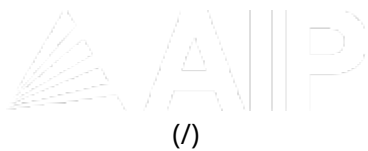


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Norman Kroll

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ORAL HISTORIES



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Interviewed by: Finn Aaserud

Location: La Jolla, California

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▼ ABSTRACT

This interview discusses: Kroll's youth and education at Columbia University; his work at Columbia Radiation Laboratory and his studies with Lamb and Melman on magnetrons; his collaboration with W.E. Lamb on shift calculations; his comments on the work of Lamb, Schwinger, Feynman and Dyson in the late 1940s; particle physics in Kroll's career; his participation in JASON laser studies; his research on the interactions of laser beams with plasmas, on visible parametric oscillators and free electron lasers.

Transcript

Aaserud:

I will mainly deal with JASON in this interview, with Norman Kroll in his office, the 28th of June, 1986. I would first like to do a short introduction of your earlier years, and you were born in Tulsa, Oklahoma on the 6th of April, 1922.

Kroll:

Right.

Aaserud:

Maybe you could say a little bit about your parents' background and their occupation in your early youth?

Kroll:

My mother was a housewife, and my father was trained as an electrical engineer. He practiced electrical engineering very little during his lifetime. I would say he was primarily a businessman, with an interest in technical matters. During my early childhood he was building residential housing.

Aaserud:

That was in Oklahoma.



Kroll:

Yes, I suppose; at least that's the first occupation of his that I remember. I think at the same time he had partnership in an oil company with a number of other members of the family. I was not aware as a child of exactly what he was doing in that or even if he was involved in that during the years he was building houses. What I remember is that I used to be taken around to see each house as he finished it. All of its virtues would be explained to me, and my mother would tell me how superior his houses were to any other houses which were being made by anybody. So my feeling is that I don't make a clear distinction between pre-1929 and post-1929, but I think that the Depression caused the family a lot of difficulties. That's my impression, looking back on what I remember from that time.

Aaserud:

Yes, it was very early in your life, of course.

Kroll:

That's right.

Aaserud:

Did that lead to any moving around?

Kroll:

Yes. It must have been in 1931 that we moved to New Orleans. Then my father really went back into the oil business. I don't know whether he built a refinery or bought a refinery, but he was in the refinery business in New Orleans. That lasted only for about seven or eight months, and then we moved back to Tulsa. I don't know quite what his business activities were then, but two or three years thereafter, he was again in the refinery business, this time in Texas. We moved to Dallas, and so from let's say 1934 or 1935 until 1940, when I left home to go to Columbia as a junior, I would say he was in the oil refinery business.

Aaserud:

Did your parents have roots in Oklahoma, or were they immigrants?

Kroll:

Both families were immigrant families, but they were both born in the United States. Their families lived in New York. My mother's family, however, moved out to Oklahoma en masse. I think they were brought out by the husband of one of my mother's sisters, who was in the oil business. He also from an immigrant family but had gotten into the oil business in

Ohio, and then had moved to Oklahoma. He got the entire family to move out to Oklahoma. My father's family mostly remained in New York.

Aaserud:

Your parents met in Oklahoma or New York?

Kroll:

Well, it's quite interesting to me. My father's brother is a well known artist, named Leon Kroll, and in fact there is a book out on Kroll's work and it's part of the Columbia University Historical Artist's Project. There is an introductory section of that book that's based on an oral interview with Leon Kroll about his early life. You'll hear the same stories I'm telling you today if you look at that, because it was he then who met the Ansen family which is my mother's family. At that time my aunt's husband — the one in oil who started the company — was doing extremely well, and in fact they engaged Leon to do a lot of family portraits. There's a good deal in that oral interview about that period. In the course of that, he met my mother and he introduced his brother to her. My father followed her to Oklahoma actually before they were married. He read in some trade journal that his rival was going to Oklahoma, and he went there.

Aaserud:

I didn't know we were continuing a tradition of family interviews here. That's interesting. What brought you to physics? When did your interest first come up and why and how, what were the influences? You can talk about your schooling first perhaps, maybe that brings us to it; I don't know.


Kroll:

It doesn't. In fact again I would say it's family background. I was always told what a talented student my father had been. My mother always used to tell me about all his wonderful triumphs, and about this particular problem that he was able to solve and nobody else was able to solve in his class. In fact, I was told what these problems were at various times, and I ultimately solved them all.

Aaserud:

It was that specific.

Kroll:

Yes, and also because my father was very proud of those particular things. Another episode 

from his college days which he always was very proud of was that he went to Columbia. As I said he was an electrical engineering student, and you must have heard of Michael Pupin, who Pupin Hall is named after.

Aaserud:

Oh yes.

Kroll:

He was a student of Michael Pupin, and he recounts a lecture in which Michael Pupin did a long complicated calculation on the blackboard and came out with an answer. My father was doing what any first class physicist would do. He didn't realize his talents went the wrong way, I would say. He had done a back of the envelope calculation while all this was going on on the blackboard and gotten a different answer. He was a very little fellow, and he raised his hand and said, "That's wrong." Pupin was not one to take that particular way of being corrected in a very friendly way. So he bet my father a million dollars that he's wrong. My father accepted the bet. Pupin lost the bet, but he never paid the million dollars, even though he had it.

Aaserud:

Did he admit it?


Kroll:

No, in fact I think the quote was, "You're right but I can't pay you." But that wasn't true, he could have paid him.

Aaserud:

But that was how far he came with him. That wasn't bad after all.

Kroll:

So in any event, I had that kind of home atmosphere. My father had some old technical books that I used to read on building radios. I started doing that sort of thing before I was ten years old, actually. Then I bought chemicals; I had a big chemical laboratory as a child, and a microscope. I used to do all these things on money that I earned by mowing the lawn. A lot of it was just the kind of collecting shopping that one does when one goes into a record store and looks at the records. I used to walk five miles across Tulsa where I lived and visit these Tulsa chemical companies. I was a little kid, and they let me look at all the chemicals stacked up on the shelves. I just enjoyed doing that. I enjoyed seeing the bottles and the 

chemicals in them.

Aaserud:

So it was the family that was the strongest influence. Did it connect up at all with your schooling?

Kroll:

Well, not at that time. I was just taking standard schooling. I was reading my father's technical books, which were not very well suited to me. But I enjoyed the sensation of reading them even if I didn't know what I was reading.

Aaserud:

There were no teachers specifically that encouraged the interest. It was entirely a family thing.

Kroll:

Yes. I would say school had nothing to do with it. Except I was a good student. All my teachers thought I was a good student.

Aaserud:

You went to school in Tulsa for how long?

Kroll:

Well, I went to school for the period I was in New Orleans, which was elementary school. I even remember my science teacher there. She was absolutely abysmal. I would argue with her and she would criticize me for disagreeing with what was in the book.

Aaserud:

No bets involved this time.

Kroll:

No, because I wasn't that sure of my ground, but I was sure she didn't know what she was talking about. I mean, was not sure that I was right, but I was sure that she didn't know what she was talking about. In any event, I must have gone to Dallas in 1934 when I was in the sixth grade — something like that. I went through high school in sort of the normal way. I took chemistry. I was very good at that because I'd had all this pre-work. I took physics

and did fine in physics, though again it was taught by an absolutely miserable teacher. She was an elderly grandmother.

Aaserud:

This was up through high school.

Kroll:

Up through high school, and I would say the physics course I took in high school was perhaps the worst science course I've taken in my career.

Aaserud:

You might have learned from it in a negative sense. What about siblings, brothers and sisters, do you have any?

Kroll:

Yes, I have an older sister and younger brother.

Aaserud:

Did they have the same kind of inclination?


Kroll:

No. The family had a great interest in art because of my uncle, and because my mother's family always felt that the arts were superior to business and things of that sort. That kind of atmosphere was present in the household, and I was myself very interested in the arts. I went through a stage where I really wanted to be a musician.

Aaserud:

About when was that?

Kroll:

Well, I became very seriously interested in music when I was about 15. I had taken some music lessons as a small child, but then I started studying piano much more seriously, and became very interested in music, interested in harmony, composition and the technical aspects of music, listening a great deal. In fact I had a cousin who was a very close friend of mine who sort of went through the same transition at the same time. We lived in the same town and we were very close friends throughout our childhood. He had also had a scientific 

interest, had also had a chemistry laboratory and also studied, taking private lessons in medicine, doing a lot of work with slides in the laboratory, also taking piano lessons at exactly the same time, and he got very interested in music. He is now professor of music at UCLA.

Aaserud:

So he took the opposite track.

Kroll:

The point is, it was a path that either of us could have gone either way; it was actually that close.

Aaserud:

Yes, conceivably I could have been interviewing him instead of you.

Kroll:

Yes. He's a moderately successful composer. He has recordings.

Aaserud:

Was that from the mother's or the father's side?

Kroll:

My mother's sister.

Aaserud:

What eventually made for your tilt towards physics?

Kroll:

Well, of course then I began going to college and it was really there that the first serious signs began taking place. I didn't decide between the two until I graduated college.

Aaserud:

So it was open even during the first year at Columbia.



Kroll:

Yes. In fact, Columbia was the first place I went to, where I had any opportunity to take any music in college. I had taken private lessons but I took music courses at Columbia. And it was the last half of my senior year that I actually dropped my music courses and decided to go into science. I would say the war had something to do with it.

Aaserud:

Yes, we'll come back to that.

Kroll:

But as of the moment I graduated from college, I still had no clear idea of what being a physicist was like.

Aaserud:

Have you kept up with the music side of it?

Kroll:

Oh, I'm very interested in music. I go back and forth. I have what I would call a serious but completely amateur interest in music. I listen to music every day. Often quite a lot — more than I have time for. I have a lot of scores. I listen in a non-casual way.

Aaserud:

Perhaps you not only listen; are you keeping up your piano?

Kroll:

I have stopped playing the piano, but that's fairly recent; maybe ten years ago. I play very little now. If you stop long enough at my age, it sounds so bad when you start that you can't stand it. Also, there are the physical problems. My shoulders ache. Arthritic type problems — that sort of thing — keeps me from really working hard at it.

Aaserud:

Are there any other influences from the home or otherwise that you would point to — religious, political, whatever? Intellectual influence I guess we've talked about.

Kroll:



Religious, political. Well, there were of course religious and political influences in the home. I don't think they have any direct scientific bearing. It certainly had direct bearing on me personally.

Aaserud:

I guess the entry to Columbia was quite natural then, since you had that earlier connection in the family, or maybe that didn't have anything to do with it?

Kroll:

It had everything to do with it, because my family's view was that it was best for children to stay home and go to college. It was my view that it was best for me to leave home—a view which I hold much more strongly now than I held then. So I made an agreement that I would go to Rice for two years and then I would leave and go somewhere else. The family said OK. From the beginning of my second year I began writing away to institutions without asking anybody, getting all the information together, and said, "Well, the two years are up, where should I go?" They hadn't really realized I was listening that carefully to what they said. But it was agreed that if I was going to go away, it was best that I go to Columbia, because at least I had a lot of family in New York and I would still be able to have family influences. So they were much more pleased at the idea of my going to Columbia than at the idea of my going to Harvard. The truth is, it's very hard to transfer anyway, to a first class Ivy League school from a place like Rice in the South. So I'm not sure I could have gotten into Harvard had I pursued it. But I did get into Columbia.

Aaserud:

But you had no personal conflict with yourself about not choosing Harvard.

Kroll:

No, living in New York was very appealing. I still had this very intense interest in music, and there's no better place than that.

Aaserud:

So now we've come to 1940 approximately?

Kroll:

1940 is when I went to Columbia and 1942 is when I graduated.

Aaserud:



Were there any particular influences at Rice? Was there anything there that pointed in that direction?

Kroll:

It was of course very intellectually enriching. The differences in intellectual atmosphere between Rice and high school was a day and night change from the start. I thought school was always like high school. So I liked college much better than I had liked high school, which I just sort of tolerated. I enjoyed myself very much at Rice, and then when I changed from Rice to Columbia, it was another shock — just as big a shock.

Aaserud:

How did you support yourself during that period?

Kroll:

My parents supported me.

Aaserud:

Maybe you could describe your first years at Columbia. What were the teachers like?

Kroll:

It will come back to me as I start talking about it. Well, when you transfer to an institution, the first thing you discover is that they had requirements which were different from the ones that you satisfied at Rice. There are various things you are expected to do to make them up, so part of my program was influenced by that. There was a famous humanities sequence at Columbia that you were supposed to take that I really hadn't had, but I managed to talk myself into the Columbia Colloquium courses. You don't know about that, but it was a famous course for juniors and seniors at the time also, in which this sort of humanities course was repeated, but on a different level. Classes were limited to 15 students, and one would meet for a long evening once a week to discuss books that you had read during the week, which were of course assigned. It was a kind of reading program that was rather similar to the humanities sequence program although there was quite a bit more in it. I took that. It was a very memorable course as far as I was concerned, mostly because I was reading these books for the first time; other people were reading them for the second. I didn't feel I had any problems competing with those smart boys from the East. I felt perfectly at home with them, and I quickly established myself among them. I took an advanced calculus course, and I also felt quite at home with that. What music courses I took, I took counterpoint — that's what I took that year. I took French. The courses I had the most trouble with were my physics courses. I took some physics, and I was not prepared

for the physics I took. In fact, I dropped the first physics course or one of the physics courses I registered for the first quarter I was there. The other one was with somebody named Falwell. I took a course in molecular physics and a course in optics. I think I did those as a junior. Falwell was not a very good teacher.

Aaserud:

So you had quantum physics at that level?

Kroll:

No. I would say the quantum aspects of it were very very minor if at all. Falwell himself was a man who was trained in classical physics. A lot of professors didn't understand quantum mechanics in those days and he was one of them, although he may have used what I would call old fashioned quantum mechanics. As far as things like real wave mechanics, in the sense of dealing with the Schroedinger equation and that sort of thing, I would say most of the professors at Columbia at that time did not feel comfortable. Falwell was one who didn't. So that was my junior year. My senior year I continued with the music course. I did not continue the colloquium for the senior year. I didn't take the senior colloquium. So I guess I was moving — well, I just didn't have time. I was interested in other things, but was still thinking about music, in which I took a course in analysis and a course in composition, maybe. In the senior physics course I took thermodynamics from Fermi. He was the most famous person I ever had a physics course with. The war had started and a lot of people were sort of only partially involved.

Aaserud:

The war in Europe?

Kroll:

The United States was already in. Pearl Harbor was December 1941, wasn't it?

Aaserud:

Yes, that's right.

Kroll:

I guess the United States was not yet in during the first half of my senior year.

Aaserud:



So you got your AB in 1942.

Kroll:

Yes, that would be June 1942 and I think Pearl Harbor was December 1941. So that would have influenced the second semester but not the first. And in fact it did. The second semester was entirely math and physics. I dropped all other courses I was taking. That was I think definitely influenced by the fact; the career decision was very marginal at that point anyway. I hadn't decided which way to go. I had the belief, which I still have, that given an equal amount of talent in both fields, you'll do much better in science than in music. In other words, the amount of talent and superiority it takes to be successful in music is substantially higher than it is in science.

