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After discussing his upbringing and education as an economist, Hitch (b. January 9, 1910) reviews his work in World War II for the War Production Board and Office of Strategic Services and his return to Oxford after the war. He then describes his recruitment as an economist by John Williams and Dana Bailey of RAND in 1947, input into the bomber study and other projects, utilization of economics in systems analysis, professional support given by others within and outside of RAND, and relations with the USAF and other government agencies.

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Interviewee: Dr. Charles Hitch

Interviewers: Martin Collins, Joe Tatarewicz

Location: At Dr. Hitch's home in Berkeley, California

Date: February 9, 1988

TAPE 1, SIDE 1

Mr. Collins: We'd like to begin this interview with a brief background sketch of your early education and family life, and then we'll move into a further discussion of your professional activities. If you can just for the record give us your parents' names, where you were born, and that sort of thing, to get started.

Dr. Hitch: Yes. My parents were Arthur Martin Hitch and Bertha Johnston Hitch. I was born in Boonville, Missouri, January 9th, 1910. My father was the head of Kemper Military School in Boonville, Missouri, which was a high school and junior college, and I attended all six years there. Then I completed my undergraduate education at the University of Arizona. I went for two years and received a bachelor's degree with highest distinction. Then I went to Harvard for one graduate year in economics, and during that year won a Rhodes Scholarship. So the following year I went to Oxford, and I was in Oxford for a long time. I was a Rhodes Scholar for three years, at the end of which time I was elected a fellow "don" at Queens College, Oxford, 1935. I was there when World War II broke out, and I stayed on. Of course quite a lot of the dons were leaving Oxford, but I stayed until June, 1941, when I went to London and joined Averell Harriman's lend-lease mission. And the war, in 1946, I returned to Oxford.

Dr. Tatarewicz: Could I ask you to just back up a little bit, and ask how you came to choose economics as your special field of study? What other fields of graduate study might you have done, were you interested in?

Hitch: Well, I was interested in two. I was very interested in law, or thought I was until I spent two summers working in a lawyer's office, looking up precedents for him, and decided that that was not the way I wanted to spend my life, looking up precedents. I didn't realize that that was such a small part of a law-

yer's business. I became very interested in economics because of the Great Depression which started with a great agricultural depression in the late 1920s, when I was making such choices. And it seemed to me from the elementary course I'd had in economics at Kemper, that here was a discipline that might help solve some of the problems of the nation and the world, like depressions. My grandfather Johnston, who had been the head of Kemper before my father, also fancied himself as a banker and started and was the head of a bank in Boonville, which was just crushed by the agricultural depression. He lost all of his fortune, which was not insignificant. And it seemed to me that economics just opened some hope to finding answers to questions of public policy of that sort. So when I went to Arizona, I decided to major in economics. I'd forgotten my interest in law by this time.

Collins: Was this a subject area that your father had an interest in and encouraged?

Hitch: Economics? No, my father started as a teacher and ended up as an administrator, and in a general way I followed that track. No, he had no particular interest in economics. He taught Latin and German until World War I came along, and German could no longer be taught. It was unpatriotic to learn German. So then he just taught Latin. But shortly after that he became the principal of the school.

Tatarewicz: Were there any professors at Arizona who had a particular influence on you?

Hitch: In economics? Not really. No. It was not a strong department there. At Harvard, on the other hand, there were several who had a great influence on me, especially Frank Taussig, who was perhaps the most successful teacher of economics in the country.

Collins: What was his particular orientation towards economics?

Hitch: He was in the mainstream of classical economics, and he taught the graduate theory course for I suppose two or more decades. So everyone who has a PhD from Harvard has learned much of his theory from Taussig, during the period that he was there. He was getting quite old when I went to Harvard and retired two or three years afterwards.

Collins: How is it that you came to select Harvard as the place to really begin your strong study of economics? Was that the natural place?

Hitch: It was the natural place. It was the place with the most prestige in the field and the strongest department. I can't think of any particular reason. It's not because I knew anyone there or anyone who had gone there. I just chose it. My father said he wanted me to go to Kemper for six years, the whole six years. He said, "If you do that, you can then go wherever you like." That was the deal. So I chose, to everyone's surprise, the University of Arizona, partly because I wanted to get as far away from home as I possibly could, and I couldn't imagine a place further away.

Collins: Is there anything else remarkable about your experience at Harvard that you want to add at this point?

Hitch: Yes, I just got very excited about economics at Harvard. I don't think I have much to say beyond that. The chairman of the department then at Harvard was a man named Burbank, who had nothing like the reputation that Taussig and others did, but he ran the department. He thought I was crazy to go to Oxford because Harvard was so much better. And he made it quite clear that nevertheless, if I was doing this crazy thing, he would like me to come back to Harvard to resume my education and get my PhD, which I never did. I just stayed on at Oxford until 1948.

