actually I only stayed there about a year, not because I didn't like what I was doing, but immediately after the war, trying to find a place to live in New Jersey was almost impossible, and so I finally--well, the place that we were renting was sold out from under us, and it was getting pretty grim, and I didn't much like the New Jersey winters and a few things like that. So I wrote to Dr. Knudsen and said, "Look, do you know of any openings there on the West Coast ?" Well, it turns out that this Bob Krueger who was one of the founders of Planning Research was at Douglas, and they were just beginning to form Project RAND there at Douglas. Apparently either just before or just after Dr. Knudsen got my letter, Krueger stopped by to talk to him, to see whether he knew of any likely people that might staff Project RAND. So apparently Knudsen told Krueger Krueger had known me of course all through graduate school, and so he got in touch with me, and I left Bell Labs as a result of this. Harvey Fletcher did not take very kindly to this, not that I was all that essential to the organization, but it was about the time that Dean Wooldridge had pulled up stakes at Bell Labs and gone out to work for Hughes Aircraft, and Harvey Fletcher was a bit sensitive about these people that were picking up and going to California and depopulating his laboratory.

Tatarewicz: Would you have stayed at Bell Labs, had you been able to find a place to live?

Frick: I think I probably would have. Yes.

Tatarewicz: So did you find the intellectual environment and the work environment stimulating?

Frick: Yes, it was a very nice place to work. That is, you had all kinds of facilities. Now, I have talked to people here, they're not here any longer, they're retired, but they came here from Bell Labs, and their description of it, particularly in the postwar years, wa that it was a nice place to work, but they were sort of behind as far as any benefits and salary scale. As Ed Sharkey used to point out, "At Bell Labs there used to be a saying that, well, the work is hard, but the pay is low. "

DeVorkin: The work that you were doing on finding sources of noise in carbon filaments, that was obviously quite applied. You'd been doing applied research. What was your general feeling about the nature of doing this kind of directed research, as opposed to maybe searching out research interests of your own?

Frick: I think I would rather search it out on my own.

Tatarewicz: Was there a chance to do that at Bell?

Frick: Oh, I think that as time went on, probably you could. That is, I'm sure that nobody at Bell Labs ever told Bill Shockley to invent the transistor. I think he probably did something about that on his own.

DeVorkin: But for somebody in your position--

Frick: Yes, I was a very junior member, and it was going to be a while before I'd have the option to do that.

Tatarewicz: So this problem that you worked on, was this assigned to you as a well-defined problem?

Frick: Well, it was one that Wegel had been interested in for a long time. And I was supposed to build up a machine for measuring this noise. That was the place where I learned about designing amplifiers, because I was going to have to have an amplifier. And I had never done a lot of electronics work myself, particularly in the sense of building equipment. Well, one of the guys in the lab explained to me, he drew a circuit diagram, and said, "Well, why don't you put this together?" So I went down and got the components and soldered it all up, and brought it back to him, and said, "Well, shall we try it out?" "Wait a minute--" So he went down to the stock room, and got himself a handful of resistors and a handful of condensers, came back and plugged in the amplifier, and needless to say, it oscillated. So he took a resistor and he bridged it across some place and looked at the scope, and if that improved it, he'd solder that in place. He'd keep doing this until he couldn't do anything more with resistors. Then he took a condenser and he started doing the same thing. Finally it got to the point where nothing seemed to make it any better, and he said, "Well, I think that's good enough. You'll get used to it." And you know, as time went on, that amplifier worked fine. But so much for designing feedback amplifiers.

DeVorkin: OK, so I think we should move on to RAND, unless there's anything else you'd like to ask?

Tatarewicz: I'm curious to know who else came from Bell Labs to RAND for the future.

Frick: Well, let's see. The only two I think of are Howland Bailey. He's a consultant here now. And Ed Sharkey--he's retired. He's consulting for Northrup at present. He's been gone from here about, I'd say about five years. He stops by periodically, and I keep in touch with him. As a matter of fact, he was one of the co-authors on this one.

DeVorkin: Which publication?

Tatarewicz: THE TRAJECTORY AND ORBIT PLOTTER INSTRUCTION MANUAL, R-4-18- PR. For the RAND period--so Krueger was making the rounds of universities and institutions of his network?

Frick: I suspect at that time, Project RAND at Douglas had maybe a dozen people.