Aaserud:

Yes. But the physicists weren't then perhaps a much bigger group. Now they are. Just in terms of numbers.

Kroll:

In terms of numbers — yes, you're right. It was a much smaller group then, but nevertheless this was apparent to me, at least it was my opinion. I think it's truer now but even then it was true. Well, you probably do not have to be as good now as I thought you had to be then to succeed in music either. I mean, universities have to some extent provided a solution for composers which is similar to that in science, in which you can have a career as a university professor and compose and you don't starve. You may not get your stuff played, which is perhaps very bad, but at least you don't starve. You have a respectable living, as contrasted with struggling along by having private students, which is what composers had to do in the past. So I think there too it's not as bad as I imagined it at the time. But anyhow, given all those circumstances and the fact that the war had started, I decided to go into science.

Aaserud:

How did the war affect the university? It didn't affect your education, it seems, immediately. You took your AM the year after.

Kroll:

That's right. I got an assistantship in physics and I continued. By that time I was partially supporting myself, though I still got assistance from my parents. I could have gotten along without it but it was more comfortable with it. And they were happy to provide it.

Aaserud:



Did you make the complete transition after the BA? Didn't you do any more music after that?

Kroll:

I continued to take piano lessons, but I didn't take any more music at Columbia, and I no longer regarded that as a career option I wanted to pursue. So I took first year graduate courses.

Aaserud:

So the choice was essentially made.

Aaserud:

You got your AM only one year after the AB. Then it took five years until you got your Ph.D. That obviously has to do with the war and the war affecting the university and your work there.

Kroll:

Yes.

Aaserud:

I'm very interested in a description of that.

Kroll:

I'm trying to remember when I started at Columbia Radiation Lab. Is there a date for that?

Aaserud:

Yes, it says "member scientific staff, Radiation Lab, 1943-1962". So the beginning year is 1943.

Kroll:

OK, then that answers the question. So once I got my Master's degree, I decided that instead of continuing as an assistant in physics, I would become a research assistant at Columbia Radiation Lab, which was functioning in Pupin. There were two classified projects going on in Pupin at that time, one on the SAM in the basement, which stood for Special Alloy Materials, and the nuclear weapons project which I think was devoted to work related to the diffusion process. And then there was the Radiation Lab, of which the

objective was to develop very short length wave radar. One centimeters was the wave length they were working on. So I joined that.

Aaserud:

Were you asked to do it or did you volunteer?

Kroll:

I sought it out. I went to both places, and I recognized that I didn't have much training. I didn't know that much physics, but what I knew about was electrodynamic theory. I still hadn't had quantum mechanics at that point. In fact, I had no atomic physics or quantum mechanics background. The truth is, it was absolutely zero. I had studied about it. There was none in my elementary courses at Rice, and I hadn't taken anything along those lines at Columbia. There was an atomic physics course which one could take, but I hadn't taken it. That was sort of old fashioned quantum mechanics, and then there was the real quantum mechanics course which you could take as a graduate student, which I was ready to take at that time. I didn't work full time at the Radiation Lab. I worked part time and I continued to take courses, so I did take quantum mechanics.

Aaserud:

There was a complete separation of course between education and the Radiation Lab?

Kroll:

Yes. And I made the decision at the time, after interviewing at both places, that it was much more likely that I would be given something serious to do in the Radiation Lab. I had the feeling I would be a bottle washer if I went to work in the SAM project. It was really for that reason I selected the Radiation Lab. Also, I had had an ESMWT course in microwave electronics. So at least I felt I had some advance knowledge of that sort of thing.

Aaserud:

How large a part of the physics student population did enter into that kind of work at the time?

Kroll:

I thought, most of them. It wasn't very large.

Aaserud:



No, of course not. How large would you estimate?

Kroll:

In the Columbia Radiation Lab, there were on the order of ten people in my situation. It was not a big laboratory. It occupied one floor of Pupin, and it had maybe five professor level type people in it, and I would say ten students. If I really tried to count them, it might not be that many. Then these were the draftsmen and the machine shop — various technicians to do things.

Aaserud:

Could you describe the work there? Who did you work for or under?

Kroll:

I can describe it. I can tell you some of the other people who were working there. A number of them have had at least reasonable careers in physics, although not many of them, I would say, from those very early days have had very noticeable careers in physics. Arthur Astrom is a person I remember who went to Bell Labs. Gordon Becker is another person who went to Bell Labs. Stanley Oller is another person I remember from those days. Tom Rusenov.

Aaserud:

These were all about your age?

Kroll:

Yes, that's right. Martin Sigmund. Lenny Lusensky. As I keep thinking more names come to my mind. A girl named Marion Sidrum. Another girl named Sobel. I forget her first name. Ruth Sinkins. A lot of people. Danny Robins. Pat Doyle. Quite a few.

Aaserud:

They all continued in physics later on?

Kroll:

No, but most — the majority. I would say Margie Friedman is another person. I would say a certain fraction of them got academic positions. A much larger fraction went into technical jobs in industry. And probably half of them got Ph.D.s. Even the half that didn't, I think went on with some sort of technical work. Maybe more than half got Ph.D.s.



Aaserud:

How big a shock was that? We've spoken about two shocks. How much of a change was it to go from the educational situation to that kind of work situation in the Rad Lab?

Kroll:

It wasn't as much of a shock, and it took me a little longer to realize what was going on. I didn't really understand what being a physicist was like until well after I got my Ph.D. Because in that respect, I was a slow learner.

Aaserud:

But it must have been a new and valuable experience.

Kroll:

It was extremely valuable. In fact, the major step toward learning what being a physicist was like was made there, and made relatively quickly. My initial job was testing magnetrons, which was what most people did. However, I was known as a graduate student who did well in courses, so it was imagined that I would indeed be a Ph.D. student at Columbia and that I probably had some theoretical ability. So Lamb and I started working on the theory of the resonant modes of magnetrons in general, and of the rising sun magnetron in particular. I took to that work very well. I think I was very quick at getting into it, very quick at getting answers, and I think very quick at making suggestions of ways to proceed which Lamb hadn't thought of. So I think I made a very strong impression at that time.

Aaserud:

You were actually able to do quite a lot of physics in that situation.

Kroll:

It quickly became all physics.

Aaserud:

That was all of it after a while.

Kroll:

Well, from the tube tester, I began inventing new kinds of tubes, and so I designed my own tubes and tested my own tubes. 

Aaserud:

Yes, so you were very close to experiment at that time. It was not merely calculations of magnetron capabilities or whatever.

Kroll:

We designed magnetrons and we measured the resonance frequencies, to see how well the theory agreed. Also I learned enough about how magnetrons worked so that I knew the kind of mode spectra. We had notions on what kinds of mode spectra were desirable for magnetrons, and I knew how to design them to get those characteristics. So I did design tubes, and everything I ever designed worked to some extent. Some worked very well.

Aaserud:

It was essentially you and Lamb?

Kroll:

Well, not just me and Lamb. I worked with Sidney Millman, whose name you might know.

Aaserud:

Yes.

Kroll:

Actually Sidney was a big influence on me at that time.

Aaserud:

In what sense?

Kroll:

Well, I somehow, I got a clearer notion of how one is a physicist from my interaction with him than I did with Lamb, at that time at least.

Aaserud:

What was the difference between them? What was the age difference?

Kroll:



Well, Lamb and Millman were older, but it wasn't a big difference — maybe five years. I mean, they all seemed very senior to me, but they were in fact all in their thirties, I believe. Now 30 seems like absolute infancy to me.

Aaserud:

Was the work there subdivided? Did you know what goals you were working toward?

Kroll:

Oh yes. The laboratory was one unit; the entire subject of the lab was one centimeter magnetrons and shorter. As time went on we began working on magnetrons at wavelengths in the millimeter range. So we built three or four 6 mm magnetrons, all of which I was very closely involved in. It was a lot of that; a lot of the publication reflects this work, by the way.

Aaserud:

Yes, I noticed that. So could you specify that from your bibliography.

Kroll:

Yes, this JOURNAL OF APPLIED PHYSICS article, for example, is basically my dissertation.

Aaserud:

Ah, that is your dissertation. I was wondering about that.

Kroll:

Item #4 on this publication list.

Aaserud:

That was with Lamb, right?

Kroll:

Yes.

Aaserud:

The PHYSICAL REVIEW article is an abstract.



Kroll:

The first thing is an abstract.

Aaserud:

That's also the same thing.

Kroll:

Yes. Those first two abstracts are simply a reflection of the fact that at the end of the war a lot of this work was declassified, and this was simply our way of taking our first opportunity to publish some of the results. Then there's a subsequent pair of papers, numbers 4 and 5, which are the papers related to those first two. Now, paper #4 is kind of a general theory as to how to calculate the resonant modes of various types of magnetrons, and some discussion of how you get various mode characteristics — how you see what the mode characteristics are going to be. Paper #5 is an example of the application of that to the invention of a particular type of tube, and it's an example of using one's knowledge of how to influence a spectrum to design tubes. There are many others which I in fact designed and built, which worked, and which illustrate interesting principles. These were never published, although they may be referred to in some subsequent things that I wrote, but they're only in Columbia quarterly progress reports.

Aaserud:

So that's an additional source.

Kroll:

And in fact here was a final report in which I wrote some things about some of those special tubes. But there are some subsequent writings on magnetrons. There are the two chapters in the Radiation Lab series which I wrote, and then there are several chapters in a book edited by Ocross on cross field magnetrons, which contain various tidbits — things that I did which were not otherwise published.

Aaserud:

This wasn't a field that you just left after the war; it went on, and it continues on your publication list.

Kroll:

Yes.



Aaserud:

So it was an interest that continued. Could you say a little bit about physics with or without this experience. That's just philosophizing perhaps, but obviously this experience was your first important exposure to real physics in some sense.

Kroll:

Yes.

Aaserud:

Had this experience not occurred, how different would your physics have been? Could you say anything about that?

Kroll:

Well, I can imagine the disastrous courses which might have happened, instead of what did happen. At the end of the war, it was obvious that I could use this magnetron work for my dissertation, but I was advised not to, because it was felt that I really needed an education in modern physics. In fact, at the end of the war I had still not passed my qualifying exams, so the first thing I did was take them, and I had problems actually because I'd never had atomic physics.

Aaserud:

This is qualifying for the Ph.D. study?

Kroll:

That's right.

Aaserud:

You had qualifying exams for that.

Kroll:

And then after you did your dissertation you had also at Columbia in those days a written final examination on the advanced courses they had — a two examination regime which I think was rare even at that time. It doesn't exist any more anywhere. It was a basic part of the Columbia education and very very vigorously defended by the people who had gone through it themselves. But eventually they became the minority at Columbia so it was abandoned.



Aaserud:

How was the transition from World War II to normalcy?

Kroll:

I was about to tell you that. So I began working with Otto Halpern. He did I think some very important work on magnetic scattering of neutrons. I think that's what he was well known for. And of course he did some important work during the war on the invention of the anti-radar thing he was famous for. That may very well still have something to do with the Stealth technology, since I know nothing about the Stealth technology. I know nothing really about the Halpern thing either except that it existed. So I can make these speculations freely.

Aaserud:

I'll have to look that up. It's not crucial for our conversation.

Kroll:

Anyway, Halpern wanted to work on the double Compton effect which was in fact a very interesting problem to work on at that time.

Aaserud:

Was he your age then?

Kroll:

No, he was much older.

Aaserud:

He was a professor at Columbia then?

Kroll:

Oh yes. He was a visiting professor. He was, I would say, a rather senior person. He must have been in his fifties. I must have been 24 when I started working with him. So we worked I would say for a while and we in fact published something. We didn't really see eye to eye on the problem, and so anyhow I decided I didn't want to use it for my dissertation. He felt I had done a dissertation and wanted me to write it up as a dissertation.



Aaserud:

So you returned to Lamb for that.

Kroll:

I just said that I didn't want to write that up as a dissertation. And we weren't in agreement about what our results meant. So I never published anything on it beyond that PHYSICAL REVIEW letter.

Aaserud:

Which was already in 1947, also before your Ph.D. So you had actually a publication history before your Ph.D. was completed.

Kroll:

Yes. I would say yes. Well, all that other work was done way before my Ph.D. The date on the MIT Radiation Lab Series — numbers 6 and 7 — is 1948. We sort of weren't free to publish until those first abstracts appeared. And then we began writing things up and they were published over the subsequent period. As I just said, I wanted to go back, in spite of the advice that I had gotten, to use that magnetron work for my dissertation. But while I was cleaning all that up and studying for my final examinations and doing all those things, the Lamb shift had been measured, and so again that was, I'd say, dumb luck. I was on the spot and in a position to collaborate with Lamb on the theory, which we did together, of the Lamb shift. I got out of that what I might well have gotten had I done a good dissertation with Halpern on quantum electrodynamics. Instead I did this work which I did before I got my Ph.D., but was not actually used as my dissertation either, because I had already decided to use the other.

Aaserud:

So how much work did your dissertation involve?

Kroll:

Well, none, because it was just this already written up paper, and I'd done that. But while all the formalities were going on, which took over half a year, I was working on this other problem, and I would say we finished the research on that in August and I got my Ph.D. in June. In fact, we were writing it up in August.

Aaserud:

Right, so that's the other part of your work with Lamb.



Kroll:

That's right.

Aaserud:

Exactly, I'm beginning to get these things straight now. So there are actually three strands of your work. There's the magnetron work with Lamb, then the quantum electrodynamics with Halpern, and it's the Lamb shift work with Lamb.

Kroll:

Yes.

Aaserud:

Right. How interconnected are they really? Are they three independent strands of work, would you say?

Kroll:

No, there's a close relationship between the Lamb shift work and the work I did with Halpern.

Aaserud:

Yes. So the magnetron work was an aside for Lamb, in a sense, too, in his career.

Kroll:

Well, I would say it's had a profound influence on his career also. I mean, Lamb became an expert in lasers, and I would say there's a very strong connection with that early work. Furthermore, the techniques used for doing the Lamb shift involve the use of microwave techniques, which again, required his familiarity with microwaves. He also did some inventing while he was in the Radiation Lab, so I would say the experience had as strong or stronger influence on him than me.

Aaserud:

So the war work wasn't an aside. It actually might have helped these developments. In terms of technique, for example.

Kroll:



In terms of technique I would say it was a tremendous boost to physics. I mean, the enormous support which physics got after the war was a direct consequence of the war work. It launched the careers of everybody who was at that age, I would say. Far from being negative, in this case it undoubtedly got people into real physics problems much earlier than they would have otherwise gotten into it. If it had not been for this war work, I would probably have done a theoretical dissertation with somebody, and I think I would have matured much more slowly. Actually, I was slow enough as it was. I expect students to mature quicker than I did.

Aaserud:

Well, these were very special circumstances, of course. I'm sure you don't feel you suffered from it later on.

Kroll:

No. It's very hard in any event to go back and decide how things would have been, had things gone differently. I mean, if you are fortunate, you tend to make use of everything that happens to you in one way or another.

