Dr. Olaf Helmer
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Interviewee: Dr. Olaf Helmer

Interviewer: Martin Collins

Date: June 3, 1994

Place: Montecito, California

TAPE 1, SIDE 1

MR. COLLINS: To begin, if you could just describe briefly your family background, where and when you were born, and then the course of your educational activities.

DR. HELMER: I was born in Berlin in 1910. I studied at the University of Berlin and got my doctorate there in 1934. Left Germany immediately after I got my diploma. In fact, my diploma was sent to me after I left, for obvious reasons, because I'm what they used to call a non-Aryan. My father was Jewish, but he had died long before then.

I went to London and spent a couple of years there, got another doctorate degree there. My first one in Berlin had been in mathematics. The one in London was in logic. I then, in 1936, came to the United States. I had tried to stay in Great Britain, but as a foreigner, I was unable at that time to get a labor permit. So I left with a little support from an international organization helping German refugee scientists; I forget what it was called. It was headed by Edward R. Murrow.

So I spent a year on a visitor's visa in the United States, trying to locate some chance of doing some work here. Finally, with the help of some friends, I got an appointment as a research assistant at the University of Chicago for one year, on the strength of which I went back to England and got an immigration visa, came back, and then spent a year at Chicago. After that, I spent three years as an instructor in mathematics at the University of Illinois in Urbana, then went to New York and spent three years there at what was then New York City College. Now it's New York City University, I guess.

COLLINS: Let me interject here and back up just a little bit. What was your father's occupation?

HELMER: My father was an actor on the German stage. There was something rather unusual about him. His primary preoccupation in his free time was to study mathematics, in which he was very interested. But he also, before he became an actor, at the insistence of his parents had to get a university degree. So he found out that the quickest way of getting a degree was in the field of law, so he studied law, and he was probably the only actor on the German stage who had a doctor of law degree. He acted mostly in Shakespearean roles, Ibsen, Strindberg, that sort of thing.

COLLINS: Did your mother have a career outside the home?

HELMER: No, she did not. My father died in 1925, when I was fifteen. We kind of struggled through the many years that I was in Germany, but I somehow managed to get myself a university education there. Incidentally, at that time, when I was a student, I met John von Neumann, who was then an instructor at the University of Berlin. Of course, I met him again later when he was a consultant at the RAND Corporation, and I got to know him quite well at that time. But that's sort of anticipating things. Maybe we can talk about that later.

COLLINS: At Berlin, and then at London, who were your principal mentors or influences with respect to your education?

HELMER: In Berlin, of course my main field of study was mathematics, and I don't know that the names mean anything to anybody, some of the mathematics professors there. I also took some physics and philosophy. For instance, I had a course with [Erwin] Schrodinger, who is quite well-known, as well as with Hans Reichenbach, who later taught at UCLA.

I don't know if this is relevant, but I took a course with von Neumann at the time in foundations of mathematics. That was a rather historical occasion, because halfway through the semester he came in one morning in his class and said he was going to discontinue everything he had been doing up to that point, which was a general theory of axiomatics and that sort of thing. He said he had just the previous evening received a manuscript from a young Viennese logician by the name of [Kurt] Gödel, and [I don't know if you're familiar with that, but he said] he was going to devote the rest of the term to discussing the results of Gödel's paper on the impossibility of proving the completeness of mathematics. He established that in any mathematical system there are always some propositions that cannot be proved, or they cannot be decided either one way or the other.

COLLINS: What was your area of mathematical concentration then in Berlin?

HELMER: It was partly in foundations of mathematics. I was also interested very much in number theory and in functions of a complex variable, and for a number of years I was not concentrating very sharply on one specific area. I was just very much interested in the general field of mathematics, so I got a pretty well-rounded education in mathematics. But I finally wrote my doctoral thesis on a subject in the foundations of mathematics, on the axiomatics of geometry.

COLLINS: Did this follow on the work of [Alfred North] Whitehead and [Bertrand] Russell?

HELMER: Not at all.

COLLINS: When you went to London then--

HELMER: I went to London. There I became a student again. In fact, if you might want to call it that, I integrated Bedford College, which was the women's college of the University of London. But as a graduate student, I was allowed to register there. I went there because I wanted to study under Professor Susan Stebbing, who was a well-known logician. I studied under her and finally wrote a thesis on some subject related to Russell's antinomies.

When the time came to decide who should be my examiners for the final doctoral exam--they have a rule there at London University that one of the examiners has to be an outsider to the university, not one of the professors at that university--I discussed with Susan Stebbing who might be the other person who should be invited, and I suggested Bertrand Russell, since I was, after all, writing about some of his stuff. So she thought it was an excellent idea, and she phoned him. He accepted, and I had a very pleasant doctoral examination sitting in her office, just talking for an hour or two about the thesis. It was very informal and very pleasant. I met Bertrand Russell again later in Chicago, but that was the first, very pleasant occasion.

COLLINS: How did you end up at Chicago, then?

HELMER: In Chicago, I was a research assistant for a year to Rudolf Carnap. [I don't know if that name means anything to you.] He was another German refugee who first, I think, went to Princeton for a while, then went to Chicago for a few years and finally wound up as a professor at UCLA. I had met him briefly once in London when I was still there, and he was instrumental in getting me an appointment at Chicago, as I said, on the basis of which I was able to apply for an immigration visa.

COLLINS: You were at Chicago and the University of Illinois through the wartime, is that correct?

HELMER: I was at Chicago from '37 to '38, then at Illinois from '38 to '41, and then I switched to New York City College from '41 to '44.

COLLINS: Was there any difficulty in being a German in America during this time period?

HELMER: Not particularly. It was difficult for me to adjust to the teaching style and the prior education level I had to expect of the students. I mean I was somewhat amazed, when I first taught mathematics at Illinois, that many of the students didn't know how to add one-half and one-third. So I had to start at a very much lower level than I had expected to, but I soon got used to that.

COLLINS: After 1944 in New York, what took place then?

HELMER: In '44, I spent one year sort of as a split personality. I was living in the Village in New York. I was teaching a course at the New School for Social Research, and every week I spent half my time in Princeton as sort of an advisor to a fellow who was very much interested in pursuing some logical studies of his own, a man by the name of Paul Oppenheim, who was a chemist refugee from Germany, too, very wealthy, and happened to be a good friend of Albert Einstein's, whom I met in Princeton at that time, too.

COLLINS: As war progressed, and you were looking to the postwar period, how did your professional opportunities present themselves?

HELMER: I forget how this came about, but I was somehow put in touch with John Williams, who later was the head of the math department at RAND. He ran a study group during the war called the Princeton Statistical Research Group, which, however, despite its name, was located at Columbia University. I was introduced to him, and he hired me on to join his staff. During the last war year, we worked frantically, sometimes all through the night. I remember one occasion where we worked steadily for about at least thirty-six hours, occasionally taking an hour to sleep on the couch there in the office to recover. It was pretty intensive work.

COLLINS: So you were part of this project at Columbia?

HELMER: That's right. Frederick Mosteller was one of the group. He was also a close friend of Williams and occasionally a consultant at RAND. He's now a retired mathematics professor at Harvard. Another person who was in that group and joined RAND later was Cecil Hastings. He, in the early days, headed the computer department at RAND. It was later taken over by someone. Let me see if I can remember the name. It will come to me later.
COLLINS: So you were recruited by John Williams?

HELMER: This might interest you, too. John Williams, after the war, in 1946, early '46, was recruited by the Navy to work at Inyokern in the Mohave Desert. It's a research outfit there. He went out there, and he asked me to join him. About a month or two after he came to California, I followed him. I was at that time getting a divorce from my first wife. I stopped for the required time in Reno to get a divorce and then came out to California.

But while still in Reno, I received a long letter from John

Williams saying that he had been recruited to come to a new organization called RAND, the RAND project, which was then run by Douglas Aircraft, and he was not going to Inyokern, but instead proceeding to this place, and gave me a long description in a letter labeled "secret" on what this project was all about--and would I be willing to join him there? So that's what happened. When I came here, I did not go to Inyokern, but instead to Santa Monica.

COLLINS: So you were, in essence, in at the ground floor.

HELMER: Yes. I joined RAND a couple of months after it was started by Douglas Aircraft in 1946.

COLLINS: Were you hoping during this time period, as you became more acclimated to American research universities, to get a post as a professor? Was that an option you were looking for?