Tatarewicz: What date would this be? Internal note 21377-1, A Histogram of RAND Departmental Growth, 29 January '71, apparently Haydon, we have May '46.

Frick: Let's see, May '46 was the start. This would have been about September of '46, so it would be right in this first interval here.

Tatarewicz: So you're still in the loft at one of the hangars at Douglas.

Frick: Yes, just south of the big hangar--with riveting guns going just the other side of the wall. So OK, well, it's somewhere between four people and 49 people.

Tatarewicz: Between May and September of '46.

Frick: I think a good share of the people that were on board at that time were people who were already at Douglas, and had switched over into Project RAND. I guess I don't have a list of who was there at that time.

DeVorkin: Well, we can probably retrieve some of it from the authors of THE WORLD CIRCLING SPACE SHIP.

Tatarewicz: We have some of that in the interviews that we've been doing. This one happens to be Douglas Aircraft Co. Incorporated, Santa Monica plant, engineering division, presents

preliminary design of an experimental world circling space ship, report SM-11827, and then there's a contract, W33-038 AC-105, and it is dated May 2, 1946. Of the ones who actually continued on as part of RAND, although not immediately, Dave Griggs ended up being head of the physics department.

DeVorkin: He's the name we've heard of. He was in Washington.

Frick: Now, Ridenour I think was a consultant off and on, but not a member. Clauser, I don't believe ever was.

Tatarewicz: That's Francis Clauser, one of the twins.

Frick: Francis Clauser, yes. Glenn Peebles came with RAND later on, but not right at the beginning. I don't know Lagerstrom. Klemperer stayed with Douglas. He was the World War I fighter pilot for the Germans. Jimmy Lipp, who was the first head of the missiles division, and I think probably he was the first employee, that is of Project RAND, anyhow. Krueger, of course. Vic Sturdevant came later. A lot of these people participated in the Spaceship business but didn't actually transfer to Project RAND. George Grimminger did. He was fairly early in the game. Wheaton was always at Douglas.

Tatarewicz: The title is "Preliminary Design of a Satellite Vehicle" on the pages, which reflects, I think, the title of the project for the cover before it was changed to something else. This was the working title. Each individual section bears the primary author's name.

Frick: That's right. It's interesting, looking through this. I was looking at it before today and I was trying to check some of the numbers in their calculations. They put out a formula and they put down the numbers that went in, and I couldn't check them. I got out my hand calculator, and no way. Finally it dawned on me, they were probably using a slide rule. Remember?

DeVorkin: You wouldn't be able to get reproducible results with a slide rule to any significant degree. That's what you're saying.

Frick: Yes. And I got out my slide rule and tried it, and the uncertainty was just about your uncertainty in reading the slide rule, so I guess that was it.

DeVorkin: When you came here, is it appropriate to ask, what did they do? Did they give you a copy of that document and say, "Read it"?

Frick: Yes. Exactly. I realized I had a lot to learn at that point. There was--another thing I noticed in it was, being an aircraft company, they computed the motion of a satellite as to what was the satellite velocity necessary, and they had a satellite velocity for an eastward flying satellite, or a westward flying satellite, because they were used to expressing velocities relative to the earth. And inertial space is a much more convenient reference system.

DeVorkin: I'd like to explore that in terms of the people that were doing this work, that brought you in, apparently that got you to read that book. I'd like to know, did they have everyone coming in read that book? Or did they have something in mind for you?

Frick: I think that this was probably more true of those of us that came into what eventually became the missiles division. That is, initially there was the missiles division, there was the aircraft division, and what was the third one?

Tatarewicz: There was guided missiles, aircraft, communications and electronics, at some point. Military worth.

Frick: Yes, military worth was probably the third one at that time. Of course, the aircraft people were primarily carrying on the sort of thing that they have always done at Douglas, and the military worth was a relatively small group that were trying to out-guess the military, I guess, and then electronics--well, that started after I came. As a matter of fact, Arnold Mengel was the first member of the electronics division.

DeVorkin: When you were faced with this evidently aircraft-oriented set of trajectories, and from your background in physics, I'd be curious, what steps did you take to refamiliarize yourself, strengthen yourself in ballistics, knowing that you'd probably be working in some area of ballistics?

Frick: Well, actually, at that time, I didn't really get into the orbital end of things that much, so I was sort of willing to go along with what they had done. Initially I was supposed to look into guidance and control problems, that is, servo-mechanisms, this type of thing, and eventually into inertial guidance, but that didn't come until later.

