# Computer Oral History Collection, 1969-1973, 1977

**Interviewee:** Mina Rees (1902-1997)
**Interviewer:** Henry Tropp
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TROPP:

With Dr. Mina Rees at the Smithsonian on the 20th of October, 1972. We are going to talk about Von Neumann and how you saw his role in some of the early decision making areas and policies and attitudes.

REES:

I had been associated with Von Neumann before the end of World War II in the whole area of the development of machines, and I relied pretty heavily on his advice about how to go in this area. I tried to get him to write something, but I never succeeded. Of course, a lot of other people tried to get him to write too, but didn't succeed. But after World War II when I went to the Office of Naval Research, Von Neumann was one of my prime advisers in general and, of course, in particular in the area of computing machinery. He made a practice whenever he got to Washington of stopping in to see me, and that was great. I counted on this opportunity to ask him about anything that I didn't understand, and there were many, many things I didn't understand about various areas of mathematics that people were interested in.

You and I were talking a little earlier about Von Neumann, and you said that Von Neumann communicated with people at their own level, and I had real experience within that. I would ask him if there was something in turbulence that I didn't understand, a question. Of course that's something nobody understood then. He would explain it all in his most fluent English and I would say, "Now wait a minute, Johnny. I don't understand that." So then he would step it down a level so that he was talking to me and not just in general. He did this with regard to anything that I queried him about.

In the machine area, the stored program concept and the general arithmetic design of the machine that he and his associates at the Institute were responsible for - it seemed to me quite central, and I think we just counted on that as a significant development for the future. I think most people - everybody I knew who was working in the general area of the development of the machines took opportunities to talk about his ideas with Von Neumann. There was just nothing significant going on that was not influenced by conversations with Von Neumann. Now whether he convinced the people he talked to or not I wouldn't be sure, but certainly nobody ignored him. I would think he played a very

central role and, of course, in the outcome his ideas were heavily represented in the major developments.

TROPP:

Going back to that wartime period, when Dr. Goldstine gave his talk on Tuesday, he recounted that well-known incident when he met Von Neumann on the railroad platform. Apparently Von Neumann was heavily involved in work at Los Alamos at the time and Dr. Goldstine mentioned how he first told him of what was going on in terms of the building of ENIAC. He did this in some detail, and my reaction was I was surprised that a person like Von Neumann, who seemed to know everything that was going on, at that point in 1944 did not know about the building of ENIAC!

REES:

He didn't know anything about the building of ENIAC at that point in 1944?

TROPP:

No. That's the way it comes off in Dr. Goldstine's story.

REES:

That's a surprise to me, because I would — I can't be sure about the dates, of course.

TROPP:

This would be — this is in the spring of '44. Actually Herman gives the exact date in his talk, because he was very careful to check the documents, because almost instantly Von Neumann was there, you know, wanting to really get into this machine and start to use it, and there was then the very famous problem that they were working on, the implosion on a sphere.

REES:

Well, you see this is the part I knew about, and I would have said around 1944 was when I knew about it. It may well be that I knew about it as a sequel to this meeting between Herman and Johnny.

TROPP:

It was a chance kind of thing. They were both waiting for a train and he said that he started off by saying he had the great audacity to interrupt the thougTROPP: of Von Neumann and introduce himself. See, he knew who Von Neumann was, but Von Neumann didn't know who he was.

REES:

Aha. I must have picked this up then immediately afterward, because what I remember was Johnny's enthusiasm for the ENIAC and convi ction as time when on that he could make it more useable which, of course, he did. But I would have said there was no time that Johnny didn't know about the ENIAC.

TROPP:

That's what I would have thought: - because he seemed to be in everything.

REES:

But specifically, he talked to me about the ENIAC with such enthusiasm that I would have expected that he was there at the birth, if you know what I mean.

TROPP:

Right. I did too, and the description that Herman gave and he, you know, remembers it very clearly,

REES:

Oh, I'm sure that Herman has it all —

TROPP:

has documents – right. It surprised me that Von Neumann, who I knew had been doing some problems on the MARK I - the same problem

REES:

Yes.