HELMER: That was certainly one of the possibilities, that that might come about. But I was just in those days very uncertain as to my future. It hadn't been that easy to get a job of any kind in the early forties. I was, on the whole, really rather pleased with this development because I was more interested in doing research than teaching, although later on, if I may anticipate, I did return to teaching for a brief time. I wound up my career as a professor at the University of Southern California and also doing a stint at IIASA (the International Institute of Applied Systems Analysis). In fact, that was my last job before I retired, except for a bit of consulting at the Naval Postgraduate School in Monterey, but that was a very minor assignment, very part-time.

COLLINS: Describe, then, your initial introduction to RAND and the sense of the possibilities of the organization as you saw it at that time.

HELMER: Of course, the first two years we spent in a rather frantic environment at Douglas Aircraft in offices attached to the big hangar where they were building aircraft in Santa Monica. [Frank] Collbohm was one of the Douglas engineers that had been assigned to take over this project, and several other engineers from Douglas were in prominent positions there. I'm not sure that I recall all their names. I think [James] Lipp was one of them, who later ran the missiles division at RAND, and--what was his name? Raymond?--something like that--who was the head of the aircraft division later at RAND. I don't recall now.

The group then was quite small. I don't think there were no more than about six or eight people, plus some clerical help. I remember, in the very early days Cecil Hastings was the computer specialist, and a couple of other people were trying to use the computers at Douglas Aircraft. They sort of converted them from their primary use, which in those days was mainly for keeping track of personnel problems, salaries, and so on. They wanted to use them for scientific purposes, which was entirely new to the engineers at Douglas at the time. In fact, they kind of frowned on that, but they finally conceded that maybe if we didn't use too much of the computer time, we could use some of it.

In those days, I think the emphasis was on what is now referred to as systems analysis. In fact, the whole term "systems analysis" was invented at RAND in those early days, and the first systems-analytical study was concerned with rocket flight, circumnavigating the earth with rockets. As a mathematician, I was involved in this on the fringes only. I can't really claim that I was centrally involved in this first systems analysis, but I had something to do with that. As far as I recall, that was really the primary preoccupation in those first two years while we were still at Douglas Aircraft.

COLLINS: Did this intellectually represent a kind of transition for you? It sounds like your academic background was very much rooted in very fundamental theoretical work.

HELMER: Oh, yes, it was, very definitely, and this was really a new departure for me. I was perhaps even naive in this respect, in that I wasn't really quite aware of the possibilities for applying mathematics to a large range of subjects, and I have a feeling that wasn't so unique. It was really pretty prevalent. Many of the mathematicians under whom I studied certainly conveyed the impression to me that mathematics was mostly something that you did out of intellectual curiosity in order to teach future students the same sort of thing. Of course, I was aware of applications of mathematics to problems in physics, but that it could be applied also to other areas very effectively, such as economics and social problems and so on, that was relatively new. In particular, of course, as RAND grew up, it applied mathematics to many areas of problems in warfare.

COLLINS: You mentioned that you worked with Carnap, and one of the things that he was associated with is logical positivism and unity of the sciences, where there was this interest in being able to explain all phenomena, or at least tie all phenomena to kind of basic mathematical and physical notions. I'm wondering how that figured into your own conception of it.

HELMER: I'm sure it influenced me a great deal. Yes, there's no question about that. In fact, in those early days I was at least as much interested in problems of logic and the logical foundations of science and epistemology as I was in mathematics proper. It undoubtedly influenced my career at RAND, too. I'm sure of that.

COLLINS: Just looking at your publications list, as I was able to go to the RAND library and print out at least a partial bibliography of your activities, one of the first things you appeared to work on was something called the jeep problem.

HELMER: [Laughter] Yes.

COLLINS: I wonder if you might describe that activity a little bit and how you saw it tying into what was going on at RAND at that time.

HELMER: That, of course, I think happened after we were separated from Douglas Aircraft and after the RAND Corporation was established as an independent corporation. Let me say something quite generally first about the situation there. When RAND was set up--I think at the instigation of General [Hap] Arnold, primarily, and I think General [Curtis] LeMay had something to do with that, too they wanted to keep together a group of scientists who had been working for the military. They wanted to make sure that that kind of body of knowledge was not being lost.

With that in mind, the assignment given to the RAND Corporation was a very loose one. It was to think about problems that might be of general interest to the future of the American Air Force, without any very specific assignment as to what we ought to look into. It was just a very general statement to the effect that we ought to think about what we thought was important for the future of the Air Force. So that established a climate which, I thought, really promoted a good deal of independent thinking.

RAND differed very much from other organizations that were given specific assignments. There was an atmosphere at RAND which was even freer in some respects than what you would find at the universities. There was a spirit of cooperation in the early days which was quite remarkable. It was sort of an open-door policy, literally. If you walked along the corridors at RAND, you found that most of the doors were open, and you felt free, if you worked on some particular problem, to walk into any other office of some colleague and say, "I have a problem here. This is of interest to you. Would you like to work on this, too, or do you have any input to provide?" It's the sort of thing that, as I say, even at the universities you don't find too often.

The result of that was that a lot of projects were started at RAND, the results of which were then communicated to the Air Force, who had never heard of this before. Some of the RAND people gave presentations that this may be something that you ought to think about, because these may be developments that you ought to foresee that might take place in the years to come that may affect the whole structure of the Air Force and the way the military machinery should be run.

It may be an exaggeration to say that we dreamed up our own projects, but we had a pretty free rein as to what we wanted to think about, what kind of research we wanted to do. Sometimes it might be very central to the problems of the Air Force. Sometimes it might be just very ancillary and with only a faint hope that it might eventually show some utility.

I really forget how this particular project of the jeep problem came up. As I recall, I don't think we had any delusion that this might be of some fundamental importance. In those days--let me think. Of course, the ranges of aircraft were not anything like what they are today, so if you were considering the possibility of carrying on military operations at some very distant point, you had to consider the need for establishing intermediate supply depots in order to supply the aircraft operating at the very front.

I think that must have been the idea behind developing the jeep problem, as to how one could most economically supply the front line by establishing depots, and the question was, where should they be established and what should be the schedule of supplies, the whole logistics of the problem. So that's how this came up. It's no pretense that this was, even at the time, thought of as being a very fundamental problem. It was sort of a tricky mathematical problem, and I was intrigued by it, and I tried to solve it and, in fact, succeeded. But it wasn't any fundamental breakthrough.

COLLINS: I wonder how you adapted to a somewhat different circumstance. In academia, you would publish a paper, it would go into a journal, and your peers immediately could grasp the significance of what you had done. The situation at RAND seems significantly different. You had to, in essence, educate your audience about the significance of your work. I mean, they didn't necessarily immediately appreciate the kind of work that you had done.

HELMER: It's true. Of course, much of the stuff that was written at RAND was classified, and so it couldn't be submitted to the usual journals.

COLLINS: But in terms of conveying the importance of your research product--in this case, work on the jeep problem--to the Air Force, how did you think about that in terms of sharing your research?

HELMER: Well, the attitude among some of the people at RAND, certainly mine, but I think it applies, too, to some of my former colleagues there, was not so much one of trying to make a reputation in the world at large, but you wanted to be recognized by your fellow workers at RAND or in the larger military community. I don't think, particularly with any classified publications that

I put out later, that I was aiming at a larger audience than that. I was perfectly happy if I got some approval from my colleagues at RAND, or if it had some noticeable effect on the people who paid our salaries--that is to say, the Air Force or, to some extent, the Atomic Energy Commission.

COLLINS: From what I can tell from your bibliography, the interest in the kind of problem represented by the jeep problem, you didn't carry that forward. You moved on to other types of things. Maybe you could describe the evolution of your particular research interests in those early years.

HELMER: A good deal of work in the mathematics division at RAND in the early years was devoted to the development of what's called game theory, partly under the influence of John von Neumann, who, as you know, was a consultant to RAND,and he showed up there occasionally. I think von Neumann was himself quite pleased with what went on there and, to some extent, even surprised, because we carried the theory of mathematical games far beyond the original area covered by the book by von Neumann and [Oscar] Morgenstern. A good deal of progress was made there. Of course, you talked to [Lloyd] Shapley. He was one of the most prominent people in that field.

COLLINS: Did you see yourself as someone who sought to find practical applications for game theory or to extend its theoretical foundations?

HELMER: I was much more interested in the practical applications, as opposed, I think, to Shapley, who was very much the theoretical type. But here again, you see, we had a vague feeling that putting military strategy into a framework of game theory might have eventually some important implications of use to the Air Force. But at the same time, in the early days, we approached this whole problem area very much as theoretical mathematicians. The field--it was just getting started, really--needed to be expanded, and there were some obvious directions in which we might want to go, and a good deal of progress was made there. It's a purely mathematical theory, which later was, in fact, applied to all sorts of problems, to practical problems of use to the Air Force or to the military more generally.