DeVorkin: You did have your first published report, as a RAND publication R-136, was" The effect of missile dynamics on flight path. " I'd be very interested to know how you came to that particular problem. We haven't seen a copy of that.

Frick: I think, I should have gotten that out; I think it's here.

DeVorkin: Is this one, I'm curious to know what the missile dynamics were that you were dealing with, the constraints, where you got those constraints, the properties of the missile that you were working with.

Frick: Well, it was mostly a theoretical treatment, and not too much in the way of numerical values. Primarily, it's the sort of thing that is in Durand's five volume series on, I've forgotten, I don't know whether it's referenced in there or not.

DeVorkin: There might be a reference in the appendix--yes, W.F. Durand, AERODYNAMIC THEORY. That was published, as a matter of fact, that was a Galcit Report, aeronautical laboratory at Caltech.

Frick: Yes. And so, in that sense, there isn't a lot that's really new in there. It's just looking at what the implications are for missiles.

Tatarewicz: How did you get to start working on this? That is, you come out here from New Jersey, you show up for work the first day, you're handed WORLD CIRCLING SPACE SHIP to read as representative of what RAND does--what happens from then on? Are you sort of set loose to find your own problem, or do they say, "Here are the sorts of things we're working on and need help on" or how does it take place?

Frick: Well, hard to say. Now, I don't know. That is, it just sort of grew, I think mostly on the basis of discussions with other people, and I think a certain amount of it would be suggestions by the people in charge of the division. A certain amount of it is the individuals going to them and saying, "Hey, I think we ought to look at this." Usually, you get very little argument, which was--kind of the atmosphere that I was used to with the project at Duke, and not at Bell Labs, but the same sort of thing here at RAND. Of course, in the early days of RAND, I don't know whether it had started there, but certainly after RAND became non-profit, there was this policy that people were expected to spend at least 20 percent of their time working on whatever interested them. I don't think we have that policy now, at least, I get a time sheet that I'm supposed to put a project number down on, for my work. But I think in the past, this has paid off. I think a lot of good ideas have come out of this 20 percent time.

DeVorkin: That time sheet didn't exist in the beginning?

Frick: I don't think it existed while we were part of Douglas. I think this was something that probably Frank Collbohm, or some combination of Frank Collbohm and Dick Goldstein and John Williams--it sounds like the sort of thing Williams would have advocated very strongly.

DeVorkin: How close did you fee, what was your accessibility to these three founders, Collbohm, Goldstein and Williams?

Frick: They were always available. Most of my, I think most of my contacts were more within the missiles division. They tell the story we used to have a man who was in charge of making coffee, around here, and this was Mr. Wilson. Oh, he was probably in his sixties or thereabouts, and he used to make coffee downstairs and then bring portable urns up and spot them around. One day, he passed Frank Collbohm in the hall. He said, "Frank, this is the strangest organization I've ever worked for. It's the only place I know of where the president is called Frank and the man who makes the coffee is Mr. Wilson."

DeVorkin: Is this something you actually experienced, or you overheard?

Frick: I heard of it. I'm sure this actually happened. Frank was highly amused with the whole thing.

Tatarewicz: What was the physical nature of your quarters in the loft?

Frick: It was a typical aircraft set-up. It had a rather rough wood floor. Most of it was a bullpen, and at one side were these cubicles with sort of half partitions around them, and group leaders were assigned to those, and the engineering aides and junior members were out in the bullpen at a whole series of desks, everybody with a Frieden.

DeVorkin: And you had Friedens, you had no other computing facilities.

Frick: There was computing there at Douglas, which I think we had access to, but I think a good share of the computing for quite a while was done on the desk type Friedens.

Tatarewicz: What types of research materials, library facilities, documentation did you have to use as grist?

Frick: I guess at that time, we were tied in to the Douglas

library, and I'm not sure just how this was sorted out. Well, I'm sure we had access to any of the unclassified material there, but now, classified, I guess it depended on whether it came in directed to Project RAND, or to Douglas. I'm not sure how the bookkeeping went on there.

Tatarewicz: What about information from other companies that were working on missles? I know there's this consortium of several companies.

Frick: Yes, well, that was a problem, even with the consortium, and that's one of the reasons we eventually became an independent corporation. Regardless of all the arrangements and everything else, it was very difficult for us to go into North American and say, "Tell us all about your latest ideas." That is, when our paycheck read Douglas Aircraft Company, they just weren't about to say anything. So I guess it finally got to the point where it was obvious we were going to have to become independent. I'm sure that nowadays, we don't have that sort of problem.