Aaserud:

I think it's the APPLIED JOURNAL OF PHYSICS paper that you refer to a dissertation by Clarkston at MIT from 1941 as kind of the main precursor of that magnetron work.

Kroll:

Yes. There's a paper by Goldstein also.

Aaserud:

So there was a continuity here.

Kroll:

Yes. I guess Clarkston and Goldstein had done the original work on what I would call the unstrapped symmetric magnetron. I think my primary contribution was to see what you could accomplish by making the resonators of unequal size.

Aaserud:

To what extent was the war work an exposure to the need to use physics in those kinds of connections? Was that the revelation or something that was new?



Kroll:

Well, when I went to the Radiation Lab, as I think I mentioned, I had no idea of what a physics career was really like. Nor did I really envisage myself in an academic career. I wasn't convinced I was good enough. I actually imagined going into some kind of applied physics in some kind of company. What I was doing I recognized as applied physics, and it seemed to me to be just the sort of thing that you would do in a company devoted to applied physics. So there was nothing at all surprising about it.

Aaserud:

So even without the war that would have been your inclination conceivably.

Kroll:

It might very well have been. It would have depended on how my graduate career at Columbia went.

Aaserud:

In this edition of AMERICAN MEN AND WOMEN OF SCIENCE anyway you're referred to not even as a theoretical physicist but as a mathematical physicist.

Kroll:

But I would not use that description.

Aaserud:

So that doesn't stem from you.

Kroll:

It probably does. It was probably one of their classification lists.

Aaserud:

Yes, the one that was closest but not close enough. But I mean this was an exposure after all to the possible applicability of physics, that you wouldn't have had otherwise.

Kroll:

Oh yes.



Aaserud:

Was this the beginning of a concern with science policy questions as generally understood?

Kroll:

No. In fact, after that was over, and I did move into quantum electrodynamics, I became very oriented toward what I would call pure science. And while I continued to do the Radiation Lab work, when I went back to Columbia, you know I spent this period at the Institute for Advanced Study. Again I worked on quantum electrodynamics there, and I certainly enjoyed that year very much. One is always looking for an academic job, and I was very pleased when I got an offer from Columbia to come back as an assistant professor, since I'd actually been given to understand from some of the people I had talked to earlier in my career that his assessment—in fact, this was Sidney Millman—was that I had really no chance of getting a job at Columbia. When I was offered one without even asking not too many years later I was naturally quite pleased.

Aaserud:

So you left for the Institute for Advanced Study without any assurance of getting back to Columbia.

Kroll:

Oh no. Although I just took leave from the Radiation Lab, I never resigned from that job. There was of course always the potential, I suppose — although it never occurred to me — of going back to the Radiation Lab as a full time employee. Certainly, though, by the time I went to the Institute, the notion of an academic career was much more real to me. The mere fact that I was asked to go to the Institute was already an indication to me that I, so to speak, ranked among my colleagues. Therefore it was not really true that I didn't have any chance for an academic career, although I didn't know if I could set my sights as high as Columbia. So by that time I aspired to an academic career.

Aaserud:

Maybe you could say a little bit about Princeton and your experience there and how that shaped your career in a little more specific terms — in terms of people and research.

Kroll:

Well, I met a lot of very well known people there, with whom I've remained in contact the rest of my life. I'd say the person that I interacted with most strongly during that year was certainly Bob Karplus. And I would say the next most strongly were Freeman Dyson and



Bob Pious. But other people who were there with whom I certainly interacted were Ken Case, Kenneth Watson, and Sol Epstein. I think Sol had already left. I'm not sure. Sol then went to Columbia so I knew him there too. Hal Lewis had just been there the year before, but I used to go down to the Institute that last half year at Columbia even before I got my Ph.D.

Aaserud:

So you had learned to know Hal Lewis also from those visits.

Kroll:

Yes. I knew Hal from those and I met Rusty Fober then also with whom I subsequently worked.

Aaserud:

How different was the work and the working environment there compared to your work at Columbia?

Kroll:

Well, it was different because the atmosphere in physics was different. There was this tremendous development taking place in quantum electrodynamics, and we were all right in the forefront of that.

Aaserud:

Which had no counterpart at Columbia.

Kroll:

It would have had a counterpart at Columbia too; it's just the time.

Aaserud:

It is the time rather than the place.

Kroll:

Yes, it got started at Columbia. I mean, the Lamb shift is what triggered this great development, and I was obviously working on that at Columbia. And in fact during that year at the Institute, the big issue was who had calculated it correctly, because there was a Feynman answer and a Schwinger answer and there was our answer. There was also a



Weisskopf and French answer. If I haven't mentioned it before, ours was the right answer. So one of the big issues of that year was, what was the right answer? There was also a very significant disagreement between that answer and the experimental results, and another very interesting question was, what was the origin of that difference? And so there was a lot of activity directed towards that. It wasn't solved that year. And then that was the year that Dyson showed how to do higher order perturbation theory and how to do normalization in a consistent way. Karplus and I carried out the first major application of that program, to calculate the fourth order magnetic moment, which calculation subsequently turned out to have some errors in it, which has been a perpetual source of embarrassment to me, but nevertheless the paper I believe was quite influential. First of all, it was quite some time before the errors were found, and during that time the illustration of the way you actually do a renormalized calculation — the demonstration of the fact you can actually do all those integrals and get an answer — was I think influential. Anyhow, that's what we did that year.

Aaserud:

That kind of mistake might just indicate the newness of the discovery.

Kroll:

They were arithmetic, as a matter of fact. I would say the thing that I learned from that is in doing a complicated calculation, you have to take the same kinds of precautions that an experimenter takes to see that dirt doesn't get in his apparatus. We had some internal checks but not nearly enough.

Aaserud:

But it got published; it was refereed, wasn't it?

Kroll:

Oh yes, it was refereed and published and was a famous paper and now it's an infamous paper.

Aaserud:

That's what you have to pay for presenting some new things, I suppose.

Kroll:

Anyhow, that's what I was doing that year. Then I had applied for a National Research Council Fellowship, which I also got, and I had to face the problem of, did I want to take the fellowship and go back to Columbia, or take the fellowship and not go back to Columbia, or

go back to Columbia and reject the fellowship.

Aaserud:

That was the year after Princeton.

Kroll:

That's right. Rabi felt very strongly that I should not take another fellowship year. He said, "You become a fellowship bum." That was what his view of the matter was. I guess I wanted to go to Copenhagen and I guess his view of the Copenhagen school was not that positive either. So he didn't want me to. He was chairman of Columbia at that time, and he was not willing to hold the job open. We reached a compromise, which was that I would come to Columbia for one quarter — one semester — and I would then take the fellowship for the other half of the year. But I would take it at Cornell rather than Copenhagen. So that's what I did.

Aaserud:

You never came to Copenhagen.

Kroll:

I went to Copenhagen in 1955. Not so much later. But only for a few months.

Aaserud:

Did you work with Bohr? I guess Bohr wasn't all that involved in physics that late.

Kroll:

That's right.

Aaserud:

So I guess the Bohr Institute might have been receding a little bit, although it was in great development on the material side. But maybe the Copenhagen spirit wasn't all that present as it had been in the inter-war period. Before we get to JASON, let's talk a little bit about your return to Columbia before that.

Kroll:

What I think we probably should talk about is my continued contacts with applied physics. Since I think they're probably relevant to what subsequently happened. I took a very, I

would say, snobbish view at that time. I thought the only physics really worth doing was pure research in problems on elementary particle physics. On the other hand, professors didn't get paid all that well, and I was perfectly willing to consult on the side for a little extra money. When I first came back to Columbia, during the summer I used to work for the Radiation Lab. I regarded that as a way of supplementing my salary with stuff that would not contribute in a serious way to my scientific career. And the fact that I could get paid for doing summer work which would contribute to what I was really interested in — quantum electrodynamics and that sort of thing — never occurred to me. I didn't think the world was made that way. I was one of the later discoverers of the fact that other people could go to Brookhaven and get paid by Brookhaven. I always felt I couldn't afford to go to Brookhaven because I had to make money at the Columbia Radiation Lab. But, while a slow learner, I did catch up, eventually. Then Columbia eventually acquired a research contract which I participated in, which paid my summer salary for doing what I considered to be worthwhile research. But by that time I no longer worked summers at the Radiation Lab, starting probably in 1954. But I did have a consulting job with Amperex in which I helped them with their magnetron problems.

Aaserud:

Is that a private company?

Kroll:

Yes. And then I eventually consulted for IBM. There I worked on parametric oscillator problems, and it was very educational work as far as I was concerned.

Aaserud:

That's the first item on your list of unpublished work.

Kroll:

But there's a published paper in that area also.

Aaserud:

Which came directly out of that?

Kroll:

Well, it's not that paper, but there's an article on an application of that theory, published in the IBM JOURNAL. So I did that for IBM, and then I spent one summer in Los Alamos in 1959. What I worked on there was magnetohydrodynamic propulsion.



Aaserud:

The Sherwood project?

Kroll:

No, Sherwood is fusion.

Aaserud:

So there's no connection there.

Kroll:

I don't remember that there was any connection. Anyhow, I worked with Francis Low on that problem. Also Eugene Parker was involved in it.

Aaserud:

So your consultancy involvement or summer activities were with the Radiation Lab, did you say?

Kroll:

In 1949, I continued my connection with the Radiation Lab. In fact I think they paid part of my salary. It was one of these businesses where my salary was partly contract-supported, and also I worked for the Radiation Lab in the summer. Then I spent the semester in the summer in Cornell on this NRC fellowship. I worked on electrons, dynamic corrections, hyperfine structure, which was I think quite a significant paper at the time. I also did some work on what ultimately turned out to be the missing megacycles in Lamb shift. That was Barangay and Feynman. And there was a good bit of interaction between us. You'll find me thanked for some parts of that work in the Barangay-Feynman paper, so I feel I made some contribution to that problem as well. It was directly connected to the hyperfine structure problem actually. Once you knew how to do one, you knew how to do the other. It's just that they were doing it and I didn't want to pirate it from them. But that sort of ties up a loose end, because that was one of the hot problems the year I was at the Institute — namely, where were those missing megacycles? By the time I came back from Cornell, or a few months after that, they were found.

Aaserud:

And then to complete the list, then you went to Rome.



Kroll:

That was a Fulbright. It was Guggenheim in Copenhagen and it was Fulbright in Rome.

Aaserud:

That was your Copenhagen experience. I see.

Kroll:

But I continued at Columbia, and by that time I was no longer spending my summers at the Radiation Lab. I think my first summer not at the Radiation Lab was probably 1952 or 1953.

Aaserud:

OK, that early.

Kroll:

Yes. There's a Kroll-Ruderman paper. What's the date of that? Here it is. That was 1954. It's #17 in my bibliography. I think the work for that paper must have been done in the summer of 1953. That means that by 1953 I was spending my summers in Brookhaven and no longer working on magnetrons. But let me look at another paper with Foley. That means that by 1953 I was spending my summers at Brookhaven and no longer working on magnetrons. But let me look at another paper with Foley. That's 1952, which means I spent the summer of 1952 in Brookhaven also. So it didn't take me so long to learn that I didn't have to spend my summers in the Radiation Lab.

Aaserud:

You went regularly to Brookhaven after that?

Kroll:

That's right.

Aaserud:

So that exhausts your consultantships?

Kroll:

That's right. But they were a suitable background for the fact that I then spent the summer at Los Alamos. That summer was the background for the invention of JASON. 

Aaserud:

What brought you to Los Alamos in the first place?

Kroll:

I think that the people who invited me there must have had something like this in mind. I think Goldberger was the one who got me to go. You must have picked up in your other interviews the thoughts that led to inviting that group of people out to Los Alamos that summer.

Aaserud:

Yes. The versions converge but they're not altogether identical, so I'm interested in your memory of that too.

Kroll:

My memory is I was invited to go out by Goldberger. I had never been to Los Alamos. I spent the summer in Santa Fe when I was seven years old. It was a very important summer in my life, of which I remembered a lot, and so I was quite interested in spending the summer in that area again.

Aaserud:

Your experience must have been quite different, though.

Kroll:

It was very different, but I experienced enormous pleasure in trying to track down the things that had impressed me so much when I was seven years old, and I found more or less everything. It was drastically changed; nevertheless, I found more or less everything, and I got great pleasure out of doing that.

Aaserud:

What kind of experience was that?

Kroll:

Well, first of all, it was where we lived in Santa Fe, which had been way out in the country and is now the middle of the city. We had a number of interesting adventures. I sort of located the place of that life. The West was not quite as fully tamed then as it was when I went back to Los Alamos. Vandalay National Monument was a place I went to as a small



child, and again enjoyed tremendously; it impressed me tremendously as a small child — big cliffs and all these ladders going up cliffs, that sort of thing. When I went back, I tried to find the way we had entered, because now there's a big road that goes down, and I'm quite sure that I found the old abandoned trail through which we entered when I was seven years old. But anyway, I just thought it would be fun to go. I had no idea what I would do when I got there. I wasn't told. But there were a number of problems presented that one might work on, and this magnetohydrodynamic propulsion looked interesting enough. This other thing that I was able to get into right away, in an afternoon I already had some ideas as to how to work on it.

Aaserud:

Yes. That was your and Francis Low's work.

Kroll:

JASON was formed after that and I was invited to join as one of the charter members. I was by now quite tired of the IBM thing, and it was supposed to be my exclusive consulting arrangement, and I was very happy to make the change.

Aaserud:

Yes, that was part of the background for it, wasn't it — the wish to have more control over consultantships, not to be asked by any industrial firm or whatever, but to have some more fixed controllable environment for doing that kind of work.

Kroll:

Not the whole story.

Aaserud:

No, it's not the whole story, but it's part of it.

Kroll:

It paid, you know, much less than I was getting from IBM, and I would say I had grown a little uncomfortable with the IBM arrangement. I had a retainer from them, and I didn't really feel I was doing enough work. And the idea of getting involved in problems of overriding national concern appealed to me. It seemed to me to be a much more worthwhile way of spending my consultant time than the way I was spending it.

Aaserud:



How was the issue presented to you and what was your response to it; what was the general and your own motivation for joining this venture?

Kroll:

Well, one of the best ways to get people to join a new thing is to tell them that they've been specially selected.

Aaserud:

Which was true in this case, of course.

Kroll:

I was certainly told that. That works moderately well. If you want to start a new program for students at the university, the way to do it is to write a note to a few students and tell them that they've been specially picked out. A lot of them will bite even if they didn't have the slightest interest in the topic beforehand.

Aaserud:

Yes, which wasn't quite the case in this instance, of course. So who approached you there and how? Was it Goldberger?

Kroll:

I think it was Townes, because Townes had taken leave from Columbia at that time to be vice president of research for IDA. Goldberger was very involved; I knew of Goldberger's involvement. It was a standard letter that they sent to people. A man named Martin Stern got mentioned.

Aaserud:

Marvin Stern, wasn't it?

Kroll:

Could be, I know several Sterns, some are called Martin, some Marvin.

Aaserud:

I think this is Marvin. Just as an aside, do you know if he's in the area? I haven't been able to track him down.