Although one has to distinguish between game theory and what is more generally called gaming, I think because of our early preoccupation with game theory, it stimulated a good deal of interest at RAND in simulation gaming, where some of the notions of game theory carried over, but not too many of them. This was a rather different area, and I found myself getting later on involved a great deal in gaming activities at RAND.

COLLINS: Could you elaborate your sense of the distinctions and the limitations of game theory as applied to the gaming activity you're beginning to talk about?

HELMER: Very briefly, game theory is concerned with setting up strictly mathematical models which you can then, if you wish, put on computers to work out solutions to particular problems, whereas in gaming, in gaming simulation, the participants play the simulated roles of decision-makers. They might be military decision-makers or others. You get a live interaction between the decisions made by various parties in the game.

We had all sorts of games that were played at RAND, some of them concerned with just military field strategy. Ed Paxson, especially, whose name I'm sure you know, was very much interested in that. He recruited quite a few military officers to participate in these games. They played the roles of, as we called them, blue and red commanders, who played out certain military actions on a simulated battlefield. In those days, only mild reliance was placed on computers, essentially just to do some of the basic bookkeeping operations that go with that, rather than to solve any intricate mathematical problems. That's, I think, largely because that was in the fifties. In those days, not much progress had yet been made in the sophisticated application of computers. That came later in the sixties only.

Shapley and I were involved for quite a few years in developing some games which were concerned not so much with actual military combat but rather with the problem of procurement. There still was a blue and a red side in the game, but the problem was not so much how to conduct a military operation but rather how to prepare for war, if it should come. Each side was given a certain budget, and they had to decide how to spend it, how much to spend on research and development, how much on the acquisition of new aircraft, and how much on the acquisition of rockets, and so on and so on, how it should be distributed, how the allocations should be made among available funds.

Then the resulting line-ups on the two sides, the actual military equipment, was then compared, adjudicated, not so much by actually carrying out a simulated war but rather looking at how much each side had acquired in terms of fighting capability.

COLLINS: Was that subsumed under the project called SWAP,the [symmetric war planning]?

HELMER: That's right. That was one of those, that's correct.

COLLINS: Again, I want to keep referring back to your training, which was more of a theoretical character. The kinds of problems you're talking about involved real-world, nitty-gritty concerns with economics and the functioning of government and bureaucracies.

How did you educate yourself about these different areas? Was it just sort of part of the on-the-job activity, or something else?

HELMER: It was part of that. Of course, you have to realize, in the first couple of years of the RAND Corporation--after it evolved from Project RAND in 1948 into an independent organization--the activity there was still strictly either engineering or mathematics, but no social sciences at all. Although it may sound self-serving, I think I may take credit for talking to John Williams at the time about the need to include in RAND's planning operations some thoughts about social and particularly economic ideas.

TAPE 1, SIDE 2

COLLINS: You were describing your evolving interest in the economic and social sciences.

HELMER: It must have been around 1950. I'm not quite sure of the years, but I think around 1950 I spoke to John Williams, who was head of my department, about the advisability of including economists and political scientists on the RAND staff. He rather liked the idea, took it up with the president, Collbohm, and subsequently arranged a big conference in New York of a large group of people who acted then as advisers as to how one might go about incorporating some social scientists in our operations at RAND.

COLLINS: Just to fix that in time a little bit, that conference took place in September of 1947, so it's a little earlier.

HELMER: It was that early? I was wrong, then. I don't know how soon after that they actually hired some social scientists. It must have been sooner than I remember.

COLLINS: I think certainly by '48 Hans Speier was aboard and so was Charlie [Charles] Hitch.

HELMER: Speier and Hitch both were at that meeting. So were, I remember, the famous Margaret Mead and Leo Rosten.

COLLINS: In those early years, then, what was your sense of the character of the interaction between yourself and the mathematics department and these other intellectual specialties at RAND?

HELMER: As I said, one of the great features at RAND, and particularly in the early years, by which I mean, let's say, the first ten years, was that we had a very open community. We felt very free to talk to other colleagues about our problems, and so there was a good deal of intermingling. Because of that, it was only natural that we mathematicians or engineers learned a lot about the ideas that circulated among the economists and the political scientists who had arrived at RAND.

In fact, I might just recall that--this was also in the very early days--perhaps the first simulation game that was organized was concerned with the Cold War problem, the whole international relationship between the United States, the Soviet Union, and other countries. Lloyd Shapley and I organized a big game in which the players represented whole countries. There was a U.S. player, a British player, a French player, a Russian player, and so on. We conducted a big operation there.

I don't know if the name [Harold] Lasswell means anything to you. Lasswell was a consultant at the time, and he helped us set this up. I remember one of his nicer contributions to that was a simulated memorandum written supposedly by Lenin that he wrote for this whole enterprise, which, particularly in retrospect, was very amusing to read. It may have been the better part of a year during which we spent some of our time on developing and playing this international game.

COLLINS: This may seem like an obvious question, but it's interesting to me that a great deal of time was spent developing the rules for these games.

HELMER: Yes.

COLLINS: Why was that? Why did they have to be so carefully structured in the sense of how this was being developed?

HELMER: Of course, you have to realize that, although simulation gaming has been used much more frequently in more recent years, particularly in the business world, in those days very little was known as to how one should do this sort of thing, and much of it was very experimental. That's to say, we changed rules as we went along. We tried something. In fact, again Shapley was very much involved in that early game. He designed some of the basic rules for that.

In order to give the participants a feeling as to just what it is they ought to be doing or allowed to do within the framework of the game, it was necessary to have some rather strict, concretely written, rules to follow, stating the available choices at each turn, so that each player knows what his options really are. Otherwise, the whole thing becomes almost impossible to handle. You have to have some kind of a schedule by which you go. At one point, player A has to take some action. In the next phase, player B has to react to what player A has been doing, and so on. In the case of this particular game, the players may be permitted to call an international conference, so there has to be time set aside for that. Again, rules need to be written as to what may take place at these conferences. Otherwise, it becomes just a general muddle.

COLLINS: How did you develop confidence in the utility of these gaming approaches, that they would actually tell you something about how behavior might be conducted in the real world?

HELMER: This is a good question. It's a very controversial one. Some people think you can actually derive from game-playing some particular implications. That may sometimes be the case, but I'm very dubious about that. I think mostly the game environment that is set up acts as a stimulant for the participants. The participants, after all, are experts in their particular fields. They might be economists, social scientists, military planners, and so on. But when they are confronted with the simulated development--say, some new treaty has been signed, and there are some military forces being built up--now what do you do? They can bring their expertise to bear under these circumstances, and it stimulates their thinking as to what they ought to be doing. If they just sit by themselves and think purely theoretically, they might not have thought of these implications. But being confronted, in a relatively realistic way, with new developments in the world to which they have to react, it's that stimulus which I think represents the biggest value of such a gaming situation.

COLLINS: Was the principal goal of gaming, then, to involve decision-makers in the games, rather than academic experts?

HELMER: Yes, that's right. Very definitely.

COLLINS: How does this relate to your interest and involvement in developing the so-called Delphi technique or projects that you worked on?

HELMER: There was, I think, a pretty clear connection. It became very clear to many of us at the time, I think, and maybe gaming had contributed to that insight, that you can't always count on being able to solve problems in any sense the mathematician might think of solving something, but that you have to rely very much on the insights of experts in particular fields as to what is the appropriate thing you ought to do in a given situation.

We began to think a good deal about the question of how one can systematize the use of expertise. I remember that in the early days--you may have heard about this before--they took as an example the question of how to predict the outcome of horse races. Did you hear about that? [Laughter] I forget why we picked that particular subject, perhaps because a lot of information was easily available. You could take the forecast of the professional horse race predictors. What do you call them?

COLLINS: Handicappers.

HELMER: Yes, handicappers, that's right. We found very easily that if you followed the advice of a particular handicapper, you always, in the long run, lost money. In fact, a good deal of money. So we asked ourselves, "Suppose you consider not just one handicapper, but several. After all, they consider themselves experts about horses." So we tried to determine whether one could somehow make use of the forecasts provided by several handicappers to do any better. Well, we looked into that carefully, and we found that by a clever combination of the forecasts of different handicappers, we still lost money, but we would lose much less money than if we followed a single one.