DeVorkin: About this activity of going to North American or some other place and saying, "Tell us what you're up to, where are the areas where you'd like somebody to explore some problems," is that the sort of thing that you were beginning to do, not only with North American but with others?

Frick: Well, I'm not sure that we were asking them so much about, what are the problems that we can do for you. Mostly it was, we were looking for basic data on--design data. And well, to be honest, we weren't going to try to build anything.

DeVorkin: Right. In your later papers on the ICBM trajectories, you had a generic ICBM, 5500 mile range with an impact accuracy identified, and you pretty much kept that one particular characteristic through a good number of years. That's about all the data you worked with, am I correct in that?

Frick: Yes, I think that was kind of the--well, whenever they would talk about an ICBM, it was always 5500 nautical miles, 90 degrees range angle. We did look at the business of high and low angle trajectories, but I think that probably the 5500 mile was kind of the generic case that we were considering.

DeVorkin: But I am curious as to, in your early work here on guidance and control and accuracy of defensive guided missiles and working on your ballistic missile re-entry loads for parachute stabilized configurations, that's 1954. I'm interested generally in this progresssion of work. At first, you seem to be

doing a number of different things, working in a number of different areas, because you were also working on the preservation of tactical air combat potential in Western Europe.

Frick: Well, that was one of these interdisciplinary studies that were multiple--that was one of the multiple reports that were put out on that particular one.

DeVorkin: It included, though, you were also working on radiation level, calculation of radiation levels in the vicinity of contaminating material. Now, you were working obviously in several different directions, all toward the same thing, this was all aspects of a ballistic missile defense system, well, mainly defensive strategy here. How much of this came out of that 20 percent time, and how much of it would you say was directed, in assignment of problems?

Frick: Well, the tactical air force business, that was definitely an assigned sort of problem. The radiation levels, that was probably 20 percent time that I was--well, I just got interested in the equations involved there. I think it probably came out of the tactical study, in that we were looking at, they were talking about--oh, fallout problems, and so on, so I thought this might be somewhat of a contribution. The trouble is, we don't have enough experimental data about fallout to really know anything.

DeVorkin: That's true, certainly, then or now. But the progression of your work, I think, is interesting to follow, and also the flavor of it, the flavor of your research seems to be very heuristic, very general, looking to broad aspects of things. I think a very important work that you did was" the Graphical Determination of Ballistic Trajectories: Through Outer Space with Compass and Straight Edge."

Frick: Yes, that was the 20 percent business.

DeVorkin: But that 20 percent, I see you referenced that paper time and again in your later papers, when you were doing back of the envelope, if you will, advanced studies of such things as Westward, as some of the other types of ballistic things. These are papers that sort of point the way, in guidance and control, in the decoy studies that you did, range accuracy and satellite recovery, they all referenced that paper, one way or another. So I'd like to get a sense of what your philosophy of, how this reveals your philosophy of research. Do you like to work with pencil and paper, compass and straight edge?

Frick: It's good fun. I've gotten so that I will use a digital

computer now.

DeVorkin: You will. I see there is a terminal here in your office.

Frick: Yes. Well, it's strange. Back in those papers about the ejected objects from ballistic trajectory, now, this was back in the time when I had not really gotten converted to the use of a computer. You'll notice, there are approximations of formulas in there. And we went to a lot of trouble to do that, and in later years, when we checked it against the digital computer, it's a very good approximation, but nowadays, nobody does that. That is, well, I won't say nobody does it, because it's handy to do that sort of thing if you want to get an idea of the trend of something, as you vary parameters. Because a digital computer, it's point by point, and so, in that sense, that's handy. But if I were doing it now, why, I'd probably use the digital computer.

DeVorkin: What was the purpose at the time? Was this what your bosses were looking for? These very broad brush feasibility studies that could point the way to maybe more precise studies that could be done by computer or hand calculation?