TROPP:

in fact on the MARK I - didn't know about the construction of ENIAC at that point in time.

REES:

This is extremely interesting, I must say.

TROPP:

In terms of some of the hardware that ended up being funded through the Office of Naval Research, was Von Neumann involved in a consultant's role as these projects were presented to you?

REES:

I don't remember that he was, but this is a misleading statement. He was a great admirer, incidentally, of Eckert. He thought Eckert was a real genius. But I saw Von Neumann so much that I don't - we never had a panel of advisors, like those that the National Science Foundation has, which reviewed all proposals for machines. We had a lot of internal evaluation and consultations with individuals. I saw so much of Von Neumann and of Herman Goldstine and of many other people, I'm sure I talked with them about these things, but I don't remember that any specific project was evaluated by Von Neumann and blessed and, therefore, funded. I do not have that impression.

TROPP:

Do you remember any discussions with him in the postwar period about different attitudes about how machines should be constructed?

REES:

I do not remember any such discussions. Our attitude, of course, in ONR was that we wanted to give any competent people with a good idea a chance to try out the idea, and we did continue the support of one of the analog machines which was built to handle crystallography, because we thought that they had a good idea there and it was an important problem, and that digital machines wouldn't be able to handle it for quite a long time and we ought to go ahead with it. Johnny was entirely sympathetic with that decision, but we didn't ask him about it in advance.

TROPP:

I think probably at that point in time you were right.

REES:

I think so too, I'm convinced that that was a good thing.

TROPP:

It was a long time before this kind of information could be handled in digital form.

REES:

And that machine did much useful work before it was superseded.

TROPP:

Do you remember who did this particular machine?

REES:

It was Ray Pepinsky, who was, I think, at Penn State at the time. He had a very elaborate laboratory there. I remember the dedication of the machine when it was in its sophisticated state brought together crystallographers from all over the world.

TROPP:

Do you remember the name of the machine?

REES:

I don't know whether it had a name. It probably had. It was a cystallographic analyzer, and it was designed by Ray Pepinsky.

TROPP:

Because that's one of the problem areas. You know, we think of astronomy and ballistics tables as two of the prime areas that promulgated so much of the machine interest, but crystallography keeps coming up and it's an area that I think is worth pursuing in terms of its role and impact as a problem area that needed machine assistance.

REES:

Well, it got quite sophisticated assistance from this machine. Ray Pepinsky went to some university in Florida and I think took the machine with him. It did a very useful job for many years. It was a good analog machine.

TROPP:

Who were some of the key people that you remember in ONR during that period, in order to get some names of individuals that I might talk to about that period?

REES:

Do you want me to focus on machines?

TROPP:

Let's focus on both machines and numerical analysis, and you migTROPP: identify the people in terms of their main areas of interest.

REES:

When you say in ONR, do you mean on the staff of ONR?

TROPP:

On the staff of ONR, right.

REES:

Well, there was a computer section of the Mathematical Sciences Division, and C.V.L. Smith was head of the section, and Gene Smith was one of the members of the section. I'm not sure that there was any other person in it. We also had a mechanics section in the Mathematical Sciences Division, and Hal Liebowitz, I think, was head of that section. There was some talk going on between the people in mechanics and C.V.L. Smith.

TROPP:

Well, when you got a proposal in, how did you normally handle it? Say the CALDIC, for example.

REES:

The CALDIC is something I can't say very much about, because I don't remember it.

TROPP:

Oh, oh. Take the Raytheon. Let's take the Raytheon.

REES:

The Raytheon machine we inherited from the Special Devices Center. We never would have proposed that for support. So that —

TROPP:

Yeah. And the SWAC? SWAC came in another environment.

