Lloyd Shapley
February 9, 1994

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Interviewee: Dr. Lloyd Shapley

Interviewer: Mr. Martin Collins

Location: UCLA

Date: February 9, 1994

TAPE 1, SIDE 1

MR MARTIN COLLINS: To begin our discussion, I'd be interested in just a brief summary of your family background, where and when you were born, your parents, and the development of your educational interests.

DR LLOYD SHAPLEY: Okay. Born in Cambridge, Massachusetts, into a scientific family. My father, just before I was born, had been appointed director of Harvard Observatory. So my older brothers and sister were born in California, Pasadena, where my father had a stint at the 100-inch telescope at Mount Wilson.

COLLINS: And you're referring to Harlow Shapley.

SHAPLEY: Harlow Shapley. Yes, indeed. So I grew up in the director's residence at Harvard Observatory, Observatory Hill, in Cambridge. Went to school. How much do you want there?

COLLINS: I'm just curious about how your interests developed.

SHAPLEY: Well, I showed rather early talent in mathematics. I had two brilliant, straight-A students for brothers, and I guess my sister, also older, was--I never got a reading on her--she seemed to be rather bright, too. Nevertheless, we would play mathematical games sometimes around the house, play with cards and multiply them, do things like that. I had this kind of boost from trying to out-excel my brothers, who were four and six years older than I was, and I did fairly well. So I had a family reputation of being the math whiz, though the other guys were pretty good, too.

COLLINS: Did you go to Harvard as an undergraduate?

SHAPLEY: Yes, I went to Philips Exeter Academy before that. That's worth mentioning, two years, then Harvard, and did well in mathematics and badly in almost every other subject. I had a very spotty academic record. Then in the middle of my junior year, I was drafted, in 1943, and spent three years in the army, three-minus years. Got discharged right after VJ-Day.

COLLINS: What part of the army were you in?

SHAPLEY: It was called the air corps. This is before the reorganization. So I was in the army air corps and was a private, private first class. I rose to corporal finally and wore another stripe, but was assigned to a secret air base in western China, a weather station which intercepted broadcasts. Weather stations had to cooperate with Chinese Communists and Chinese Nationalists.

COLLINS: Did you have a technical or a scientific role in this?

SHAPLEY: Yes, I went to weather school. I was not a forecaster. I was just a weather observer, you know, how to plot maps, you know, how to interpret them. It was a ten-week training course getting into the weather, which, I guess, a lot of people who did get into it, whom I've met since, got into that.

But this base also involved a certain amount of cryptoanalysis. We had to break codes. We didn't have to, but it turned out it was a kind of emergency where no one could change the code. I was put onto that because I had a high score on what the army called IQ. We called it IQ tests, where I was mathematically good. But I was still sort of an undergraduate-type mathematician, had no clear career objective. So as a result of that, this record here shows me a Bronze Star. This was a medal for breaking a code. That's when they promoted me to corporal, which meant I got $56 a month instead of $52 a month. I advanced at a very low level very fast. But that was an achievement there. Of course, the experience of being in China at that time has affected me ever since, as far as my interest in the Far East, and especially the reopening of China which occurred ten years ago. I've made several trips since then.

COLLINS: So were you discharged then after VJ-Day?

SHAPLEY: Discharged, yes. On VJ-Day we were out there. We had to get to China by flying over Burma and the Himalayas. It was called "flying the hump," before the word "airlift" was invented. So within a few months, I was out and returned to college for the spring semester at Harvard.

COLLINS: Did you have any role while you were there in contributing to the bombing raids on Japan?

SHAPLEY: Yes.

COLLINS: What was your function in that respect?

SHAPLEY: On the same base, there was sort of a small group about the size of the "M.A.S.H." cast, if you remember what M.A.S.H. was like. There's a lot of residents there. The Korean War was still fought with the same kind of jeeps, you know, they had surplus stuff. And except for not having nurses in the same sense, we were really about as funny as--there were about fifty of us in this compound.

They were trying out the B-29s, the new big bombers that were going to subjugate Japan. They were twice as big as the ones that were-or at least had double the load and the range of the ones that were used in Europe. They were produced without any real testing when they were first sent to China to make their first runs, as I said--sent to India, and they would fly over the Himalayas. So they built an enormous airfield right outside our door, and 100,000 Chinese worked on it. It reminded one of the picture of the pyramids being built, long lines, carrying. You couldn't have concrete, nothing like that, and not have machinery, particularly—-this was inland China--but a lot of manpower and a lot of good will. Everyone was sort of in favor of us: we against the Japanese. It was a kind of popular American situation. It was a cobblestone, two miles long, could carry these great heaviest planes that had ever been built.

So a great construction project went on, and eventually something called the Twentieth Air Force. And the commander of this was the famous Curtis LeMay,[1] who, of course, hadn't become famous yet, but he was a big bomber person. Let's say, six months before the bombing began in earnest from the Marianas, Guam, Saipan, he started smaller raids. They eventually got up to about 100 bombers. They had four airfields, of which ours was one. So we had these twenty giants, and they would fly over and they'd bomb eastern Japan, and later on they bombed targets in Korea and Manchuria. The timing was such that about the same day they started the raids--it was June 1944--that was about the same time as Rome fell, the same time as D-Day. And, significantly for the Pacific war, it was about the time of the initial American landings on Saipan and Guam, the island-hopping. I don't think the Philippines had been landed in yet. That was about the chronology of the war.

So, in a certain sense, even though it wasn't laid out, it was clear that, yes, the Marianas were the right range for these planes to hit at any point in Japan. So about the time we started our things, about every two weeks, there would be a raid go off. So that's some private history, and I saw how we fit in the world. It was interesting--high technology.

COLLINS: Your weather activities were basically to help coordinate the activities of the airfield in terms of appropriate times—

SHAPLEY: No. It was called the weather center. It was gathering from everywhere, including intercepting Soviet weather, because that's the source of the weather, and then made long-range forecasts. As far as the B-29s were concerned, they didn't like to park on the ground in China. The Japanese had the capability of coming over at night. In daytime, they were sufficiently opposed by the Flying Tigers. And there's not much in the way of radar business there. But if

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[1]Curtis E. LeMay (b. 1906), U.S. Air Force general, pioneer of strategic bombing concepts in World War II. Commander in chief, Strategic Air Command, 1948-57; chief of staff, U.S. Air Force, 1961-65; American Independent Party vice presidential candidate (ran with George Wallace), 1968.

the planes remained parked on the ground, they could be bombed at night maybe. So anyway, the B-29s were shuttling back and forth from India just carrying the fuel and the bomb loads they'd have to carry, because the question of getting tonnage across the hump was a serious thing. We were stretched to the limit, and all we carried was ammunition and fuel for the Flying Tigers. things like that, coming across the hump, not up to where we were in Szechuan, but in Kunming.

Anyway, it got into a period that every two weeks they had enough fuel ready, so then the planes would come over in the evening. It was pretty hard to land at night because you didn't have a lot of aids. We were some many miles, a great distance farther west than in Chungking. So the Japanese would have to, in fact, stage their bombers up to reach us. So they bombed occasionally. They never hit anybody much. We had tremendous air advance warning because of all the walkie-talkie radios that we'd distributed to the Chinese everywhere.

But particularly because of that setup, they wanted to know will the weather be good three days in advance over the target area. They wanted what they called a long-range, which was about as good then as they are now. After a little while, you'll get good accurate forecasts, and you hope for alternative things. But the weather, see, moved down from Siberia, the fronts, down from Siberia, across China into Japan. So we had a lot of radio receiving things, and then we'd prepare our maps based a lot on sort of extra classified stuff, intercept data. We got some Japanese intercepts and a lot of Soviet stuff that we could take out of the air better than they would send us, because the cooperation wasn't in any sense close. They were not, in fact, involved in the western war. You needed masses of data, because the quality was very poor. So we were handling that kind of stuff and digesting it, forecasting, and then we'd send them out to all the airfields within range on a particular day.

So at various times we were at sort of the center. Our colonel, commander in the air force, would go over and brief LeMay on the weather. Not only that, we even intercepted U.S. Navy. They used the air force for keeping track of weather in the Pacific, which didn't affect much except hurricanes as they went from east to west. So the possibility of a hurricane coming over Japan, we don't hear about it from the northwest. They come from the east. And predicting where they'll hit shore—I remember one time in which the navy had been reporting on a hurricane that was sort of marching at the usual speed across. Everything depends on whether it will turn, and where it was at any given time. And then missing broadcasts, and they lost track of it, and they gave us a coordinate, but it was straight extrapolation. It could easily have turned. We couldn't predict. But in that case our guy had to tell them, "Well, you either go or don't go," and he had to sort of grit his teeth and say, "Go, I think." Explaining it was all the navy's fault if we missed. And, in fact, it turned out we came out over Nagasaki and were right in the eye of the whole hurricane. It just turned right up. That was good because our bombs were simply dropped into the middle of the hurricane and probably splashed in the bay. The hurricane did much more damage than we could have done, anyway, to the port and the harbor and all that. Typhoon would be the word, not hurricane. So those kinds of anecdotal thing.

I had an interesting experience, and it was much more than I could hope for, being just a draftee. I used a little bit of my talents. I think it maybe earned me a letter of recommendation when I applied to the job at RAND, from at least one guy who was aware of what I had done cryptographically, as an amateur. So that's the beginning of a career.

COLLINS: At the end of the war, what were you thinking about in terms of your further education?

SHAPLEY: Well, I went back to Harvard and finished. So that was a winter, summer, winter. That would be class of '44 for alumni purposes. But June '47, I finished Harvard. George Marshall gave the commencement address and announced the Marshall Plan. I was there.

But then I didn't know what to do. I wasn't set to go to graduate school. I wasn't aimed that way. I didn't have a concept, really, even what a mathematician would be. I was sort of not well-organized. My brothers will quote my father as saying, "You know, this guy may be on our payroll for life." And I have a younger brother who was more to worry about than I was. So I wasn't generally, obviously, headed to an academic career.

So I was hanging around a bit at my parents' home in New Hampshire and traveling here and here, visiting old buddies from China, a little bit of that, going around with them and making desultory applications. Willis [Shapley]--you know, they were on my side--he suggested I might be interested, and he mentioned two things. One was called OEG [Operations Evaluation Group], which is a navy operations research thing that was sort of starting up with somewhat of the same ambitions as RAND, and that was in Washington. And then he mentioned this RAND, not a corporation, of course. It was Project RAND. This was in fall of '47.

So I had a résumé of some sort and sent it off and got a telegram back. You don't just pick up the phone in those days, you know, just fly here and there. I'd come back on a slow boat from India--rather, a fast boat, but it still took a couple of weeks. Been around the world and all that. So air travel wasn't that big. Anyway, California is a long ways off to drop by for an interview. But they liked the résumé, and John Williams, who was sort of my mentor throughout the whole RAND experience, as long as he lived, sent back, "Okay, you can start at $300 a month." I think it was $305 a month, something like that, I think, five dollars more.

As part of the informality, please report on January 1st. Of course, that's a holiday in most cultures. But anyway, I took what they called an unscheduled flight, which is a way of beating the price-fixing, because there was a great surplus of airplanes, even then in '48. Mostly DC-3s, the workhorse. They had an army name, too, but they were all over the place. And so they had these non-regular airlines that regularly flew across the country. I think that flight was Boston to Buffalo to fuel up a bit and to Chicago somewhere, not the new airport, but the old airport, whatever it was, and then all the way to Burbank. That was really a long flight, and I think there weren't too many on the plane. So that's how it went. You spent about a day crossing the continent.

COLLINS: Just to go back to Harvard for a moment, you did graduate with a major in mathematics?

SHAPLEY: Yes, I was a math major, and mathematically I was distinguished, and then I was on the William Putnam team--you know the Putnam rigmarole--twice. I guess once before and once after. They had people try out for it, and then you solve problems for a while. It's like a team. It's a competition.

COLLINS: Do you see your interests crystallizing around any particular aspect of mathematics at that point?

SHAPLEY: No, no, no. Mathematics I didn't have a good comprehensive view of, even from the undergraduate part.

COLLINS: Who were your influences then at Harvard in terms of your interests?

SHAPLEY: The ones that I remember mostly were courses, and, of course, other people that I keep meeting now were part of it. Among the teachers, one made a very--I guess took me by surprise, anyway--was Quine, William van Orman Quine, who was a mathematical logician. And I hadn't, prior to that, had or even done any reading in a formal logical systems, mathematical logic. He presented it beautifully. I just gobbled it up, learned how to be logical in the sense mathematical logic . And of course, a good introduction to the group of ideas that culminated in [Kurt] Godel's Theorem, which he had difficulty completing, completing a logical system. Quine taught out of his book, which was to have solved the problem, although it developed that there was a flaw in his proof. So in the middle, he passed out a little printed sheet called "Core Agenda." You know, "Read this." I don't know if you know something about mathematical logic. Maybe you don't.

COLLINS: Yes.

SHAPLEY: As part of the course, I think he had us read [Bertrand] Russell and [Alfred North] Whitehead, which was a very ancient, but the first really serious attempt to do it, two great big volumes. And there's Russell's Paradox. Russell and Whitehead have the theory of types, where there's a hierarchy of types and elements of a set have to come from a type. So you have sets of sets: you can't get out of those circularities that lead to the set of all sets that don't contain themselves, that sort of stuff. And that's had flaws, too. By then they had been sort of worked out. But Quine was proposing an elaboration of that system which seemed to work for a long time, enough so he published papers and books, and people worked on it, and then the devil reemerged.

The hope is to get a system of axioms that's strong enough to prove everything is true without proving even one thing that's false. Because when you prove that one thing is false, then everything can be proved false. Every false thing can be true, too. And that's, of course, was

what, let's say, clarified by Godel. So I, at that point, was excited about something that was frontier, something I hadn't really thought of, implicit in so much mathematics. Of course, I knew what a proof was from Euclid. I think Exeter taught us out of the Greek, except translated. Well then, you knew what a proof was. So that kind of thing.