Kroll:

I have no idea where Marvin Stern is. I think I've only met him a couple of times. I know I've met him once but I'm not sure I've met him more than once.

Aaserud:

He was at Los Alamos at the time.

Kroll:

I don't even remember where I met him.

Aaserud:

You had a letter before or after you came to Los Alamos?

Kroll:

No, no, at the time I went to Los Alamos, I didn't have any idea that it was a trial run for this sort of thing, and so it came completely out of the blue.

Aaserud:

But your participation had been discussed in concrete terms already at Los Alamos?

Kroll:

It was not, no. Not with me. I wasn't at Los Alamos that long, because there was the first Soviet High Energy Conference and I went to it.

Aaserud:

So in that sense you were not one of the founding fathers of JASON, so to speak.

Kroll:

I was a charter member. I mean, there were certain people who were involved in the preliminary discussions and who were on the steering committee initially. Besides Goldberger, I don't remember exactly who the other three were. You probably know.

Aaserud:

Watson and Brueckner?



Kroll:

It could be. There were four.

Aaserud:

Townes?

Kroll:

No, Townes was the vice president. Could it have been Murray Gell-Mann?

Aaserud:

Yes, it definitely could.

Kroll:

The composition of the steering committee changed over the years, so all those people were surely on the steering committee at one time or another. I just don't know if they were on the first one. I think they were the first ones. But I went to the first JASON meeting. I signed the first contracts.

Aaserud:

There's also a separate strand of developments with summer study 137.

Kroll:

Yes. I was not involved in that.

Aaserud:

But that's also a precursor of sorts.

Kroll:

Yes. I think that is a precursor which maybe Stern was most directly involved in.

Aaserud:

That was Wheeler's conception, I think.

Kroll:



Yes.

Aaserud:

Had you been approached by other people or the same people for that?

Kroll:

I may have been invited to that also. I just don't remember any more. Again, various attempts were made to get people to go out to Los Alamos, or to go to Livermore. Teller was starting Livermore, and he came to the Institute and tried to recruit people to get involved in that. Again, I regarded all those things as distractions from real physics.

Aaserud:

You still have that attitude?

Kroll:

Well, I had it mostly then, and having discovered that I can spend my summers in Brookhaven, which I like as a place very much, and where I can do what I want to, I think I must have turned down invitations to go to Los Alamos. That I accepted this one was probably due to the fact that Murph asked me rather than somebody else, so I didn't have to take any initiative of trying to get there, and partly that having gone to Brookhaven every summer for the past six or seven years, my family was ready to try something different.

Aaserud:

A lot of factors were involved.

Kroll:

Yes.

Aaserud:

Were you close to Murph at that time?

Kroll:

Oh yes. Close is too strong; adult males tend not to get very close.

Aaserud:



No. Great respect at any rate.

Kroll:

That's right, and we were very friendly, I would say.

Aaserud:

Had there been any prior discussion with the same people or other people about a similar kind of arrangement, or was this your very first exposure.

Kroll:

I don't remember enough. I remember that Murph had suggested it. He suggested it at a time when my mind was open to doing that sort of thing, in a way that it might not have been had it been suggested a couple of years earlier.

Aaserud:

It's impossible to sort out those things entirely. We would have to have some kind of independent documentation if we were going to discuss that kind of thing.

Kroll:

You know, I think what we're doing is something like what one does with a psychiatrist, and things begin to come, recollections begin to bubble up as you talk about periods of your life. That part of your brain evidently begins to be activated, and probably if I talked about this period extensively enough a lot would come back to me, that I don't remember now.

Kroll:

My decision to go to Los Alamos that summer was not a big deal at the time, even though I think it had important consequences.

Aaserud:

It's more by hindsight than by planning. What did your tenure in JASON eventually amount to? How long were you a member of JASON?

Kroll:

21 years.



Aaserud:

21 years continuously.

Kroll:

Yes. 1960 to 1981 is my recollection. The 21 years is right, the stop and start I'm not sure of.

Aaserud:

The origins of JASON of course are connected to the development of the Institute of Defense Analysis. That is the institution that it formally started under.

Kroll:

Yes.

Aaserud:

And it contracted for ARPA exclusively I think during the first years.

Kroll:

I don't know that.

Aaserud:

And ARPA was in some sense a consequence of the reaction towards Sputnik. Was that kind of discussion explicitly involved in the argument for creating JASON?

Kroll:

Not to me. It may well have been, but I was rather an innocent about the whole thing. I didn't know what ARPA was until I got to JASON. As far as I knew IDA had been there since the days of Jesus Christ, you know.

Aaserud:

So it was more the lure of collaborating with Murph and others than any political motivation?

Kroll:

No, there was a national policy issue. I wasn't unaware of the national policy issues. I



followed sort of everything that went on in the world very carefully, actually. For instance, I was very fully informed about the Baruch Plan and the Lilienthal Plan and all those things.

Aaserud:

So that was then an important part of the motivation after all.

Kroll:

Again, Charlie Townes is certainly a man I respected a great deal. He asked me to do it. Naturally he tried to sell it to me, tell me what the things would be. I certainly had enjoyed my summer at Los Alamos and the little exposure I got there to some of these things, because besides working on magnetohydrodynamic propulsion, this was interesting anyhow. I mean, it was sort of part of the space program. I learned for the first time a little bit about nuclear weapons work, I got some introduction to particle beam problems, all of which is stimulating to the imagination. And I was very much emotionally involved in the Oppenheimer business. I'd been at the Institute, I knew Oppenheimer well, and so it was something I followed in great detail; I bought the transcript, and read it from cover to cover. I sort of read everything and was fully cognizant of all the national policy issues. So being invited to get involved in them in this kind of insider way was quite attractive to me.

Aaserud:

So the Oppenheimer case didn't affect you negatively. Conceivably that could lead to some kind of suspicion about that kind of involvement.

Kroll:

Well, it may have had a bearing on my initial reaction to being asked to go to Los Alamos, actually. I could imagine that it did.

Aaserud:

But it had both kinds of effects; it also made you aware of these kinds of connections and the possibility of making a positive contribution perhaps too.

Kroll:

Perhaps.

Aaserud:

Brueckner I think indicated to me that there were some strong discussions at the time about

the validity of an institution particularly like JASON — whether there were other possible arrangements that would be better in furthering these means. Were you involved in that kind of discussion?

Kroll:

Well, in the initial pitch, one of the primary motivations was to get the new generation of scientists involved in advising the government. There was a senior group of scientists who had gotten deeply involved in advising the government as a result of the technical contributions to World War II, and so there were lots of people who were going to Washington all the time, of which Oppenheimer was one, Rabi was another, Kistiakowsky, later on I think Seitz — a long string of people that were very much involved in that. There was no avenue to recruit a new generation into that — and people who were essentially academic scientists and who commanded the respect of the scientific community at large, and who would at the same time be in a position to provide government advice. One of the primary motivations, I had understood, of getting JASON started was to develop a new cadre of such people. Again, I thought that was a good thing and I was honored to be included. Now, there may well have been a discussion of whether there were other vehicles. I wasn't involved in those.

Aaserud:

I'm not perfectly aware of all these developments, but I think Bethe for example would have preferred some more open avenue towards general exposure of all physicists. He may have thought that this was a kind of too elitist arrangement, that this excluded too many physicists.

Kroll:

I would argue that the elitist aspect was effective.

Aaserud:

Yes, but since we both don't know this discussion in detail we should probably drop it.

Kroll:

Yes. There is something called POPA now, which you must be well aware of. It's more representative of what maybe Bethe would have thought would have been a good idea. It doesn't play the same role. I think it plays a good role too, but it's not the same one. It's no substitute for what JASON was trying to do.

Aaserud:



No, it's entirely different. It's the insider versus the outsider kind of approach, I guess. That could be a valid description of the difference.

Kroll:

I mean, the idea of an open insider is, it seems to me, almost a contradiction in terms.

Aaserud:

One main objective was to educate the new generation and expose the new generation to these kinds of questions. To what extent was this new generation new to these questions, and to what extent was it a positive educational experience for most people joining?

Kroll:

Well, technically it was broadening, because I think I worked on a wider variety of physics problems than I would have done before. I regarded that as very valuable and very beneficial. It may well have detracted from my work in pure physics. I certainly haven't done as well or as much as I would have liked to have done. I think, no matter what I had done, that would probably be true, but in any event that is true. I don't have any conviction that had I done no JASON work whatever, I would have done any more, but maybe I would; how can I tell?

Aaserud:

There's a very important difference between three generations of physicists here. You have the generation that educated you — that had this science policy experience during the war, so to speak. And then you had your generation that was educated.

Kroll:

No, that's really not true because the people that had the science policy experience was again a small elite group, quite senior, and not the people with whom I had direct contact. I mean I had some contact with Rabi and Oppenheimer, but not really as teachers.

Aaserud:

What about your teachers like Lamb and Halpern for example?

Kroll:

Well, the people I would identify as my teachers would be Lamb and Millman. I would say that Halpern's educational role in my life was really quite minimal.



Aaserud:

But the JASONS of your generation may not be typical of that generation either.

Kroll:

There was a certain group of people of my generation who went to Los Alamos instead of to the Radiation Lab. I think the contribution that made to their career was much stronger, because the problems that they worked on were much more closely related to modern physics. The level of theoretical activity was higher and I would say the level of theoretical direction was higher. I think for those people it was just a major and very positive influence on the development of physicists.

Aaserud:

To what extent was the establishment of JASON an effort of the older generation to educate the new generation, and to what extent was it something active on the part of the new generation seeking their own approach to science advising?

Kroll:

Well, I really can't answer that question, because the people who were so to speak brought up in Los Alamos did have a quite different experience from me; I think that was probably true of Goldberger. I don't remember which of those people were brought up in Los Alamos, but I imagine Goldberger was one. Anyway, those people who had the Los Alamos experience may have been much more sensitive to this kind of thing — the people who had been involved in the atom bomb, who felt very motivated to become involved in government interaction thereafter, and were involved in all the discussions after the bomb as to what we should do. The people who were involved in those discussions and the subsequent developments went through various plans, and may very well have continued an interest. And there were various other classified projects which people of my generation did get involved in, like the work at Convair.

Aaserud:

Convair I think was the prior experience of a number of people leading to the JASON— including Brueckner, and probably Watson.

Kroll:

Brueckner, Watson, Dyson. I think that was just not part of my experience.

Aaserud:



But the Rad Lab experience must have led towards that kind of view, if not as strongly as for the people at Los Alamos.

Kroll:

I think it was minimal. I think on the other hand the ability to jump right into applied problems and get answers was something that probably did contribute to that. And then electromagnetic theory is an important subject in applied physics, and I had a kind of expertise in that area which I would never have had if I'd just taken a graduate course, and then done a dissertation in particle physics. So I had had a kind of knowledge that was of use.

Aaserud:

Well, we could probably agree at least that this was more a physicists' initiative than the defense people's initiative; the initiative of forming JASON was an initiative of the physicists.

Kroll:

Well, there must have been a matching group within the defense establishment or it would never have happened. I mean, how did IDA come into existence? IDA was there, and I would imagine that the whole FCRC structure developed in response to a need to have quality technical input at non-government salaries.


Aaserud:

Of course there had to be some positive response, but I think the positive creation of JASON was more a physicists' creation than anything else. There were developments in IDA at the time, seeking to expand the work of IDA at a general level.

Kroll:

But there must have been high level physicists involved in government consulting who saw the specific need. I think Wheeler was part of that. I think Wheeler's 137 project was undoubtedly started for exactly the same reasons JASON was started — the need to pass the flag along, so to speak.

Aaserud:

Well, one crucial part of it of course is that there was a close interrelationship. Otherwise it would never have worked. I don't think any similar attempts have worked in any other country. I think it was Gordon MacDonald who told me that he had been involved in trying 

to establish something like JASON in England. It just doesn't work, because it's too much of a divide between the academic community and the defense community, and that's very different in this country. Let us talk in a little more detail about the organizational structure of JASON, which probably has evolved in time anyway. It's the senior advisors, it's the chairman and the steering committee, later I think the project committee came.

Kroll:

After my time. Well, it may not be after my time, actually.

Aaserud:

I think Ed Frieman told me that he was the creator of that concept, so that would be during your time then. That would be the mid-seventies that he became chairman.

Kroll:

Yes.

Aaserud:

So that's the structure. What was your place in the structure? You weren't a chairman. Did you participate in the steering committee?

Kroll:

Yes, I was a member of the steering committee.

Aaserud:

Throughout your period?

Kroll:

No, for a certain period. I forget the exact times. The organization did change quite a bit. I probably remember a few things, if you're interested in the evolution of it.

Aaserud:

I am, very much.

Kroll:

At the beginning there was the steering committee and the vice president of IDA — he was



involved in getting it started. There was a very close and supportive relationship between IDA and JASON. It wasn't always true, as things evolved. I guess a man named Garrison was president of IDA at that time.

Aaserud:

So he would be Townes's superior in IDA.

Kroll:

But I think Townes really had the rather full responsibility for the scientific direction of IDA, and was fully sympathetic with JASON. The initial arrangements were in fact retainers, and I had a retainer, rather than a regular consulting fee. That didn't last very long.

Aaserud:

Could you expand on that distinction?

Kroll:

Well, you're sort of on call, and you don't keep track of your time and you get a certain fee. I mean, that's one of the ways people consult for outfits, especially if you're prestigious and they want your name on the thing. They give you a retainer, and then they have the right to call upon you from time to time.

Aaserud:

So it's an administrative thing mostly.

Kroll:

Yes. Well, it's a different kind of relationship in the sense that in consulting, you have to keep track of your time and charge it. The other way you just get a check. That's how it started, and I don't know if everybody was on retainer, but some people were and I was one of them. That didn't last long. I think it soon turned into a time charge thing.

Aaserud:

This is the relationship with IDA.

Kroll:

Yes. But I think that this rather loose structure — it had only a steering committee and the



vice president — lasted for a long time, and there was also initially I think no discussion of how if ever the steering committee would ever change.

Aaserud:

There was never rotation or anything like that.

Kroll:

Nothing, that's right. The main changes that occurred during that time were the various vice presidents of IDA. Townes resigned, then I think Keith Lee became the vice president. After Keith, it was maybe Jack Ruina. And after Jack Ruina, Elliott Montroll. And then of course the leadership of IDA changed over that period too. Was Maxwell Taylor the next president after Garrison?

Aaserud:

I have to get that straight, but it sounds right.

Kroll:

And then Al Flax took over, and the Vietnam War produced great strain. During all this time, I don't think the organization changed very much, though I was on the steering committee during part of this time, as I recall. Oh, I left out Gordon MacDonald. He was also one of the vice presidents. I think Hal Lewis was vice president of IDA when we broke up, so to speak. During the time that we broke with IDA, I think Hal was the chairman. Is that clear? Do you remember that?

Aaserud:

I think so, and Hal Lewis was chairman of JASON then, wasn't he?

Kroll:

He was chairman both when we broke up with IDA, and then when we resumed with SRI, and there was a period when JASON didn't exist. You say, was I continually with JASON? The answer is, no, because JASON didn't exist for three months.