REES:

SWAC came - well, but we did decide to support that, and there I would say that we talked among ourselves and with the Bureau of Standards people and with the people in the field. Now this would be a case where we would have talked with Herman Goldstine and Johnny Von Neumann and some other people, probably Richard Courant or somebody in his shop, because this was early on, and it was after we had made the

decision to establish the Institute for Numerical Analysis, and we had decided on Numerical Analysis as a major thrust. We were, after all, responsible for the development of mathematical results that were of importance to the Navy, and all of us who were concerned with machines — and the two Smiths and I were the principal people in the Office of Naval Research who were concerned with machines — were convinced, and I think this was probably as a result of discussion with the kinds of people that I've been talking about, that there was reasonable danger that we would develop the machinery and not have enough sophisticated knowledge of how to handle it so that we would be able to exploit the machines adequately.

The decision to establish the Institute for Numerical Analysis had that component in it. The decision to build a machine out there seemed to us to be justified at that point in history because the Bureau was demonstrating, or had demonstrated - I don't know the chronology exactly - that it could put together something that worked when it built the SEAC in the East, and the decision that we should have a machine at UCLA, at the Institute for Numerical Analysis, seemed to us to be sound, because we wanted to develop the numerical processes so they would be ready to exploit machines when they were on the market in a large way. And the people there just had to have a way of trying things out on the machine.

TROPP:

It's interesting, because if one looks at the chronology and this meeting I attended was called the Twenty-fifth Anniversary of SIAM and SIGNAM, really of numerical analysis in the sense that it's about twenty-five years ago that we think of numerical analysis as beginning to be born as an independent area of mathematical research.

REES:

Well, of course there was numerical analysis in the early days.

TROPP:

Right, it's been around for, you know, a number of centuries.

REES:

But, of course, the problems became different when you were talking about the kind of fast operations that even then we envisaged. Now I don't know that we envisaged the kind of speeds that we have attained though you'll find that there are writings that indicate that we expected them. But it was a different magnitude of problem. I mentioned Courant earlier, because the whole exploration of the use of the machines for the solution of differential equations introduced mathematical problems that were simply different in kind from the ones that had been introduced in the earlier application of the Hollerith machines.

TROPP:

That's right, particularly when suddenly you were faced with the problem of second order partial differential equations, and this introduced a whole new area.

REES:

And then there was much work that went on on rounding errors and the impact of this on numerical analysis. Johnny was interested in that and I guess that's a paper that does exist, isn't it? And, of course, a number of other people were interested in that.

TROPP:

Right, and it was a direct outgrowth of the early papers on matrix inversion and they knew they could run into very serious rounding errors of a serious magnitude.

REES:

Yes, serious enough to invalidate the results actually. So it was a recognition of the appearance of vital problems in major areas of mathematical concern important to the Navy, that made us reach an internal decision taken on the basis of a good deal of exploration, that this was a basic thrust that we had to support, that the development of the machines would simply be without meaning unless they had the necessary mathematics developed. Now as you probably know, but you may be too young to know it, there simply wasn't very much interest among mathematicians in this kind of problem.

TROPP:

I remember that very well.

REES:

We searched for people who did have an interest. At Courant's - well, it wasn't called the Institute at that time - the Mathematics Department of NYU - there were people who were interested. I actually collaborated on a paper with Eugene Isaacson and Courant on the differential equation problems. Among the astronomers there was a certain amount of interest. At that time there were just a handful of astronomers: Brouwer at Yale, and a man in the Midwest whose name I can't recall, and maybe one or two others who were working on trajectory problems. It simply wasn't the vogue for astronomers to work on that kind of problem. With the coming of NASA and their concern about trajectories there's been a lot of thrust in this direction. But when I was looking among astronomers I found these two, and particularly Brouwer, and we supported him, which seemed very strange to some people for us to pick up an astronomer in that way, but we did quite significant things in the development of methods for astronomy that could be used on computers.

TROPP:

Was Menzel at Harvard ever involved in that area or was he interested in other things?

REES:

No, he was interested in other things primarily. He was entirely sympathetic to the area, but he didn't do any work that I remember.

TROPP:

He mentioned in our conversation one day that related to your idea about applied mathematicians interested in these areas, that he could find only one sympathetic ear in the Mathematics Department at Harvard, and that was Birkhoff.