Collins: Who were your principal mentors when you were at Oxford?

Hitch: I suppose my principal mentor was Oliver Franks, now Lord Franks, who was then the don in philosophy at Queens. I was reading, as the British say, the honor school of philosophy, politics, and economics, so I had to do a good deal in philosophy as well as in economics. There was one very good tutor that I had at Oxford in economics named Redvers Opie.

But apart from him, I felt I had rather bad luck in my assignment of tutors at Oxford and came away finally thinking that the tutorial system was not the source of Oxford's strength. It was very chancy. So much depended upon who you got as a tutor, and there was seldom very much you could do to control that.

Collins: How do you feel the Oxford experience helped extend your understanding of economics?

Hitch: I think mainly by having to teach it. I think that before you can really understand a subject you have to teach it. And that's what I did at Oxford, I mean when I was teaching, undergraduate teaching. And I think that's the principal thing I got there. Well, there was something else. It just happened, at that time at Oxford economics was expanding as a field that students were interested in, and the colleges were taking on econo-

mists, not in large numbers, but there was a group of about I suppose 15 or 20 of us, most of us quite young and very congenial. And we did a lot of cooperative work, which is pretty unusual in academic departments. We had an Oxford Economists Research Group which met regularly and which undertook projects. So those were experiences that were valuable to me. Many in that group went on to very distinguished careers in the field.

Collins: Let's move on to your work with Averell Harriman at the start of the war.

Tatarewicz: Before we do that, could I ask a couple of questions about what type of philosophy you were being exposed to at Oxford, and if that had any interaction with your thinking about economics?

Hitch: The answer to the last question is, no, I don't think it had any influence. It was Oxford-style philosophy, modern and semi-modern philosophy, and to some extent I enjoyed it but it didn't really take. I never thought that I was going to become a philosopher or was ready to teach philosophy, although my examiners told me I did very well in my philosophy as well as in my economics. I was surprised to hear it.

Collins: So this would have been Bertrand Russell and G.E. Moore?

Hitch: That's right. Those are two of the names, and also [Immanuel] Kant.

Collins: Okay. So what was it that you did as you began to work with Averell Harriman?

Hitch: I made studies of the British wartime material controls for him, and he passed them on to what became the War Production Board in Washington. But I was not engaged in any of his diplomatic negotiations on Lend-Lease. He wanted studies of the way in which the British were controlling steel and other materials, raw materials, in the wartime economy. It was something that the War Production Board should have available as inputs in designing the controls they were probably going to be putting into effect at a later date.

Collins: I'm a little unclear. What office or entity were you working for as you did these studies?

Hitch: Well, I was working for the Harriman Office in the American Embassy, but necessarily with the National Steel Board, the relevant war ministries, and the economists in the War

Cabinet office. The British had developed quite different sorts of control systems for each individual material, so that you had to treat each one separately. And you had to question, how could they be coordinated when they were so separately managed? So that was what I did. And I would write reports on this and they would be sent back. Then after Pearl Harbor I was sent back by Hariman to the War Production Board to work with it in the development of U.S. material controls.

Collins: This would have been back in Washington, D.C.?

Hitch: Yes, back in Washington, D.C. What we developed and put into effect after long debate was the so-called Controlled Materials Plan, CMP, and it controlled allocations of steel, copper, and aluminum, the three major metals. By the time I finished that, I had been transferred to the War Production Board and was drafted. They didn't protect their employees at the War Production Board. I was assigned to the OSS, Office of Strategic Services. Then I was shipped over to Britain to join an Anglo-American unit called RE-8, located in the Home Office at a town called Princes-Risborough. It was charged with assessing the effects of the air raids on Germany and the Continent from photographic cover taken before and after the raids.

RE-8 had earlier, following German raids on UK cities, developed methods for estimating from roof damage the effects on the economy of a city and its factories. We also measured the effectiveness of high explosive and incendiary bombs which led to our recommendation to increase the proportion of incendiaries in the mix of bombs. It also enabled us to make good estimates of the proportion of bombs hitting the target city. In the early years of the war that proportion was very small. But it improved.

By the time I reached Princes-Risborough the methods and factors developed from damage to British cities and industry were being applied systematically to German cities and industry.

Collins: That meant assuming that German reactions to air raids were basically the same assuming the British?

Hitch: That's right. That's right. Now our reports were not popular, especially with the RAF, but we just went ahead and ground them out. In the bombing survey that was taken after the war, they discovered that if anything we were too optimistic about what the effects had been.