Aaserud:

That was in connection with that transition?

Kroll:



Yes. The transition was not completely smooth. And JASON did in fact stop. As far as I know, JASON did not exist as an organization for some three month period.

Aaserud:

But that didn't mean that you got a letter that you were fired and then a letter that you were employed again?

Kroll:

You didn't get fired, but you got a letter saying you couldn't make any more charges to JASON, and as far as I was concerned, I was not in JASON. I wasn't doing it any more. I thought, maybe if this SRI thing ever worked, it would start again. But it was probably days after that happened that I got a phone call inviting me to consult in other contexts and other ways. So I was not separated from the work at all, in fact.

Aaserud:

But the purpose of the separation from IDA wasn't to discontinue JASON?

Kroll:

I can't answer that question.

Aaserud:

The separation was connected with IDA having too many employees. That was the formal reason for it, I think.

Kroll:

I think that it could well be that JASON was getting to be a little sufficiently prickly to handle that IDA viewed it as a mixed blessing. I felt that Flax was happy to get rid of it, and that this other thing was to some extent a pretext. I have no idea whether that's true but that's my feeling about it at the time. Maybe that was because I felt that it was natural for them to feel that way, given all the trouble that JASON had made about Vietnam. I mean, JASON was causing trouble inside in its attitude towards the war, and at the same time it was drawing a lot of external flak.

Aaserud:

Yes, and it was during that period it was difficult to find another sponsor. In consideration of such difficulties, perhaps the transition happened quickly after all?



Kroll:

Well, I think that if we had agreed to SRI's initial terms, there wouldn't have been any problem with the transition. I think JASON wanted to preserve its independence and its integrity, and was unwilling really to be governed in any significant way by SRI. And I think the SRI relationship was not smooth either. SRI felt that they could not allow JASON to do things that would endanger their ability to get other contracts. So that fear on their part was a source of conflict. Again, I'm imagining that, but I think it's probably correct.

Aaserud:

Were you a member of the steering committee during the transition period?

Kroll:

Yes. I was back on the steering committee.

Aaserud:

Does it make sense to talk about specifically important members during particular periods? Of course, Goldberger was crucial during the first six or seven years, I would suppose.

Kroll:

Yes.

Aaserud:

Are there any grey eminences or whatever that you would call them? I'm fishing now, of course.

Kroll:

I would say all the people who chaired the steering committee were obviously important members. I'd say that all the members of the steering committee were also important members. A person who certainly has been and is an important member—I don't think he participated so much earlier on—is Marshall Rosenbluth. I'm trying to remember. When he was at GA he was a little bit limited in how much he really participated. I don't think Marshall was ever a member of the steering committee while I was in JASON. I think he might have been since.

Aaserud:

He's been a member for a long time.



Kroll:

Yes. Of the people who were never members of the steering committee or chairs some place, I would say he's the most important member I remember. Dyson's another important member, although the amount of actual work he did for JASON was not really very great.

Aaserud:

Was he a member of the steering committee?

Kroll:

I think he never was. I already gave you one blanket list — those are the members of the steering committee and the chairmen. Outside my blanket list, I have to sort of look for which of the other people were specially prominent. I guess Freeman did not do that much work for JASON, although what he did was always quite interesting. People who manage JASON projects, that's another list of people that by and large are important members. That's a big job managing a project.

Aaserud:

I guess that differs from project to project. Freeman Dyson has done some projects on his own, I think.

Kroll:

I mean these big projects where you bring in a lot of briefings and organize a lot of JASON people to participate, and a report comes out at the end. I mean people who have run things of that kind are certainly important members. First of all, those big efforts have impact, by and large, and the people who organize them become known for having done that. Often they're organized at the initiative of the people who do them, so all those things make the people who do them important.

Aaserud:

What about selection of members? Was it the steering committee that took care of it whenever a new member was invited? I guess people didn't apply to become members of JASON?

Kroll:

Well, at the beginning there were no new members. But as we began to age, it occurred to us that maybe we should worry about continuity, so new members began to appear. Now, I was often unaware of how they appeared. New members would just show up. I would see

them, and I assumed the steering committee selected them. I would imagine that people suggested their names or the steering committee thought of them themselves, and that people would be invited out and some of them would take it and some of them wouldn't.

Aaserud:

So it was someone's colleague or something like that.

Kroll:

Some people were asked to join. There was a conscious effort to get people to make suggestions. I certainly made a number of suggestions and some of them became members. It's not been easy to get young people involved. A certain number do get involved and like it, and a comparable number don't.

Aaserud:

Yes, the average age has increased significantly since the early days, of course. The person taking over as chairman now, he is the first chairman —

Kroll:

— not from the beginning. Well, I don't remember if Ed Frieman was a charter member.

Aaserud:

He was not a charter member, neither was Nierenberg, but they started early on. Nierenberg didn't start from the outset because he went to Paris with NATO at that time. And I'm not sure about Frieman. He wasn't a charter member, but he started very early, I think. I think it was a year after the creation or something like that.

Kroll:

I didn't know it was that early.

Aaserud:

I had an interview with him recently. I should remember. I'm pretty sure that's what he said. Most of the people in JASON throughout the time have been theoretical physicists. Was it the same people who discussed problems together in JASON who also collaborated in the academic sphere towards open publication? I notice on your publication list that there are quite a few collaborative ventures with JASONS that are obviously not JASON work as such.



Kroll:

Well, it clearly had an effect. Marshall was here for a long time, but we never wrote any papers together when he was here. It was as a result of our JASON work together that we have collaborated on a number of things. The same is true of Ken Watson. My writing with him certainly started in JASON. On the other hand the work with Ruderman actually started before JASON existed. And we have collaborated in JASON but none of the work we did in JASON was published.

Aaserud:

I'm through about half my cards here, so I don't know what that indicates.

Kroll:

I don't think I'm up to another two hours.

Aaserud:

Reasons for people leaving, constancy of membership — how important was Vietnam and how important were other developments in that respect? It wasn't all that much of a watershed, as far as I have been able to see.


Kroll:

A few people left, I think, because of Vietnam, either because of their own feelings about the whole enterprise of collaborating with a government that was involved in something like the Vietnam War, or because they couldn't stand the heat. Being unloved is a status that's very difficult for some people. So there was the ideological reason, or it was just too uncomfortable for them — which in a way could be in part ideological. I mean, people are telling you you're doing something very immoral, and you don't feel that you can really defend yourself. You might not have felt it too immoral to do if people hadn't made you defend yourself, but the fact that you find it difficult to defend the moral character of what you're doing might be a motivation.

Aaserud:

It's hard to distinguish completely of course. Are there specific cases you would care to mention, or is that something you would rather not talk about?

Kroll:

I suppose I shouldn't. I mean, I think mostly people don't want you to talk about it. You can  interview those people yourself.

Aaserud:

Yes, that's a more sound way of doing it, I suppose.

Kroll:

You can easily, I imagine, find out who resigned when, and go talk to them about it. Have you interviewed any such people?

Aaserud:

Not really.

Kroll:

You've identified them?

Aaserud:

Not in detail. Actually I haven't. I've spoken to people who have resigned. I've spoken to Francis Low, for example, but he resigned much before, which he was happy about having done. I'm going to interview Hal Lewis who I think left later. So actually I have interviewed no people who have claimed explicitly that that was their reason for leaving JASON, so if you could give me a hint.

Kroll:

Well, actually, if you're doing this project, let me recommend that you do something that you may not have been doing. I think you should identify the people who left and try to interview them. I can think of two people offhand that I remember as prominent members who left. One was Leon Lederman, the director of Fermilab. You haven't interviewed him?

Aaserud:

That's right. That's in the Henry Foley papers, I remember that now. No, I haven't interviewed him.

Kroll:

Extremely prominent man. Of course, everybody will tell you about this. But he is a prominent member who resigned. Certainly as prominent as Francis Low. Another quite prominent person who resigned is Mat Sands.



Aaserud:

That I didn't know.

Kroll:

Let's see if I can think of any others. Well, then there were a lot of other people who left. You mentioned Hal Lewis. Steve Weinberg has left. I think Murray Gell-Mann is no longer a member of JASON. All very famous people that you might want to talk with.

Aaserud:

You mentioned Sands. He's at Santa Cruz.

Kroll:

Santa Cruz, yes. There are lots more. I tend to forget who they all are.

Aaserud:

Yes, but I could call up Matthew Sands on my way, I'm going up there.

Kroll:

Then there are a number of people who tried JASON out for short periods of time who may not have been asked to stay. The initial period, once things got codified, I think turned out to be a three year period. You might find it interesting to talk to some of them.

Aaserud:

Yes, certainly. And of course I should talk to non-JASONS; I mean people in the agencies who have connections with JASON too.

Kroll:

Now, a certain fraction of people leave JASON because they're asked to leave, and that's probably not easy to find out who they were. But it's a fact. You should at least know that it's a fact.

Aaserud:

Yes. For what kind of reason?



Kroll:

Non-productivity.

Aaserud:

There haven't been any big leak questions or anything like that?

Kroll:

Not that I know of; in general one doesn't know in detail. It's my impression that it's for non-productivity.

Aaserud:

Is that also essentially a steering committee decision, or do you get hints from higher levels?

Kroll:

I'm sure it's internal, entirely internal. You're not supposed to know about it, but people gossip. So I think by and large everybody does know.

Aaserud:

That's important. It's important to get opinions of all kinds of JASON members who have left for different reasons, and who have stayed on. That was people. We're coming to projects now, I hope. Maybe we could start talking about the selection of projects — how they have been selected, both as a function of time and at any one time, what different kinds of projects have been chosen, whether there are independent projects leading to a specific result, whether they are advised as to how other agencies or other people should do their projects. I'm sure that there's a whole spectrum of tasks.

Kroll:

Yes. I'm sure that's varied over time, how that's done. And I can probably be least helpful to you in connection with that. I think by and large projects are brought to JASON.

Aaserud:

But not forced upon them.

Kroll:

Not forced upon them. I think it must be the case that the administrative staff person — I



don't know what title that job has.

Aaserud:

The executive secretary?

Kroll:

I don't know what it's named, it's the job Don Levine had.

Aaserud:

Yes, I had a long interview with David Katcher.

Kroll:

David Katcher was the first one, and then, Don Levine was another one who had a very long tenure at JASON.

Aaserud:

And there were Jack Martin and Joel Bengston in the meantime. You can correct me on that.

Kroll:

There was another man, Turner, Robert Turner. I don't think Joe had that job very long — it's my recollection — and I don't know that Martin had it very long either. Do you remember?

Aaserud:

I don't have the dates now. I know that Jack Martin had it for a much shorter time than Katcher had it, but I don't have the specific dates. Maybe I should talk to Joel Bengston while I'm here. He's been out of town last week but I hope to see him next week.

Kroll:

I think he had it for a very short time. And I think that Martin also had it for a very sort time.

Aaserud:

Turner I'm not sure that I know about even.



Kroll:

Well, he was perhaps the only one that was fired.

Aaserud:

OK, that may be a reason.

Kroll:

He may have been aware of the fact that he wasn't very much appreciated.

Aaserud:

For what reason?

Kroll:

He wasn't good at the job. Anyhow, the reason I brought the subject up is that it was an important part of that person's job to visit the agencies and find problems.

Aaserud:

Even to the extent of finding problems; not just maintaining contact after the problems were decided?


Kroll:

I would say maintaining, finding — just knowing the people, making the personal contacts that would make it natural for them to think of JASON when various kinds of issues came up.

Aaserud:

And also making the work understandable to the agencies. David Katcher told me that he had a hard time writing up the results from JASON meetings and trying to communicate them to the appropriate people.

Kroll:

I'm not trying to describe all the aspects of that job because I don't know them. I was simply talking about the role which that person played in the selection of projects. And I think that person does have an important role in getting projects brought to the attention of JASON, and I think he interacts very directly and continuously with the chairman of the steering 

committee. I think that's undoubtedly the main avenue of projects into JASON. Many JASON members are members of other non-JASON committees. Problems come to their attention there, and as a result of problems that come to their attention there, they may suggest problems to JASON. So those are, I would say, the two major avenues.

Aaserud:

And there are briefings, of course, but those are the projects brought to JASON.

Aaserud:

Main projects during your tenure — what do you consider particularly important, to the extent that you can talk about them?

Kroll:

To the extent that I can remember them.

Aaserud:

That too.

Kroll:

Main projects, not just the ones I was involved in? It's hard enough for me to remember those.

Aaserud:

Well, let's start with your own projects.

Kroll:

It's very hard for me to remember even what I did.

Aaserud:

OK, let's limit it to that.

Kroll:

I can try to do that sequentially.



Aaserud:

Of course, we have your reports.

Kroll:

Yes, but that's just a small sample, although they may be suggestive. The first specific JASON project I remember working on is this nuclear explosion in space problem. It's a paper. I think it's referred to here, "Scheme for detecting nuclear tests." It's the effect on the propagation of low frequency waves. That's a JASON report. It probably is not findable, but I may have a copy of it somewhere actually.

Aaserud:

Did you work with anybody in particular on that?

Kroll:

No, it was me.

Aaserud:

It was you exclusively. Did that have any particular effect for example on test ban negotiations? Could it be put into any context like that specifically?

Kroll:

Well, I think it was. It certainly had not much of an effect, if any. The question was whether nuclear explosions could be detected by monitoring low frequency transmissions. When there were space tests or extra-atmospheric tests, I'm sure that low frequency transmission was monitored to see if there was an effect, and I'm sure that there was an effect. On the other hand, I think the Vela satellite is the means that was chosen for detecting such tests.

Aaserud:

But it fell into that general debate anyway.

Kroll:

Yes, and I think it is probably true that if you got a Vela signal, one would look for confirmatory signals in radio wave propagation. That's what was discussed in that paper. So there was that project. Now, the question of microwave breakdown came up in connection with the same context it would come up in right now, namely that ABM electromagnetic weapons. It was then in the microwave region and that's still a subject. I got a telephone call

from some committee. One of my colleagues asked me what I knew about this subject, not knowing I'd written this. One of the things that's characteristic of defense science is that many things never get finished and loose ends don't get tied up. So much work is done over and over again.

Aaserud:

That might be one function of JASON, to avoid that to some extent, to ensure the continuity of it.

Kroll:


Right. All these nonlinear optics questions have come up in the question of laser beam weapons. I assume they are very current right now in the SDI business. This whole business was thoroughly studied in the sixties, when they also were trying to develop laser ABM weapons.

Aaserud:

That was also the context of your own report. Could you say something more about the context of it?