REES:

Mhm. The senior Birkhoff, you mean?

TROPP:

As you say — right — it just wasn't vogue, and nobody cared.

REES:

That's right. Now Birkhoff, of course, was a great man, like Von Neumann, and therefore —

TROPP:

There was no other person that he could talk to in the Mathematics Department about his difficulties and get some advice, but there was nobody else who was even interested in what was going on in the observatory.

REES:

Yes. Well, incidentally and quite peripherally, my contact with a couple of astronomers at that time established my interest in seeing that astronomy should get better support. Later on with the Geophysics Section of the Office of Naval Research we started the support of astronomy with the help of the American Astronomical Society, and we had a fantastic program going for practically no money. That was the beginning of government support of astronomy, which has now leapt beyond the limits of all possible support. But this was just a little project.

TROPP:

That was the beginning of what I remember as being the major geophysical year. That was kind of a peak.

REES:

But, of course, they build the big machines with government support now, and there're still big machines and big efforts going on. Well, that's off the point.

We were committed to develop methods of numerical analysis that would enable the Navy to study its problems and, of course, would advance the general scientific uses of the machines. As time went on, it became clear that machines were going to be possibly even more important in business applications than in scientific applications, but we did not undertake specifically to develop methods there. I think they probably have been developed adequately by industrial companies or will be. I don't know whether anything is adequate up to this point, but the Institute for Numerical Analysis became the sort of main pure effort that we had. That was the big thing that we were supporting with the expectation of developing methods of numerical analysis. We had a lot of other things going, of course. As I mentioned earlier, the Courant Institute, which was not called that at the time, was doing work in the development of numerical methods for the solution of differential equations. Some of this was going on at the Institute too, though the main effort at least —

TROPP:

Weren't there some things going on at the various atomic energy installations because of problems that they had?

REES:

Yes. Those were not supported by ONR.

TROPP:

Right. But this did give some of the thrust to the development of numerical analysis.

REES:

Well, that had been going on at Los Alamos even during the war.

TROPP:

And then at Oak Ridge later on.

REES:

Yes, Oak Ridge became an extremely important locus of work in the field.

TROPP:

But in terms of the Institute for Numerical Analysis, with the mathematical community more interested in the pure mathematics as opposed to the kinds of problems that you were talking about, how were you able to generate interest and get staff at Los Angeles?

REES:

Well, you mentioned a major thrust of the mathematical community. But one of the people we got — two of the people we got were the Lehmers; and their concern, their interest in the use of the machines, was in support of number theory.

TROPP:

Right. Which they are still doing.

REES:

which was thoroughly acceptable. But you see here were a couple of very good pure mathematicians, thoroughly acceptable to the mathematical community, who wanted to use the machines. By this device we lured people in who then became interested in other possibilities. Another person who, of course, became one of the directors of INA, was C.B. Tompkins, who was a very good pure mathematician who had been lured away during the war and —

TROPP:

You might tell me something about him, because his name is very important and it keeps coming up. Of course he's not alive for me to talk to, so it might be worth digressing for a moment to talk about Tompkins.

REES:

Well, he was one of Marston Morse's students and had proved an important theorem with Morse, and had established his potential before he became a Naval officer in the Second World War. And it was because — you see, the war came along too soon for him to have established himself strongly, but this was such an important theorem that everybody expected a lot of him, and he was lured away. During World War II he worked on cryptanalysis and I guess that's about as much as I'm allowed to say at this point on that subject, but did some very important things there. Now that was an area where the machine had much to contribute, so that he became quite sophisticated in the kind of applied mathematics that is relevant to the cryptanalytic problems. This meant that he

was using the full machinery of parts of pure mathematics as well as numerical analysis associated with it.

I hope I haven't talked about something that's classified. This sounds to me like the kind of thing that's all right.

TROPP:

No, this is all right. As long as we don't get into any details. [Laugh]

REES:

All right.