So that was nice and a course, that I enjoyed more or less. Talking about Quine, his personality also was part of it quite a bit. I don't know how well he remembered me. But the early years of RAND, what we'll be talking about later today, I guess, would be the RAND summer, which would bring in great flocks of pure mathematicians who were looking for a way to spend the summer. Quine came, I think, one or two summers and did some game theory and other things like that. So I had a reinforcement of that, and it was kind of nice that I knew him. So that's one, and there's other people.

COLLINS: What were your first impressions of RAND when you got out here?

SHAPLEY: Yes, the first impression. Maybe I was making too much of a fuss about it, but I arrived January 1st. It turns out not only were we taking that day off, but that was maybe a Thursday, and they decided to take the Friday off and the whole weekend. So I get there, and the guard was saying, "No one is going to be here for four days." So I had to sit around for four days. Ran out of money. I remember that. Because the banks weren't open either, and I didn't have an account. Then I got in, it was a very free and loose, friendly kind of an organization.

I think, throughout my education, in a sense even now, I'm not all that disciplined in getting places on time or going to bed when I should so that I can get up when I should. And RAND is sort of nice--it's open all the time, twenty-four hours. People work at night, late, and you can just go in there. Of course, you have to get past the guard, and you have to get your clearance and so on, which took me about a year. But even without that, I had access. I didn't have access to the [unclear] stuff.

The other part was that there's enough interesting here to do. At that time, the mission of RAND was very open. You'll have better statements of it, but essentially the air force was wanting to keep contact with the scientific community on any pretext. They didn't have a task for them, but they wanted--the air force, in particular, and maybe the military more generally. "Hap" Arnold was the guy with the power who did it. We were painfully aware that here is the first time where a war has been decided or profoundly influenced by the technology that was produced at that time. Usually the military advances help you fight the next war. The tanks were invented in World War I, but they didn't really turn the tide. They might have if they had played them differently. By the next war, everyone had tanks, and they didn't have trenches anymore, you know. So the warfare would take place; the change would be between wars. And here are, in this war, things like radar, not to mention the bomb, and others, proximity fuse, all kinds of rather important, useful things that maybe didn't exist at all before the war.

So the military felt that, yes, we should keep in touch with the scientists after the war.

This was Arnold's theme, as I got it second-hand or third-hand: "So let's just not give them an assignment. Give them some money and say, 'You think of some problems and tell us about it.'" It was a kind of wide-open contract with the air force. This led to people like [John] Williams putting together a rather motley crew of people. He hired philosophers. We'll talk more about that. And he hired crazy students from the math departments—me. I didn't get my degree in '47. I finished all the requirements for the degree, but the last semester I also failed two courses, and I needed two to complete whatever the number was to qualify for the degree. I took four courses over the load you take, and I failed two, and maybe got a couple of As in the other courses. The math courses I was generally good at and music courses sometimes. So they said, "No, you're on probation. We can't give a degree to someone on probation. If you survive for a year, you'll get the degree next year." You see, and what am I supposed to do? Get a job or something. It was very vague, what I was supposed to do, and they wanted me to write a letter, and I figured, "To hell with this." So I wasn't all that gung-ho about Harvard. My performance at Harvard hadn't been that much. Of course, a great deal of education takes place in a person hanging around--of course, I'd hung around Harvard beforehand.

Somewhat interesting, as a personal note, since you know Willis. He started at Harvard and possibly got into roughly the same kind of--things weren't working out. Too close to home. That's part of it, anyway. So he transferred to Chicago, did well there, I guess went on to a bit of graduate school, managed to get into a job where you can get drafted or you can get a wife. Both my brothers stayed out of the service, but did great work, I suppose. And partly, I think, Willis's experiences led my parents to send me--I wasn't happy about it--away to a boarding school, so I'd have some kind of independent living away from home. Not that I would necessarily go to Harvard afterwards, but they saw that Willis's problem was that he'd never been away from home.

COLLINS: You're talking about your Philips Exeter experience then?

SHAPLEY: Yes. I was going to Philips Exeter for the two years. There was a private school you went to before that in Belmont which is--just take the bus. So I ended up not poised to go to graduate school. I didn't know what I was going to do. I was a big music lover, but I had no technical skills there. So I even went to a course at the Union Conservatory one summer before I went off.

But then RAND's signing me was when things started. I must have looked like a kind of oddball. I think OEG did not--I think they made an offer for me, too, also. Simultaneously, I think I got an offer from both. But there it was a very different thing. The guys kept saying, "We run a tight ship here." I was sort of not the navy type. I was more the army or air corps type. I got through without serious disciplinary problems, but I also got a rather open-ended job in the weather, take the night shift, and if someone had a problem with the [unclear], I could work on it. So I flourished in that kind of atmosphere, and RAND was such an atmosphere.

Fairly early--this was reasonably soon after the publication of Theory of Games and

Economic Behavior, [John] Von Neumann and Morgenstern's opus, a big thing. I think it was published in '47, a so-called second edition, which simply has an appendix added, which they didn't have finished in time before. So it had appeared and had not made not very much of a splash, got big reviews. Von Neumann, anyway, was well known in mathematics already. But nothing had happened beyond that. Von Neumann was multi-careered. He went to a number of different careers, as you may or may not know--quantum mechanics, operator algebras, and then this big burst of mathematical economics, including Theory of Games. And then somehow he invented the computer, and was one of the big movers on that. He's credited with thinking of the internally stored program. Until then, computers used a kind of a massive production line. Anyway, that's a separate thing. He was involved with RAND almost from the beginning. He was on the list of consultants, and he was being consulted about what RAND should do and be, and continued on the technical side, anyway, contributing to the development of computer science, in which RAND played quite a role during this period, as you would have heard. I was around, too.

That meant Von Neumann was visiting frequently. I didn't meet him early, and this would be early 1948. I started January 1, 1948 at RAND. But possibly because of that, or because there was a "Let's look at new stuff...." And [Olaf] Helmer and [J. C. C.] McKinsey and other people like that. Helmer is around, incidently. You might even want to talk to him.

COLLINS: I might at some point, yes. He's not here at UCLA, is he?

SHAPLEY: Well, yesterday he was. Actually, the medical center. He's retired and living in Santa Barbara, and since he was down for some check-up yesterday afternoon, he stopped at our house with his wife.

COLLINS: What is McKinsey's first name?

SHAPLEY: J. C. C., three initials. McKinsey.

COLLINS: It's Mc, isn't it?

SHAPLEY: M-C-K-I--and from then on it's sort of standard. He died long ago. Well, his degree--I'm sure he had a degree--was somehow in mathematical logic. It may have been philosophy department or math department. He was into more or less mathematical logic. So he's a logician. And Helmer, as I recall, has two degrees, one in math and one in philosophy or something, both from Europe.

So, anyway, in that climate they said, "Let's look at game theory." The Cold War hadn't even been announced, I don't think. I forget when. [unclear] wasn't quite in the mind-set of RAND, the way we thought about the war. The way you thought about the war was largely the result, maybe inevitable result, of what RAND had to drum into the heads of the military. They

still thought their purpose was to win wars, you see, and winning war was a mind-set, whether you win if you don't fight it. So that was part of RAND.

So, anyway, John Williams was one of the founders. Helmer goes back two years before I was. He was with Williams when Project RAND was cooked up. He'd give you an insight as to that. Another person who was there, I think when I was there--if not, he came almost immediately after--his name is Dalkey, Norman Dalkey. He, in fact, still has an office at UCLA, and I see him around campus. He's emeritus, and would be a source of insights. Again, he and Helmer, they collaborated later on a good bit in what is called futurology, or forecasting the future, or combining different people's predictions to get a better prediction. Delphi was a kind of code name for that, the oracular notion of Delphi.

While we're hitting that, there was a person, I guess my age or my status at RAND, named Roger Snow. Roger Snow, a mathematician who hadn't done any graduate work at that time or was just beginning to, which is where I was, you see. And so we got this--I know it wasn't daily, but maybe weekly--seminar. We'd get together and someone would work through another chapter of Theory and Games. It is in the other office. It looks like a Bible.

COLLINS: I've seen it, yes.

SHAPLEY: You've seen it. So someone would read a chapter. Then the group would do it, and someone else would take the next chapter. Another participant in that seminar, whose name will turn up frequently, I think, is a guy named Paxson. You've probably heard of Edwin Paxson. About five years ago, I had in my math class Edwin Paxson, Jr. Yes, he was the son of Edwin. Among other things, the phrase "systems analysis," I think was either Paxton's or described what he was doing, what RAND was doing. He came out of Cal Tech. He was a mathematician and thought more entrepreneurially. He was into decision-making. He would brief the army all the time. But also he kept up with the mathematical work of proving a theorem. That's not all you do.

COLLINS: There was a circle of people.

SHAPLEY: So there was a circle of people. Williams is sort of presiding genius. He was assembling--the division was at that time called MW, for military worth, and so the memos come out that, and eventually it was called mathematics. But always it had things in it that weren't mathematics. We had statisticians, not only statisticians, but quite outside, it was the seedbed for several other departments, social sciences of all types. So John Williams, in a way, represented one-half of the contradictory, the non-engineers.

The president of RAND was [Frank] Collbohm for a long time, and the idea that this was Douglas Aircraft, and we were to look at the aircraft after the next generation. That was their idea of deep long-range planning. Whereas, Williams, at least, epitomized in many cases, and was heavily contributing to, the long-range, wide idea of RAND.

So you want to get maybe some more slants on that, because I think a lot of the early history of RAND is [unclear], and there would be real confrontations. Whoever decides the budget would fight it. Several times Williams had to go over the heads of--well, the president of RAND, the administration, was Collbohm-- to the trustees, and the trustees would be more [unclear] types, when it came to approve what Williams wanted.

COLLINS: You mean in terms of a budget for the department.

SHAPLEY: Budgets and how to allocate and what areas to go into. They want to start an economics department, they want to start a social science, all these different things. [unclear] in many different ways with the hard science and soft science and the [unclear] of mathematicians. So there was kind of an intellectual free-wheelingness, if you call it that. That's not a good word. Or we might also say just kind of an academic research as opposed to developmental type research.

So RAND spread and maybe made big contributions, and maybe didn't. Satellites were proposed very early. It's possible they knew about it, knew about the rockets. They had a physics division, which was more secret. They were doing H-bomb work, I think a lot. I think it was an extra level of classification, but also it involved an immense amount of calculation, when you look at nuclear reactions and you have to integrate all these particles in some way, which consumed gobs and gobs of computer time. So that was a stimulus for bigger, faster computers, which was felt here. Of course, this also came out of Los Alamos and other places, the military support for--each generation being super-relative to the previous.

That's another part of it, and, of course, computing has something to do with mathematics. So there's a lot of having these two elements at work at RAND and a kind of political--I won't say right and left. That's not quite right. But, anyway, there was kind of a political attitude of RAND's mission simultaneously all the way until, say, Williams's death, which was in the sixties, I think. It's not clear who finally won, if anyone did, but when there was a crisis, as when the air force [unclear] and the Congress finally comes up with a final budget number for each year, you know, it may be up a million, down a million. That's a lot of money, you know, for things like that, or it could be a big slash.

You don't worry about that until one of those occasional cuts come, or squeezes come from the congressional support or government. Air force, usually, support. Who would cut? You know, who would be squeezed? One period somewhat later you get ahead, but you probably hid it in many ways. The punch was so severe that Williams said, "I'm not going to fire any professional people in my department." He put himself on leave but kept coming in. His second-in-command, who was Alex Mood, similarly. They cut their salaries, their effective salaries, to zero. They fired all the secretaries except one, and all the research assistants and everyone else, because of the great budget cut of, probably '55, something like that. At that time, I was at Cal Tech, and they said, "Keep coming over." I would come over once a week. "But just don't put in any time." One of these great confrontations where Williams stood firm. Then

wherever possible, some members of the department got into other departments, so he salvaged. He eventually had to lay off only one person, who was the most recently hired. So when things like that would happen, then you felt very much the two sides, because other parts of RAND could support him.

COLLINS: When you're referring to the two sides, you mean the practical-oriented work focused on technology, versus things that were more intellectual in character?

SHAPLEY: Yes, I think so, though it would be also sort of engineering versus science, and science includes some technology. Engineering is not entirely technology. It also involves development to an extent, not really, but the idea, at least. I think the correct version of the story is RAND's name came from the phase "R and D," cross out D. Other people say, "Well, it's RAND, research and no development." But I think that's a slightly slippery story, that the "AND" is just the "R and," but then someone else would develop. Anyway, that's the attitude then. They would stop short of development.

The big test of that was when one of their research projects on spotting aircraft, air defense things, was so successful they said you can have a big contract to train people, because they learned how to train people to write backwards on these screens so you could see which airplane was [unclear]. I mean, a great sort of setup, these things you now see in the movies. That was spun off as the SDC, Systems Development Corporation. And that's the erasure of that from RAND. It was getting into development, and they said, "No. Won't have it." And it was kind of a symbiotic relation for a while. I guess the trustees were overlapping, and the corporation, RAND Corporation, I think, owned the SDC stock. There's a considerable corporate history in there you will have better.

So, anyway, that idea. But still a lot of those people were simply Douglas engineers, electronics division and so on. You can see the bias here, but here we have the engineering school on campus right next--I'm on the front tier where my math office connects. But then there's the liberal arts college involving math, physics, chemistry, you see, which are hard sciences, if you like, if math is a science. We have this geographical arrangement with UCLA [University of California at Los Angeles] campus. This is the north campus where everything is sort of soft and mushy, and the other side is all--so in that sense it's not between--when you say technology, okay, I accepted that, but it's not.