Kroll:

Yes. Again, there was a specific proposal. The proponents were arguing that nobody knew anything about microwave breakdown and that you couldn't believe anything that was in the literature. So, there was money spent trying to get people to study the subject. The argument was made that studies in the laboratory were irrelevant because there are no walls when you're doing a weapons context, so you couldn't prove anything from the experimental observations that were made. This report studied some of these questions and claimed that one could predict quite well what the breakdown thresholds would be. It discussed the potential effects of the loss and argued that they couldn't possibly have a significant impact on the main results. There was a man named MacDonald who did experiments at the same time, again supported by federal money for that purpose. He confirmed everything also, so we at least concluded at that time that the breakdown limits that people would naturally have thought existed were in fact right. There had been some other work, in fact by some of my colleagues — some technical work that also supported the view that the subject was very murky and that you really couldn't predict. In fact, one of those colleagues came to argue with me, saying, "How could you conclude this when we concluded the following?" He had done a calculation from scratch, where I had used scaling arguments. So I looked at what he had done from scratch, and pointed out a few cross-sections that he had imagined that you could actually look up and they weren't what he'd imagined. And it would affect the results. He agreed.



Aaserud:

There may be a relationship between what you want to find and what you actually find.

Kroll:

I don't remember whether he said the breakdown situation was worse or better. It was just quite different from what I got. So I don't remember which side he was on. I no longer remember which side.

Aaserud:

You didn't have a political discussion about it.

Kroll:

We have our professional integrity and professional standards to defend.

Aaserud:

And they come first.

Kroll:

I think that was what was involved in the discussion.

Aaserud:

I remember the last time we talked, you mentioned some outcome of that work or some continuation of that work, I think with Watson. Is that correct?

Kroll:

Yes, that's right. We came back to the problem again in the context of laser induced breakdown. This was microwave breakdown, but lasers also cause atmospheric breakdown. And there were some puzzling experimental results which as far as I know still have not been explained. I haven't followed that field very carefully, so there may have been a solution. But we therefore re-examined it for the purpose of trying to explain those results. And so there is a paper on laser breakdown, and we specifically looked into the question of whether the quantum effects were important.

Aaserud:

Was that also JASON work?



Kroll:

Yes.

Aaserud:

That was a few years later, of course.

Kroll:

Yes.

Aaserud:

At a more mature stage of technological development.

Kroll:

And that got published.

Aaserud:

Well, next on your list is your laser summer studies. When we talked together last I think you traced that back to Woods Hole in 1963, to Falmouth in 1967, and to San Diego in 1974.

Kroll:

That's right. I'm not sure that the 1963 study was JASON. I think Keith ran that one. And the 1967 study I ran.

Aaserud:

So these were collaborative.

Kroll:

They involved large numbers of people. These are what I mean by big studies that make thick reports in the end. And the 1974 study, I don't remember who ran. I don't think it was me.

Aaserud:

The one publication here has you as the sole author, and it says JASON Laser Summer Study, 1967. That's the Falmouth thing, I gather.



Kroll:

Yes, that's right.

Aaserud:

But that might be you as the head of the project.

Kroll:

That's right.

Aaserud:

You also mentioned JASON as a pioneer in electron laser.

Kroll:

Free electron lasers.

Aaserud:

Which one of those is that?

Kroll:

None of those. No, the 1974 study contains in fact the first — I would even say published, since that part has been published — claim and to some extent demonstration that the free electron laser was a classical device. Again, my laziness about publishing, my snobbish attitude towards applied physics, lasted even until 1974. That whole area of free electron lasers has subsequently become a fairly important part of my career. At that time I was still very diffident about it, and I did not publish. Even though I wrote to Madey at the time and pointed out to him that the free electron laser was a classical device, that had no impact on anything, and the paper on that subject was not published by me. It was published by I guess Scully and collaborators, even though I had actually done it one or two years before. Not only done it; it was a matter of the public that I had done it, though not to Scully, evidently.

Aaserud:

So even then you did separate between your theoretical activities and later applications.

Kroll:



Yes. Anyway, the JASON work in free electron lasers started it really. That was the first JASON work on free electron lasers, and although that is the first place that the classical nature of the free electron laser is pointed out, I don't know how much impact that had on the free electron laser field. But then there were subsequent JASON summer studies — a whole series of them — and later in the seventies. And those were very important. In fact, the thing that is being built at Livermore and the thing that is being built at Los Alamos are all based upon JASON work.

Aaserud:

So JASON has been very active in that. I don't know if you have had contact with Joan Bromberg. She is heading a large historical project on the history of lasers, and she's interviewing people. You haven't been approached by her? That's too bad because she has more specific expertise in that field than I would have.

Kroll:

Well, maybe free electron lasers are not what she's interested in.

Aaserud:

I think maybe she's interested in the earlier stuff essentially, so that's probably why, and as you say you entered it somewhat later, a little reluctantly, perhaps.

Kroll:

Well, not really. I've actually done everything. I wrote one of the first papers on the optical parametric oscillator which is also a kind of laser, and there's a range of the laser spectrum which is dominated by optical parametric oscillators.

Aaserud:

And after all you were head of the JASON study group earlier, so that goes without saying. I don't know how long we should keep on with your specific JASON projects. We have reached number 2 here only. The next one here is in 1968. It's called "Transient effects in stimulated Raleigh scattering."

Kroll:

In fact there were a whole series of papers, some of which are classified, on the propagation of high powered laser beams through the atmosphere, which is the context of that work.

Aaserud:



For what purpose?

Kroll:

Well, why do people want to propagate high intensity laser beams through the atmosphere? It's the same reason they do now. A very old problem, this modern SDI stuff. And why are you interested in transient effects? Because you send very short bursts, and therefore things which you couldn't possibly sustain over long periods of time really work for short periods of time—because things don't have time to build up.

Aaserud:

Was that done in a specific defense context at the time? Was that part of a larger study, ABM or whatever it might be?

Kroll:

Well, that 1968 paper is probably an outgrowth of the 1967 laser summer study. It's just the published output. The fact is, there's a PHYSICAL REVIEW paper on that subject also.

Aaserud:

And then in the next report you go into the sea — well, not entirely. It's "Lithospheric Propagation for Undersea Communication" with Frieman.

Kroll:

Yes, there's a JASON report on that. Also, there is an open publication on a different but related matter.

Aaserud:

That's in the JOURNAL OF GEOPHYSICS?

Kroll:

Yes, that's not actually. This is spheric propagation and detection of electromagnetic signals a mine puts out on the ocean floor. And there's some unpublished stuff on that same general area involving various suggestions inside the Defense Department. I worked in that area, and played an important role in determining the expenditure of large sums of money in the wrong direction. That initial paper was written in response to a very thick proposal which got a completely wrong answer, and subsequently other proposals appeared which also got wrong answers, which I worked on.



Aaserud:

Then if we have exhausted that, then the next report is with Watson. It's in 1973, and it's "Multiphoton Detachment of Negative Ions."

Kroll:

Yes, that's all in connection with the problem of laser breakdown.

Aaserud:

So these three, numbers 3, 4 and 6, are intimately related, in other words. And then it's free electron lasers.

Kroll:

Yes.

Aaserud:

That's the pioneering study we were talking about.

Kroll:

Yes. It was not only pioneering but of course it continued. We continued to write what I would say are important papers in the field.

Aaserud:

And then the last one is from as late as 1979. It's with Morton and Rosenbluth and that's "Free Electron Lasers with Variable Parametric Wigglers."

Kroll:

That paper was an invited paper in the first special free electron issue of the series of, I guess it's PHYSICS OF QUANTUM ELECTRONICS, which sort of devotes an issue more or less every year to free electron lasers. This was the first issue and this was the invited paper in it, so it's been a very important and influential paper. That doesn't exhaust all the things I did for JASON. Let me see if I can think of some other things I was involved in. Well, I mentioned these various committees, that JASON members tend to serve on.

Aaserud:

Outside JASON you mean, essentially.



Kroll:

Yes. Well, there were even some inside, inside JASON. I don't know to what extent those are classified or not, so I guess I can't really say too much about them. I was involved quite a bit though in underwater sound type of things. That work was classified, but there was actually a rather extensive involvement in that type of project, which I'll call Navy type projects, in which Ed Frieman was very much involved. If you want me to list major JASON projects, I'd say that's one of the major JASON projects — the involvement with underwater sound problems. It gave rise to a published book, which will probably be the dominant book in that field. This is on propagation of sound in the ocean. It's just been something that's been a major project every summer for years. I don't know if it still is or not. There were big Co2 studies, mentioned in a little—

Aaserud:

Was the Navy involvement before or after the 1973 study or concurrently?

Kroll:

Mainly the involvement began actually during the JASON hiatus. It goes back that far. It goes back to the transition between —

Aaserud:

— IDA and SRI. That's early seventies.


Kroll:

That's when it started, and I would say it continued until I resigned really. Maybe not quite then. Before I resigned I had really begun working exclusively on free electron lasers. I would say it actually could well have been 1979 that I was still involved in that. My consulting was no longer exclusively for JASON. I consulted some for Physical Dynamics and some of the Navy work I did for them. I'm not completely clear in my mind, what I did for who. They were very closely related.

Aaserud:

But the consultancy or involvement has mostly been with lasers; would you say that was the most prominent part of the JASON work?

Kroll:

Yes, but not overwhelmingly. If I were to look up the number of days I charged for each of these things, I don't know that there would be a preponderance on lasers if I looked back 

over the same thing. Maybe there would, if I counted all those laser summer studies. It could well be that the preponderance would be laser related.

Aaserud:

You mentioned when we discussed the last time that the JASON work was a source of broadening your general interests, and that in that sense at least the JASON work influenced your general attitudes and work in physics. Could you expand somewhat on that? Am I right in saying that?

Kroll:

Did you write down anything about that?

Aaserud:

No, just that observation. Nothing in particular. That's why I'm asking you now.

Kroll:

I don't know how to elaborate on it. Look at my published work. A lot of it is clearly related to things that I did in JASON. My general interest, and less snobbish attitude toward applied physics, certainly has a connection with my JASON experience. So did also, the policy type things, in which I had very little involvement personally. Nevertheless, the fact that all those policy issues got discussed, and that my friends were very interested in them, and all of that certainly played a role. And I was certainly involved in some studies. I was involved in studies related to the ABM treaties.

Aaserud:

In JASON?


Kroll:

In JASON. I don't quite remember the exact context.

Aaserud:

These were more science policy things than technical things?

Kroll:

Well, there were technical components, and I tended to look at technical issues that such things raised. But the project as a whole had a quite significant non-technical component. 

So I certainly was involved in projects where that was the case. And I certainly found that interesting and broadening too, in the sense of exposing me and making me address national policy issues.

Aaserud:

To what extent did the JASON discussions involve those kinds of more general discussions as to the context of the specific technical work?

Kroll:

Well, some JASON studies were intended to be non-technical. I mean, they were asked to consider policy questions, and clearly when we were asked to consider them, we considered them. And some people are especially comfortable with that sort of thing, and feel, in fact, that's sort of what JASON should do. Some people feel that people working in JASON should do problems in JASON which they would not do otherwise and which they would not do as part of their regular scientific work; they think that that's what it's for. Others take a different view.

Aaserud:

What has it been like in practice in that respect?

Kroll:

Variable. I guess the people who think it should be what you wouldn't otherwise do — that's true for them. The people who think otherwise, do otherwise.

Aaserud:

That depends on what field you're into, of course — the possibility of making a separation. The oceanographers for example have a harder time with that, I'm sure.

Kroll:

That's right.

Aaserud:

The theoretical physicist certainly has an easier time.

Kroll:

That's right. But one could take the view that once I became a free electron laser physicist, I



should stop working on free electron lasers for JASON since I would do that anyway, and instead work on something else for JASON. There were people who felt that way — in a minority, I would say, a very small minority. But the fact that some people felt that way was at least brought the issue forward.

Aaserud:

You had that discussion at various points?

Kroll:

Yes.

Aaserud:

There are a couple of papers with Watson here. One is called "Theoretical Study of Ionization of Air by Intense Laser Process," openly published in PHYSICAL REVIEW as I said, and that was in 1972. And there's the 1976, "Inelastic Atom Scattering with an Intense Laser Beam," also in PHYSICAL REVIEW. Are those JASON developments?

Kroll:

Yes. The first one in fact was directly related to the issue of whether quantum effects would significantly affect breakdown in the optical region. The paper on the atom-atom collisions, and also the multiple ion detachment which never got into the PHYSICAL REVIEW were stimulated by some experimental results that one couldn't explain. We were not able to explain them after writing the paper either, but the paper itself was interesting, and in fact other people took up what we did and wrote subsequent papers on the same general problem.

Aaserud:

To what extent do you value JASON as an interdisciplinary experience — getting exposed to other kinds of problems, being forced to applying your physics but also being exposed to other fields?

Kroll:

It would be nice if it were. That's true to some extent.

Aaserud:

But not a big part of the truth.



Kroll:

Well, attempts have been made to broaden JASON; they've been only moderately successful. JASON now has one professional mathematician in it.

Aaserud:

But there's never been social science component; that has never been thought of.

Kroll:

No, I don't know that it's even been thought of, but there have been attempts in other directions. I mean, we have electrical engineers, computer engineering types. They fit in well. But attempts to get biologists involved haven't worked either.

Aaserud:

No. But Allen Peterson has been a member from the beginning. He's an engineer. I hope to interview him while he's here. And well, Walter Munk of course too was with it from the beginning.

Kroll:

I don't think Walter was in it from the beginning.

Aaserud:

Well, practically speaking wasn't he? After one year or so?

Kroll:

I don't think it's that early.

Aaserud:

OK, I'm going to talk to him on Monday so I will find out. Gordon MacDonald was early.

Kroll:

They're all early, but not from the very beginning.

Aaserud:

No, not from the very very beginning, that's for sure.



Kroll:

Also that means not after one year, either.

Aaserud:

When I'm going to pursue this study, it seems important to have some sense for at least part of what JASON has done in terms of projects in some detail. As a case study, so to speak, would you have any suggestions for what kind of work I could look into? Of course, I can't look into everything, both for practical reasons and for clearance reasons. Is there a set of problems or a problem that would shed especially useful light on the work of JASON during the first 10 to 15 years? That's essentially the period I want to limit myself to.

Kroll:

Well, there were some major studies in JASON relating to the Vietnam War.

Aaserud:

But I don't know how typical they are.

Kroll:

They're not typical. But they're significant. I mean, some of them are referred to in the Pentagon Papers, although there was something called JASON East, which was not really JASON, and which did a Vietnam study, which sometimes JASON's name gets associated with.

Aaserud:

That includes higher level PSAC people and others?


Kroll:

High level people. Yes, that's right.

Aaserud:

Wiesner, Kistiakowsky?

Kroll:

I don't remember who it was any more. I think there was a major JASON Vietnam study in 1964, if I'm not mistaken, and then there were several after that. 

Aaserud:

I don't know easily accessible those are, both from a JASON point of view and from a clearance point of view.

Kroll:

I really wouldn't know. They might not be as inaccessible as you think. There are certainly a lot of titles that you could look up. Anyhow, this style of having things which go on year after year like the Navy studies — well, I don't think that really got started till 1970, so that's something which is very characteristic of the current mode, but which was not so characteristic of that earlier period.

Aaserud:

That was the time when other contractors came in. As I said, I think from the outset ARPA was the only contractor.

Kroll:

Well, if you want confine yourself to the ARPA-IDA period, that's at least a well defined period.

Aaserud:

It is, but I think the transition too is important, because a lot of people have indicated to me that that involved important changes, like different clearance status for different people working with different agencies. The Navy tended to be more strict than other agencies, for example.