That was the origin, I think, of the development of the Delphi method, which was a systematic way of trying to combine the opinions of different experts in a particular field. You may already have looked at the original Delphi study that was conducted at RAND, which was concerned with, at that time, a classified project about the effect of atomic bombing on the United States.

If I remember correctly, I think the question was posed in this way: suppose the Soviet Union decided to attack the United States with atomic bombs of the Hiroshima-Nagasaki size and directed their attack primarily at industrial installations. How many such bombs would be required in order to reduce the industrial output of the United States during the subsequent year to twenty-five percent of what it would otherwise have been? I think it was put in this precise form.

This question was put to, I forget how many, about twenty or so experts in various fields related to that, some military experts, some experts in the structural viability of industrial plants and that sort of thing. Most of them, I think, were RAND members, a few were RAND consultants, a group of about twenty people. They were simply asked, "What do you think is that number?"

At first, I think the spread of numbers as to how many bombs it would take to achieve that stated result was enormous, I think from fifty to five thousand or so. It was a ratio of about one to a hundred, at least. I don't remember the exact figures. But then we went through several rounds of the Delphi process, and in the last round the interval had shrunk considerably to something like maybe from one hundred to less than three hundred. The ratio of largest to smallest estimate was then, in fact, less than three. It was about two and a half, rather than a hundred as it had been at first.

We felt encouraged by that, because after having gone through the Delphi process, the participants themselves felt much surer about their judgment. They, I think, all agreed that their original estimates in some cases had been way off, and they now felt rather confident that the final number that came out of the

Delphi study was something they could live with, that they found acceptable. So this was a particular item which was of some military significance, because this was something we could present to the military in Washington since this was the sort of thing they had to go up against.

COLLINS: As part of the process, or the technique, you would have this group of people sitting around a table all together, or would they be sampled separately?

HELMER: No. One of the features of the Delphi process is anonymity. That is, the participants are asked individually to give their opinions, and then in a second round they are told what the spread of opinions is, but they're not told who had which opinion. So the particular authority that might have been vested in some special person was eliminated, so everybody was on an equal basis.

Then in the second round, people were asked to look at the spread, to review their own opinion, and if their renewed estimate was outside what's called the inter-quartile range, then would they give a reason why they thought their opinion was so different from that of the majority. These reasons, again anonymously, were fed back to the whole group. The group was then asked to consider these reasons and give them what weight they thought they ought to have, and, in the third round, give one more estimate. Now, if their estimate again was outside the new inter-quartile range, would they give a counterargument. Why didn't they accept the arguments that had been presented anonymously? So some counterarguments were elicited. These were presented again to the whole group anonymously, and then in the final, fourth round, the participants were asked to reconsider, to consider the arguments for a particular position, the counterarguments that had been presented, and give their final estimate. So it's sort of an anonymous discussion that takes place.

We did a lot of experimenting subsequently with that notion, and we found that generally there is a contraction of views as a result of that. We then later on applied the same idea to other areas, such as to technological forecasting on a large scale. You've probably seen the study that was conducted by Ted Gordon and myself, in particular.

COLLINS: In the Delphi method, there seems to be, as you describe it, a kind of downgrading, and that may be the wrong word, of the methods by which people arrive at their judgments. In other words, you weren't too concerned about how one arrived at an opinion, but just what that opinion was.

HELMER: Yes. Well, that's true to some extent, but, you see, if someone really had a good reason for giving his opinion which was different from that of the others, then he was given the chance to present his argument in giving a supporting or counterargument in this anonymous discussion. So it wasn't entirely suppressed, but we primarily relied on the intuitive insight of people, regardless of how they arrived at it. But they had at least the opportunity to present their reasoning if they wished to.

COLLINS: I would assume that in assembling your original sample of experts, one selects people who you assume have some ability to address this question.

HELMER: Oh, sure. Yes. This is very important, to select the right kind of people, of course. You can't just take any person. You have to be sure that he brings to bear some kind of expertise that's relevant.

COLLINS: I'm interested in exploring what the limitations of this were and how far it went as a kind of technique for dealing with these kinds of problems.

HELMER: Let me say a couple of things about that. The whole approach of the Delphi procedure was met with a good deal of distrust within RAND, particularly among the social scientists, who had never heard of such a thing before, and they didn't really like it. They were opposed very largely to putting any qualitative thoughts into a numerical format, which Delphi requires. They kind of distrusted it, and not much support was given within RAND to that, until much later, when they found that the Delphi method had sort of taken over and was used by lots of other people outside. Then it suddenly became the method that was invented at RAND, and at first they didn't really quite like to admit it.

What really put the method over was an application that was arranged at TRW--Thompson, Ramo, Wooldridge. You know about that?

COLLINS: Yes.

HELMER: They used it first, within their own company, to get some ideas from staff levels, from the lowest to the highest within the organization, as to where the firm ought to go, what prospects there were for future developments, and so on. They got all sorts of advice from many levels within the organization. They were very pleased with the results, partly because of the actual substantive output of the study, but partly also because it established a good climate within the organization. People who had never been asked their opinion before had been consulted somehow, and they felt they were more of a part of the actual running of TRW. And so it established a good climate with which the management was very pleased. So they undertook to write this up, give talks about this method at industrial conferences, and so it became quite well-known as a result of that.

COLLINS: That was about what time period when TRW applied it?

HELMER: That was around 1965, thereabouts. In fact, the person who was primarily responsible for carrying on the study at TRW, was an assistant to the vice president, I think. He is now here at Casa Dorinda. [Laughter] It's another one of those coincidences.

COLLINS: Was this method ever applied to a group of military officers? Did they ever participate in this?

HELMER: The method really took over on a large scale, much to my surprise. A few years ago, someone made a computer search of military documents involving the Delphi study, and there were at least a thousand titles. The Delphi has probably been used, I would say, maybe as much as in ten thousand locations all over the world. It was used on a very large scale in Japan at one time, and I think they're still doing this sort of thing there. In about 1970, they ran a very large study on the economic future of Japan, in which several hundred Japanese economists were involved in running this Delphi study, one of the largest-scale studies of the kind that I'm aware of. But it's been applied all over the place.

COLLINS: Just to reorient our discussion a little bit, thinking back to the 1950s, [interruption] if we could just have you characterize a typical day or a typical month in terms of how much of your time was devoted to particular kinds of activities. We talked about your working on the war gaming, on the Delphi method. You were also concerned, judging from your bibliography, about problems of target selection.

HELMER: Yes.

COLLINS: How would you characterize how you divided up your time and how much energy you devoted to these different activities? I know it certainly varied over time.

HELMER: I'm not sure that I really recall that too well. I think that probably, at the time, I usually concentrated pretty much, maybe ninety percent of my time, on one particular project, but there may always have been a few others into which I was drawn by other people, or which in the back of my mind I was interested in, and I wanted to take up at some future time. But usually I think I concentrated on one of those things most of the time.

COLLINS: Maybe we could talk a little bit, because it touches on perhaps some other relationships within RAND, your interests in target selection, because it seemed to me it involved, in contrast to the Delphi method work, a genuine interaction with the economists at RAND. Can you describe a little bit your research activity in this area?

HELMER: I'm not sure that my recollection on that is very clear, because I'm not sure of the sequence of events anymore. For instance, I don't know whether the target selection project preceded Delphi or came afterwards. I'm not certain anymore whether it was at about the same time period, or if it came earlier. Maybe you have some clue.

COLLINS: My sense is that they kind of overlapped.

HELMER: They overlapped to some extent. That may well be the case. Yes, that's right. They were probably more or less in the same general time period, but that doesn't mean that I split my time every day between those two. But which really came primarily first, and which later, I'm not sure of anymore. All I remember is that the actual gaming activity, I think, on the whole, came somewhat later.

COLLINS: Perhaps another way to phrase this is to characterize the evolution of your research interests. The jeep problem represents a kind of mathematical logistics problem. Then there was, I think, an interest in assessing the probabilities of getting bombs onto target, and that seemed to evolve into a consideration of target selection. But you also had an interest during this time period in developing war gaming techniques, and I think a little bit later than the Delphi method is where it comes in. Is that a reasonable statement?

HELMER: Yes, I think that's right. That's true.

COLLINS: You indicated that with the social science people, and in the Delphi method, there was a kind of distrust of this. Did that limit your interaction with the social science department?

HELMER: Not that particular thing, but in general. You see, what I liked very much about the early days at RAND, which I pointed out several times, was the spirit of cooperativeness and openness within the staff, which was fostered very largely through John Williams, who really played a very prominent role, although he was just a division chief, but I think he had a great deal of influence about the way RAND was run in the early days. As I say, he was instrumental, in fact, in bringing in economists and political scientists.