Also, the personalities shape things a lot. Williams was very liberal. He was, from my point of view, the ideal kind of department head. Under him we never had to write a progress report. Unthinkable to anyone else. Any other bureaucrat says, every six months you have a progress report, what have you done, all of that. He considered it was his duty, as head of the department, to know what his people were doing. He could write the progress report. He was somewhat of an astronomer, and he had messed around in science and had been in operations research, had an attitude of a scientist. Wisely enough, I don't think he actually did any science. What he would have done is counted meteors, or something like that, some astronomical project

he was on for a while. He is a very amusing person to describe.

COLLINS: Let me just pause for a minute and flip the tape over.

TAPE 1, SIDE 2

COLLINS: I'm interested how you've developed intellectually in this environment. You came into this as a very junior person, which is kind of unusual, as I've researched the development of the departments in RAND. Many of the people who come in are reasonably well advanced either academically or in their profession. So you're something of an anomaly in a sense.

SHAPLEY: I didn't feel that way at RAND. RAND had anomalies. RAND was itself anomalous at that time.

COLLINS: So it's interesting to me to understand a little bit why John Williams would seek out someone of your particular educational level, and then how he helped nurture your intellectual development.

SHAPLEY: Yes. Okay. Well, I sort of [unclear]. How much my scientific background mattered, I don't know. I was Harlow Shapley's son. He was an astronomer. He had heard of me. I must have come across in my résumé--résumés are now pretty thin. I had scores of various tests and so on. Not a big--it was an extraordinarily [unclear]-looking thing. I had the typewriter. And then somewhere in there it may have struck him that I was probably eccentric in that way, and possibly very bright, because there was something on the SAT. There's a graduate intelligence test, verbal and nonverbal, I think still, and those scores were high, and that looked good. So I think he went for those high scores. But also, maybe, they hadn't gotten very many people, and he wasn't sure what he wanted except he wanted to get potentially--which maybe fizzles out--people fizzle out, people that maybe look attractive that way. I expect he got--because I put down as a reference an astronomer and astrophysicist named Donald Menzel, who, in fact, succeeded my father as director of the Harvard Observatory, and he was also in cryptoanalysis, or at least aware of the whole field, during the war. And he knew about my Bronze Star medal, that thing. He had known about it when I got back. I may have met him after coming back. He had heard about it. The other things like that, I had some kind of reputation. Anyway, he knew me as a bright kid, and he has daughters the same age, and we messed around a lot growing up. So that kind of family friend, someone who could maybe appreciate my talents and express it in a way that might look good to a possible employer. So I imagine he sent a letter of recommendation on to Williams and probably had the good words. I know he did send a recommendation to the navy OEG. Back in the war, he was a naval person of some sort, and there there's Admiral, or Captain, Engstrom.

COLLINS: As in the unit of measure?

SHAPLEY: Yes, as in, except spelled E-N-G, so not an A with a circle. See, I did go down to

an interview, to Washington for OEG, in that job-hunting time, and he probably was there. Menzel had spoken to him. Now that I remember it better, yes, they did offer it, and I don't know, I'd have been a very different person. If I'd succeeded there, I might have just been a big mess. Who knows?

Anyway, California sounded good. I'd been around the world, and one of my friends had come to California. I could look him up. That was not very much to base it on, why I came, but it fit in. And, you know, the pay scale. I kept feeling that was still low pay--$300 a month. And it was also starting pay, because other people have the same experience. There was sort of a six-month review, and then people got substantial raises. So you weren't gambling a lot of resources, and then within six months I'd made an impression, anyway. So then it went up right away to $450, or something like that.

So that's sort of my parameters. But what happened during this period was that we started this game [unclear] various ideas, and someone worked on something, and McKinsey worked out a theorem on something. I spent some of the other time working with Roger Snow, who is like me, same kind of kid. He came out of Florida. And a more senior person, a statistician named Girschik, who was fairly well known, Abraham Girschik. Blackwell and Girschik, were in kind of cutting edge mathematical statistics at that time where they had notions of decision theory, I think. Anyway, he was at RAND, and, again, he died not too long after. I think he had left RAND and gone up to Berkeley or Stanford within a few years, and then died of penicillin poisoning. There was nothing wrong with him except a penicillin reaction, one of those casualties. But, anyway, he had a kind of paternal help. He was older than us.

There was a whole group, though, that none of us were young by other's standards. Of course, I was older because of being in the war, but even then, later on my wife, who was not of that age--she's my age--but she said, "All these people are all the same age," meaning Williams, Helmer, Dalkey, Girschik, McKinsey, and so on. They were within a couple of years of each other, kind of a cohort. About ten years older than I am, I guess, something like that. And I think probably Girschik was with that. It seemed, among other things, that Williams was picking young people in a sense, but maybe people who had reputations or certainly degrees. People like Roger and myself and so on, we weren't research assistants or anything. We were just there in the community.

COLLINS: So how did you fit into this group? What kind of responsibilities did you have?

SHAPLEY: There was very little to do. We just said, "Let's work on a problem." So I'd say, "Why is this true about games?" We made some progress, and finally I guess I broke it, but Roger was also working on it. And Girschik, you know, started talking around this problem. Abe said, "Write it up." So a write-up is there somewhere in the files, our notes, not even mimeographed. Things came out sort of purple, we had a crank thing and a fluid comes out, and you get an oscillator process, some kind of duplication process. And so this became a military-worth memorandum or something--MW, blah, blah, blah.

Paxson, on one of his trips to the East, maybe to interview people or recruit people--he was a person that took trips. When anyone from RAND went east at that time, since you crossed the continent, you might as well visit everyone there. I mentioned the travel situation. So he particularly stopped off at von Neumann's and talked to von Neumann about the latest things, what's happened at RAND, and all this and that. And so he carried with him this paper that Roger Snow and I had written. Girschik said, "No, you did it all. Keep my name off." So it turned out to be Snow-Shapley or Shapley-Snow, alphabetically, and it was more Shapley than Snow, actually. Roger kept saying, "I don't really understand it yet." This was a work of mathematics where I had not really even read very many math papers as published and didn't have any clear concept that I was doing anything special except solving a problem. He showed it to von Neumann. Von Neumann got very enthusiastic, because the mathematicians had not really risen to this great work they'd put out. There had been one or two other papers in the literature, maybe, like Kapansky [phonetic] had one or something that even referred to this great minimax theorem business.

So von Neumann, partly, I guess, for his own ego, said, "I want to encourage this work," even though he was really not working on game theory anymore. He was working on computer ideas mostly. So he wants to encourage it. So it came back a big rave review or maybe a letter. Paxson said that von Neumann was all excited about this, and he'll publish it, and he'll sponsor it in any journal you name, and so on. So in a certain sense, this very small group hadn't really formed very much yet in John Williams's section, as it was called when I joined it. Then it was called military works or something.

So Roger and I became sort of stars. His gamble had paid off. He got these undergraduate people. Where I didn't have the degree from Harvard, I just knew that I was going to get it in a few months if I was a good boy and that kind of thing. So at that point, my stock went up, and Roger's went up. At least these two kids--and there weren't all that many of us around--had something enough to get a real pro like von Neumann interested. So, of course, stop everything else while we write this paper and send it off. This is my first contribution, Shapley-Snow. I call it my piece, really, though I mean it's helpful to have Roger in the thing, but he kept saying, "You write it, and I'll read it."

So anyway, this was in my career and answers your question, I think. It took a great jump upward at that point towards what I've become. I write papers. I went to graduate school and so on.

COLLINS: So that focused your interest for some years?

SHAPLEY: I was focused on it. Anyway, game theory, I think, was made for me, because I was always messing around with great big game-like models, the sort of thing that now they call "Dungeons and Dragons." We used to do that with my friends in school and so on. So I maybe had that mentality, too, but also the mathematical push, because this was a kind of mathematics that I had generated, quite apart from the way it's applied. I've always enjoyed the mathematics

of it.

COLLINS: As you developed your interest in this, that's a question. I mean, you pointed to the tension between engineering and other aspects.

SHAPLEY: No. Oh, no. I had no association with engineering. They were people who did electronics. I flunked electronics at Harvard.

COLLINS: But in the sense of where your interests lie, was it in the application to real-world situations?

SHAPLEY: I think so. I was sort of interested in what RAND was trying to do. The idea then was go to the future, space travel. All these things were, to me, glamorous, and the challenge often came, though, in mathematizing, and that was probably deeper seated than what I use the math for. The problem I get on, from whatever source, if it starts out mathematical interest, I'll follow through to the end. If there's simply an interesting application, well, maybe someone else can do it. I'm right in there in that close equation. The mathematical discovery is the really exciting part. You create conjectures, but you discover results. I think that's the way. I don't consider mathematics--you don't create facts. Of course, the facts are always there, but you discover them.

COLLINS: But you do attempt to define interesting problems.

SHAPLEY: Yes.

COLLINS: I guess the question is--

SHAPLEY: The question was the creative step. The answer is discovery.

COLLINS: I guess my question is, where did you look for problem ideas? What was the domain that seemed to interest you?

SHAPLEY: Well, game theory served very well in that way because it spreads out so quickly into so many different fields. Once you get multiple decision-makers in the same model, all kinds of things can happen. So there's almost never been a time, after the very beginning of that seminar at RAND that first couple of months, where there was not a game theory problem ahead of me, in front of me somewhere to see. It's also not only unexplored but unstructured, because I think my main contributions here, the big thing--someone wrote a preface in my book with papers on my sixty-fifth birthday--it's mentioned somewhere here probably--and he, I think correctly, said that for many years I set the agenda for the field by my work. In other words, I was trying to cover the problems that I could see so I'm building the theoretical structure as well as [unclear]. I'm not sure I'm hitting your question.

COLLINS: Maybe I'm not asking it very well. You mention the von Neumann-Morgenstern book as the Bible. One could use that and say, okay, there are lots of mathematical ideas in here, and they ramify, and you want to figure out, solve additional problems raised by this book. On the other hand, you're working at RAND, and you've got specific military problems like mounting an attack against the Soviet Union.

SHAPLEY: No. I wasn't doing that.

COLLINS: Or the operation of markets.

SHAPLEY: RAND was never really trying to pull me into--I sometimes got pulled into war-gaming things or that sort of problem, but the great thing about RAND for me is they let me do what I wanted. I succeeded in doing what I wanted. All kinds of timekeepers, other people were there saying, "No, we want to see something else." And eventually I was outside the scope, I guess, of your time. I eventually was getting National Science Foundation (NSF) grants, which were supporting me at RAND and not really drawing on RAND. But this is well after RAND had left this wide-open phase, and had much more of a job shop and big matrix projects and disciplines and how many hours per week and all this stuff. I resisted that successfully by getting NSF grants. I stayed at RAND for maybe ten years longer than I would have otherwise, because almost all my support came from there, NSF grants which helped me to do research on whatever it was that attracted me at the time.

That's not to say that my problem section wasn't invented, at least at the beginning, because at the beginning RAND considered RAND in some kind of vague personalization, which is not really accurate. But RAND, as it applied to me, was interested in game theory, all kinds of game theory, and you could sort of build the field. So, of course, in a sense, the von Neumann-Morgenstern was a central thing, and I was getting dug in until I read the whole damn book. I know it, more or less, by heart and know all the mistakes they made and so on and other turns they might have taken and didn't take.

One of the early ones works was this thing which led to the Shapley Value. It was called the Shapley Value, so my name is an adjective for that solution concept. In that case, I can sort of trace the ancestry of how did I think of it.. But more specifically, one can look at the--what's the word? Not paradigm. But anyway, the layout of the field as defined by von Neumann and Morgenstern, exactly where, when they decided to do this, you could have done something else, and this would lead to the Shapley Value. So I can sort of provide a foundation for my work by following their arguments up to a certain point and then saying, "No, this is more important." Sometimes you can use a unique answer. If you insist on uniqueness, you have to give up something else and then went a different way. So it was a kind of rank and file. Sometimes it was rather neat that way.

But if we're going to continue with this in-depth, I said I have five years of graduate study at Princeton still ahead, and a lot of my shaping of where and how I made up problems and

whatever arose from the Princeton period. I eventually did become very much academic.

COLLINS: You were there during this period, we're talking 1948 to '52, before you go to graduate school. Were you or others in the mathematics department attempting to extend the game-theory problems that you were looking at to specific real-world problems?

SHAPLEY: Very few people. Some did. Or others who were looking at those problems anyway would try to use the game theory. I think of some one like, say, Herman Kahn, the physicist. He was worrying about the world, the Cold War and all that, and he had more than a smattering, but not too much more than a smattering, of game theory combined with a great mathematical talent and so on. So he could dig in as deep as he wanted there. He would use game theory, extract it, and gave these brilliant briefings, convincing that we should dig holes [unclear] or we shouldn't have. I forget which way it was. And couching a lot of it in game-theory language. Not all of us would agree with it. We understood it a little bit better than he did.

That kind of effort and a similar status of the same thing, Albert Wohlstetter, more or less the same period, having maybe more influence. They were very different in some ways. And to some extent, when they were using game-theory arguments--they would call it game-theory arguments--it was RAND's reputation in game theory that gave some kind of bridge to it. Now, I don't know to what extent it really means that we have--all the good game theory is at RAND, which is not quite half of it, let's say. The rest was at Princeton. But there was double counting if you went back and forth. In other words, RAND had an idea that they knew how to use game theory, and these people who were really applying it to their problems were not themselves too much game theorists. Sometimes what they did, they'd work with us. There would be joint things, something like that, and to some extent maybe we developed some game theory, you know, the game theory of deterrence, things like this, or foundations of deterrence, that sort of philosophical basis, and sometimes, but not so much, the technical basis of game theory was part of the whole RAND impression on the air force and the Cold War problem.

We could flatter ourselves at the fact we were back there proving theorems that had something to do with this academic-like reputation. These are experts in the field and here's what they're saying. But then, of course, it can go both ways. One would make a joke about game theory.

COLLINS: Let me try to characterize what we might call your functional role at RAND, and see if this is accurate. Did you see what you were doing then as a resource that other people could apply to the specific military problems that were of interest to the corporation?