Tatarewicz: I wanted to ask whether it was just chance assignment that got you into RE-8, or whether you actually went after that

assignment? You had heard of it?

Hitch: No. RE-8 was originally in the War Office. It was a British organization. When we got into the war, they were very anxious to make it a joint organization, which would include doing the same sorts of reports on American Air Force raids on Germany and the Continent. The head of RE-8, who was an RAF officer named Duncan Dewdney, requested that OSS send him somebody who could be his deputy in the organization. And it was OSS which selected me to meet that request from the British side. Amusingly, I was supposed while abroad to disguise the fact that I was in the service but no one at home was supposed to know that I was in disguise, so my wife and others in America who were writing me addressed their letters to Private Hitch. It was a very open secret in Princes-Risborough that I was masquerading as a civilian.

Tatarewicz: Why did they want you to be in disguise?

Hitch: Because I was a private.

Tatarewicz: Oh, okay.

Hitch: And Wing Commander Duncan Dewdney didn't want a private as his deputy. It was all right to have a civilian but not to have a private. Actually, they bucked me up to first sergeant before I'd been there very long, but that was still much too low for this assignment, so in Princes-Risborough I was always in mufti.

Tatarewicz: When you were trying to connect the evidence of bomb damage with the economic effects, did you find that the group you were working with had already developed the intellectual tools to do that?

Hitch: To a very large extent, yes. Yes, that was done quite early. And it was in the main completed by the time I got there. But of course there were lots of special cases that kept coming up. You couldn't apply these general factors to aircraft production or to ball bearings or to petroleum or to transportation, where you had to get quite special approaches in each case. But the basic framework had been developed before I got there.

Tatarewicz: And what types of other people were in this group? How big a group was it?

Hitch: Oh, of course the key people were the photo interpreters. We had a lot of well-trained photo interpreters, and we needed a lot because it's long, tedious work, piecing together what hap-

pened from high altitude. Then there was a biostatistician who provided a lot of the intellectual leadership in the early days, named R.B. Fisher, not the famous statistician R.A. Fisher, but R.B. Fisher from Oxford. There were mathematicians, engineers, statisticians, scientists of various kinds, many I think biological, although I can't think why. How many altogether? I think there were two although three hundred. And only 25 miles from Oxford so that on weekends I could go to Oxford and occupy my rooms in college.

Collins: How did this unpopularity of your assessments manifest itself? How were your reports used and acted upon?

Hitch: Well, we weren't involved in that, and I can't give you a full answer. I knew they were sent to and used by the American Air Force people, and of course they were sent to bomber command. That's where they were most unpopular. But they never tried to stop us. They let us go ahead, and it was common knowledge among the people in the business that we had probably the best dope on what was happening.

Collins: Was there any attempt in your reports to suggest changes in tactics that might improve the effectiveness of the bombing?

Hitch: We did that very seldom and were careful not to gain such a reputation, but as I have indicated we did point out that the mean area of effectiveness of the incendiary bombs was much greater than for the high explosive bombs, and this they did apparently want to know, and used.

Collins: So the implication being that it would be better to use more incendiary.

Hitch: More incendiaries, fewer high explosives. Now that wasn't accepted by everybody. Some people felt the psychological effect of big bangs frightened people and accomplished something that the fires didn't. That's the only important case in which I think we made any suggestion. That wasn't our business. It was just to report what the effects were.

Tatarewicz: As deputy to the commander of this group, the leader of this group, were you involved in the administrative aspects of organizing this activity?

Hitch: Yes.

Tatarewicz: Was that a new experience for you?

Hitch: I suppose it was.

Tatarewicz: Directing other people's work, organizing the flow.

Hitch: Yes. That was my first experience.

Tatarewicz: How did you learn how to do it?

Hitch: I just did it. I didn't have any courses, didn't go to business school, or study management economics. Oh, after I'd been there a year and a half, they brought me back to start working on Japanese targets. The Army Air Force, the Navy, and the OSS set up a joint office in the Pentagon, a Joint Target Group it was called, the JTG, to make studies of Japanese targets. And it was a group, I'd say, considerably smaller than at Princes-Risborough but still large. By that time I was a first lieutenant and I was the deputy head of one of the three major directorates. A naval officer is the head, and the naval officer thought it was dreadful that I was just a first lieutenant. He went to General Sanford, who was the head of the JTG, and pleaded with him to let me get a discharge and permit me to operate as a civilian with an appropriate salary and rank. Sanford just turned to him and said, "No, by God, it's all right for my majors and colonels to be taking orders from a first lieutenant, but not from any goddamned civilian!"

Tatarewicz: So you were introduced to professional soldiering.

Hitch: Yes, I was.

Tatarewicz: The system of prestige and relationship of soldiers to civilians.