But much to our distress, as time went on, we found that among the social scientists, particularly the political scientists, there was much more of the sort of academic relative secrecy among them. They did not welcome the idea, as much as we had done, of cooperating with people in other fields. They were not intrigued with the possibility of a multidisciplinary approach to problems. That was a disappointment to us, and it led, I wouldn't say to friction, but at least it did not continue to foster this open atmosphere that we had established in the early days of RAND.

COLLINS: Clearly, it seems a thread one could read into a lot of your work and the work of the mathematics department is a real genuine belief that you could mathematize a lot of these complex social and political problems, and my sense is perhaps that the people in the social science department, and particularly the political scientists, didn't share that belief.

HELMER: Absolutely. You're quite right. It's sort of understandable, considering their background, although my feeling is that there is a lot more reliance these days on mathematical and statistical approaches within even the political sciences than used to be the case in the early days. One of the few people who really sympathized with our approach was Lasswell, and there were a few others within the political science profession, but not very many. Many of them distrusted that.

COLLINS: What about, then, your interactions with the economists? RAND economists, a certain fraction of them, were very comfortable with mathematics.

HELMER: Yes. On the whole, our relations with the economists were quite friendly. Of course, there were people who rose to some extent in both areas. [Albert] Wohlstetter was one of those who was trained to some extent in mathematics and logic, like myself, but who had always been very much interested in economics and in applications to wider problems. So there was a bridge via some people like Wohlstetter.

Wohlstetter happened to be a good friend of mine. In fact, I was kind of instrumental in bringing him to RAND in the first place. I had known him and Roberta Wohlstetter in New York, and when the question came up of hiring people in the social sciences, I suggested that maybe they ought to consider Albert and Roberta. I contacted them and found out that they might indeed be interested, and it was worked out to their satisfaction. Particularly Albert played a very prominent role. I forget if Roberta ever was really on the staff or merely a consultant, but she worked a good deal for RAND. I think she was actually on the staff for a while.

COLLINS: Were there any particular members of the economics department that you had good working relationships or close working relationships with?

HELMER: Not too much at that time. I had had no training in economics myself, although I was quite interested. But I developed a better knowledge of economics later on, after I left RAND, in fact. In fact, particularly when I was at IIASA, I put together a world economic model, and so I got involved in economics a little more deeply at that time. But in the early days at RAND, through constant contact with these people, I learned a little bit about it, but never in any very systematic fashion.

COLLINS: I wonder whether it intersected in your work on target selection--that is, in the development of RAND's literature on nuclear strategy. The issue of target selection was one that economists seemed to make a contribution to.

HELMER: Oh, yes, we certainly relied on inputs from the economists, but my recollection is that we pretty well accepted their inputs on faith, rather than getting involved ourselves in these substantive aspects.

COLLINS: In the case of the symmetric war planning, the SWAP project, would this have been the case as well, in terms of procurement issues and the ability to organize resources to be able to conduct a war?

HELMER: Oh, yes, true. But I don't know if any fundamental economics was involved there. This was to some extent a matter of logistics, to some extent a matter of just bookkeeping, and some knowledge, of course, of how the actual operating costs relate to the acquisition costs of the materiel. This doesn't really go into any deep economics. This is just some basic knowledge that one has to get, and some of it certainly came from the economics division.

COLLINS: In the kind of work that you did, did you have much involvement with, or much use of, RAND's developing interest in computers and its applications to these kinds of problems?

HELMER: We made some use, of course, of the developments with regard to computers, but at least I myself, I can't say had anything to do with the actual development itself. I may have made some suggestions at times. I once proposed that we ought to spend some effort on developing a reading machine, which met with some positive response, but I had nothing to do with any further developments there.

COLLINS: But in terms of the kinds of projects you were interested in, having the capability of computers was not essential to the kinds of things that you wanted to do, is that fair to say?

HELMER: I can only think of one minor instance of that kind. When we played around with further developments and refinements of the Delphi method, we once set up a communication system, a computer network, if you like, on a very small scale within RAND, where the respondents each had a terminal at their disposal, and we ran a Delphi study using these terminals instead of operating through the mails with questionnaires. So we were able to run a brief Delphi study within hours instead of weeks. That was the sort of forerunner of what was done later on a larger scale. But I can't think of any particular instance, aside from that, where I was involved directly in this sort of thing.

COLLINS: What I'm trying to get a fix on here is your research style and the way you preferred to carry out projects, and my sense was that you had a different approach than some members on the staff.

HELMER: By the way, are you interested in contacting some people who were involved in the early days in the development of computers? There's Paul Baran. I don't know if you're planning to interview him. He is in the Palo Alto area. And there is Paul Armer, who ran the computer establishment at RAND for quite a few years.

COLLINS: And Willis Ware, of course.

HELMER: Willis Ware, yes. I don't know what became of Willis Ware.

COLLINS: He's still hanging around RAND.

HELMER: He's still at RAND, is he? What do you know. He must be getting on in years, too. [Laughter]

TAPE 2, SIDE 1

HELMER: The story of the mugging of Mel Dresher reminds me that one of the early applications we tried of the Delphi method was finding out what could be done about the increasing crime situation in the United States. It wasn't a very serious large-scale effort. I understand that RAND now has a project under way on a very large scale that's trying to cope with the crime problems. Is that correct? You probably know about that better than I do.

COLLINS: Right.

HELMER: But that's only an aside.

COLLINS: I'm curious about how you put together a study, and I'm thinking to draw a contrast. For example, when Wohlstetter did the base study, a large part of his effort was empirical. He went out to bases and military facilities and looked at how things worked and got a feel, essentially experientially, of how to approach the problem. Did you do similar things for the projects that you did, to somehow get a certain level of understanding of the military situation, to visit operational sites, or was it more gathering information in different ways?

HELMER: It was more the latter. I got information from colleagues, from reading the relevant literature, and that sort of thing. Except for some occasional trips to Washington to talk to people there, I don't recall going to any military installations. The only such trip I ever undertook was to the desert in Nevada to look at the atomic bomb site there. That was instructive, but didn't have an awful lot to do with the actual work I was doing at RAND.

COLLINS: For example, when you were doing the target selection work, would you go and visit a factory and get an on-the-site sense of operations and the potentiality of damage, or again was it just something that was drawn from the literature?

HELMER: I did not personally, no, but some of the people participating in the study certainly had relatively more intimate knowledge of how a factory was constructed, and what one had to look for in order to assess possible damage from an atomic attack.

COLLINS: Since we've talked a little bit about the Delphi method, I'm interested in your elaboration of this into the notion of forecasting, or futures research. When you originally thought of the Delphi method and its applications, did you think of it in those terms, or was that a later kind of elaboration of your thinking?

HELMER: As I recall, originally we merely thought of the best way of combining expert opinions, regardless of whether it was concerned with the present or the future or some technological development, or whatever it was. In fact, as I mentioned, one of the early studies was one of horse races, where we tried to combine the opinions of supposed experts in that field. And then the next study, you would hardly call that a forecasting study, the one on the effectiveness of an atomic bombing campaign on the United States.

But then we began to think about the mission of RAND, to do some work that might be useful to the future of the Air Force. So that inevitably involved forecasting, forecasting both of technological developments of relevance to the Air Force and, of course, international developments that might lead to war or affect the conduct of war. So then that made us think about the future and the possibility of applying expert opinions to making some reasonably reliable forecast of what might happen in the future that would be of relevance. So that's how we got into that.

Then the next big step after such minor experimentations with improving the Delphi method, which were really just strictly methodological, we then conducted this major technological forecasting study, which involved, I think, over eighty experts, some of them drawn from Europe and elsewhere. I, in fact, went to Europe to solicit the participation of some people there. Perhaps the two most prominent participants in that study and in Europe were Bertrand De Jouvenel, who died a few years ago, a prominent French futurist, and the Hungarian-born Britisher. What's his name?

COLLINS: We can fill that in later.

HELMER: He got a Nobel Prize essentially in the field of optics. It's too bad that I sometimes have a problem with names. It must be my age. (In fact, it was Denis Gabor.)

COLLINS: But it was basically the same set of techniques that you described to me previously when you originated the technique?

HELMER: Yes, that's right. Of course, the important thing there is, first of all, to identify the right kind of experts, and it's hard to do that systematically. You essentially start out with a few people where you feel quite sure that they are good in the field. You might ask their opinions as to who else ought to be consulted, or you look at the literature and see who has published in the field, and so on. So you gradually accumulate a list of potential participants, and, of course, you need many more than actually will participate, because you have to expect that some of them will refuse.