SHAPLEY: I was frequently reduced to justifying when necessary what I'm doing in those terms. Whether I saw it that way, or whether this was sort of a ritualistic thing, it would come down to saying we must write a preface to every paper explaining that this is vital to the United States, blah, blah, blah. And it was almost laughable, but we did something or other.

COLLINS: This is a document that I found recently that I just wanted to share with you. It builds on this discussion about game theory, kind of peculiar in a way. It takes place in 1955, and it's a letter from Larry Henderson to Trevor Gardner, who was the special assistant for research and development in the Department of Defense. But apparently some consultant at RAND, a guy named A. A. Albert, who was based at the University of Chicago, felt that the game theory stuff, if I'm to take this letter correctly, presented inflated claims about what it could do for solving military problems.

The response was that Alex Mood wrote a letter back and gave a brief historical synopsis of game theory at RAND. Basically, his claim was that there was a lot of excitement at the beginning about the use of this theory and its potential application of military problems, but as RAND became more familiar with military problems, their depth, their complexity, the initial enthusiasm about the utility of game theory waned a bit, and then it became less of an intellectual tool for addressing military problems.

This is 1955, and I thought that was kind of interesting. It seemed to have, by his suggestion, a burst of interest and activity, and then a kind of decline in terms of its practical value. Is that a characterization that rings with your experience?

SHAPLEY: Is this Mood's letter?

COLLINS: Yes.

SHAPLEY: Mood, of course, among other things, was trying to keep alive in the Defense Department and all that, and this is what maybe he would say. I mean, a good way to try to explain, you know, you give something to the critics and you give something to his audience saying I would sometimes, you know, try to justify my work, necessary in some sense. So you understand that it's a directed message that you're reading there.

COLLINS: Precisely.

SHAPLEY: But I have one comment that may help a little bit, because I have used this many times in different ways. As game theory has made impressions--made an impression on economics, on this and that--or sections of game theory, because game theory is sort of a compartmental business, the pluralistic theory. But I'll tell you, the measure of the success of an application of game theory is whether the field to which you've applied it can now use it without mentioning games. You see? And it reflects the idea what a bad name game theory is, because it invites jokes from Fulbright[2] and people like that. Was Fulbright the guy who had these Golden Fleece [Awards]?

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[2] Senator William Proxmire.

COLLINS: Yes.

SHAPLEY: Someone in Washington was always afraid that something RAND would do would get a Golden Fleece, you know. That means, you know, half the budget would disappear the next year. So there's what's called the giggle factor now, the giggle factor. This is an aside, but a recent event is that they cut all these Search for Extraterrestrial Intelligence (SETI) projects out of NASA, just got an amendment through, and the response by the community is the giggle factor. Someone said, "Oh, you should look for intelligent life on earth first. Ha, ha, ha." You know, and then they'd make a joke. Something that's sort of vulnerable for that kind of stuff, and game theory may be worried about that.

So among other things, if you apply game theories to political science, you get some nice results. The field adopts the results and manages to explain them in their own terms, you know, export them, and you end up not mentioning games. Very much the same in economics. One of the big developments post-von Neumann and Morgenstern in game theory is called "the core." You hear that word. It doesn't matter too much. It's a solution concept related to von Neumann's work, but not directly, and it helped to clarify a great deal of theoretical economics in the fifties, at the time when that field was becoming more mathematical, you know, when they were proving things carefully and so on. So now they talk about the core, and the work is there, and it's all embedded in economics, and they don't even call it game theory. Now, of course, I come here and teach economics courses and game theory and, "You've heard of the core haven't you? Well, we invented it." That kind of result.

But, anyway, the [unclear] and game theory provides--you don't necessarily have to have a field called game theory. You can say, well, yes, many of the historical roots of game theory involve economists. Edgeworth--you wouldn't know him. Or maybe you would know him.

COLLINS: I don't know Edgeworth.

SHAPLEY: Edgeworth is Francis X. Edgeworth. He wrote in the 1880s. He was ahead of his time. In some ways. He was well known as an economist, but he had this wild book which is called *Mathematical Psychics*. This was when psychics--psychology hadn't been invented yet, so this really turned out to be a book on economics and utility theory, the preferences of people. A strange title. And it has stuff that other economists couldn't relate to, and then it turns out he simply anticipated this whole core theory. It was my economist roommate at Princeton who unearthed this work of Edgeworth's--no one else had paid much attention to it--and worked into the field of game theory.

So now the field is such that things were happening which weren't considered part of the same field called game theory, and they probably would continue to happen. But because, probably von Neumann and Morgenstern and at RAND and people like me, there is kind of a unified field. There's no departments yet of game theory, but there is plenty of journals exposure to it. And it has a property that the results that tend to--I don't want to say be useful in

necessarily a practical sense, what is practical--but applied to some field outside, you know, a big application to theoretical economics. You still aren't at the retail store level or anything like that. Sometimes we are.

So it has that property and so it's relatively easy to write what Alex Mood was writing, saying, "Okay, excite me." Yes, he touches all these things and a lot of math, and we can use high-powered mathematics, whatever comes out of it doesn't maybe affect people very much. But then the subsidence afterwards--what has happened, you see, at that point, maybe at the time he wrote it, they now knew what deterrence was. They didn't know what deterrence was, and it was sold to them, inspired by, and couched in terms of game theory. So people heard about this as strategic analysis, Wohlstetter and so on, where should the basis be? A lot of early decisions, having to [unclear] piece the unfought war, can be traced to game theory and traced to the fact that RAND was, first self-consciously, looking at all kinds of game theory, a lot of which wouldn't apply anywhere. And then it looks like game theory is very big, and then suddenly, this particular thing that made an impression on the military has now been absorbed into their thinking, and there's no reason to call it game theory anymore, or reason to fund other game theory.

COLLINS: So in a sense, you're saying it's internalized to a certain degree in RAND's activities and the military's activities.

SHAPLEY: Yes. And so it's not simply it failed to deliver. I would think. It did deliver and then the delivery was absorbed, and then there was no more particular hunger, from that point of view.

COLLINS: But in terms of the focus of individuals within the mathematics department, game theory and its elaboration—

SHAPLEY: In RAND?

COLLINS: At RAND. Did that continue with the same intensity as in 1948-49?

SHAPLEY: The math department itself shrank, but to the end--in fact, after the end--they were still doing game theory. I mean, this is what you do in the math department at RAND. And other things, but there was a lot of game theory and a lot of--a thing that was also highly concentrated at RAND was the development of linear programming. It didn't start there. [George] Dantzig started it beforehand. He came to RAND and there was a large group doing this. A lot of the mathematics is similar to the mathematics of game theory. Many of us have contributions in both fields. From the very beginning, George Dantzig was sort of the ruling person in that area who constructed the field the way he thought it would, and he was very much on the applied. He said if a problem has an application, you ought to sell it. Generally, all kinds of computations arise. Oil refineries needed it a lot. So there was always a connection with the linear programming community and industry, if you want to put it this way, or any kind of

planning and computation.

On the other hand, game theory didn't parallel that, but there was someone like Ray Folkerson, who was another colleague of mine, though he arrived somewhat later than when we're talking about so far, but certainly after I came back from graduate school. He and I were very close and worked together. And he worked a lot in linear programming and in computorial mathematics and applied mathematics and so on. We could contrast him once that, if there's a problem, he and George Dantzig would take it up together or be both aware of what the interesting problems were. And if it continued after the first blush of looking at this kind of problem, there might be a network full of problems of this sort, but that theoretical interest, Ray would stick with it, you know, forget the thing and deal with the mathematical abstractions and do mathematics on it. He was a mathematician of my style, I guess, too. Whereas George, interests, he would be only incidentally interested in any theorems that come out of it, but you would be glad if he could find another application of something and work about that. So it was kind of a useful tandem, because they were good friends. They represent opposite poles of the applied and the theoretical in the growing mathematical field, which is sort of what you're asking about in terms of game theory.

But in game theory, there's much less application in that sense. The application that occurs will often be to other theoretical things or to political science in a very abstract way, namely, interpreting the Constitution. So, in fact, one of the very early applications, in the certain real sense that people were paying money, lawsuits, about fair representation. The decision of the [U.S.] Supreme Court in the early sixties saying, "You've got to straighten out the state apportionment systems. Legislatures are not giving equal power to the people." This is a very game-theoretical problem. And since the Constitution is itself an abstraction, as it says--we have a game theory axiom, too, saying all men are created equal or something like that, all persons are created equal. This is also true of accounting, which starts with principles, and you end up with punching numbers, so we do get applications of game theory in that way. And when it happened, it was sort of pleasant.

Once, only once, I went to a scientific meeting of accountants, because the title of the meeting was "Using the Shapley Value in Accounting." Someone thought Shapley was dead. [Laughter] This was many years ago. And they thought, "Well, what was that?" So I turned up at it, and the accountants, turns out, they understand arithmetic, they don't understand mathematics. So it was an interesting cultural thing.

So I brush with applications, and they're fun to talk about, amusing consequences of my real work, which is uncovering the truth in ways that you can dimly understand.

COLLINS: A noble enterprise.

SHAPLEY: Yes. And RAND let me do that, at least the people in the math department. At one point, they claimed that I and Ray were the two highest paid professionals at RAND. Of course,

administrators start where the professionals leave off. I'm not sure if that's true. And then I got NSF grants. Some cut back. I got the grant. Ray didn't get his grant. So Ray left and I stayed.

COLLINS: You've drawn a contrast between the early period of RAND, where things were a little more freewheeling, and a later period when things were more bureaucratized. When do you mark the period of change from the early ambiance to the later ambiance?

SHAPLEY: Gee, I wonder if I could hang it, something I could date. I can sort of see this. It wasn't gradually, but in fits and starts. RAND went away from this one great--what metaphor I'll use--one great contract, air force contract, with quite wide provisions that RAND could do whatever they see fit, to having many other clients and also much more specificity in the contracts and the jobs they undertake. [Unclear] this was happening against the background of occasional shocks from budgets going up and down. I guess somewhere in there--and this is what you'd have a much better eye of--some people in RAND in one way or another were drumming up work. RAND's charter, I think from the beginning, said public governmental bodies will be our clients. We will not do--what's the word? Anyway, the sort of thing Arthur D. Little,SRI, does for industry. But this is quite widely interpreted, apparently, because we had contracts with the Dutch government, the city of San Diego, all kinds of things like that, and urban transportation.

The way I describe RAND to people, as far as I could know, they tried to pick, at least at the beginning, special interests in types of problems that were maybe loosely enough drawn, but anyway, which concerned methodological. New techniques would be needed to define, to carry out this research. And they made a big thing about having mixed teams. We have talent in many fields. We can bring them together, consultants, so we can study urban transportation or chemical equilibrium that causes the bends, all these various projects you've probably seen lists of. And that, after a while, became more a description of RAND than from the very beginning. How that transition, and when the big landmark of that is, I'm not terribly sure.

See, my first time at RAND I was there less than two years. I don't know if you have the dates right. It says something about '52. I left to go to graduate school in '49, '48-'49. And then I went to graduate school in Princeton, returned five years later, having, however, spent every summer in between at RAND as a consultant. So I had a continuous thing. It wasn't foreordained I'd go back. They gave me the nice offer when I got my degree. I said, no, I'd wait a year. By [Al] Tucker himself. He was [unclear]. I had a Sloan Fellowship, I think. No, which I could have taken instead of going back to RAND. And he was somewhat disappointed. He thought I should be academic, and I guess I didn't disagree so much. I just discovered that at RAND I could do academic research, so I expected this, and I wasn't, as I say, going back planning to spend a long time there. But at that time, there still was just an awful lot of interesting game theory, and Princeton was the place where--there weren't too many places that were otherwise interested in it.

John Nash, at that time, had a similar choice, and he went to MIT [Massachusetts Institute of Technology], was on the way to becoming a first-class mathematician. He had immense

genius. But in the course of his undergraduate years, he had already published about four or five papers on game theory which were momentous in their effect. So he went the other way. I didn't particularly want to be where he was, but that was not the point. And then he had the mental breakdown a few years later, so that episode ended. I remember the choice there, should I go back to RAND or not.

COLLINS: I'm still not clear on your--maybe you're not either--on the choice that you made there, if you had the option of going into academia at that time.

SHAPLEY: Well, surely you understand the difference.

COLLINS: Did teaching not appeal to you?

SHAPLEY: Teaching didn't appeal to me at all, but academic research did. I'm not sure how much I inherit. My father, you see, taught his first course in Harvard after he retired. [Laughter] He went there as director of the observatory. He founded, he created the graduate school, and, of course, he was producing graduate students for the whole country for a considerable time. So in that sense he was an educator, but he never actually lectured in front of his students, undergraduates or anything like that. In astronomy you don't have--I mean, in undergraduate astronomy you learn the constellations. It has nothing to do with the science part, really. So he was a professor all of his life, but as a director, as an administrator, he had to retire at a certain mandatory age--for administrator it was probably sixty-five, but he'd gone to sixty-eight as a professor. So he didn't for three years. He went down to Harvard Yard and taught the undergraduates.

So the idea, the concept, of a non-teaching professor was--I mean, no, I didn't really think teaching was good. I had thought I'd attempt to teach some calculus or something my last year at Princeton. And then there was a big reentry on the teaching basis when I came to UCLA about ten years ago, twelve years ago. The plus was I had some graduate students whom I could really train and imbue with my knowledge, whatever it is. Most of my colleagues in game theory, they have generations of students of students. I had nothing like that. But the other side is I have to teach [unclear] students, which I've learned to do, but I don't know how to do it very well. So I'm not a big teacher, no. Research is my thing.

COLLINS: Okay. Let's pause here.

TAPE 2, SIDE 1

COLLINS: Did the question of working under classified auspices within the military present itself as a question to you during this time period, after you'd finished at Princeton? Was there a difference at RAND, in terms of your mathematical work and your ability to freely communicate with your colleagues as you wanted to or needed to, as was typical of an academic environment?

SHAPLEY: You're talking about a difference over time, or just for the moment, just the difference?