Hitch: Yes.

Collins: What was the name of the directorate in which you were a deputy now?

Hitch: Physical Vulnerability. Physical Vulnerability. And they put me in that directorate because of all the work on physical vulnerability that had been done at Princes-Risborough. They thought I'd had my breaking-in there, could bring the Princes-Risborough findings into use for the Japanese targets.

Collins: What did physical vulnerability mean here?

Hitch: That meant what we were just talking about: the greater effectiveness of fire bombs than high explosive bombs, and the

physical vulnerability was the vulnerability of the buildings to attack by one or the other. I had no inkling of the atomic bomb. And I'm pretty sure that nobody did except possibly General Sanford himself. Nobody else in the JTG organization.

Collins: How far did this targeting study of Japan proceed? Was this something that you continued to do till the end of the war?

Hitch: Yes. Well, until the atomic bombs were dropped. By that time I was out. I got a discharge. I had a request for my release from the Office of Reconversion, Office of War Mobilization. Judge Vinson was the head of it, and he requested that I be released to join his office, which I was. But it was pretty much over by then, and I went back to Oxford in the spring of 1946. Is that right? Yes.

Collins: What were your intentions as you went back to Oxford? What did you want to do?

Hitch: I felt I would just stay on indefinitely. They'd been very good to me. They made up my salary while I was a private in the American Army, and I had tenure, and I liked my teaching role in Oxford before the war. And of course by this time I had a wife. I married my wife in Washington, and she went back with me. And I decided after a couple more years there that that wasn't the way I wanted to spend the rest of my life, tutoring undergraduates, even if they were good ones.

Collins: Was this an outcome of what you experienced during the war?

Hitch: I had a very interesting, exciting war, and it opened my eyes to all sorts of possibilities that I hadn't thought of before. And when RAND got in touch with me, I was ripe to be plucked because it was clear I was not going to spend the rest of my life at Oxford.

Collins: When did RAND get in touch with you?

Hitch: RAND got in touch with me in the summer of 1947, when I was a visiting professor at the University of Sao Paulo. I was down there for part of a term, I forget, a couple of months. Our summer, their winter. And I was teaching their graduate economists about national income and national product statistics, when I got a cablegram from John Williams and Dana Bailey. I knew Dana Bailey, who was another Arizona Rhodes Scholar, about seven years younger than I, but he was at Queens also. He was at Queens as a student when I was there as a don. And how he got to know John

Williams, I don't know. I have known but I just can't remember. But this cablegram was jointly signed by Williams and Bailey. Now Williams was head of the mathematics division. I think he called it "military worth" at that time. Dana Bailey was then for a brief time head of the electronics division. He was a physicist, a cosmic ray physicist. But he did not stay long. In fact he left before I actually got to RAND to take another job, so we did not overlap at RAND. But it was John Williams who was the moving spirit. He learned about me, I suppose, from Dana Bailey.

Collins: What was the gist of this letter that they sent you? Was it an invitation to talk with them about RAND?

Hitch: Yes. Could I stop off in New York on my way back from Sao Paulo and attend this conference in New York to talk about RAND, this new enterprise that they were just founding? Well, it sounded interesting. I didn't know Williams but I knew Bailey, and it sounded respectable and interesting, and I was able to work it out. That was right on the way home to Oxford.

Collins: This Conference of Social Scientists, as it was called, seems to be a fairly crucial event in RAND's early history in terms of the future composition and character of the organization.

Hitch: Yes.

Collins: Can you describe your experience there? How did RAND present itself, and how did you feel about what RAND was trying to do?

Hitch: Well, I felt that RAND had only the vaguest notion of what it was trying to do. The leader in this whole enterprise to get economists and social scientists in was John Williams, and the discussions that he held with me and other economists there really took a very odd shape. Of course much of the meeting was devoted to various people standing up and saying, "Well, RAND could do this and RAND could do that." I contributed, I think, a little to that, and talked about assessment of the economic effects of bombing and about the relative strengths of various economies to support major military enterprises. And I talked about the importance of cost analysis, which was something that some of the economists were qualified to do. But looking at all those things together, they didn't add up to a lot. I was sure there were opportunities here, if we got some bright people and got them moving, but the effects of strategic bombing, after the development of the A-bomb, didn't look like a terribly interesting, challenging subject. It was just too easy, and cost analysis

is not an awfully exciting thing as a rule.

Collins: Could you briefly describe what cost analysis is and what its relevance was to this discussion at this conference?