COLLINS: In the example we talked about, of an atomic attack on the United States, the question the experts addressed was formulated in a very precise kind of way. For questions or issues relating to forecasting the future, are the questions formulated differently, or do you try to give them like, "Will we have a ten percent economic growth in ten years?" Or is it something more qualitative?

HELMER: We were well-aware of that problem. Whenever possible, we tried to formulate questions in a very precise manner, but it isn't always possible. For example, an instance of a very precisely formulated question about the future was the following: when will a computer become available that can understand English and can answer a standard IQ test and score at least 150? That was one of the questions put to the experts, which, again, at first yielded a wide range of divergent answers.

Another one that was relatively precise: When would it be possible to produce energy through thermonuclear fusion? That's relatively precise, too. In fact, I remember that the answer was around 1990 at the time. This was asked in 1964, I believe. That was a little optimistic. If you ask people now, they think it's still somewhat in the future, maybe 2010 or so, so that was a little optimistic. But anyhow, that's beside the point.

But in many other cases, it's not that easy to make a question absolutely precise.

COLLINS: In the case of the fusion stuff, what would you expect a policy maker would do with that kind of answer that says fusion will be available in 1990? Is it to say to the policy maker, "It's good to go ahead and fund this stuff"?

HELMER: It might very well. It might be, for instance, a warning to the people constructing the traditional atomic power plants, because if you can switch over to fusion, it would be presumably very much cheaper, if it can eventually be achieved. So there would be less point in sinking enormous funds into the building of atomic power plants.

COLLINS: Did you attempt to attach a confidence level to the answers that came out of this process?

HELMER: Only informally, in the sense that along with the precise date, which was the median of the final answers, which we considered the forecast that was achieved by the study, we also published the remaining inter-quartile range, so that gives some idea of how much even the expert opinions still differed after they had been through this rigmarole of the anonymous discussion. That gives you at least an informal, intuitive feeling for how reliable the answer really is.

COLLINS: I'm trying to get a little better fix on your interest in this. Historically, attempts to predict future conditions have always been fraught with problems, and typically things have not happened in that way.

HELMER: Forecasting obviously is of great importance. I mean, whenever you do any kind of planning, you are doing it against the projected future environment, whether you're just planning business operations for the next year, or whether you're doing long-range planning, as in the case of RAND, of military procurement, where you have to talk in terms of decades rather than years. You have to have some image of what the future will be like, or else your planning doesn't make any sense.

COLLINS: How would you compare your work with, for example, Herman Kahn's? If you look at a book like Kahn's on thermonuclear war, essentially what's happening there, or seems to be, is that he's trying to imagine futures, different scenarios. How does that compare with the kind of thing that you were thinking about and doing?

HELMER: I think the difference is that what Herman put out were essentially his personal opinions, opinions of one person. Now, he was a very bright person. He was a real genius. And so one might want to put a great deal of weight on his opinions. Yet it's just the opinion of one person. Sure, in arriving at his forecasts, whatever it was, the future of nuclear war, whatever, he certainly ingested the opinions of other people, but it was essentially his condensed opinion that he himself had formed, whereas in a Delphi study, it's a combination of all the experts participating.

One might, of course, argue that in many cases, a dozen experts together might not be as good as one real super expert like Herman Kahn. It's hard to argue about that.

COLLINS: Typically, when you talk about future research, or trying to elaborate the future, do you have a specific time horizon in mind? Are we talking like five years or ten years or fifty years? I'm sure it depends on the problem.

HELMER: It depends on the problem, of course. Roughly speaking, something like maybe in the next twenty years or so. But it's only because in the next couple of years, things are not going to change very much. It's very unlikely. If you go much beyond twenty years, things become so fuzzy that, even if you take a bunch of real experts, their opinions will vary widely. There's too little to go by. Everybody's aware of the fact that something may intervene that's completely unexpected, and so it becomes kind of futile to try to make forecasts much beyond that. So something like ten to twenty years is probably the horizon I'm thinking of most of the time in my own work.

COLLINS: Did this have any connection with the work of the Air Force in the mid-1960s on what they called their Project Forecast, which was a revisiting of the work that [Theodore] von Kármán did right after World War II to look at the technologies that may be potentially useful to the Air Force? Was that related to what you were doing?

HELMER: It was not formally related at all to that. In fact, I'm sure I saw the study at the time, but I have really no recollection of that, and I don't think that our work was directly related to that in any way. It was fully independent. I forget, at what time he conducted that study?

COLLINS: I believe it was '64, '65.

HELMER: It was just about the same time. I sort of vaguely remember that, at one point, we actually recommended that if that study was redone sometime later, they might want to consider using Delphi in it, but I don't think anything ever came of that. I have only a very vague recollection of it.

COLLINS: Shifting backward again a little bit, one of the distinctive products or contributions that RAND thought it was making during the 1950s was this notion of articulating systems analysis and what systems analysis was. Did you see yourself contributing to that kind of general exercise of the institution, or do you see yourself working apart from what was going on with respect to that?

HELMER: In those days, I think I was much more engaged in learning what it was all about. I don't think I had much to say about propagating the idea or any notions about systems analysis. It was much later, when I was at IIASA, that I actually taught the subject. But not in those days.

COLLINS: You mentioned over lunch that [Edward] Ed Quade was a close colleague, and he'd organized, or helped to organize and taught this course in the mid-1950s on systems analysis.

HELMER: At RAND, you mean.

COLLINS: At RAND. Were you involved with that in any way?

HELMER: No, I was not involved in that. In fact, I'd forgotten that he did that. Now that you mention it, I recall it. But I don't know whom he was teaching.

COLLINS: It was military officers.

HELMER: Military officers, that's right, yes. No, I wasn't involved in that. I think that maybe, as an adjunct to that, we probably demonstrated some of our gaming techniques to the participating officers, but it was not a central part of the course.

COLLINS: I want to explore who your most intimate colleagues were in the mathematics department and the character of your interactions. We talked a little bit about that over lunch, but it would be useful to do that for the record here, about those people who you were close to either in a working sense, or in a social sense.

HELMER: I was working very closely with people like [J.C.C.] McKinsey, with Mel Dresher, Lloyd Shapley. I think those were the principal ones with whom I worked, and to some extent with [Norman] Dalkey, although Dalkey was not primarily interested in game theory, but perhaps in the logic of game theory rather than the mathematics of it. Have you interviewed Dalkey?

COLLINS: No.

HELMER: He's at UCLA now. I think he's retired, but he still has an office there. I think those were the principal ones in the field of mathematics.

Outside that field, I was a close friend of Albert Wohlstetter's. I worked with him occasionally, but not very often. Occasionally I did some work with Paul Armer, who was running the computer establishment at the time. There were some people in the--what was the department called that had to do with weather and that sort of thing? There was Will Kellogg.

COLLINS: Right. That department changed names several times over the years.

HELMER: Yes. I forget what it was finally called. Our department changed names, too. It was originally called the military worth department in the early days. [Laughter] It was later more adequately called the mathematics department. Socially, close friendships developed between my wife and myself and other RAND families besides the Wohlstetters, such as the Hitches and the Barans.

COLLINS: Certainly during this early period, RAND was a fertile crossroads for a lot of academic people who would come through, and you've mentioned von Neumann before. What was the nature of your contacts or interactions with him?

HELMER: We had some fascinating sessions with him, because he's a man not only full of ideas, but he had an incredibly fast mind. It was always astonishing to us, if you put a particular mathematical problem to him, it often occurred that he just sat there for a minute, thought about it, and then came up with a solution, something which might have taken us weeks and months, if we ever got it.

COLLINS: Were there ever any summer visitors who came through RAND who were interested in the same areas that you were interested in, and that you had contact with and perhaps kept up contact with after that?

HELMER: There were some people, I don't know if they were summer visitors, who came for a while. Whether they were there as consultants or on temporary employment, I'm not sure. People like Ken Arrow, for example, an economist who got the Nobel Prize later. There was another economist whose name I can't think of right now, who I think went to Pittsburgh. (He was Herbert Simon.) There was [David] Blackwell, whom my wife mentioned. He's a statistician, now retired at Berkeley. There were probably numerous others. I can't think of any right now.

COLLINS: Did you ever in the course of your various studies take the results of a study and go brief at the Pentagon or other Air Force sites, like Wright Field? Was that part of your experience?