COLLINS: Well, we can talk about it as a historical phenomenon.

SHAPLEY: I can consider what RAND was like even before. Of course, I have some idea of what's academic. First of all, I had arrived at RAND. I was one of two people whose clearance took a long time, over a year. And RAND always, I think, has been set up in such a way as they accommodate people who are not cleared. You know, there's "need to know" and separate things, and maybe some pre-cleared section, which I guess at the beginning, things were so small you didn't notice it. There was no separate place for people. This period was also circa 1950, which is a period of, say, [Senator Joseph R. McCarthy] McCarthyism or pre-McCarthyism. That era came to a boil around there. So clearance was a big deal.

And then my father was a liberal kind of person, and got a certain amount of attacks from Un-American Activities Committee type of people. He was on McCarthy's first list, I think. McCarthy started off as a big bang. He was just a senator, and he stood up somewhere and said, "I have in my hands two hundred Communists working for the State Department, and Harlow Shapley is one of them." He was not a Communist, by any means, and he never worked for the State Department. I forget. He counted how many mistakes there were in that one accusation. It tended to fizzle. But, nevertheless, he was in that kind of trouble, insofar as there was trouble in being cleared. I don't know if he needed clearances. He'd need passports sometimes and it would be hard to get a passport. Eventually you get a passport. And at one point he was cited for contempt by Rankin, who was the chairman of the Un-American Activities Committee, and Congress didn't uphold the citation. So he won the fight there. But, nevertheless, this connection--nothing I ever did or said, particularly, was involved-- was the presumed reason for taking so long to get me cleared when I came to RAND.

One of the things you might mention about John Williams in particular, and his whole philosophy of how to be a leader of a scientific thing, was that he would not bother people with this. He never spoke to me about a clearance. I never worried about it. The interface between me and the people who were worrying about it and so on, was simply I was insulated from it by him [unclear] writing bureaucratic reports and other kind of distracting things from just sitting and thinking. So that was a special feature of Williams's administration.

So, anyway I had no problem at RAND. My clearance was deferred, but on the other hand, I'm not aware of anything particular--I knew there were secret things, but they were talking about new kinds of radar, [unclear] and so on. And there was the nuclear physics division. I don't know when that started, but it was even inside a separate barrier inside the RAND building, after the new building or the present building was constructed.

So there was a kind of game in the early game theory. We looked at various types of games, and one type we called duels. It's a game where the continuous time variable and the

players sort of maneuver and sometime you have to act. You know, in the classical duel you walk, draw, that kind of thing. So it had this name, and mathematicians use any name to define any kind of abstract thing. But for some reason, this caught some classifier's eye who said, "This has got to be restricted. This is military. Anything about a duel." I mean, how long since--well, it was more than that. When you talk about duels, there are two sides that are trying to kill each other. It got, I think, the lowest level of classification of anything in this area, and no one stopped calling these things duels. So there's a whole bunch of memos that eventually were declassified. So there was a little bit of interference, which meant you couldn't send these things to people maybe, you know, to other people outside RAND. It would be restricted. And even now, a couple of theorems, I think, became well known, but the reference is to some classified RAND thing.

I think there was something that I did on reconnaissance. It was called reconnaissance. This is an idea where, if you have somebody [unclear] matrix, and reconnaissance means if you're allowed to peek at the other guy's matrix before you move and how much is it worth. The idea is to set up a model where you can either get information--or not information--and measure the amount of information by how much it gains for you, not by how much paper it covers and that kind of stuff. So it's a kind of game [unclear] information theory and it applies to statistics very well and to theorem and Shapley, Carlin, B_____. Anyway, a couple of Cal Tech people, who were consultants, and we worked on this. And this got classified, because reconnaissance is clearly a military concept. And so this couldn't come out. No one worried too much about it.

I believe in other postwar operations research places, you know—there was one person who is much revered, whose name is something like Koopman [phonetic], who was big on what is called search theory. And this is not particularly game theory. How do you search an area? How do you search for someone who's hiding, or who's trying to be found? The downed aircraft or the submarine problem. So it was sort of a bunch of problems. You find them in operations research textbooks now. I think it's Koopman. Anyway, all his work—he wrote the master summary of the field shortly after the world war ended. I guess some of this was during the war, and that became classified. And so years later, we were trying to, us on the committee, to help award an operations research prize, some annual prize for past operations research. So we suggested Koopman, and we found that the stuff is still classified, and his work was known among the people who went on to be the main people in the field. He was still alive, I guess, at that time, or we wouldn't have considered him.

So it was that kind of thing that only really brushed against me. I was never pulled off something. Of course, I got my clearance before I left the first time. I was cleared and then I was always cleared. But unless they bring me into something--like once there was a thing that took several months, a big war game, a big board and two rooms and referees going back and forth moving these chips around. I was somehow involved in that, designing and running it. And then officers would come in from various army or air force places or students from the Air Force Academy to play this game. We had a setup downstairs where you could play this game. It was pretty smoothly done. A certain amount of computer interlinkage, but mainly it was to give the

cadets, or whoever they were, an idea of what global strategy is. The North Pole, and the [unclear] go out, colored plastic star-shaped things when something gets blasted. So it was a lot of that kind of game fun. And then this used classified data in general, so we couldn't really--they say we shouldn't even talk about the form of the game now. Nothing's classified.

So almost never. I said we brushed up against it, and usually I'm already doing something I don't really--I wouldn't want to do again. It was away from my career.

COLLINS: So, in a sense, did you regard this as a price of doing things that you--

SHAPLEY: Well, if I'd been wanting to do this, and if you do it, then I would have felt this as a constraint, but it was not the fact that it was classified that was a problem. It might even be kind of an incentive. It gives a certain drama of doing something important, because these were bombers and eighteen million people there under this piece of plastic. And, in some sense, I guess I was interested in current events and the progress of the Cold War and this and that, and a little bit of insight into the thinking that goes behind it, and, I guess, loyal and proud of early RAND work.

Wohlstetter is the one who deserved most of the credit of convincing the army that [unclear] goal. First strike capability, second strike capability. If you have second strike capability, you will deter war. If you have first strike capability, you may actually increase the danger of war. This is a definite game theoretic idea that does not come to mind readily, and this led to things that would have been unthinkable before. I guess the first S.A.L.T., the first real treaty we had with Russia--not the test ban, but some kind of S.A.L.T, I think it was--in which each side agreed to limit the amount of defense, the number of cities they will put defenses around. This is very strange, but both played into the whole logic of deterrence and what the real danger was that war would happen over everything else, you see, and if someone can defend themselves too well, and that guy can strike you with impunity, or with less impunity.

So that's the concept there. This is very much game theoretic analysis, thinking without a lot of number crunching. So that was RAND's, once or twice removed. But it's a level where claims made by the people that promote these things were, in fact, game-theoretically sound. It's oversimplifying the problem to change it. So that's the point we could say that this is pretty good, that we are at least capable to say that they're saying the right things to the air force. And it was having an effect on the stability of the whole world. So there's some kind of pride there, and there were various times when I tried to explain what I'm doing to someone who isn't really interested in mathematical or science, I can say that kind of thing. We think differently about these things, and perhaps better. Not being happy about the Cold War, but somehow the alternative was a lot worse.

COLLINS: To follow that thread a little bit, from how I understand the development of the Wohlstetter study, for example, I don't get a sense that Wohlstetter thought his problem through in specific game theoretic terms. I think he approached the problem in a somewhat different

way.

SHAPLEY: Well, I could look at some of the literature. I think you may have missed something or not recognized the game theory in his game theory. But he was rather explicit about this. He wrote a paper. He wrote about game theory. Among his RAND things he wrote, I remember one had a flashy title. It was [unclear] non-mathematician, a non-game theorist looks at game theory. It was called something like *Sin and Games in America*. Maybe you'll get that title, at least. And he thought about game theory. And, of course, as many people in game theory do, correctly or otherwise, think they see game theory better than someone else does. [Thomas] Schelling was another example of that, who thought carefully--he was a smart guy--and didn't really understand game theory real well, but had some good thoughts about it.

Wohlstetter was there for a considerable amount of time. I remember several times, once in particular, where Olaf Helmer, he'd done something which was bad game strategy. We refer to them as game theoreticians and say, "This isn't right, what you're saying here." Some particular point, whatever it was. And had to face this guy who is--have you or will you interview him sometime?

COLLINS: I have.

SHAPLEY: And probably he's mellowed, or done the opposite since then. I haven't seen him in years and years, but he was always a courteous person and a debater and so on. He would have these critics. But then he would, at some point, usually when we'd do it, he would somehow try to shield himself from criticism by saying, "I am my own worst critic." You see, as a kind of rhetorical device. And, of course, you would see that. You know, "I worry even more than you do about what I'm saying, and therefore I am right because I have done more worrying." I remember that phrase as being a little irritating. If you could get him to write, you could nail him. I mean, just to make a point about something on this thing. And he would certainly carry out these kind of discussions in game theory. He talked to game theorists. He was using game theory. Not that all that comes out of what he does was really attributable to game theory, and, as I say, game theory has a way of being absorbed, you see. The logic of the Cold War as it finally developed was enough. You didn't have to call it a game, you see.

Of course, you've had, with interviewing, perhaps, got him on this point at bit. It would be vaguely interesting for me to read your book, if I survive long enough. But that's my remembrance of Wohlstetter. He wasn't a game theorist. To some extent we can say how much in fact he was a user of game theory, and to the extent he drew anything out of the RAND atmosphere, I think it came partly from brushing against people like Olaf and me, and partly from brushing against Herman Kahn, who is a very different kind of person who eventually fell out with RAND, because he had his view of game theory. It was limited. Eventually it was [unclear] serious mistakes and what he promoted about bomb shelters. But he's gone now. You must have heard about him.

COLLINS: Yes.

SHAPLEY: Eventually, he said, he would grow so big the world couldn't contain him. [Laughter] You have the idea. He had a kind of variable weight which varied between like three hundred and five hundred pounds. One time, I think, he was sort of [unclear] getting bigger and bigger. Eventually I think he went up the high end. I don't think he wasted away. I'm kidding a little bit, but he was that kind of person you'd kid about.

COLLINS: Let me ask your perceptions about something else, and that is, you focused on game theory but, as you were pointing out earlier, RAND defined its reputation, its corporate image, around the notion of systems analysis and interdisciplinary concentration on problems. How did you see the attachment of game theory to systems analysis, and did you have a sense of whether systems analysis was a real thing or just a kind of concatenation of techniques like game theory and linear programming and other kinds of things? I'm just wondering as someone sitting in the mathematics department, how you perceived it and reacted to it or contributed to it.

SHAPLEY: Well, it was clear from the beginning that it was a kind of catch phrase or buzzword. More than a word, two words. And it described something that didn't have another name at that time, I don't think, at the beginning anyway. Systems analysis. The idea, of course, is you look at the big picture, and whatever your picture is, mine is bigger. You know, my big picture is bigger than your big picture, that kind of one-upmanship, implicit, but more it had to do with you have a problem, even though you seem to narrow it down, you should look at the context carefully. And this, of course, is a big watchword in game theory. I tell this to my students every day almost. Game theory has all these techniques, and which one you use depends on the context more than the actual definition. I won't give my lecture now. I just mention this relationship. Suit your work or your theoretical apparatus to be sensitive beyond the boundaries of the problem as it might be narrowly defined. And I think that is at least part of what systems analysis is. Systems analysis also has the idea of being much more of an engineering kind of thing. At least, if you go to the engineering department here, they have a systems department and so on, and that has to do with pulling things together. Engineers, of course, have their world, their view of the world, a really tremendous view. And the idea is sort of considering all the adjoining problems at the same time. That's how I view, at least what they meant at the beginning, about systems analysis. It seemed from the beginning to be part of RAND's early pitch, if not continual pitch, is that they have interdisciplinary teams. If you're going to do systems analysis, you don't get one group of specialists. It's like doing an environmental impact report or something like that. They cover the territory.

Of course, since then the word has changed a bit, and the new [unclear] participated in the birth of something called the International Institute for Applied Systems Analysis. You know of this. If you don't, you should, because this is RAND all over again. Its early phase, twenty years later, was doing just the same thing with some of the same people, I guess including me, but I wasn't quite that close. This is in Austria. Do you know about it at all? "YASA" they say, because it sounds like a nice Austrian name and it's actually IIASA.

COLLINS: I've heard about this through Gus Chambers.

SHAPLEY: In the great Laxenberg [phonetic], with an "a," a suburb of Vienna, having a palace, a big white elephant which is then used for making institutes. What do you do with a castle nowadays? And it was formed in Austria between the two worlds, of the two-world world. So it had Eastern European and Western European, and members of the institute were academies of the various countries, national academies. The budget was international in that sense. And they're applying sort of systems analysis. Well, systems analysis, looking at problems from a systems point of view, which I would just repeat the words. It's multi-layer problems and world problems. Let's do hunger. It wouldn't have to be that big. And also a good deal of not only interdisciplinary, but international teams. So the East Europeans and the West Europeans would work together on how we can prevent the spread of smog or something like that.

Of course, that didn't have a military base, didn't look at military problems or even problems of world security, and they weren't particularly involved in arms negotiation. But that was there. They had a very broad mandate. People came in, a mixed bag of people. Usually the East Europeans were people getting a vacation in Vienna, so they were political hacks who could come from Russia and get a year at the "YASA." Occasionally, you get some real people there, but it was a lot of inter-cooperation. I don't know if it's a success or not. It's hard to say. But it exists, and they were going through the RAND thing, and maybe it's worth mentioning one of the early directors was--what's his name? Mel Levine? Someone who had been young at RAND before. They were wandering around in a kind of brave new world, amateurish quite in a sense of what is a problem. Okay, I mentioned it.

Anyway, the word "systems analysis" may have acquired a certain color, because this thing--well, you've heard it a lot--is well known internationally, and it may be now the word has changed its meaning because of that.

COLLINS: But sitting at RAND and seeing systems analysis develop, what kind of intellectual meat did you think it had? What exactly was it describing from your perspective?