Hitch: Cost estimating. Yes. Well, clearly the central core of research at RAND would be comparing different weapons and techniques and delivery systems, and one of the things you wanted to know about each of these that you're comparing is how much it costs. So you'd better get somebody who's good at that, if there is anybody good at it, to do that for the organization. That's one place where certain economists' skills could be fitted in right from the beginning, in the kind of systems analysis they were trying to do at RAND. It never occurred to me at that time that the economics division would itself play a major role in the systems analysis game. I think that was because we didn't realize how badly they were being done.

Collins: Do you recall whether John Williams or others from RAND at that time presented this notion of systems analysis and its potentialities for the kinds of studies that they were talking about?

Hitch: Negative. John Williams was not interested in systems analysis. He was interested in basic research.

Collins: Was this notion of systems analysis, do you recall, brought forward at the conference?

Hitch: It was brought forward, yes. A little was said about it. Of course those that were under way were classified, couldn't go into much detail on that. But I expect it's pretty certain that [Ed] Paxson, for example, described his bomber study, in which he was comparing prop planes against prop jets against other conceivable characteristics of bombers.

Collins: Did you see this notion of systems analysis as taking the kinds of studies that you did in assessment an extra step, or operations research activities an extra step? Did you see it as a new kind of tool, or did it seem like something that had essentially been done in other forms before?

Hitch: It had been done in other forms before and in fact it had been done by economists before, to a limited extent. There were a number, for example, of economic studies, cost benefit studies, on the supply and use of water.

TAPE 1, SIDE 2

Collins: I lost track a little bit of where our discussion was when we turned the tape over. Do you recall, Dr. Hitch?

Hitch: I think I was saying that there had been various systems analyses done before. Economists didn't call them systems analyses; they called them cost benefit analyses but they're essentially the same thing. I could say more on that if you want me to. But Roland McKean, when he was at RAND, did a cost benefit analysis of water projects. I think water is a good subject matter for such studies because there's a lot of money involved and a lot of things you can measure. The other was the operations research fraternity, which developed mainly during the war, and they were doing what they called systems analyses. Did you read my paper in which I discussed the wartime convoy problem? And of course it was from that that the RAND approach to systems analysis developed.

Tatarewicz: But you weren't really impressed by your first contact with RAND at this conference on social science?

Hitch: No, I wasn't very impressed. Let me tell you one amusing thing that happened. Clearly John Williams had been given the job of recruiting these people that he wanted, economists and social scientists, and there were three fairly senior economists attending the conference. One was Ed Shaw, who was a professor at Stanford. One was Allen Wallis, who was a professor at the University of Chicago, and is now Under Secretary of State for Economic Affairs. John tried talking to each of us. It wasn't clear that he was getting anywhere, really. And he thought of this device. He asked the three of us to meet together, and he opened the meeting by saying that they very much wanted an economist to start an economics division, and they'd be very happy to have any one of the three of us. He asked the three of us to remain behind, and talk it out among ourselves, and decide which of us would go to RAND as the director of the new economics division. Well, that didn't work. We sat there looking rather embarrassed and not knowing what to say. Nobody volunteered at that point.

Collins: Nobody was genuinely interested.

Tatarewicz: Did you know Williams by reputation if not personally?

Hitch: No, I did not. Oh, I think I remember how Williams got in touch with Dana Bailey. They both went to the University of Arizona at the same time, and they were both avid stargazers at

that time, and that's where that friendship and that link got started. And then it was Dana who told John Williams about me.

Collins: So what feeling then did you leave with?

Hitch: Well, I think John sensed that I was more interested than the other two, and he followed up and made me a specific offer and went after me. Oh yes, he invited Nancy, my wife, and me to come out to Santa Monica for the Christmas vacation for further talks, which was a welcome thing to do from winter in Oxford. It was the hardest decision I've ever made in my life, I think, but I did decide to go with RAND.

Collins: You mentioned that the sense of RAND's mission as expressed at this conference was fairly vague.

Hitch: Yes.

Collins: What then attracted you to the potentiality of RAND?

Hitch: Oh, I don't know. I don't know how I can put it in words. There were quite a number of factors. One was that it was a very good offer. Another was, I had found the Princes-Risborough experience very exciting, and while this was different it in some respects promised to be similar. And I'm trying to remember whether I really had a close look at one of these RAND systems analyses before making my decision. I'm not sure. I'm just not sure. Ed Paxson had the biggest one going at that time, comparing different characteristics of bombers that might affect the choice of a preferred bomber. And you know, he treated it as a transportation problem. His criterion was, how to minimize the cost of transporting various tonnages of bombs from various bases to various targets in Russia. To minimize the cost of transportation. Not a word about the vulnerability of the bombers or anything else except transportation costs. Oh, he did give some weight to the number of people killed, but this was to the number of flight crews killed, not people on the ground. As I say, if I had seen this I suppose it would have convinced me that I should come and do something about it, but I'm not sure whether I did or not.