HELMER: Very little of that. There wasn't much of that. I once went to the Army War College to give a lecture there on some of our gaming activities, because they became interested in what the Air Force was doing. So I went there and gave a lecture, which was well-received. Let me think. I was in Washington on a number of trips, but usually just as a member of a group that went there, and I didn't brief anybody in the formal sense of that word. I was just participating in a general discussion there on a number of occasions.

I don't know if you want to go into this at all, but you know that when I left RAND in 1968, it was in order to set up with a number of my former colleagues an institute of our own, the Institute for the Future. Before we got to that point, we were invited to give a presentation in Washington. In fact, on one occasion we gave a presentation at the press club in Washington on our ideas for setting up an Institute for the Future. We wanted to apply some of the ideas that had been used at RAND, particularly ideas relating to the future, to apply those to larger problems of our society. That was the general idea behind that, so that was one occasion where, if you like to call it that, I gave a briefing. But that was not within the RAND context.

COLLINS: When you say "your colleagues," whom were you referring to specifically, who were part of this group that set up the Institute for the Future?

HELMER: It was Paul Baran, whom we mentioned a number of times. He joined the institute for about three years and then went off on his own. There was Ted Gordon, who never was an employee of RAND, but was a consultant when we ran that long-range forecasting study in the mid-sixties. He was one of the prominent members to join the organization. Let me see, was there anyone else from RAND? I can't think now. There may have been one more. Yes, indeed, Arnold Kramish joined the institute staff for a while.

But originally, there was a group of about eight people who had formed a committee, some of whom were RAND members, who met about once a week to talk about the possibility of setting up such an institute. Paul Baran certainly was one of those. So was Paul Armer, who did not join the institute that we set up, but he was very much active in preparing for it. There was George Mandanis. I don't know if the name means anything to you. He was at SDC, Systems Development Corporation. He then set up his own firm near San Francisco. He was one of the, if you might call it that, founders of the Institute of the Future.

COLLINS: I'm wondering if we can try to characterize some of your research activity, in talking about your training, very much oriented in theoretical mathematics and logic. At the time that you worked over at RAND, was there a sense that you can identify of the limits of one's ability to model and apply mathematics to the types of problems you were interested in? Did you ever at any point along the way say that that was, in essence, a failed project, that there are some things that just couldn't be modeled in that fashion?

HELMER: Well, that depends how you interpret those. The simple answer is no. I always felt that one could somehow model things, but it depends on how. I certainly wouldn't want to maintain that everything can be modeled in the mathematical sense. Whether, for example, you consider the Delphi approach a modeling effort, that's questionable. But I certainly think that problems, no matter what they are, can somehow be approached through some kind of a systematic effort, whether it be a mathematical model-building or some other construct. So in some sense I'm always very optimistic and positive in that regard, but I don't mean by that that I feel that everything can be mathematized.

COLLINS: Did you, during your time at RAND, belong to any professional societies, like the Operations Research Society or any similar organizations?

HELMER: I think I probably did, yes, belonged to that, but it meant so little that I don't even really remember.

COLLINS: Let's talk a little bit about your work that you mentioned over lunchtime with Nicholas Rescher on the epistemology of the inexact sciences, because I think it ties into this discussion a little bit. Can you describe the motivations for that work and what you were trying to achieve there?

HELMER: I think that whole work was sort of a forerunner of our ideas for setting up an institute that would deal with larger societal problems, applying methods similar to those that we had developed at RAND and applied to military problems. It was very much in the same line. The later ideas that were more specifically addressed to studying the future of our society through the institute we wanted to set up, they really came out of that development, so this was very much of a forerunner to it.

COLLINS: I want to explore a little bit the significance of the title of the work. By looking at the epistemology of the inexact sciences, what were you attempting to do here? It sounds like a kind of theoretical foundation project, to give some rationalization to the effort.

HELMER: Our feeling was that the social sciences were in some sense lagging behind the physical sciences, because you didn't have these exact methods, empirical methods and statistical and so on, that you can apply to study physical problems in physics or chemistry or biology. And we tried to analyze a little more clearly what really the differences were, and to what extent some of the methods used in the physical sciences could be transferred to the area of the social sciences. And so, in that sense, it's concerned with the foundations of scientific thinking. That's really what the word "epistemology" means. I don't know if that really explains it all, but I think it gives you a little bit of the motivation behind it.

COLLINS: How does that relate to your educational experience and professional friendship with Carnap and [Hans] Reichenbach?

HELMER: It had, certainly, something to do with that. Just my training in logic, in particular, and in logical thinking, and my interest in the interdisciplinary application of scientific thinking certainly derives from that to some extent. There's no question that I owe a considerable debt of gratitude to both Reichenbach and Carnap for the influence they had exerted on my intellectual development. Another philosopher, incidentally, with whom I had a good deal of fruitful interaction is my old friend Carl Hempel of Princeton University (now retired).

COLLINS: You mentioned with the Delphi method that there was not a lot of professional kinship or sympathy with the people in the social science element of RAND. Were they interested at all in this kind of theoretical elaboration of your approach as represented in this work?

HELMER: There were one or two people there who really took an interest in that, but, by and large, I would say that they weren't antagonistic, they just ignored it. But there was also the basic mistrust of anybody who wasn't officially a member of their field who wanted to say something about that. The feeling one got was that they thought of an outsider trying to enter their field as a charlatan.

COLLINS: I'm wondering where your interest came in as you worked on problems. Certainly in the 1950s they were all military problems, or principally military problems.

HELMER: Mostly, yes. Not exclusively, but mostly.

COLLINS: But as you talk about the Institute for the Future, the concern seems to be to be able to apply techniques to a much broader spectrum of social phenomena. How did that interest develop in your research or professional activity?

HELMER: I guess it came about very gradually. I think a lot of us felt at the time that some of the efforts we had directed to military affairs were perhaps somewhat futile. We were all discouraged by the Vietnam War in those days. I remember doing a lot of thinking about that and even had dug up some notes on that subject that I wrote. I was very dissatisfied with what RAND was doing there, and I felt that maybe once this whole mess was over, we ought to see if we couldn't do something more fruitful by applying our analyses to the broader field of our society.

Some of it came about just sort of incidentally. As I mentioned before, it was perhaps because of what happened to Mel Dresher in Washington. We used Delphi to apply on a very tentative basis to the whole problem of reducing the rate of crimes of violence. It was then used merely as an example of how Delphi could be applied to such problems, but it certainly led us in that direction of seeking broader applications of the method.

COLLINS: Could you talk a little bit about your working relationship with John Williams over the years that you were at RAND? You've talked a little bit about his bringing you to RAND, but we haven't talked too much on the record about the working relationship that you had over the time that you were there.

HELMER: How should I describe it? I wouldn't say that we ever had a close relationship as friends on an equal basis. I always acknowledged him as being, in a sense, my superior within the organization, but that's primarily because he sort of defended me vis-à-vis the top management. I think he himself was a very inventive person, and he sensed that my thinking was somewhat similar to his own. My thinking was less conventional than is usually the case, and he appreciated me as sort of a producer of ideas, some good, but some of them bad. But that established a bond between us which was appreciated by both of us. I'm not the kind of person who ever was able to push himself very much, and so I needed someone to defend me, which he did very effectively.

COLLINS: When you say "defend" you, were there any instances in which your work caused controversy or problems in the organization?

HELMER: Well, there may have been a few. As I said, the development of the Delphi technique at least caused some mistrust among the social scientists, and since at that time they had very much the ear of the president of the corporation, it was not easy to continue this kind of work vis-à-vis this pretty clearly expressed disapproval. So it was an instance where certainly Williams's presence was very essential to me.

COLLINS: How would that disapproval express itself? Do you mean just from social scientists as colleagues or somehow through the administration?

HELMER: It was also through the administration. I don't remember details of this, but I think that on a couple of occasions we tried really to get under way some RAND projects directly concerned with some military problems that would use the Delphi technique, and we were rather clearly told that we would not have the support of the administration. That was pretty clearly felt there at the time. It didn't always take the form of someone officially saying, "No, you must not do this." But within this general informal atmosphere at RAND, the opposition was felt, rather than made formal.

COLLINS: How does one put that in relationship to the kind of general atmosphere that you suggest of openness, of the ability to choose and direct your own research activity? What you're suggesting is that there were on occasion some constraints on that.