Frick: Yes, I think that's probably true, that they were more interested in sort of the broad brush trend type of study. Now, it wasn't till along in the sixties that I actually began to get converted to the computer, and the reason for that was, they installed this JOSS system. It's a--stands for JohniAC OPEN SHOP System. I'm not sure about the second S. But it was the first interactive computer we've had around here, and the guys who developed that, it was developed here, had a remarkable feeling for the interaction of a computer system and the user. Like, I didn't have to know what was downstairs. I could teach myself how to use it, and there were a lot of people around here who were using JOSS long before they had an instruction manual. The method of using it was either trial and error, or word of mouth from somebody else, that, "Oh yeah, you can do so and so," and it's one of the most useful systems that I've run onto. Now, it has limitations. It doesn't have the capacity of the big machine. But that was my introduction to computing systems, and that really sold me, and so then later on, I learned some Fortran and went to the big machine entirely.

Tatarewicz: I was going to ask about that. Fortran existed, of course, in the early sixties. But you said that you started on the JOSS system, the JohnIAC system, even so. So you weren't ready for Fortran or it wasn't available to you?

Frick: No, I still had the feeling that there's too much protocol in this Fortran for me. But, well, in later years I've gotten used to it, so at least when I write some Job Control language, I don't look at it and say, "That's ridiculous."

Tatarewicz: Let's talk about your outside contacts, in the area of ballistics, I think in the mid-fifties. I'd be very interested to know, again, you mentioned Herrick, and others who were around, of course....I was wondering if, after you started working in this area, if you started talking more and having seminars or being in contact more with people in celestial mechanics, again, Samuel Herrick or Robert M.L. Baker, or people of that sort, or the people at JPL? What kind of contacts did you have?

Frick: We've had some meetings. I've been at meetings where Herrick was present, although I didn't actually have any direct contact. My nearest contact with Baker is, my son took his course at UCLA, but I don't know him myself. I've used his book.

DeVorkin: I'm interested in identifying any kind of connections that RAND would have had with these people at UCLA, or with other universities, in augmenting your expertise in celestial mechanics. Paul Herragot? or others who were big in orbit theory in the fifties. Did you become part of that community at any time, do you feel?

Frick: No, I don't really think I did. I've been to various meetings, particularly in connection with some of this gravity gradient stabilization. Some of the people involved in that. Not as much on the orbital mechanics, though. That is, for a long while, I'm afraid I was approaching orbital mechanics in a different way than some of the conventional ones, that is, I didn't start out with orbital elements, and that sort of thing, although I have gotten there just from a different route.

Tatarewicz: Did you read any of the literature, any of the journal literature in celestial mechanics?

Frick: Well, yes, to some extent.

DeVorkin: You have Moulton's CELESTIAL MECHANICS.

Frick: Yes.

TAPE 2, SIDE 2

Tatarewicz: What I'm after is, was celestial mechanics to you a tool that you picked up and learned when you needed to pick it up and learn it, or parts of it, in order to do something else?

Frick: Yes, I think you would put it that way.

Tatarewicz: Rather than doing something else because you're a celestial mechanician. You know, there's a difference in approach here.

Frick: I think it's more that I looked at it as a tool. There are some areas in it, oh, like the business of the equations for how do the orbital elements vary as a result of various perturbations and so on. I haven't gone into that in nearly the amount of detail that some of the other people have. That is, for our purposes here, it's kind of a case that our celestial mechanics is pretty much spherical earth, no atmosphere, on occasion we'll introduce orbital regression, and advance of perigee, and this kind of thing, but we're just taking the oblateness term, and that's as non-round as the earth gets, for our purposes.

Tatarewicz: Did you ever feel that in attempting to solve one of these problems that you'd been asked to or had chosen, that you were hitting the real limits of your knowledge and comfort with the subject, and needed help? And if so, who did you turn to?

Frick: Well, I think the one around here that--well, the two of us get along quite well in collaborating--is Ted Garber. He's a co-author on some of these reports; and he has a lot of background in areas that I'm not as familiar with. When we have worked as joint authors, it's been a joint operation, and I think I have some expertise in areas that he doesn't, and so it works out very well.

DeVorkin: When you needed his expertise, or someone needed yours, was there anybody who was worried about punching the clock, as to how much time you were spending on this project or that project? I'd like to know how it changed with time, if you could give us a scenario.

Frick: Well, as to time, in the dim distant past, nobody really paid any attention. There was a time back probably in the early fifties that they decided things are getting kind of out of hand, the administration doesn't really know what people are working on, and they instituted a sort of a very rough keeping track of time. Now, this has gotten more and more rigorous. At present, we every week get these time sheets, and down here, we put project numbers, and across here we put how many hours we spent on each

one.