But I did decide that I'd give RAND a try. It was obviously a somewhat speculative enterprise, after three years. Of course there were other factors. My wife did not like it in Oxford. She didn't like the climate. She didn't like the way women were involved or not involved in Oxford society. And she was quite happy to find a good reason for going back.

Collins: How were your interests in economics developing? You'd had your academic training, you'd had your wartime experience, you'd had some opportunity to reflect after the war. What did you see as the major areas where you could be original or that you thought were really ripe for investigation or study?

Hitch: Well, the one that really intrigued me was the economics of government expenditure, which had been almost completely neglected by the whole economics fraternity.

Collins: Was this idea something that you were thinking about when you were considering departing Oxford?

Hitch: Yes. And I have now been plowing that field for quite a long time, all of my thirteen years at RAND and later in the Pentagon. And then at Resources For the Future. It was my economics agenda for my whole career.

Collins: Okay. We can develop those ideas in more detail as we go along here. So John Williams had, in a sense, persuaded you to give RAND a try.

Hitch: Yes.

Collins: What were your first impressions, as you came out and began to work on the kinds of things that you felt needed to be done to establish this economics division?

Hitch: Of course the first thing was that I had to hire some people, and that occupied a good deal of my time in the first two or three years. And then I wanted to get various things under way which I thought we certainly would want to be involved in. One was cost analysis, where I hired Dave Novick to run the department, which he did in his inimitable manner.

Another was logistics, led first by Stephen Enke and later by Charles Zwisk. And then I had some ideas about how we might compare the relative strengths of the Russians and the Americans in supporting military programs. It was based on an assumption that there was some rationality in the Russian pricing of, in particular, producers' goods. If there was rationality in it and the prices bore some relation to the factors of production needed to produce these producers' goods, and since the prices were available, I thought you might be able to learn quite a lot by comparing the relative efficiency of the Russians and the Americans in making different kinds of products. I got work on this started quite early with the help of some people at the Stanford Research Institute and Batelle Memorial Institute. I suppose that was the

first project I launched. As it turned out, it was something of a blind alley. There just didn't seem to be that much rationality in the Russian pricing.

Collins: How were you able to reach that determination?

Hitch: Well, we just couldn't make any consistent sense out of it. You know, we couldn't say for this type of product it looks like the Russians are better than we are, relatively speaking, and for others, not as good as we are, and so on. We couldn't find any plausible patterns that looked like we had something to sell. So we gradually wound that one up. But by this time I was beginning to get interested in the Paxson systems analysis and other systems analyses going on. There I saw an opportunity for us.

Collins: What was the initial reception by other departments at RAND to the notion of establishing an economics division? Were you immediately welcomed in as colleagues, or did you have to go through some kind of period of adjustment and acceptance?

Hitch: I would say that we got accepted pretty rapidly. I was thumbing through the annual reports you sent to me to see where they start acknowledging the role of the economics department in systems analysis. Here's one, 1953, which is after only five years: "Economic Criteria and Specific Costing Data are Regular Contributions to RAND Systems Analyses." I would say we got accepted with remarkable rapidity. Of course, to a limited extent that was because we were doing things like cost analysis that they would have had to do and they really weren't interested in doing, but they recognized that you had to have it. Yes, 1955, two years later, there is a specific reference to the--this was quite general--use of economic criteria for selection of preferred weapons systems. That was 1955, where our role was clearly recognized. And I'd say that on the whole our relations were quite good. It was put to this test in the Wohlstetter Base Study, which, it points out in this annual report, was centered in the economics division. That was one we did mainly by ourselves, and perhaps the most influential one that RAND did up to that time.

Tatarewicz: I do want to talk about the Wohlstetter Base Study, but before getting to that, I'd like to know what your relations with Frank Collbohm were like. Or perhaps to put it more generally, when you arrived at RAND who were you talking to, to get a sense of what you were expected to do for RAND?

Hitch: I wouldn't say I got much help from Frank Collbohm on it.

And in this case I can't say I got very much help from John Williams, because he wasn't interested in the systems analyses. His real interests were in basic research and computing. There really wasn't anyone who took me under his wing and introduced me to everything that was going on. I certainly got much help from the other economists, from Dave Novick and Stephen Enke and Armen Alchian and Roland McKean, who were all contributing.

Collins: These were all people that you hired after you came to RAND?

Hitch: Not all, no. Stephen Enke and Armen Alchian had been taken on by John Williams as economic consultants, so they had been there, not full-time. They both held chairs at UCLA but they spent a good deal of time at RAND, and I'm sure helped in breaking me in at RAND and introducing me to what was going on in the various areas.