TROPP:

We know who the people were who were involved and these — in fact some of it's in various public, you know, publications like *The Codebreaker*, names do show up.

REES:

All right, so I haven't said anything that I'm not allowed to say. You might as well delete that part of it. Tommy Tompkins was his, his "code" name. He turned up very soon after I went to the Office of Naval Research. I found him a fascinating mathematician. I hadn't any idea who he was or why he was there. It turned out that he knew a great deal about the status of machines and the work of Engineering Research Associates, which was later incorporated into —

TROPP:

Right, because they had done some of the early machines for the cryptanalytic people.

REES:

It turned out also that ONR had relationships with them that I hadn't found in the records. So that Tommy was a very important person in this whole development. I would say that he was also one of the people I talked to a great deal about the development of the program. He was both a very able mathematician and a person who understood machines and their uses. It was as a result of his work during World War II that he was diverted from pure mathematics to numerical analysis. He was also one of the key respectable mathematicians who were active in the numerical analysis game. Abe Taub was another.

TROPP:

That's another I'd like to have you talk about, because he's rather difficult to talk to today about that period.

REES:

I can't remember where I picked him up. I knew him as a mathematician - oh, I know, yes. During World War II I was Technical Aide in the Applied Mathematics panel; he was Technical Aide in one of the divisions of OSRD, and I'm not sure what the division was. I think it was explosives, but I'm not sure. He and I had a good deal of contact in that connection. We talked about the mathematical problems there and we often met in Washington and elsewhere in connection with the problems that we were both involved in, so that he and I became very well acquainted both personally and mathematically. I'm not sure where he went after World War II. Do you know?

TROPP:

He was close to Von Neumann —

REES:

He was close to Von Neumann.

TROPP:

and for a period, I think, he might have been involved in the ENIAC project. I know he was at the Institute level and from the Institute he went to Illinois, where he was the only mathematician that I know of, of stature anyway, who was connected with the ILLIAC, which was in a sense, a first major copy of the Institute machine.

REES:

Yes, and he was a very important element in their decision to go that way.

TROPP:

That's right and the fact that they had him there made it possible for them to go the way they decided to go.

REES:

Well, my remembrance is that I picked him up at the Institute after World War II. I'm not sure of that. In any case, he certainly knew what was going on with the Institute machine, and when he went to Illinois he provided the flow of information from the Von Neumann and Goldstine Project to Illinois. Now it was somewhat later when the Navy was confronted with the problem of cooperating on the protection of the continent, the whole

Dewline development - there were also some Navy ship aspects of that and I think that's as much as I should say - but I recognized that we were going to have to develop some Navy machines and I turned to Illinois where the combination of Abe Taub on the mathematical side and a very, very sophisticated engineering group was very important. As you know, Illinois has continued to develop very significantly.

TROPP:

RigTROPP:. They're now up to ILLIAC IV, which is currently in operation.

REES:

And they're doing some very important things with PLATO and various kinds of educational projects.

TROPP:

How about somebody like Barkley Rosser, whom again I don't know very much about.

REES:

Berkley Rosser, now let's see. I'm trying to remember. Was he at Aberdeen during the war?

TROPP:

I think so. I think he was part of that group with Veblen and others.

REES:

Yes. Well, you asked earlier on, how did we break into the respectable mathematical community and the generic answer to that is, we identified respectable mathematicians who were interested in machines. And we've mentioned, of course, Von Neumann,

TROPP:

The Lehmers.

REES:

the Lehmers, Tompkins, Taub, and Rosser. Rosser was one of our sort of star identifications. He was a logician as well as other things, so that he was sort of easy prey on the design of the machines, and he was also something quite important in this connection, and you'll get this from John Curtiss, I guess. John Curtiss had been at Cornell and Barkley and he were fast friends, so it was very natural for John to suggest

that we try to lure Barkley into the scheme of things. Barkley is a very good mathematician and so he just was natural prey. We invited him. Everybody wants to go to UCLA, and we invited him and he accepted.