Tatarewicz: That's for a two week period?

Frick: For a two week period. Now, I'm not sure how accurately we all conform to this, but it's a far cry from the 20 percent operation.

Tatarewicz: At the time, in the first few years, RAND had only one blanket contract with the Air Force, to which everything was going on, and it wasn't until perhaps I guess the mid-fifties, that there was even more than one contract number to bill time to.

Frick: That's right. Well, at that time, Frank Collbohm--initially anyhow--was against the idea of having multiple contracts. He said, "We have a commitment to the Air Force, and until we fulfill that, we aren't in a position to take on other contracts." Well, now I think Air Force is less than 50 percent of our total commitment and this does introduce a lot of bookkeeping problems. That is, the bean counters worry about how we charge the computer system. Do we pro-rate it over all the contracts, when some of the contracts aren't using it? Or how do we do this? And there are some fearful and wonderful solutions that come for this problem, most of which we don't like. The attitude of a lot of us is the machine's downstairs, it costs them just as much whether it doesn't turn over one bit in the course of a month, why do we get charged so much a CPU second for the work we do? Somehow it ought to be an overhead item, but thank God that isn't my problem.

Tatarewicz: Yes. But even when you run a blanket Air Force contract, what seems to happen, as RAND expands, and expands in numbers of people very rapidly, there are lots of individual research projects going on, and people are investigating lots of different things and calling on one another for help. Somehow or another, somebody is worrying about each one of these individual projects and worrying that they get done and that they get the appropriate help. There must have been conflicts of scheduling, conflicts of human resources, and I was just wondering about how that was handled in the very early days, and how it evolved in the first five years?

Frick: Initially, the structure that was being used was based on these various divisions or departments or what have you. That is, there was the missiles division. There was aircraft, all of these. And then, as time went on and they began to get these interdisciplinary studies, where a project leader would be

appointed and he would draw on people from the various divisions or departments or whatever the structure was at the time. Well, at that time, the power in the organization was with the division heads. He controlled the people under him. This poor project leader over here, trying to cut across lines, had absolutely no leverage on so and so in this division is supposed to provide me with certain outputs. Well, he didn't have any jurisdiction over that guy, other than that he was in his project, and the only jurisdiction he had was to go to the division head and convince him that this ought to be done. This was certainly unsatisfactory from the point of view of the project leaders trying to get a meaningful result out. Well, now the pendulum has swung the other way. Now the project leaders and program managers control the people, and they control the money, and the department heads--well, it's been said that the department head now is sort of an office boy who shuffles papers up there, and really has no jurisdiction over the people under him. I'm not sure that either extreme is the right way to have it, but right now, I would say that it's primarily the programs and projects that control the activities of people. And come salary review time, the department head collects the data, but the thing that carries weight is what does the project leader or program manager think of this individual. Well, you can see where the individual is, as far as what he's got to do.

Tatarewicz: One of the ways that RAND has been described, especially in the early days, is as a place where you were free to work on almost anything you wanted to work on. That is, you were hired in and then you just sort of looked for something interesting to do, and you did it, and by and large you were allowed and left alone to do that, in a place in which if you needed resources of any kind, you could get them. If you needed travel, you could get it. If you needed computing assistance, be it human computers or mechanical or electronic computers, you could get it. I was just wondering if you could, from your own experience, in the early days of RAND, tell me how accurate a picture that is, in the sense of how much direction you were given or how much freedom you had?

Frick: Well, I think in the early days, certainly that was very true. We are a lot more organized now. Now, I've got a small project. It involved 12 days of my time. And every week I get a document like this, which I don't entirely understand. It tells me all about how much has been charged on the project, and so on. Well, there are already eight of the twelve days are gone.

DeVorkin: This is called a Project Labor Status Report.

Frick: Yes.

DeVorkin: And it's in a large format chain printout on a sheet from some high speed printer.

Frick: Well, now, in the early days, a 12 day project, no one other than maybe the department head might know that you were doing something on this, why, nothing like this would be done. So that's one of the things about what the availability of a digital computer and a text processor and all that sort of thing does, because in fact, we get sort of annoyed once in a while. On the week that the payroll is run, if you try to submit a job, for computing, you have these long waits, particularly on printout, and the reason it will be long is, well, you know, here are a bunch of 50,000 line jobs that are being printed, all of which is the payroll.

Tatarewicz: And it's not the paychecks, it's the information.