Tatarewicz: If RAND wanted you to build a new division, an economics division--it's not a teaching university, where somebody wants you to start an academic department, it's pretty obvious what you need to do. But at a place like RAND, was it just left entirely up to your creativity to build the division and find something for it to do?

Hitch: Yes.

Tatarewicz: You're referring to?

Hitch: Jack Hirshleifer. He designed his own study and carried it out, and nobody interfered with him, either from outside or inside RAND. Read the preface in his recent book.

Tatarewicz: So they went to all this trouble to recruit you and entice you there and then just said, "Build the division and do good work"?

Hitch: That's right. And I recruited Jack Hirshleifer and told him the same thing. Not build the division, but do good work, and he did.

Collins: Still there must have been kind of a feeling out period, in which you figured out how this economics division could be most useful to RAND and how it could best augment the activities of the other departments.

Hitch: Yes.

Collins: Do you have any observations about that process?

Hitch: Well, Roland McKean and I wrote a book called Economics of Defense in the Nuclear Age, and it's all there.

Collins: The book, written I believe around 1960?

Hitch: Yes.

Collins: Just lays out the theoretical elements of how the impact of technological change, the availability of nuclear weapons, and all of that affects the defense enterprise.

Hitch: Yes.

Collins: But it doesn't really address the internal specifics of how RAND did its work.

Hitch: No, it doesn't. It doesn't do that. It wasn't intended to do that. But that was the mode that we worked out, by which we did our work.

Collins: How, or did you, draw on resources outside of RAND, say in terms of working with universities or developing your own relationships with the Air Force?

Hitch: Well, first, with the universities we did from the very beginning establish a close relationship with the Stanford economists. Ed Shaw was at the New York conference, and I got to know all of the Stanford people quite well and used them pretty extensively. If you look at those summaries of personnel in these reports, you'll find a lot of consultants among the economists. Now some of those were Stanford. Some of them were from a good many other places. It was our custom in the economics division to bring economists out for a summer, and those who contributed, we'd invite back another summer and another summer. Other divisions did that, too, but not as much as we did.

Collins: How did you decide what problems or issues these consultants might work on, or was it up to them, in the RAND spirit?

Hitch: Mainly up to them, in the RAND spirit, yes. Well, I have to make a distinction. On this project of economic strength to produce military goods, there our relations with the people who were working on it at SRI and Battelle were specified by us. They worked on the details, but the general character of the work was specified. But that was exceptional. When we got Paul Samuelson to come out for the summer, or Tom Schelling, we just said, "Look

around, see what's going on that interests you and pitch in."

Collins: So there was a general understanding that they would be sensitive to the kinds of issues that RAND was interested in.

Hitch: That's right.

Collins: But when it came to selecting something specific, it was up to them.

Hitch: That's right. Just like Jack, who was of course an employee, but it was the same with the summer people.

Collins: What general benefit did you feel the economics division gained by having this ongoing relationship with the university world?

Hitch: I think it was on the whole very worthwhile. I think we learned a lot from it, and I think many other economists around the country had a favorable impression of RAND because of it.

Tatarewicz: This study of the impact on or the ability of the economy to sustain a war, if I'm correct, was also making use of the Russian Institute at Columbia University?

Hitch: No. No. That was related. That was related but that was different. We did do quite a lot of work under Joe Kershaw of a more conventional sort, on the strength of the Russian economy, and that involved the people both at Harvard and at Columbia. The Russian Research Center at Harvard and--what did they call it at Columbia?

Tatarewicz: The Russian Institute at Columbia.

Hitch: Yes. Well, Abe Bergson from Harvard was the most active of those consultants, and he came out frequently and gave a good deal of direction to that study. The people in the government who were most interested were in the CIA, so I established relations with the CIA to make it something of a two-way street.

Collins: Just to follow through the university thread a little further, how would these consultants communicate what they were learning to, say, the RAND staff or ultimately perhaps to the Air Force or other clients themselves?

Hitch: Whenever an employee or a consultant wrote something he wanted to communicate to others at RAND he would propose and issue it as a Research Memorandum. There were lots of Research

Memoranda, which were informal and mainly for internal use. Some later became RAND Reports after review and revision. RAND Reports were our principal formal mode of communication with our clients. I'm not sure I'm answering your question.

Collins: Well, I think that's one answer. Were there other informal mechanisms by which the economics division staff interacted with these consultants?

Hitch: Yes, we held seminars. If somebody that we brought in for the summer had been working on his own on something that was of general interest to RAND or to economists, we asked him to talk to a seminar.

Collins: Was there a sense in which these consultants became part of the RAND ambience, RAND life, and generally participated almost as employees?