HELMER: Well, it's not that so much. I think there was a gradual shift. In the early days, particularly in the early fifties, there was this openness which was just very clearly present, whereas gradually, as we moved into the sixties, there was less of that, and there was a clearer dominance of both the political scientists, primarily, but also to some extent the economists and the engineers, who were opposed to this kind of freer thinking and collaboration. They were distrustful of anything that looked as though it was an attempt at interdisciplinary approaches to problems. They wanted to confine things more clearly to the areas in which they felt they were competent.

TAPE 2, SIDE 2

COLLINS: Another one of the identifiers of RAND, or one of the things that people identify as a hallmark of the organization, was this sense of interdisciplinarity. I wonder how you interpreted that kind of character of the corporation. In a sense, you've suggested it was the ability, at least on one level, to walk around offices and walk into people's offices and talk through a problem. But in what other ways did you conceive of the meaning of interdisciplinarity in the RAND context? Are there any other ways in which you would characterize it, or the lack of it?

HELMER: I think this was partly a learning process, too. I think we gradually realized that in many of the problems, whether strictly military or not, it was just more useful to bring in the opinions from people who were not directly related to the military, but who might have some expertise in other areas that were perhaps a little more remotely relevant.

Since we did not have any formal specific assignment from the Air Force on what we should work on, if someone had an idea that here's a project area which might turn out to be useful in the long run, then we felt free, without any direction from management, to just say, "Here's a new problem." You talked it over with a few friends maybe in your own department, and then you figured, "Well, in order to make some progress here, maybe we also need an economist and an engineer and a political scientist." So you just walked into their office, told them what you were thinking about, and asked whether they might want to participate. It was very informal.

Very often we just formed groups of that kind, which came together, maybe dissolved after a while, but there was a good deal of interdisciplinary cooperation, because we felt--or at least whoever started these little projects felt--that this wasn't just a project which could be handled in one disciplinary area, but which needed inputs from other areas.

COLLINS: Now in some cases, when you look at RAND publications, like the Wohlstetter study, you can see authors who represent different disciplines. When I look at your publication list, your coauthors are almost always other mathematicians. It sounds like for the types of projects that you worked on, perhaps the notion of interdisciplinary was in this kind of informal sense that you've described.

HELMER: Well, that's probably true. There're some exceptions to that. Membership in the math department should not necessarily mean that these were just mathematicians in the strict sense. For instance, Herman Kahn was in the math department, but his interests were much wider than that, and he was not primarily a mathematician, even.

The one study which I did together with Dalkey and Thompson on--what was it? A funny title. Moscow, Dayton, and Geneva Steel study. It was the study of the vulnerability of certain targets. Dalkey clearly was a logician. I was supposedly a mathematician. [Fred] Thompson, although he was in the math department, was very much interested in engineering problems, so he brought that kind of expertise to it, too. So there was a bit of a mixture there. Or the early gaming study of international relations--what was the name of it? I think it was called COW, C-O-W. What did it stand for? The W stood for war. I forget what it meant. Anyhow, it was an early study where Lasswell participated, also.

But there were a number of people from various departments. In fact, the guy who formally headed that COW project was a psychologist. So there was definitely some cooperation from different departments. But what you were saying is correct, that most of my publications, to the extent there was more than one author, were coauthored by mathematicians. But Nicholas Rescher, for example, is not a mathematician. He was just hired temporarily by the math department, but his field definitely is philosophy.

COLLINS: Another area that seems a little bit allied to what you were doing that was developing at RAND during the fifties was organization theory. Herbert Simon spent some time there, and there were some other people on the staff who got interested in that.

HELMER: Herbert Simon was the other economist whose name I couldn't think of. He was a Nobelist.

COLLINS: Did you have any contact with that activity?

HELMER: No, not really. Not very much.

COLLINS: Getting back to the war game stuff, one of the images that has been created about RAND in the fifties is that there was a lot of enthusiasm for war games. I wonder whether that's an accurate characterization, from your point of view. Certainly one gets the impression that people played it at lunchtime and after work, and that it was a way of bringing people together, in a sense. I wonder if you might characterize your understanding of the role of war games as part of the RAND culture during this period that we're talking about.

HELMER: I think that the group that was interested in war games as such was quite small. It did not go throughout RAND by any means, although occasionally, for instance, in Paxson's efforts, he drew in people from various departments for maybe a month at a time. I would think that maybe twenty percent of the RAND personnel was involved in war gaming at one point or another, but it certainly wasn't pervasive throughout the organization. Among this smaller group, there was a good deal of interest.

But then you're speaking of doing war games at lunchtime. That's something else. What was done there regularly was to play so-called Kriegspiel. That's not a war game. That's just a form of chess with limited information. Do you know how the game is played?

COLLINS: Yes.

HELMER: I think it used to be played in Princeton. I don't know if it's still the case. And it was taken over from there to RAND. It was played every lunchtime. It was a fascinating game to watch. Have you ever played it or watched it?

COLLINS: I haven't played it, no.

HELMER: Do you know chess?

COLLINS: Yes.

HELMER: You know how it is. You have two boards, and you have to guess what moves the other guy has made. There has to be an umpire who tells you whether your move, in view of the position of the other player, is legitimate. If a piece is taken, he takes it off your board, and the other guy simply is told whether it was a pawn or a piece that was taken. So that gives him a little bit of information, but it's very limited. But to watch it and see how gradually both sides know pretty well where the other player's pieces are on the board is just amazing to watch. It's fascinating. It's a nice game, but it's just a game with limited information and, except for the name Kriegspiel, has nothing to do with war games.

COLLINS: But in its formal qualities, it has some of the aspects of gaming.

HELMER: Sure. In any kind of real war games, you have also limited information against which you have to play, that's true. But the structure of it is so different. It has really very little to do with it.

COLLINS: To wrap things up a little bit, first of all, are there any aspects of your RAND experience that we haven't talked about that fit into our discussion here today?

HELMER: No, I think we more or less touched everything that I can think of at the moment. I think we covered it pretty well one way or another. You're a good interviewer, probably from many years of experience.

COLLINS: Right. I want to, as a way of summary, return to this theme of your starting with the theoretical and moving into very practical kind of things, because it seems to have been an important element of your professional life. Did you have any specific feelings about working on military problems? Certainly it seemed a very natural part of the postwar environment, having come out of World War II. But thinking back to your telling me about your dissertation defense and having Bertrand Russell sit on that, it would be difficult to think of Bertrand Russell sitting down and working on problems of the British military.

HELMER: That's right.

COLLINS: The culture of mathematics and logic didn't readily lend itself to that kind of interaction, and I'm wondering whether that's a kind of thought that occurred to you during this or afterwards about this professional path.

HELMER: Sure, it occurred to me, but I've always felt that there are some wars which are justified and some which are not, and clearly, perhaps because of my background, I felt that the war against Hitler was certainly utterly justified. The world just couldn't go on with that sort of thing, dominating people all over the place.

I had very different feelings about Vietnam. I felt very sympathetic with [Daniel] Ellsberg, because I think that the general public just wasn't well-informed of what was going on. I don't know that many people at RAND actually spoke up on the subject, but I do know that there was a good deal of disapproval of the way things had been handled there.

COLLINS: With respect to Ellsberg, you mean?

HELMER: No, with respect to Vietnam, the way that the war was conducted. Our feeling was that (a) there was really very little justification for that war in the first place and (b) if you wanted to win it, then that wasn't the way to do it, the way it was done. RAND contributed awfully little there. They should have done much more than had actually been done. Our troops, I felt, and I think some of my colleagues felt, were not given the full support that they should have had. We should either have gotten out, or we should have ended the war much sooner than actually happened. This is really almost in the nature of an aside, and it's just a feeling I'm expressing.

COLLINS: As a way of summary, are there any threads or themes that you would point to about your RAND experience?

HELMER: Looking back on this, I feel very good about having been part of this experiment. I really think that RAND, the way it was originally conceived, and the way it was certainly conducted for many years, I think was a very good experience. It was a useful thing from the national point of view, and it was very pleasurable, personally, to participate in this kind of an effort.

I think it was very unique and unusual because of the internal structure and the rather informal mission that was given to RAND when it was first set up. It was really a novel institution. It was not like anything that had been done before. It was exciting to be part of that. I learned an awful lot during those twenty-two years I was there, and I developed considerably. I was very naive when I first started, and I feel I learned a great deal from just being part of that and from my colleagues and their wisdom and all that.

COLLINS: Naive in what sense?

HELMER: Naive in the sense that I was still very much of a formal mathematician when I first started and not really fully conscious of the immense possibilities of applying mathematical and logical thinking to larger problems.

COLLINS: That might be a good place to end it right there. Thank you very much.