Frick: It's not the paychecks. It's multiple copies of things that are going all over to various people in the organization. Well, I just put my printing on high priority and let it go at that.

Tatarewicz: It seems, in talking to you about comparing Bell Labs and RAND, one of the things that attracted you about RAND was the lack of explicitly tasked or highly structured assignments of problems. And I'm just wondering, to what degree you have had to guard your own professional commitments, chart your own course, and be very wary in fending off problems that you don't want to work on, in order to maintain that.

Frick: Well, I don't think I've had too much trouble that way. I have tended to stay away from some of these interdisciplinary things. Now, that Tactical Air Force study was one of them that I did get involved in, and even in that, I was able to single out a specific area which had to do with the coverage problems in bombing an air base. This was kind of a separate entity, which was useful to the rest of the project. On the other hand, I tend to stay away from the things where--well, I'm not a very good general. I don't think I want to try to make the decisions that a general has to make. And some of these studies get into that kind of thing, and I wasn't trained for that and I'd rather stay away from it. I don't think I particularly want to get into such things as, say, the State Department has to look at. I think, well, based on the publications there, I tend to stick with something where there's a specific problem that's subject to analysis--that is, mathematically--and see what I can do to solve

it.

DeVorkin: I'm interested in your feedback that you get from the work that you do, and the community for which or for whom the papers and reports are produced. Most of them were classified, many of them were classified Secret when they were done.

Frick: Yes.

DeVorkin: We'd be very interested to know how you felt working in a classified community, not being able to discuss these things with people who were not in that community.

Frick: Yes, that's true. Well, I haven't had too much difficulty that way. Of course, I don't think that you get as much prominence or what have you, since it doesn't go into the open literature. That is, well, now, for instance, Izzy Rudnick at UCLA has recently been nominated to the National Academy of Science.

DeVorkin: I knew him from his textbooks and that sort of thing.

Frick: Yes. Well, Izzy has done a very good job, and I'm all for it. I don't think this would happen to somebody working in the classified area, because they just aren't that exposed to the -- well, to that many people. I'm not sure I would have gotten into the National Academy of Science anyhow, so that's all right.

DeVorkin: But it is an interesting question. Did you see it as an acceptable trade-off to you? You were given the facilities, the wherewithal to do something that you found interesting and exciting, but it was a trade for that. Did you see it as such?

Frick: Not really. If the job is interesting, that's good enough.

DeVorkin: What kind of feedback did you get within RAND or from patrons, coming back to you and asking for more, for clarification of the document? I'm curious, in one case, I was looking at the publications provided here, and on the ejection of objects from an IBM satellite, RM-1701, it said in the front, "The distribution restrictions here were not suitable for distribution in some manner or form," and I'd like a description of what that means.

Frick: Well, what happened here, on this one in particular, was, at the time this was put on it, it originally was Secret, and I received a call from a guy, I think it was Martin in Denver or some place like that, a guy wanted to reference this in a report he was writing. And he said, "Can you get it declassified?" I

said, "Well, I'll see what I can do," so I got in touch with one of the local Air Force officers and asked him to review it for declassification. Yes, I guess it had already gone down to Confidential at that point. Well, he looked it over, and he came back, "You know, I just can't see letting this go into the open literature. Why don't we make it For Official Use Only?" Well, that was worse than nothing, because it will never get out of that. And so--well, as a matter of fact, I talked to Malcolm Palmatier when we were getting that form set up, and he's going to see what he can do to get rid of this. It's one of these things that they have a big backlog of stuff in publications that should be reviewed and declassified, so he just put this on the list, and we'll see, because if you're going to assume that the Russians can't apply Newton's Laws to simple problems, why, I think we're in bigger trouble than I would like to believe.

DeVorkin: So your audience for this of course was technical people in the military as well as other contracting agencies, large industrial aerospace firms.

Frick: Yes.

DeVorkin: The people had security clearances that could read them, and you did get feedback from them, asking you about them.

Frick: As a matter of fact, on this particular one, there was a group of people--

DeVorkin : That's RM 1701.

Frick: 1701. There was a group from here that went over to Lockheed, within the last month or so, and Lockheed was giving them a presentation, and they flashed a slide on the screen which showed --wait a minute--showed this on page 14 of RM 1701, this business, and somebody in the RAND group asked them, "Where did you get that?" "Oh, that's some work that was done by R.H. Frick at the RAND Corporation, back in 1956." Well, all of these guys that were there are people who had come into RAND within the last year or so. They were a little bit shocked that something 30 years old was still in use.