Hitch: Yes. Yes, that's right, for the summers. Occasionally we'd get one out for a year but usually just for the summers. And yes, we took them into our social life. It was a good thing for RAND and for us and for them.

Tatarewicz: How easy was it for you to recruit people? You were after all asking people to leave the academic world and join an organization which is neither fish nor fowl at this time. I mean, it was a very novel idea. It's not even a typical research institute.

Hitch: No.

Tatarewicz: Could you tell us something about the way in which you did your own recruiting, and the kind of reception you got say from people who decided not to come to RAND?

Hitch: Daniel Ellsberg. I recruited Dan Ellsberg. I don't know; I think the proof is in the results. We did get some very good people to come and join the staff. There were certain people that you just obviously weren't going to get. You weren't going to get a Paul Samuelson, under any circumstances, to leave MIT and come to RAND. And there were many in that category. But if you go to a slightly younger and less distinguished group, I think we were pretty competitive. We'd get the best people out of the best universities, including Dan. Dan was a junior fellow at Harvard. They considered him the brightest thing they'd had practically forever in economics. We thought he was quite a catch. We didn't know what we had.

Collins: He was a very productive worker for a number of years?

Hitch: No, he wasn't, actually. That was not my impression. No, I think this may have been one of his problems, that he never really produced anything substantial. Except The Pentagon Papers.

Collins: A different sort of production. Well, we've talked a little bit about the university connection to RAND.

Hitch: Yes.

Collins: Let's talk about how you built up your relationship, I assume primarily with the Air Force and then with the CIA, how you went about establishing a relationship with the outside clients.

Hitch: Oh, it's hard to generalize.

Collins: Well, you mentioned the CIA. Let's look at that example.

Hitch: Yes. That one was a fairly straightforward, conventional economics, national product, national income. These measurements either did not exist at all in Russia or were done in such a way that they were unusable for our purposes. And we did sit down from time to time with the CIA people to discuss our problems and our progress.

Then, I didn't personally have a lot of contact with the generals. As I think you may have gathered, my principal interest had been improving the quality of the work produced within RAND. When [Albert] Wohlstetter would take his base study around and present it, I frequently went with him. We learned a lot about Air Force attitudes by bouncing that against them.

Collins: Was that your first extended experience with briefing the Air Force on a study or had you been involved in that activity before?

Hitch: There was one important prior example, which occurred in 1952. We'd had Edward Teller as a visitor at RAND. He was a fairly frequent visitor. But on this occasion he briefed us on the status of the H-bomb, and Frank decided, Frank Collbohm, that this was something we should brief the Air Force on. We made a little study involving about four or five of the divisions, and took it to the Pentagon and other relevant parts of the Air Force, including NATO.

Collins: This must have been just when the H-bomb was being considered for production. It was under discussion about whether or not the Air Force and the AEC [Atomic Energy Commission] should proceed with actual production of the H-bomb.

Hitch: Yes.

Tatarewicz: This is the study on the effects of the H bomb and the implications that those effects would have on planning and operations.

Hitch: Right, and I'm trying to recall who made the briefings to the Air Force. Yes, in addition to myself, there were Bernard Brodie of the Social Science Division, Ernie Plesset, head of the Physics Division, and Bob Schairer, head of the Aircraft Division. All four of us participated in the Pentagon briefings, but I was sent alone to brief Gen. Norstadt, then NATO Commander.

Collins: How do you recall that interaction? Here's an instance where several elements of the RAND organization were brought together to assess this development, and a very important development, the H-bomb. Do you recall how well that worked?

Hitch: Well, let me say that was an unusual way for it to work in RAND, to have that many diverse people involved. But I think it worked fine.

Tatarewicz: It was unusual to have that many people from that many divisions and that many diverse backgrounds involved on a study at that time?

Hitch: No, that's not quite accurate. You have as many people from as many divisions on a good many studies. What do I really mean? In a sense this was done by four people from four different divisions. It wasn't a big study. It was one that the four of us just did by ourselves, working together, because it was regarded as just very highly classified at that time. And so there were just the four of us, and we did the study, and then we went to all these places and then gave the briefing.

Tatarewicz: How would that contrast with the way a similar study would normally be done at that time? I'm trying to understand what made this a different way of operating with this study.

Hitch: Well, I'm trying to answer your question. In the first place, it wasn't a systems analysis; it was just a quite conventionally organized study. But it didn't involve a lot of people from all over the organization, which made it different.

Collins: It sounds analogous to the kinds of assessments that you were doing during World War II.

Hitch: Yes. Yes, it wasn't an optimizing systems analysis. It was more descriptive. You know, I'm going to have to abandon you. I think I warned you.

Collins: Yes, certainly.

Hitch: Herb York is having lunch with some of us on the campus.