Kroll:

Well, I just don't know whether there were compartmentalized clearances before that time. I know we were told when we entered JASON that we were getting across the board need to know, and that there would be no doors closed to us. Now, whether that was true throughout the IDA period, I couldn't really swear. Has there been any discussion about contracts with the CIA in any of your interviews?

Aaserud:

I haven't discussed it, no.

Kroll:



Because when you say ARPA was the only contracting agency, there were ultimately contracts with the CIA.

Aaserud:

At that early period?

Kroll:

I don't remember.

Aaserud:

And the NSA.

Kroll:

I don't know how sensitive that is. In the old days CIA used to not want publicized even the fact that there were contracts that were classified, and that may still be true for all I know. But I suggest you look into that, just to see if that information is available. I mean, I wouldn't pry, but it may be that it's now open.

Aaserud:

JASON contracted with the National Security Agency too, of course, which is not the most open agency in the world either. And of course, there is a danger that if I only concentrate on the most obviously open projects, then that might tilt the conception of JASON, so there are hard questions here.

Kroll:

Now, a major issue was certainly ABM.

Aaserud:

Yes, I've been thinking about that.

Kroll:

Again, that work had a lot of continuity. I was involved with some of that work. That was one of the things I did.

Aaserud:



That shouldn't be too hard to get at, I would think, and that has obvious continuity into the present period too, of course.

Kroll:

Well, I must say that the work that was done undoubtedly had a bearing on the test ban treaty. So I would say that's important work that JASON did. And then in that period that you mention there were these two major laser studies. At the break, the Navy projects started, and they were a continuous and important thing at least throughout the seventies. They may still be important, and probably are as a matter of fact. Then, there are several related projects of a Naval character; it's not all just one thing. The environmental studies are certainly more recent.

Aaserud:

That's more recent. It was exclusively defense I think during the first maybe 15 years; maybe not quite that long but close to anyway.

Kroll:

The Vietnam is certainly another very important thing, so it'll be identified from that period. Navy projects, Vietnam studies, the laser studies, and the ABM studies.

Aaserud:

How related are the ABM and the laser studies?

Kroll:

They're the opposite sides.

Aaserud:

So that would be interesting in it's own right too.

Kroll:

There are probably others. Well, counterinsurgency is another topic. I don't know exactly what auspices the counterinsurgency work was done under. Part of it was Vietnam related.

Aaserud:

I should get a list of the reports. Or a list of the titles of the reports. That would help.



Kroll:

You should have had that before you did these interviews.

Aaserud:

They're hard to get at.

Kroll:

They won't give you a list if you just ask?

Aaserud:

I haven't gotten it yet.

Kroll:

You have asked?

Aaserud:

I have asked, yes.

Kroll:

I see. Nierenberg won't give them to you?

Aaserud:

No. Well, actually, there is an archive at the MITRE Corporation.

Kroll:

Closed archive?

Aaserud:

Yes, exactly. I don't know exactly what is left there now. It's perplexing. So I don't know to what extent I could get that. I'm going up to look at Charles Townes's papers. He has given me access to his papers. And that involves a lot about the origins, of course, but I don't think very much about—well, it could — the early years of JASON. That remains to be seen. So I'm hopeful about that. The Henry Foley papers at Columbia University has some reports in them, but very few, and it's essentially what he's been involved in, of course, so it's not

representative for JASON generally speaking. But that is a serious question. Well, I'll be seeing Nierenberg on Monday and I'm going to pressure him further on that.

Kroll:

Yes, I guess Henry's the only JASON member who exited in that manner.

Aaserud:

There's a lot of interesting things in his archives, but it's not full — that's a problem. What I also hope to do while I'm here — which I also have not succeeded in — is to get a sense of how a JASON meeting is conducted, to get a sense of the atmosphere there. Now, Nierenberg didn't think it appropriate that I show my face in the environment, so I will have to do this indirectly by asking individual members.

Kroll:

Nierenberg is no longer a member of the steering committee, is he?

Aaserud:

Yes. He's chairman until January. Will Happer is taking over in January.

Kroll:

I knew Will was taking over. I thought that had already happened.

Aaserud:

No, no, I think that is decided formally at this meeting and then he enters in January. I haven't been introduced to him. I tried to call him up. Well, he's at Princeton anyway, so I could see him there.

Kroll:

You didn't get invited to the JASON party last night.

Aaserud:

That's right. I was hoping to, but I wasn't. I had some feelers, too.

Kroll:

You tried to get invited.



Aaserud:

It didn't work out. Well, a person tried for me. I didn't try too hard. I didn't put any pressure. Not for the party, but it's a great opportunity for meeting people.

Kroll:

I understand why you wanted to go.

Aaserud:

I don't even know which people are here. I have asked about what people are here on the basis of my own list of members. It would be much more practical just to get a list of who are here.

Kroll:

They won't tell you who's here? I didn't realize there was this resentment.

Aaserud:

No, they didn't give me a list.

Kroll:

I know Bill has been very interested in the history of JASON.

Aaserud:

Well, he may just be too much involved; that he may have too much work and doesn't know what's the basis of my problem.


Kroll:

Well, they are of course very busy.

Aaserud:

But he's taking the time to give me several sessions of interviews about his career. So it's not that.

Kroll:

The atmosphere of JASON is of course very frenetic. There are lots of briefings going on all 

the time, lots of outside visitors.

Aaserud:

Do you have briefings during an introductory week, then work, then writing up of the result — is that how it works?

Kroll:

No, the briefings go on the whole time. People usually sign up for several projects, and they find they can't attend the briefings of all the projects. If they attend all the briefings they're supposed to attend, they can't do any work. And one can't imagine how anything is going to come out of any project. The project directors are frantic.

Aaserud:

It's such a short period for so many things.

Kroll:

They're going to have to spend all this money and they don't see how a report is going to come into being; they're going to have to write it themselves. They often do.

Aaserud:

To what extent are people from the agencies looking over their shoulders, so to speak? The briefings are conducted by the agencies, right?

Kroll:

Yes.

Aaserud:

That's from the Defense Department or the Navy or whatever it might be. Are they kept continually up to date on what's being done while they're here?

Kroll:

Well, visitors come and go. It depends upon actually how interested people in Washington are in what JASON is studying that summer. There is a wrap-up session which is primarily for the sponsors.



Aaserud:

Yes, it's a part of the summer session.

Kroll:

The way that's been conducted has actually varied over the period of JASON. Initially there was a wrap-up at the end of the summer study. People would come out from Washington, and they would come listen to the wrap-up sessions. Then they began moving the wrap-up sessions more towards the beginning of the session, because they found it was very hard to get reports written before the wrap-up session, so the hope was that after the wrap-up session, people would write their reports. Well, of course, then it's harder to wrap up. From year to year the level of the people they sent out would vary. Some years it was much less than others. I would say, it seemed to me to be evolving in the following way. The wrap-up was actually for JASON, so that the different JASONS would know what different JASONS were doing. So there is certainly a wrap-up which is primarily for the JASONS, for which I think the sponsors are certainly invited. They're free to come, but may not come, and then there are subsequent presentation meetings in Washington which take place the following fall. That's the way I think it works now, but I really can't be sure of that.

Aaserud:

Specific question: are there any old timers in any agency that you might think of that happen to be here for a briefing or whatever that it would be particularly useful to talk to?

Kroll:

You want to interview agency types?

Aaserud:

Yes, I do, because I'm interested in the impact question, and I think I would have to approach that side in order to even start answering that question.

Kroll:

I think you'll have to travel to find them.

Aaserud:

Yes, that's fine. I'd like to know about people who not necessarily are here now, but who have played a role throughout a long period.



Kroll:

There's all the heads of ARPA and the heads of DDR&E.

Aaserud:

And IDA to some extent too, I guess, has played a mediating role to some extent.

Kroll:

The heads of DDR&E—Herb York and Johnny Foster.

Aaserud:

Yes, he's in Cleveland. I should get to him.

Kroll:

Bill Perry used to come talk to JASON now and then. I'm having difficulty remembering who the heads of ARPA were. I mean, they're a natural group. But there's a long list of such people.

Aaserud:

Yes, and there are ARPA archives that I should look into too.

Kroll:

Jack Ruina is somebody you should talk to because I think he is both head of ARPA and vice president of IDA.

Aaserud:

He was. I have an appointment with him, and he was there for this crucial early period too, so he's on my list.

Kroll:

But the man who can give you a lot of names is Charlie Townes.

Aaserud:

Unfortunately he is vacationing in South America when I'm in Berkeley.



Kroll:

Townes is worth a trip.

Aaserud:

I'll look at his papers then and then I'll have an even better background for interviewing him. I really look forward to that, and he was very positive to the enterprise. What about differing or similar political views within JASON? Have they played a role at all in formulating projects, choosing projects, discussion of projects?

Kroll:

I think there's a fairly broad spectrum of political views in JASON. I don't think there are any extreme right wingers in JASON. Certainly no extreme left wingers because they'd have trouble getting in.

Aaserud:

Needless to say, yes.

Kroll:

Although there have been some, I would say, quite left leaning people who have been in, who haven't stayed. It was not for clearance reasons. They just didn't like it.

Aaserud:

Yes, and some people are more vocal than others too, of course.

Kroll:

There was a Junior JASON project once. Have you heard of that?

Aaserud:

No.

Kroll:

These were various attempts that were made to recruit young people into JASON — various ways of doing it. But the one which has worked has been to pick out a few names that are known to people already in JASON, and to suggest them and invite them for a summer and then if they like it they may be accepted, if it's mutually agreeable, for an initial

appointment, which runs three years. I think that's the present way it happens. Then later after that they will become regular members.

Aaserud:

When did that start?

Kroll:

Oh, I think maybe Ed Frieman made it happen.

Aaserud:

Mid-seventies or whatever.

Kroll:

Mid-seventies. But there was an earlier experiment in which a number of promising sort of assistant professor types were invited by IDA to spend a summer in Washington, and to work there. A number of people were invited out to do that, but among the people I think were invited to that was Charles Schwartz, and you probably do know about him. There was clearly no very strong discrimination against those kinds of views.

Aaserud:

Yes, he did participate one summer. Maybe that was it. He did participate in a summer session once, I think.

Kroll:

I think it was the Junior JASONS summer session.

Aaserud:

I thought that was in the sixties.

Kroll:

It was in the sixties.

Aaserud:

Yes, because he didn't become that extremely vocal until the Vietnam affair, I suppose. I think that was what triggered his campaign against JASON.



Kroll:

Yes, but it wasn't well separated from Vietnam. I mean, Vietnam had its precursors. There was an involvement in Vietnam. There was of course the Free Speech Movement in Berkeley, but that was a symptom of what had already been going on in Vietnam — it wasn't the beginning, and that was in 1964. I came here in 1962 and Junior JASON was after I came here, probably the summer of 1963. There was already significant Vietnam involvement at that time—obviously, since the Free Speech Movement started in 1964. There was clearly some ferment going on. So it may well be that the failure of that recruitment method is directly related to the fact that it was not timely.

Aaserud:

Yes, yes. So that was just discontinued? It was attempted for one time only?

Kroll:

Yes, I think it was a one time thing; at most two.

Aaserud:

The demand for secrecy, has that afflicted your work as a physicist in any way?

Kroll:

There have been a couple of things that I would have published that I was told I could not publish. I never agreed that they should not have been published.

Aaserud:

Yes, but other than that does it affect your communication with other physicists, for example foreign physicists or outside JASON physicists?

Kroll:

No, but the free electron laser field has become quite classified and I'm sort of out of it now, and I don't like that. I would like to work in it, and I'm not willing to do it on a classified basis. So to the extent that that's happened, I am unhappy with that.

Aaserud:

Because you think it shouldn't be classified?



Kroll:

Well, I think it shouldn't be as classified as it is. I think there are aspects of it which it would be not inappropriate to classify, and which I might work on if I were willing to do that. I don't want to suggest I think there's anything wrong with doing classified work. I just don't want to spend my time on things I can't publish any more. And I don't mind that I did it in the past, either. But I don't want to do it now.

Aaserud:

So your departure doesn't have anything to do with this? What was the background for your departure from JASON?

Kroll:

It was just what I said. Having been in it for 21 years, and being in my sixties.

Aaserud:

Enough is enough?

Kroll:

I don't have enough energy to do as many things as I had to do then, and I decided that I didn't want to spend this part of my life doing classified work.

Aaserud:

Could you say anything about instances of outspokenness by JASONs and the reaction within JASON to that.

Kroll:

What do you have in mind? It's certainly an interesting question.

Aaserud:

Garwin I guess is the most prominent example — the SST discussion. Most JASONs I think have tended to want to keep a low profile. On the other hand, there is always support for people like Garwin who says things that agencies react to, as long as it's not obviously stating or going beyond what he actually is allowed to say. There's a very strong reaction within JASON to any suggestion of an exclusion on political grounds. So I was just asking about this general problem.



Kroll:

You're trying to say something, and you're not trying to say it very clearly.

Aaserud:

OK, the question is, to what extent has such instances of outspokenness affected the discussion within JASON, and affected the cohesiveness, or influenced it in any other way? I don't know if that's clear enough. There's not a hidden agenda behind my question.

Kroll:

I don't understand the question.

Aaserud:

OK. It is a problem, of course, that a JASON person is interviewed by the press, or is in some advisory capacity, and is using material that he has learned in JASON, whether or not he talks about things that are classified or not. I was just asking about that general problem.

Kroll:

Well, I don't know if it's a problem. I think JASON people as a group are much more reliable to keep classified matters to themselves than is typical in the federal government. Leaking classified information for political purposes is a very common occurrence. It's not something that JASONS do, by and large. I mean, I couldn't swear there have never been any instances. I couldn't put my finger on any. To name something that's close to me, there are these famous WALL STREET JOURNAL letters by Lowell Wood, in which he used classified information from the free electron laser program. That's the only way I learned about it, because I was already excluded from classified information. But nobody else could say those things. Even though he said them and they were published, people who were part of a project could not even repeat the things he said, other than quoting him. They couldn't repeat any of these facts. This has had a very negative effect on science. You raised that subject before; I must say something about it. This is a specific instance that I can think of. There is a very high powered microwave free electron laser at Livermore, which is based upon this JASON work. In fact, some details which I formerly thought were classified were reported at the APS meeting last April. It just happens that that technology and that type of device could play an important role in accelerator development. And the fact that it is to some extent classified does interfere with accelerator development. The classification, in my view, is quite unnecessary. And so, this is a negative example, and also an example of the other thing — leaking, selective leaking. It bears on all those subjects.

Aaserud:



But you wouldn't say that any JASON people have crossed that boundary that you know of?

Kroll:

That I know of, that's right.

Aaserud:

The uniqueness, the uniqueness of JASON — from the outset it was a pretty unique organization. It may not be as unique any more. Would you have any comment on that?

Kroll:

I wouldn't know enough to know how unique it is now. It is a vehicle for maintaining the connections between the academic technical community and technical problems of the various government agencies. It's certainly the one I know the most about and it may well be the most successful of the organizations that do that. And it's shown great resiliency, I would say, in the fact that it still exists and still enjoys a certain amount of entree. It's gone up and down in various periods of its history, but the fact that it's still there and still functioning in more or less the same way means that there must be something good about the format.