DeVorkin: We find an awful lot of graphical data, data presented graphically in your articles, while in the WORLD CIRCLING SPACE SHIP I saw very little of that. Is this a hallmark of your own work? Do you do your own graphical work?

Frick: As a matter of fact, in this, yes. I find that well, I'm not good enough with a drawing pen any more to compete with the

reports department.

DeVorkin: You take a certain pleasure in producing those kinds of materials.

Frick: Yes. Some of the work in the orbit plotter manual, was done professionally by one of the gals up in reports. At that time, she was--I think she was in her sixties at the time. She was a German. She had come up through the apprenticeship line over in Germany. And she was good. She said that when she was an apprentice, she spent two years just drawing with pencil. They wouldn't allow them to touch a pen for two years. And then only very simple things. And she's long since retired, though.

Tatarewicz: It's getting about 12 now--perhaps we should bring this session to a close.

DeVorkin: Yes. ....We reserve some time at the end of each interview for you to ask yourself a question. In talking we may not have asked the right questions. We've covered an awful lot of ground, from your early training up until your joining RAND, and some of the early atmosphere and a few things about projects later on. I was wondering if there's any area, especially regarding the early years at RAND, that you think we should pay particular attention to, that you feel strongly about?

Frick: I think we should have stopped off there. OK... Now, let's see--

DeVorkin: It was, whatever you think it's important for us to know, especially about RAND as a place to work.

Frick: Well, let's put it this way. If I didn't like it as a place to work, I wouldn't have stayed forty years.

DeVorkin: Did the atmosphere here change in the Sputnik era, just around Sputnik? You were of course working on all these "what if?" scenarios, the coming ballistic missile system, that was more or less operational at different levels. You were looking at problems that ranged not only at first with the delivery and use of decoys, but later on, satellite recovery systems, so that that might have had something to do with reconnaissance.

Frick: Yes.

DeVorkin: And the study of the large aperture. This is something, again, another paper that--"Fuel Requirements for Attitude Control of Large Orbiting Apertures."

Frick: Yes, this had to do with some of our radar people who were thinking in terms of big disks in orbit, and what do you have to do to control them, and so on.

DeVorkin: What I'm trying to say is that, with the coming of Sputnik, planning must have changed, in terms of time frame, time scale. Things became more rushed. Was there a reflection of that here at RAND?

Frick: Not as much, in the sense that we're a little bit removed from the actual production kind of thing, and I'm sure that some of the companies who were involved in quick trying to get something off the pad in orbit, and so on, they were a lot more rushed. I wouldn't say that it affected us that much. Now, it affected me at home a little bit. My kids would come around and say, "How come the Russians are doing all this so well and we aren't?"

DeVorkin: How did you answer them?

Frick: Well, I told them "We're doing our best." But--

DeVorkin: You could have told them a lot more, but there was--

Frick: Yes. And it was a little hard to tell them how well we were doing, when on the front page of the paper would be something toppling over on the launch pad.

DeVorkin: Exactly. Did you ever have a feeling, when you were doing these advanced feasibility studies or looking for avenues to get a better control over some of these very technical problems, did you ever have the feeling that you wanted to carry the project through a little farther than the feasibility study and maybe carry it through to a requirement study, and posssibly even get involved in production--you know, see some project through completely?

Frick: No. Not really. Over the years, we've had this problem though with people, of the guy who comes to RAND and he wants to get away from the aircraft company, he's tired of all this hardware business, and so he comes to RAND and he begins doing these feasbility studies and so on, and pretty soon, his hand begins to itch for a screw driver or a soldering iron, and he goes back to the aircraft company. Well, no, I've never been in an organization where there was actually a piece of hardware as the output product. It's always been a report.

DeVorkin: So you've never been in that position of seeing something through to a hardware production aspect.

Frick: No.

DeVorkin: And you didn't miss it, then.

Frick: No, it didn't bother me in the slightest. Now, if I had stayed at Bell Labs, possibly I might have gotten into such a position, although even there, if something goes into production, it goes to Western Electric, so the labs as such don't really manufacture anything. No.

DeVorkin: Well, I think that's about it.

Tatarewicz: I guess it's an appropriate arbitrary place to stop. Until the next time, we thank you very much.

Frick: Well, I know I've enjoyed it.