SHAPLEY: Was it descriptive? It's a methodology, isn't it? I'm backing away from how I describe it. Well, group methodology, how to run an institute like RAND or an institution, a think tank. And the word is used a bit, and you sort of knew. Often Paxson would use it more than anyone else. It was his banner, so to speak. I'm not sure what you get. Of course, you've been doing all this, so you know a lot of these answers more and trying to get something more. But I think, when you get an annual report from RAND of the right epoch anyway, and they'll talk about systems analysis and talk about mixed teams, and disparate teams, and then lots of examples. At least one could extract this, I think, from the annual things. And the idea, I think, a methodology which you can apply to all kinds of things, especially where a shift took place, when could that have been? In time, I don't know. Where somehow RAND went more and more onto the--civilian is the wrong word--non-military. More and more of the contracts were from

NIH [National Institutes of Health] and HUD [Department of Housing and Urban Development], those places, all kinds of contracts.

There was the one where they're getting the computer model of the estuary of the Scheldt, the place where they had bad floods in the Netherlands, and they were going to build a dam, and this would cause the oysters all to die. And a few oyster fishermen were mad, but they weren't strong enough. However, there's a green party that said no, you must not let a single oyster die just to keep this flood. The flood had already killed like ten thousand people in the surge barrier. So there was a question. But they wanted to analyze it and try to get some kind of objective thing. So the project was stopped, and RAND was hired as being a distant person, because the local politicians, the government people, were all too closely involved with that.

So RAND has this big study [unclear], and it was not only about oysters but also the migratory birds. I saw a part of it because my wife was involved in part as a programmer and another mathematician close to me was involved. I didn't mention Norm Shapiro. He's there now, too. He came with me on my second visit. When I came in '54, he came in. We were classmates. But before that, I think he had no particular idea about RAND. A computer model of the constituents of the water, salt water, or fresh water and different things described differently and then the whole food chain was a model. They sort of did a big thing.

So this is branching out into, in this case, very much ecology in the formal sense, the interrelationship of species. And RAND getting away from the military, more and more specific projects. I was rarely involved in one of these things, but the impression I had was always they put together a team across RAND departments. There was sort of a matrix organization. They talk about the departments or the divisions. These things changed from time to time. In one direction, the discipline is the other way, or maybe the contract's the other way. And the contracts were served by drawing people from various departments as needed. Hiring people, I guess, was a big one.

COLLINS: My impression from your account of your own work is that it was the exception for you to be drawn into one of these teams.

SHAPLEY: Yes. And I couldn't have stayed at RAND as long as I did unless I had done this, or unless I got outside support, which is what I did, because I just wouldn't have gone for that stuff. I mean, it's interesting, and I like to read science and all that, but fairly soon--quite apart from this thing about do I take that fellowship and launch myself into a classical academic career--I would be getting feelers or job offers. My reputation had grown. Say, by 1960, I felt I was easily employable at many universities. I stayed at RAND for a while because of friends and everything else. I had a relationship with RAND, so I could do what I wanted, which is mathematics, game theory, or whatever interests me, keep my own hours and so on. So I had something that I could stay and live with, but that depended on getting National Science Foundation grants, which came from the economics section of the National Science Foundation, and that's part of the reason we're sitting in this economics department now here.

Because I became plausible as an economist, as well as a mathematician, when I went outside. But I had always had that kind of escape, I mean long-term escape hatch, and had I not got the NSF grant, I imagine I would have done what Ray Folkerson did, my peer who I mentioned before, and taken a job at a university.

So in that sense, once I got in that state, I was certainly exceptional at RAND. I think the turnover at RAND--maybe you have statistics of the average half-life of a RAND employee--but my impression was that it was, and perhaps was intended to be, or was accepted as quite short. People would spend two or three years as a RAND employee and then go off and get into [unclear], or projects happening and ending. And so many of the projects, almost by definition, the RAND projects, except for grants--the National Science Foundation wasn't my client. They were simply supporting me. The general scientific community, I guess, was my client, if enyone was. Wth publication, they said, but now the client had to sign off, all this stuff.

But most of the people at RAND, much more as opposed to the early part of RAND, came out of academia. They had skills and all that, and they're good guys, and most of the people are, in fact, doing research on the side, some sporadic time. You may have clocked it sometimes, as I did. Every contract should have a kind of intellectual overhead or giving eight percent of release time to write scientific papers or something like that. I guess it's like in a college, they give you summers off, as it were, and pay you for it. But once you have that kind of thing, it's quite natural that people, if they do a big urban transportation study, and they've met a lot of people in the field outside, that and they're rather easily employable, and RAND is a way station.

In the latter part of RAND, beyond your scheme, I guess, is the RAND Graduate Institute. And you might realize I was associated with that. I helped design it at the beginning with Charlie Wolf [phonetic] and a few others. And for many years, even after I came here, I would teach occasionally for the RGI or RGS, as it became, Graduate School. Again, that was part of the percolation of RAND, people not necessarily staying for a long time. If someone gets a ten-year pin, he's probably worked in facilities and maintenance or something like that. But I even got a twenty-five-year pin. Somehow the award for a ten-year thing, they give you tickets to a show or something, or you select some kind of gift. Then there's a twenty-five year award, which is worth about a quarter of the ten-year one. The incentive is not to stay even longer. I'm not sure that hasn't been just worked out that way, anyway. Worth seats at a better show.

COLLINS: There are some people at RAND, though, I think, who invested their professional identity in the organization.

SHAPLEY: All right. Name some.

COLLINS: I can think of somebody like Jim [James] Digby, for example.

People who worked on the kinds of problems that he worked at, Rand was pretty much the place

where he could do it. But clearly your professional identity was with the mathematics community, and RAND was a place where you could exercise your professional skills.

SHAPLEY: Yes, though the mathematics community actually disappeared. I mean, the RAND mathematics—

COLLINS: But I'm speaking of--

SHAPLEY: But if there's a larger community, I'm not sure. We don't have that many mathematicians today. A lot of mathematicians earn a math degree, but they'll be in a department. I mean, I understand. Your point is well taken.

COLLINS: Did you have contact with Ed [Edward] Quade as he was developing the systems analysis course?

SHAPLEY: Yes. Oh, yes.

COLLINS: What was the nature of that contact?

SHAPLEY: Well, among other things, he was head of the math division for a while, so quite a while he was my titular boss after Williams. I'm not sure if he immediately was after Williams. I think Harris. In some order, Ted Harris, who is a mathematician, he was the head. But then his number-two head was a more administrative person, because Ted was very much a research statistician, who eventually went to USC [University of Southern California]. But I think Quade may have been sort of number two under Ted Harris, as Alex Mood had been under John Williams for a long, long time. No, I can't remember the sequence of things.

But gradually the nature of RAND changed, so they didn't really want a large math division, although it didn't affect too much, because the [unclear] at this point was almost partly administrative. Except if you had a conscious math division, they're going to try to hire good mathematicians, like a math department in a university, and you go for math ability. And that, somehow, may be not what was essential. I mean, competence you wanted there at RAND, but creative mathematics was not prized, except for, as I say, exceptional people or those that were otherwise able to do some of that quality of work there.

COLLINS: My question about Ed Quade pointed more towards the mid-1950s, when this systems analysis course was developed, and it was presented to the military and a few other groups as a way of indoctrinating people into systems analysis techniques.

SHAPLEY: I guess I don't know. I guess there must have been a systems analysis course.

COLLINS: I was just curious whether you had any contact with him at that time.

SHAPLEY: We knew him quite well. He was doing that. I don't know. Is he alive?

COLLINS: He passed away a couple of years ago, but I interviewed him.

SHAPLEY: Yes, that's what I thought. And probably Sylvia is perhaps still alive.

COLLINS: But I interviewed him.

SHAPLEY: Oh, this has gone on for a while, too. Someone will write a history of this history.

COLLINS: Please. [Laughter]

SHAPLEY: Follow you around with the microphone. And also, he was a great bridge-player. He and Sylvia were just total bridge-players, and Marion and I were somewhat bridge-players, that kind of connection, and lived not far apart. And Quade was also, incidentally, involved in the Austrian thing. I was trying to think who from RAND was there. I think Quade maybe. He may have been the director of it or something. He had a systems analysis course--again, later parts, just to connect a little bit, the *Policy Science*. In that period of RAND, *Policy Science* is a name of a journal edited by Quade. It was another attempt to pull together a field when there wasn't a field, and it was the "one way or the other" policy studies, different ways of saying it.

RAND got this grant from Ford, I guess, along with seven other academic places where at some level they had graduate programs in policy science. I was involved in that, too, and Quade was, to some extent, involved. Charlie Wolf, whom you probably should talk to.

COLLINS: I've talked to him informally, yes.

SHAPLEY: The culmination of his career was his immersion and construction of this policy science institute--institute within an institute. Before then he was economics.

COLLINS: I notice on your résumé you were, or have been, with the Operations Research Society of American (ORSA), and you've got 1981 on there as a date. This organization was formed in the early 1950s. Did you have any association with it during the fifties and sixties?

SHAPLEY: No. A lot of people were involved in that. There's something called Mathematical Programming Society, which is a name for linear programming, because linear programming includes nonlinear programming, and there's a lot of people in that. ORSA is much bigger. It allies itself with something called the Institute of Management Science, which comes out TIMS, because IMS was already taken as the Institute of Mathematical Statistics, you see. So often you'll see TIMS/ORSA. They'll have joint meetings, and they publish journals in common. The TIMS is more the industrial side of operations research. ORSA is more the academic side.

I was involved with operations research people. I joined the society rather late. I think maybe they gave me a prize, and I said maybe I should be contributing to running this thing,

because the society awarded me the John Von Neumann Theory Prize of TIMS/ORSA. I joined for those two things. Had to go to a meeting and get a metal thing this big. It's made out of some material that sounds good. It's a metal medal. It's not really gold, but it's quite heavy, is a way of putting it. I think it's packaged in lead or something. Anyway, it's a nice medal with a picture of Von Neumann on it.

And other times I was awarding this prize to other people. I mentioned this research theory person. So I was involved in some of that activity. I was in no sense an organizer. Some operations researchers always struck me as being more applied than I was, and yet then people start publishing in their journals and write a few article things, and I'd have to look in their journals. After a while, the main journal would have instead of just six--every other thing would be called either "A" or "B," and they were separate editors, and one had more theory and one had more applications. And it got into this within Operations Research Society. There, of course, were operations research societies in Europe and so on, and it's an international thing, but this is the American part of it.

Incidentally, sometimes I'd go to a meeting, be invited to give a paper, and once they got me on a committee to try to decide whether we should really have a separate journal, because too many of the applied people were saying, "I don't like getting a journal where I can't understand the paper. I should be able to understand the paper." Of course, that's not true in a math journal. Mathematicians will send all the math papers or something. But that kind of thing.

So there was this journal called--there's usually one lying around--they said on the committee, "Let's combine with TIMS," which also had a journal called *Management Science*, and it also was splitting into theory and otherwise theory. So they cut it the other way and founded a new journal called *Mathematics of Operations Research*, which is specifically mathematics of this field. The people who become members of either one of these learned societies--and they have lots of members, you know, it's in the thousands or tens of thousands--would have an option. They can either get cheap rates--as members you're getting a journal, but many of these things were basically just subscribing to a journal when you become a member. And so you'd get options. So this sorted out and removed a certain amount of tension. So I've been on the advisory board of this ever since, editors and section editors.

By the time they got it sorted down this way, at least at the beginning--I'm not sure what the latest thing is, I haven't seen any reports--at least one-quarter of the papers were game theory. So it was really a place to publish game theory, which was not necessarily easy to find in the old days. So in some ways that's mentioned probably here, too. I'm not sure, if I'm not making it up, maybe get the date. I'm editor of the math of operation research. So about '75 is when this thing happened. So I had a small influence on this.

COLLINS: But your interest didn't really develop in this area or become affiliated with this area until the seventies and early eighties, then?

SHAPLEY: Well, this area includes game theory, so this is not true. In fact, I probably may have published some papers in operations research journals before that. It's more a question--I'm interested in game theory, and game theory is a significant part of OR [operations research] and is recognized as such by these other institutions.

COLLINS: What I was trying to get at was, did you have this connection in the 1950s.

SHAPLEY: Yes. As soon as I was doing work. One of my early papers I published in another journal called the *Naval Research Logistics Quarterly*, which for all those words in it was basically an operations research journal for the navy, and they published a lot of game theory from the very beginning. I had several papers in the sixties and maybe even in the fifties. So I think I claim I was in the field.

COLLINS: So did you have contact with people who identified themselves as operations researchers?

SHAPLEY: Yes. Yes.

COLLINS: What was the nature of that interaction?

SHAPLEY: Well, academic types would include people like George Dantzig, a linear programming person who was at RAND. Various people at RAND were, one way or another, for people.

At Princeton, I was supported at Princeton by a project. How do graduate students get support? Well, I was at least partly supported by Professor Tucker, who was my mentor at Princeton, my professor Al Tucker. He had this continuing grant for many years from the Office of Naval Research, which supported game theory and operations research. Game theory was maybe in the description of the grant, or just what he used it for. So the growth of game theory in the early years was supported by RAND, turning people loose on it and having summer consultants come in, in great numbers, who often were math people, usually working on some kind of minimax to figure out if it applied. So they said, "Well, this is a mathematical thing." And the other coastal thing would be Princeton math department with Tucker the providing genius. He still lives. You can interview him in Princeton if you want.

COLLINS: Let me just pause here.

TAPE 2, SIDE 2

SHAPLEY: So I regard myself as, yes, I was doing operations research. I would describe myself sometimes this way. I can give an example if you want. When was it? A group of us, extramural, were all in favor of integrating the schools in Los Angeles, voluntary buses that people could sign up for in the school board and so on. So at one point, people would propose

to the school board, with some of the people on the board actually in favor of it, a system of busing to equalize distribution of races in the school. It was sort of an integration thing. And this is a network problem and a linear programming problem, how to design a busing schedule to achieve approximate--or certainly improve overcrowded schools, usually the inner city ones. And so it would be mainly busing them to less overcrowded schools in the suburbs. So this was a great thing.