TROPP:

I guess in terms of the Institute and listening to Alston Householder this week, I got the idea that during its few years of existence, depending on who was there or who was the current head of it, each year they kind of had a different emphasis.

REES:

Sure, but we were quite deliberate about it, getting different kinds of people there.

TROPP:

Yes, that's what I wanted to ask you about. It was that deliberate rotation of people and areas to concentrate on.

REES:

Well, of course we didn't tell them, "You must concentrate on this, but it's inevitable that a man does the things —

TROPP:

It turned out that this was what happened. That was deliberate on the part of your office?

REES:

Yes. John Curtiss was heavily involved in this. This was not our decision alone. He was running it, as a matter of fact, (laugh) so let's admit that he was —

TROPP:

Right. He ended up being the Director when apparently you couldn't find anybody, so he just went out and —

REES:

Yes, but we did get some very good people. We tried to get them from different parts of pure mathematics. Of course I've said this before. I'm just convinced that all parts of mathematics have potential applications. So I still think this was a good idea. Of course Alston was there and had a very great impact on what happened.

TROPP:

He's so modest that I think it would even help … if you would talk about Alston as you saw him. I knew him by reputation very early in my own career.

REES:

How did you come to know about him?

TROPP:

As a young mathematics professor and somebody suddenly telling me that undergraduates ought to be introduced to numerical analysis, they ought to have a course in it, and I'm talking about the late fifties. And I thought, numerical analysis, that's not an area that I ever studied. And at that point in time when I started looking at the literature I found out that one of the most prominent names associated with that phrase was Alston Householder.

REES:

Well, that's right, and I think that the war did this to him really. He had the potential interest, but as you know he became involved in atomic energy matters and we were all responsive to the demands of the time, and when John and I were looking for somebody who was just a natural for the development of numerical analysis, no one was more natural than Alston Householder. He had a very great impact on the development of the field and, of course, has carried on significantly.

TROPP:

To me in 1959, the phrase numerical analysis and Alston Householder were almost synonymous.

REES:

Well, I think that's pretty limited.

TROPP:

I had a limited viewpoint, you know, when you asked me the question. I had never even heard of this as an independent area of mathematics. You did applied things in various courses and you had a course called applied mathematics for physicists and engineers, this kind of thing, which was more applied differential equations.

REES:

Yes, but that was on differential equations.

TROPP:

And to suddenly realize that out of this wartime environment a new independent branch of mathematics had suddenly blossomed. It was brand new to me.

REES:

Well, as I said earlier on, this was really responsive to the clear need. Those of us who were responsible for basic policy in the support of mathematics identified this as a field where there was no question that stimulation was needed in order to use the machines. Now as you know, many of the machines are used in unsophisticated ways, but even there, there are some important developments.

TROPP:

I think the point that I was making is, something that was new to me then and still doesn't happen very often, that suddenly out of an area of mathematics that's immersed in certain other branches, something evolves that becomes independent in and of itself. The fact that you founded an Institute for Numerical Analysis indicates an early realization of the independent intellectual area of research, where it really didn't exist before. I mean, sure, we can trace things back to Gauss and Euler and, you know, every major person worked in particular aspect of what we now call numerical analysis, but as an independent area of study it really didn't exist.

REES:

Well, that's right but then, of course, computers didn't either.

TROPP:

Computers didn't either. Computer science is another example of an independent area that just evolved within our recent lifetime, but to me numerical analysis was unique because it was my first experience with the blossoming of a brand-new branch of mathematics.

REES:

Well, of course, Alston wrote that book; which is very decisive in making an educational institution recognize that a field exists.

TROPP:

Right and so if somebody's going to teach a course the first thing you do is, "Where can I find a book?"

REES:

Yes.

TROPP:

The only book that anybody knew of at that point in time was Alston Householder's

REES:

Yes.

TROPP:

and this is why the two have become synonymous for me.

REES:

Of course one thing that's important about him is that he moved easily from a university to a government laboratory, and he was acceptable in both worlds; and relatively few people satisfy that requirement.