Aaserud:

Well, the general environment has of course changed since that time. There are more organizations, more institutions now that do similar things. And there are more scientists within government too. So from that point of view it could conceivably turn out to be an anachronism at some point.

Kroll:

You're probably much better equipped to evaluate that than I am, since it's the general subject of your study.

Aaserud:

If I try to relate that specifically to your work in JASON or to work in JASON generally, I would guess that that might have lead from projects to evaluations of other agencies or other people independent of projects, because there are ever more projects done elsewhere. Is that an observation that's valid?

Kroll:



I'm not again quite sure what you're saying, but you've reminded me of some activity in JASON that I haven't mentioned that is probably worth mentioning. Certainly one of the things that JASON is frequently asked to do is evaluate programs, or evaluate proposals, and so those proposals are often presented to JASON in the form of reports written by some other group. In fact, people from that group may come and brief JASON on that, and the main function of JASON is actually to review that proposal and make recommendations — is it something to pursue, is there something wrong with it? That sort of thing. I don't know if that remark has any bearing on your question or not.

Aaserud:

Yes. But has there been an increasing activity to that effect as compared with other kinds of activities in JASON during your tenure for example?

Kroll:

It was variable, I would say.

Aaserud:

There's no systematic trend.

Kroll:

Again, my recollection is related to what I was personally interested in.

Aaserud:

Are there any institutions or organizations that you have been competing with and/or collaborating with on projects, like RAND for example?


Kroll:

Certainly, RAND does that kind of thing, and—well, Bob LeLevier is somebody who's always been a central person. Have you interviewed him?

Aaserud:

No, I haven't.

Kroll:

He's a person who's different from the average JASON in that he's never been part of the academic community. He's always been part of what I would call the consultant technical 

community. I think he was maybe initially at RAND, and he's probably been at a lot of different organizations over that time. So he's a good person to talk to about that, and also about the relationship. But certainly RAND does that kind of work. We were part of SRI which could also be regarded as a rival organization. IDA certainly as an organization does similar kinds of studies, and the only thing that really distinguished JASON from that kind of thing is that it's made up of people with substantial positions in the academic community, whose primary role is in the academic community. Otherwise what they do is quite similar to what those other organizations do.

Aaserud:

Does that affect its work in relation to the other agencies? And in what ways if so?

Kroll:

I would say that it interacts with those, in the same way that it would interact with the technical community that was completely within the government—straightforward interaction, evaluation, etc.

Aaserud:

But they probably do more direct scientific advice. I mean, there's more good scientific argument in JASON than in the other groups.

Kroll:

No, there's a lot of purely technical work in these technical groups, and I told you that some of my naval work was done for Physical Dynamics. It wasn't really that different in character. There was a difference, and that is that the agency had much more control over the direction of the work, and that is one of the things which I think distinguishes work in JASON from work in any of these other agencies. The internal autonomy is much higher in JASON. You can choose what you want to do and also how you actually carry out the work.

Aaserud:

The preponderance of theoretical physicists — do you think that's accidental, or do you think theoretical physicists are particularly suited for JASON types of tasks?

Kroll:

Well, most of the work is theoretical. But there are all kinds of theoretical physicists. And Garwin is not a theorist, he's an experimentalist.



Aaserud:

There are exceptions, of course.

Kroll:

But he truly is very good at doing the kind of theoretical work that's involved in JASON. Nierenberg is an experimental physicist. Happer is an experimental physicist. It's not so rare, as you can see. Peterson is experimental. So you can see actually that some of the most prominent members are experimental. Watson's another experimentalist.

Aaserud:

OK, so you think I'm exaggerating the preponderance.

Kroll:

It's probably true that it's a majority, but preponderance is probably the wrong word.

Aaserud:

I think the originators possibly were theoretical physicists, but that may have changed.

Kroll:

The four original steering committee members were theorists, yes.

Aaserud:

How has JASON worked or succeeded as a training ground for this new generation of physicists?

Kroll:

Well, there are a lot of new people in JASON. The answer is, it's clearly working. If it were not working, the organization would have aged much more rapidly than it has. That is, it would have aged a year per year. I don't know if that's the useful plot you can make — the average age as a function of time.

Aaserud:

But the slope of the curve would certainly be greater than zero.



Kroll:

Yes, it's greater than zero, but it's less than 1. As I said, there are lots and lots of people in it. One always is sensitive to the fact that there are not many young people in it.

Aaserud:

Yes, which it was at one point, of course.

Kroll:

Very young? I don't know. It wasn't very young. I certainly thought of myself as a young physicist. I was 38, however, that's not that young.

Aaserud:

That's right. Zachariasen was pretty young when he joined, but that might be the exception.

Kroll:

Actually Zachariasen may have been a Junior JASON recruit. I mean, I don't think it failed with all of them. Although he may have been going to Los Alamos independently.

Aaserud:

I think he started earlier than that campaign.

Kroll:

That campaign was pretty early. That campaign was in 1963, only three years after JASON started.

Aaserud:

Actually I should ask him that. What about JASON as a springboard for other science policy activities.

Kroll:

Oh, it's very much that. Very many people who were involved in JASON get involved in other kinds of science policy activities. Although there are people who get involved in other science activities not through JASON. I don't know what the avenues have been.



Aaserud:

And the opposite too, I think. When I spoke to Ruderman, he made the distinction between two categories of JASONS — one that used this as a springboard toward further activities, but the other group, in which he include himself, were the physicists who would never have joined JASON, hadn't it been for the fact that they could continue their careers fully as academic scientists outside JASON. So I guess that might be a valid distinction of the kinds of people, and you would probably belong to the latter.

Kroll:

Yes, certainly. Very few people who have been in JASON have used it to do that to the extent that their academic careers disappear. There's not been any real motion of people out of academic life, who were ever in it. There are people in JASON who were never in academic life. I mentioned LeLevier. Actually, Garwin is another one.

Aaserud:

Well, that's one of the things that make JASON particularly interesting for me, because it straddles that divide between academic science and scientific advice.

Kroll:

Also, the university community, and the nongovernmental not in university. Well, of course it works for IBM. It is rather few companies in which one can do this sort of thing. It's probably no accident that there is nobody from Bell Labs. You know they work on very similar problems. It's sort of unique that IBM can have somebody who's a member of JASON, as Garwin is. And LeLevier is sort of the unique example of somebody from the consultant community also. There are many other people one could have imagined being part of it, who are not. It's an uncommon mode.

Aaserud:

So you think that the academic community, or the academic mode of work, is particularly conducive to JASON activities.

Kroll:

Well, no. It's the main point of JASON, actually.

Aaserud:

Yes, it was originally, of course.



Kroll:

In fact, I don't fully understand. Well, I think IBM is a unique kind of company and that's why IBM can have somebody like Garwin. Why it's LeLevier rather than some other member of that consultant community, I really can't say. That's interesting, I really don't know. I think he was a charter member, actually.

Aaserud:

There are lots of questions to pursue here. The final big question, towards the end of the list there, which you didn't answer very optimistically is the impact of JASON. If that can be measured at all, how should one measure or approximate it. What impact has JASON had. Of course it differs a lot for different periods, and for different projects. But generally speaking, which role and impact has it had? Would you have any point of view on that?

Kroll:

Well, it's clear that many JASON projects have had an impact. Whether the impact was decisive or not is very hard to tell. If JASON didn't exist, many of those people would have found other ways to interact with those same projects, possibly. In any event, the projects might have gone on anyway. In the free electron laser work, for example, I think JASON had a major impact. However, I don't think it was so major that if none of us involved in it had existed, the program would be very different from what it is today. I think the things that we invented, other people would have invented within a month or two of when we did it. We may have sped up the process of recognizing certain problems and issues, but I don't think it would have happened without us.

Aaserud:

But what about the mere existence of JASON — the possibility of coming to JASON for that kind of problem? Or would it just have been a matter of going to other places with the same problem and getting the same kind of treatment of it?

Kroll:

JASON didn't invent the free electron laser. The man who did was supported by the Air Force. His own particular interests were not along the directions that we worked, but they would have found other people who would be interested in that, if it were not us, and some of those people would have ended up with similar ideas. In fact, I would say many other people had similar ideas. We were just quicker in getting them developed and in publicizing them. We weren't as quick in publicizing them as we should have been, so we've been scooped there too, but clearly by people we told it to.



Aaserud:

So there was a competition in that sense between different kinds of agencies. I suppose that to answer the question of JASON's impact I would have to at least go into the agencies and see to what extent the advice of JASON has been taken seriously.

Kroll:

Most policy makers, I suspect, will tell you that they shop around for advice until they get the advice they want to hear.

Aaserud:

Yes, if they're that honest.

Kroll:

And that JASON's a great place if it gives them advice they want to hear. That helps them further the policies which they consider to be the appropriate policies.

Aaserud:

Well, that's not the usual self-image of JASON.

Aaserud:

Do you have any recipe for how to answer the question of JASON's impact? It probably would be best to look into the agencies.

Kroll:

I think you certainly should talk to the agencies, but I don't think the question can be answered in general. I think you have to answer it in connection with specific projects. I mean, all of these things have some impact. I commented that my work on the [ceceric?] propagation for example had the impact of preventing some very wasteful programs from being undertaken. Now, would those programs have been undertaken if I hadn't given that advice? I really don't know. But certainly more money would have been spent. Very often, before you undertake a program, you give people money to do expensive experiments and things of that sort, and in this particular case, I think expensive experiments would have been done. So, I don't know how one quantifies an impact of that kind.

Aaserud:

Of course, it's the opposite impact question too of whether the existence of JASON and



JASON kinds of involvement has had any effect on physics and the physics community. That's another question. I don't know if it's a valid one, but it's a possible question to ask.

Kroll:

The question is not well posed from my point of view.

Aaserud:

I mean, whether the involvement in applied physics questions for defense or government has in some way or other entered the academic way of doing physics; if it has some impact on the way physics is done generally.

Kroll:

Well, there's no question about the fact that physics has become more applied than it was when I started, and that the image of applied physics is more positive.

Aaserud:

Not only from your point of view but generally speaking.


Kroll:

I mean, this department at this university for example began with the intention of splitting into a department of pure physics and one of applied physics. People were initially appointed with the understanding that that was what they would eventually do. When the time came to do it, they elected to not do it, and so the department never split. I would say that the department has however become more and more applied. Not only that; the engineering fields are — many of them — so physics-oriented, that they are also just applied physics in themselves. I would say the other two engineering departments here are really applied physics departments also. Computer science has changed that to some extent. I would say that computer science is a branch of engineering which is not applied physics. It's a discipline in its own right. But the others are really not distinguishable, as far as I can see.

Aaserud:

Then of course the question is, can that be connected up with what we've been talking about at all?

Kroll:

Well, I think it certainly is connected with the growth of science and government support of 

science. Also, as science evolves, there are lots of kinds of applied problems that you see how to deal with in a scientific way. There are all kinds of strange problems and very messy problems for example in solid state physics or material science. At the time I entered physics, one would have considered the problems to be boring, and as problems that you couldn't approach scientifically — they involved the specific properties of specific materials that were rather random in character, and it just was not interesting science. I mean, it was George Washington Carver type of science, which is valuable, but not what attracted people to do physics in the first place. That's really changed tremendously, because one now sees how to do all those problems. There's been a tremendous evolution in the appreciation of how to get hold of these very complex situations and see universal aspects of them. Take this great revival of nonlinear science. When I was a graduate student, nonlinear mechanics was applied mathematics, a completely dead subject or a subject for mathematicians. It's changed. And so I would say that a lot of the development of applied physics is a part of that general movement; it has very little to do with JASON, and puts JASON in a comfortable position. It's in concordance with that, but I don't think it's driven it.

Aaserud:

No cause and effect, just a part of a general development. That would be your conclusion.

Kroll:

Yes.

Aaserud:

I have gone through my JASON questions essentially. Of course, we could briefly just wrap up your own career, since we've talked in general terms about JASON — your years at Columbia, your move to San Diego, and your career here. You moved here in —

Kroll:

1962.

Aaserud:

1962. Thank you. What was the background for your decision to change quarters, so to speak?

Kroll:

They asked me.



Aaserud:

They asked you, yes. You don't say no.

Kroll:

Well, I thought about it for quite a while. I was not brought up in New York City. It wasn't my natural habitat. And while I like New York very much, I was not a New Yorker who couldn't imagine living somewhere else. There were things about life in New York which were unattractive to me. They were most apparent to me in the fall, because I would spend my summers outside of New York. I would go back and be re-exposed to the things that I didn't care for about it.

Aaserud:

Not just climate but other things as well.

Kroll:

Just that managing your daily life is complicated, you know. It snows in the winter. I used to park my car in the street and have to change it from side to side every day. Many people have said I left Columbia because I wasn't willing to garage my car. That's not true, but it is a factor. I didn't enjoy that. The fact that you had to get into an elevator every time you wanted to go outside is a minus about New York life. You know, you do something for 20 years, and you feel like doing something else. It's like I quit JASON after 21 years. I believed that a midlife change would be invigorating for me.

Aaserud:

And it was?


Kroll:

Yes, I would say so.

Aaserud:

What was physics like here when you came in 1962?

Kroll:

Well, there was an exciting and attractive idea about this place. They were going to start a new university, recruit all the best people in the world and build this great big department. Well, rather shortly after I came here, it became apparent that the demographics upon 

which this development was based were somewhat awry. Then came the Free Speech Movement, and the student disturbances, and all that stuff, which led to disaffection between the voters and the academic community. It became quite apparent that we weren't going to grow that fast, so the whole character of this place and the development here changed. Starting in 1967 we went into a rather long lean period.

Aaserud:

So it was a disappointment in that respect.

Kroll:

Yes, but I considered that I had been a willing participant in the deception, that I could have seen that it wasn't going to be quite like all those things said, and that I was quite willing to deceive myself for the purpose of justifying in my own mind the change. You know, it's a big thing to go from a major very prestigious institution to a completely new place, and I had to give myself reasons I was willing to do that. So among them was a belief in the vision. And the vision may well come true. It's just a little bit delayed. We're in a big growth stage right now, and we may have another chance.

Aaserud:

So how did your research change as a result, or did it?

Kroll:

It's hard to say. I mean, it continued.

Aaserud:

But you were more able to define your own space, perhaps?

Kroll:

No, I was free to do what I wanted at Columbia. I mean, I worked on different sorts of things here than I worked on at Columbia, but I might have made those changes at Columbia also.

Aaserud:

Students?

Kroll:



Well, I directed the research of many more graduate students at Columbia than I've done since I've been here. I would say by and large the graduate students at Columbia are better than the students here. We had our best students at the beginning. When we had all this publicity, all these great people were coming here. They have not been as good since. This is not for publication!

Aaserud:

Are there any particular contributions that you would point to while here, researchwise or otherwise? Is there something you would mention in particular?

Kroll:

I guess I'd prefer not to. I'll let other people evaluate my work. And the publication list is there.

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