We got together and focused, and a couple of people in other departments who were less mathematical, but equally zealous in the cause. So we borrowed some time somewhere. I guess it was published as a RAND memo at one point, or it was the kind of thing that was acquiesced with at RAND. It wasn't a RAND project. We even went down then to the school board and presented the thing with slides and charts. You know, how the buses would have to go, what were criterion, explaining what's going on.

In order to do this, when you appear as an expert witness at a hearing, which I don't do very often, one takes qualifications, so I pointed to my publication list in network flow and operations research. I didn't say anything about games or particularly math. It was a correct and useful description, if I wanted to promote that sort of thing to that kind of audience. And so I just remember I was an operations researcher by title. I wasn't a member of the society then yet.

COLLINS: Let me turn that question just a little bit. Part of what was going on in the 1950s was the attempt by some people to have operations research established as a recognized kind of academic discipline. You establish a journal. You create a society. You start university courses. Phil [Philip] Morse at MIT was very keen on this sort of thing.
SHAPLEY: Yes. Early fifties, even before.

COLLINS: Did you see yourself as part of that interest, or as contributing in a tangential way?

SHAPLEY: I guess it was part of my perception of RAND from the beginning. It was described to me as that kind of thing. We'd hear a lot about--there's both Morse and Morrison. Were they both Phillips?

COLLINS: Yes.

SHAPLEY: Yes, that's part of the confusion. Of course, there were other things at RAND of a hardware sort but this was what RAND did, in one sense, and you would describe to people, operations research. It's only somewhat later I would even be tempted to say that they do game theory, because it took a while for the momentum to build up.

For example, as I was being hired in '48, or the navy people I interviewed in Washington in '47 in OEG, Engstrom, whoever it was, that was operations research. That would explain what it was. So I was aware of, yes, something had been created, and then you'd hear about it more and more. The society itself had no meaning one way or another. But then later on, when the

people that had been [unclear] wanted to talk about sort of structural-type changes like these journals. I was quite at home in being in that group and talking about this thing.

COLLINS: But that was some years later.

SHAPLEY: I would describe, when my parents wondered, "What the hell are you doing?" I think my father was afraid to ask me. He thought we were making wars or something. We never quite got him over that. He said, "He's at RAND. Of course, we were making peace. I was trying to explain the difference between peace and war. But it's okay. It was good relations.

But operations research becomes something that was sufficiently spoken about in general so that you could mention it, and someone would know what you're talking about. It would be a way of describing the game theory as part of operations research. I didn't think of it early on at all as part of economics. That was a later growth, of either economics or myself, the theory. All three together. So to explain it one way and say, "No, we're not figuring out new plays for the 49ers or something like that. We're not studying games. It's something else." So sometimes, at least when I mention game theory as what I do, is prepare to carry that language.

COLLINS: We've been going a little while. I've just got a couple of other question areas I want to pursue, and then we can wrap things up. One thing, in looking at the documents from the mid-1950s, that I get a feel for, with respect to the mathematics department, was that, with respect to game theory and linear programming and dynamic programming, there was a certain amount of ferment there. You had Dantzig and Belman. There seemed to be a lot of intellectual energy at this particular point in time. I'd like you to elaborate a little more, if you can, about your relationship with Belman and Dantzig and the way you saw your game theory interests relating to what they were doing, if there is a relation.

SHAPLEY: Are you talking about ferment in time, or in place?

COLLINS: Well, obviously--

SHAPLEY: No, it wasn't obvious to me. What I meant was, there was this time where mathematics was breaking loose and becoming something more and then coming back together. It is not a RAND sort of thing.

COLLINS: If you see a distinction in time and place, I'll let you separate that.

SHAPLEY: I'll cover both, because it comes back to RAND a little bit. A term—I don't know anyone else really uses it--I say paramathematics, like paramedics or paralegal. That is, the center of mathematics is a pure mathematics, and it has boundaries and math societies that have been going for 150 years. And then, in this immediate post-war period, things that were mathematical in many ways, but were not within the traditional algebra analysis, geometry, logic, with that formulation, started becoming interesting and important, and more and more

mathematicians get trained. They go not into academics, but they use it outside. So there's a difference in the employment, to put it another way, of all the mathematicians, the fraction of them who are dedicated to the pure mathematics, becomes somewhat less in the various defenses and expansions. Computers come in. And at first it seemed this was all mathematics with computers. And they could [unclear] and so on.

But in watching university departments around the country as they evolved, some held to the pure, and then eventually some other department would appear, called maybe mathematical sciences. Have one common name, Department of Math Sciences. It's not necessarily antagonistic, but somehow things that didn't belong in mathematics were still enough mathematical to need the word "mathematics" to describe what they do. So with the word "science" attached. I won't mention statistics, which is sort of a different kind of thing. Then these disciplines, of which game theory is a minor one. But I was involved in it, so I could [unclear]. A lot of departments aren't interested even now. I get students, I can't sell them the math department, some math departments because they still remain that way. Other places, among which are Princeton and UCLA, the math department simply expanded. Within the math department, they did the OR, they did the linear programming, did the game theory, and old-fashioned applied mathematics, which is a jargon word we can't use anymore, because it means partial differential equations, which is what it was used for to train engineers. So we say mathematics applications or something like that to get that context in. The point is the situation is ambivalent enough that both of these developments were logical, and neither one really prevailed over the other.

During the forming time of RAND, as I said, there were really only a couple. I mean, I don't have that synoptic a view, but Princeton was outstanding in linear programming, of even probability theory, which means mathematically respectable statistics. Of course, some statistics departments somehow have a different purpose and context, and then they often separate or partially separate out, even if they were embodied with it, but that leaves behind the mathematical core of statistics, which is a reasonable mathematical subject which is not really quite covered in [unclear] theory and probability. And that was happening in Princeton when I was there. Feller [phonetic] was the point man on that.

So in the early years of RAND, universities wouldn't touch the stuff we were doing in math. We were looking for [unclear]. We didn't think of it that way. Maybe Williams was, but let's use mathematics more broadly and much more finite mathematics as opposed to calculus and the traditional curriculum. And so he wasn't interested if people had math degrees. That was part of my story. He kept saying, "You don't have to go to graduate school. We'll love you anyway." But I did and so on. It was very important that I did.

So we have this growth of mathematics in the surrounding paramathematical context. So what RAND found in the early years, John Williams found that he could very easily interest people from traditional mathematics departments, which are most of the math departments around, in coming to RAND for the summer and not have to do [unclear]. They can look at this

new stuff, develop it, dynamic programming, the whole business, more commonatorial [phonetic] kind of network flow theory, which is somehow not quite mathematics, but it was a big thing that RAND was important in. Not to mention game theory, which has so many mathematical facets. You wouldn't know the people. I mentioned Quine, the logician, [unclear] logical problem, in the sense of form games, and he's contributed to it, was good at it, and did the right thing.

Because of the state of mathematics at that time, it became easy--with the right budget, anyway--for RAND to bring in lots of summer visitors who were classical, often mature. Albert was an algebraist. He didn't, of course, quite come believing in RAND the way some of us others did.

COLLINS: You're referring--

SHAPLEY: A. A. Albert, the one you mentioned.

COLLINS: Yes, okay.

SHAPLEY: He was, you know, a summer visitor. He was there several times, I guess. But his comments were not on the mathematics part. He was a pure mathematician, but he wasn't above --they called it consulting. This was not research; it was consulting. Now, of course, we call it research and we can sell it. So that was the setup.

Since math needed to be developed somehow and all the mathematics that's associated with computer science, information theory, for example. The other vogue word at that time, which survived in Russia but not here, was cybernetics. About the same time as Von Neumann and Morgenstern came out with the book, Norman Wiener, you know, sort of came through with very high-powered mathematics and really hit information theory and set it up. Which led then to, say, Shannon, who probably never got a--no, he eventually got a chair somewhere, but he was a Bell Labs person who developed information theory. That was another thing which RAND didn't do much, but was part of the ferment.

One feels maybe this is overwhelming, but that mathematics has to get its problems from the real world. So every century or so, it should revisit the real world and then come up with new problems. I mean, continually problems of motions of the solar system, that and then all the mathematics of the great people, the [unclear], the [unclear] people, then the thermodynamics and then the Maxwell and the electricity and all that, and [Albert] Einstein, everything. All this touches the physical world, at least, if not the human world, and then pushes mathematics in a certain direction, accelerates the growth in certain quantum mechanics [unclear]. Well, that needs a mathemetician to straighten them out. So something like that maybe was happening in the human side of human activities. Game theory is part of that, decision-making, decision sciences, all the complex things of a society.

So that was happening in mathematics, the universities, through their math departments, weren't really staffing that kind of enterprise, and there was not really yet very much of a big National Science Foundation which would splash a lot of money around in math institutes. In fact, until recently, they only gave small summer grants to mathematics people, compared to what they would give to the physicists. That was one of Harlow Shapley's reasons he worked very hard for the National Science Foundation.

RAND filled the need at that point in the first, say, five years of existence. There was a lot of stuff to be done. You needed mathematical foundations, a lot of problems, at least mathematicians would consider important, if they considered the importance of their problems at all, but were mathematically significant. Someone like Dantzig was very much aware of applications. Others were just poking along, not caring whether it was going to be applied or not. So that kind of growth was happening. RAND did it. The universities didn't for quite a while, or only a few universities did.

I don't have good figures on this, but I would think maybe every summer there were ten, you know, real math professors, flocking around at RAND, an interesting thing to do. And mostly I think, they were stimulated--maybe California is a great place in the summer--but maybe they were coming because there was new mathematics in the air at RAND. And that, of course, couldn't last forever. Surely academia has taken over that burden, and they don't need RAND anymore. Even if growth is continuing, it now grows because you have math sciences departments and grant money comes in that area much more and so on. So something I could observe, being a game theorist.

I'd like to say Williams has an awful big contribution. I mean, he was the length and shadow, a one-man type of person. He had a broad shadow rather than a long shadow. As he foresaw in the math department, the so-called math department, he had hired a psychologist and all kinds of people.

Do you know computers? Do you have a computer?

COLLINS: Yes.

SHAPLEY: If you're actually going to program, you use BASIC. Everyone uses BASIC. This is at RAND, you know. It would have been clear to you that BASIC is simply a slightly inferior version of DOS. One of these guys, [John] Kemeny, who was, I guess, a mathematician. He was also something else. He was Einstein's assistant and a physicist. He then became a big man at Dartmouth and visited RAND one summer and saw computers. It was just incredible. He wrote almost a poetical memo. And this is just a JOHNNIAC, not the IBM, which was used for things more experimental and had all these rooms full of tubes that glowed, and you'd have to replace them when they stopped glowing. Anyway, you take one of Lewis Carroll's puzzles--Lewis Carroll had a lot of problems where I think we had people with different occupations and different names, all these logical puzzles of that sort. You just feed this thing into this computer,

and it would think for about a minute, and then light up and give the answer, you know. Of course, now it would be a second, not a minute.

But the interesting thing was that Kemeny, John Kemeny, when he did this--he recently died, actually--became well known in the--

COLLINS: How do you spell his last name?

SHAPLEY: K-E-M-E-N-Y. First name is John. You're tempted to put "F" in, but that's Kennedy. And he was, I think, Hungarian by birth. He was president of Dartmouth. He was a mathematician at Princeton. He was a little bit ahead of me, not much, and wrote some good textbooks on the new mathematics. Getting away from calculus for freshman is a good thing. At a young age, became president of Dartmouth because all the old people died at once or something. He had done a little consulting with RAND, I guess, before and was somewhat [unclear] about it. And he came back and said, "My gosh, every student should have a computer." This was long before anyone had done anything like this. You could date it with the dates of JOSS. I'm not sure what it was.

COLLINS: Right.

SHAPLEY: The second time he returned there, by then someone named Shaw, Clifford Shaw, was the computer scientist who single-handedly developed JOSS, the software. The idea of interactive time sharing on a computer was RAND's cutting edge. You had this typewriter, and you type on it, and it had a double ribbon. You had typewriters once, in your life. You're not that old. And this was a very modern typewriter because the carriage didn't go back and forth. That's the point at which they say "carriage return," when nothing returned, and even now computer programs have CR. And what's a carriage? I mean, good heavens. These things survive. But a little golf ball that jumps back and forth along, you see. Also, it was electric in the sense that the keyboard worked electrical switches and everything. So this became a remote station, which was not common. You had to go to the computer room, and you had these punch cards, and you'd feed them in.

So just to have even the idea of a separate workstation was all very early RAND. And they put wires under the floor. Every room had a plug for computing power, and eventually, had like eighty stations, I think, around, as many as you wanted. You could order one. It would be this typewriter, and you could type in red and green. Then, of course, there's a black box, which was, in fact black, and was a box, and then the cable would go in there, and the black box had a buffer so that you could store what you had in there, and then the time-sharing would go around, depending on the number of users, will spend a tenth of a second, a little spinning wheel somewhere down in the basement, and all this was conceived.

Shaw wrote the software for the interactive language. Even the phrase "user-friendly" hadn't been thought of. And he hated all computer people that said, "No, this is dreadful." Partly

this is because they were being made unnecessary. You see? If you had a problem you'd go and get a computer programmer and, you know, who uses FORTRAN or something and go to the machine and then come back, a big science of how to talk to their clients and so on.

Now, this [unclear] this thing, and when you type, it's in green. It got green off the shelf, black-green instead of red-black for the double ribbon. So all this was there to use. And then you had paper coming off a roll, and it goes off on this little vertical thing the typist used when they typed. The letter would go up here, and they could read it, and then fall off on the edge, if you want, into the wastebasket below, whatever you want. You had to dial the computer, you set a program, the language was beautiful. You had to spell things rightly and punctuate. No shortcuts for the hackers. The computer people were furious. "You could have a much more powerful language. You just have one little button, and you get ALGOL or something." Some great better program or something like that. They didn't understand this was for the masses.

The personal computer concept, well before IBM or Apple or anything like that. No [unclear]. Actually, this is almost better, because you'd have a record of your thing, and if you wanted to make a run, you know, you can run the program. Most of the instructions were either two or three . . .

So, anyway, Clifford Shaw was the genius of that. It was vastly successful, as it was copied other places from RAND, and then the big copy was--of course with full permission--that Kemeny took it to Dartmouth, and it was scaled back a bit. For example, one of the revolutionary steps of JOSS was they used lower-case letters. It was almost like reading real words, not the breathless telegraphic style. But Kemeny then implemented Dartmouth. They had machines all over the place. They were teletype machines based on [unclear] network all over England. But, anyway, the name BASIC was for this language you learned. So, anyway, this is cutting edge, very much cutting edge. No other place has--IBM would send people over and look at it.

COLLINS: Just to return to your depiction of the mathematical community. Does this, in part, explain then your reason for coming back to RAND, in the sense that this was a more hospitable place to do the kind of thing—

SHAPLEY: It would be sufficiently hospitable. The immediate alternative--I wasn't job-hunting otherwise--but I had entered somehow into this fellowship application. [Telephone rings. Tape recorder turned off.]

Yes, I had a very clear idea of what I could be able to do at RAND, I mean, and would go on doing. It was fun. I had a lot of mathematical--I learned an awful lot of math. I just learned a lot of math at Princeton and I really got educated mathematically. But the game theory load was still there, and the other employment thing was nice there. I don't think the money mattered much. They offered me more than I thought they'd offer me.

COLLINS: In speaking of the emergence of these newer areas of mathematics, many of which seem to be represented at RAND, did you see these things as--to use a later word--synergistic in the sense that they were useful in terms of your own mathematical thinking and development with respect to game theory? Was it good to be around somebody concerned with linear programming and somebody who was concerned with numerical analysis and computers?

SHAPLEY: Oh, yes. I'm sure that was part of it. Especially when I was in graduate school, every summer I went to RAND, drove across the continent about nine times in a Chevrolet. Nine and a half--I won't go into that. So I had a very clear picture of what RAND meant, because what I would do, whether it was summer or not, would not be affected much. Because I could tell that, a permanent offer, and I wouldn't mind living there.

I'm trying to think of the order of events. I got married, I think, about a year after I came back permanently. But I think that really all happened after I came. But there was nothing comparable on the East Coast that would help me get on with my life. So that was part of it. There's some personal things about it--not a lot. But I didn't have anything that would be cramped. It would be interesting problems. I didn't necessarily think I was going to do game theory all the time, and I had no inkling of the economic growth I would come in on. In fact, a number of my RAND staff are not in game theory. There's [unclear] problems, and there's chemical problems, which are in the course of collaborating with people. It's a free atmosphere. We moved around clearances. You had no security pretty much. Someone could just walk in and talk about something. Do I have any idea about it? Maybe I do sometimes.

So it was that kind of--I certainly felt at home. Actually, later on, I didn't. I felt much more isolated when I thought about it, but by that time I had so many outside connections, travel became easier, and phones became cheaper. It just turned out that my close intellectual associates, after, say, ten years more of this, in the seventies by then, the majority were outside RAND. So it was less to keep me there, and I was, as I mentioned earlier, reassured, if I worried at all about it, that I could get a job academically. As I would frequently say, "Well, I'm not interested," when someone would ask me.

And then eventually a time came I said that I would be interested, and I limited myself to two offers to consider, because I had about a year to think about it: Stanford and here [UCLA]. So it was here. So things worked out very nicely for me, without being unhappy. If I had moved sooner, I don't know, it might have been different. And I might well have moved sooner, but by the time I moved, I could say, "Well, I'm not really interested in what anyone at RAND is doing," except insofar as personal friends. I would listen with interest to what they say. But subject-wise, not so. Conversely, no one at RAND was really interested in what I was doing by then. The math department had been swallowed by something called--for about a week, you know, for about a month or two--mathematics and information science. You know, this was the person who had been head of math still had a titular job in the math section of this somewhat larger department. And then he left. That was probably Tom Brown. And then they said, "Let's just drop the mathematics, because, after all, mathematics is one of the information sciences." We all

said, "Ha, ha, ha!" Their big picture is bigger than our big picture. So there was no real mathematical group [unclear].

Also there's always somehow computer things that are going on. I've always had a peripheral interest in that, so I got quite involved. Computational kind of mathematics. Chemistry was one of the papers, more like computation of chemical equilibrium. RAND was a leader for a while and had the only program in the world that could take something like a sheep's blood and run it, simulate it, make a mathematical model of it, something which is very they call an ill-conditioned problem in mathematics. You know, you have blood, and they'd have one part per hundred million of something, two parts of a hundred million of something else in the blood. The constituents differ so much in scale that the theory is there for what happens. You have to go double. triple precision, and things blow up as you try to approximate.

And so that led to something called geometric programming. If you ever hear that term, it handles chemical equilibrium. But for a while, that whole profession simply reffered to the RAND chemical equilibrium program, if someone had a problem of that type. Anyway, I got into that kind of thing, and that's not game theory at all.

So my economics connection grew independently of all of that. That was mostly from outside RAND, almost entirely.

COLLINS: When you return from Princeton and you're at RAND, this was also a very active time at RAND in terms of developing the notion of the strategic defense.

SHAPLEY: I guess so. You know the dates.

COLLINS: Yes. I'm just curious about your awareness and connection to these other developments within the corporation, the elaboration of nuclear strategy and that kind of thing.

SHAPLEY: I mentioned a thing that took a lot of time at RAND, this big war-game thing.

COLLINS: Right. Do you recall when that was?

SHAPLEY: Yes, this was after I returned from Princeton. So this would have been in '54/'55, academic units for a while. The thing was called SWAT, Strategic War--some acronym, S-W-A-T. You'll find it mentioned under that in some index.

COLLINS: What was the abbreviation?

SHAPLEY: S-W-A-T.

COLLINS: There was something called SWAP, I remember that.

SHAPLEY: I can thumb through here faster.

COLLINS: Symmetric War Planning Game.

SHAPLEY: I remember SWAP. I may have the wrong one for this. But this was not that symmetric. That was another one. [Unclear] where to look. This index is almost everything. These are authors. SWAT? If I could remember even who was number one. Someone who was very big in it whom you might meet, that you probably should probably interview about it, is Milt Wiener.

COLLINS: I know of him, but I haven't.

SHAPLEY: He's been around a long time. Among other things, he's now doing the nostalgia. He's the company man on the alumni association or something. But as I recall, he was definitely--and an air force officer named Jones. Bill Jones was much involved in this kind of thing. Olaf Helmer and I were maybe the main mathematical people in it.

I'll look under "gaming." Gaming is somehow not [unclear], but it's sufficiently [unclear]. Well, maybe I won't find it that easily.

COLLINS: What I'm interested in, is your general interest in the development of this subject, and whether you kept tabs on the debates that were going on within the corporation on this subject.

SHAPLEY: Yes. If I'm not mistaken totally, because things did happen in the summer when I was there sometimes, it seems to me, my encounters with Wohlstetter probably were in the post-1954, after I returned full time. When I came for the summer, it would be for like two months. Maybe I was just involved in something that summer visitors would be around anyway, I being one of them. So that was there. I can remember around 1960 we had a considerable interaction with Herman Kahn on what was one of his specialties mathematically called the Monte Carlo method, which has a kind of game. You know what it is.

COLLINS: Yes.

SHAPLEY: Use of massive. The direct technical connection, we were trying to solve some very large games. Sometimes we'd tackle the electoral college, which has fifty players. It had forty-eight players and then had forty-nine and fifty and fifty-one, as in a relatively close period of history, Alaska, Hawaii, came in to bring it up, and then [Washington] D.C. got the vote, and there's the census in there. So we had a cluster of voting games. Voting systems are games. There's a calculation you make on them, which is maybe too hard, or maybe we could do it with Monte Carlo. And so we got interested in the Monte Carlo method and how you accelerate the convergence, and you run these things in great number.

So some papers I never directly collaborated with Herman, but with someone who worked with Herman named Irwin Mann, who was a kind of mathematician. He was kind of Herman's assistant, maybe you've [unclear]. In fact, he would have at least anecdotes about RAND. He was hanging on for a long time. Irwin Mann. So there's some Shapley-Mann things on the market called "methods applied to the electoral college" and game theory in general. So that kind of problem. But this is while Herman was still there in the sixties. He must have left fairly soon after that, and he left during a time when basically RAND didn't want to stand behind his particular ideas about preparedness. But that came at the end of a period to which he had made a great splash at RAND and in the air force as well, talking, his version of sense: how to think about the Cold War and "How to Think About the Unthinkable," was one of his titles, and so on. And so this is within their area, things I was aware of just because of the personal contact that was going on. I probably had some opinions of my own. I never briefed the air force or anything like that.

COLLINS: Was there any change in the life of the mathematics department towards the start of the [John F.] Kennedy Administration? A number of RAND people left the organization at that time.

SHAPLEY: Yes. It devastated what was called the cost analysis department, I think it was called--cost for short. I mean, devastated. Charlie [Charles] Hitch was an early--is he still around and interviewable?

COLLINS: He's around, but he's not amenable to being interviewed.

SHAPLEY: Not available one way or the other. But he was very much involved in the very early formation back in 1946 or '47. Certainly 1948, after I was there in the summer, they brought together a lot of social scientists to try to expand. This is part of Williams and Hitch, and Hitch was a major mover of that. He, I think, joined RAND when that came to fruition. He must have been an earlier thing, and whatever his reputation. Then he left RAND at the time of Kennedy with [Robert S.] McNamara. He was the number-two comptroller. A lot of RAND people came out, not so much from the math department, but some people we knew.

COLLINS: I'm just wondering whether you had a sense whether that affected the tenor of the organization at that time.

SHAPLEY: Yes. Yes. I got the question. Massed amount was not--what's the right word? In tune, in a certain sense, to the cost for even the economics department at that time, so that the effect on RAND in general would not be detected, except for cross things.

This is something you could--if you really wanted to see, I can give you an address, if you want to talk to Olaf Helmer. Among other things, one of his rather close friends that he collaborated with in a way outside of RAND, was named Robert Smith, Bob Smith, and he was

in cost analysis. Olaf would also be good on Wohlstetter, and he would also be good, of course, on John Williams and 1946. I told you I was talking to him just yesterday, so I, among other things, asked, "I'm going to meet this guy. Do you mind if I suggest he might call you?" He said, "Okay, call me." He's in a retirement community outside of Santa Barbara, two hours' drive away. You might want to do something one way or another.

COLLINS: Okay. Yes, that's a good suggestion.

SHAPLEY: And, of course, with this we had a little bit of rehashing of some of these things. We would ask, "Whatever became of so-and-so?" And he mentioned Smith. I hadn't thought about him, and I didn't know him well. But he was in cost.

Another person at that time who would have this, he was at RAND for several years. He came, in fact, roughly in my cohort in 1854--1954. That was my grandfather. 1954. He came from Princeton. Mathematician turned economist now, a rather big one, named Herbert Scarf. Scarf as in muffler. Herbert as in Herbert. He said, call his foundation. He's at Yale University, which is mostly economics. I'm pretty sure he's in economics. People hold joint appointments around there, and he is still very much a practicing mathematician, but he's also made many contributions to both the game theory, particularly game theory, and economics, arriving on the scene as a mathematician. So we have a lot of sympathy for each other, because we had to explain to economists what they should be doing sometime before they got recognized.

So he and my very much closer colleague, or equally close colleague, Martin Schubik [phonetic], also was a frequent RAND visitor, and we worked together on almost everything I did in economics for many years. He's, again, on the faculty at Yale University. He would have spent, not just summers, but maybe he spent a year at RAND once he was involved in gaming. He got a lot more broad view of RAND, because he worked in many departments. Scarf in particular worked with the cost department, and he had friends there. During one of these crises, he's one of the people that John Williams didn't fire, because he could get him a job in something called logistics, which is an upstairs department.

And the push which got Herb--until then Herb wasn't into economics at all. Then he proved some theorems and logistics and stock. I don't know, something about when to buy and sell stock. I don't know, when to reorder. Some theorem that's associated to him. And then, carrying his mathematical talents, he went to Stanford and economics in Yale and back and forth. He's at the National Academy, like some others are. But he was around at that time and would have also at least a view of what's going on that you might not catch, because he was a RAND employee. He was not just a visitor. A RAND employee for, I would say, about five years.

And on that checklist, since we're doing it, Olaf said, referring to you, "He should talk to Bob [Robert] Specht." And I can't not imagine you're not talking to Bob Specht.
COLLINS: I have talked with Bob.

SHAPLEY: The question is how to stop talking to Bob Specht, or listening. But Specht is a great mind, presumably reasonably authentic. I mean, he has his own spin on things.

COLLINS: Of course.

SHAPLEY: Specht also goes back. He was number two under Williams in the math department at a time either before or after Alex Mood. You have to fit it in right. Or maybe you've talked to him.

COLLINS: I have. I've got something from Specht.

SHAPLEY: I didn't want to omit that. We all liked Robert Specht.

COLLINS: I was going to ease up on the questions now, but if you have any other reflections about our conversation, you certainly can add them to the record here.

SHAPLEY: Gee, pay off some old scores?

COLLINS: Let me get another tape. [Laughter]

SHAPLEY: Do a job on Poundstone [phonetic]. Is it Poundstone?

COLLINS: Oh, yes, the book.

SHAPLEY: Maybe there's some other RAND book even worse, but Poundstone is more recent. I think it was him. Well, okay. I'm counting on you to set the record straight.

COLLINS: We'll call it quits.

SHAPLEY: Von Neumann wasn't like he said at all.

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