TROPP:

How did the mathematical community, even in, say, the UCLA environment, react to the establishment of an Institute of this limited a nature, as it was in 1947, '48, '49?

REES:

Well, we didn't — it wasn't established that early, was it? What was the date of the establishment of the Institute for Numerical Analysis?

TROPP:

Probably '48.

REES:

Well, the mathematicians there were glad to have it. This was partly because we brought very good mathematicians onto the campus to be associated with it. I think the Lehmers came early on and any mathematics department wants more good mathematicians around, so this was just great. It also gave an opportunity for students to get jobs and any mathematics department that's giving a Ph.D. needs jobs for students, so there were many attractive features. It also gave them an opportunity and many of them took it, to work during the summer on the numerical analysis type of thing. Later on I established, and

actually became Director for one summer, another institute out there during the summer that was classified, but used numerical analysis and a number of the people there were involved in that, so that the general thrust of applications in mathematics became very acceptable on the UCLA campus. UCLA when we first went there had some good mathematicians, but it was not one of the great mathematics departments in the United States, so the chance to bloom and become more distinguished was also very welcome.

TROPP:

How did you get interested in applied mathematics?

REES:

It was World War II, and I think this was true of most of the people. Well, of course the Lehmers are different because the Lehmers were on their own thing and, of course, Dick was at Aberdeen during the war and he knew the possibilities of the machines more than other people would have known. Of course he was a very varied kind of person. But during World War II any of us who became involved had to work in applied fields, so that meant that we were all doing numerical things. Now most of the people went back to their old game after the war, but in this classified project that I mentioned, I got quite a number of them to come and spend the summer, so that you had the same kind of group of people working on a special problem where application was needed.

TROPP:

Do you remember — maybe this is a postwar attitude — what Veblen's attitude was towards machines in general?

REES:

I don't believe I can answer that question in any significant way. I never remember discussing them with Veblen at all. By the time World War II was over, Veblen was an elder statesman and I don't remember having any discussion of this problem with him.

TROPP:

People who were close to him don't remember having them either. One gentleman recounted a comment that Veblen's wife made about machines in Veblen's presence, but he doesn't remember Veblen either agreeing or disagreeing. [Laugh] So he didn't have any — he didn't know whether that was a repetition or whether that was her own thing.

REES:

I don't have any reason to think that Veblen cared one way or the other.

TROPP:

I would gather that having somebody like Veblen at Aberdeen also, somebody of that stature, helped attract —

REES:

Oh, there's no question, people went to Aberdeen — I mean, mathematicians were glad to be there because Veblen was there, there's no question about that. So in that regard he certainly had an impact.

TROPP:

It's interesting when I look at the origin of ONR, you know, the concept in some of the early documents as to how it was created and what its role was to be, that you got involved in the machines per se, but also, as you indicated, felt that it was more important to concentrate the energies on things like numerical analysis. In terms of the machines themselves, you indicated you didn't feel this was as important a thrust as the other.

REES:

Well, it isn't that I didn't feel it was important. It was after all, basic. And we did support machines but, of course, our budget was not adequate to have a vast program of machine support. We supported the Mathematics Division of the Bureau of Standards where there was numerical analysis going on too; but we did support, I'm pretty sure - now this I'm not reliable about - I have an impression we did support the SEAC as well as SWAC - but, of course, we always supported the Institute for Advanced Study machine and we inherited these very expensive machines - the Whirlwind and the Raytheon machine from the Special Devices Center. I have an impression we also gave some support to Illinois. Now whether that was on numerical analysis or a machine, I think we gave support there.

TROPP:

I would guess from what I heard about the size of their initial budget, I think Louis Ridenour managed to get a hundred thousand dollars originally. They must have had some outside support, because that machine cost a lot more than that.

REES:

Oh yes, they did have, and of course as I said earlier, we turned to them when we were trying to get some qualified people to attack some problems for the Navy, and that budget would not have been ONR budget. That was a big Navy budget, and that would have been lots and lots of money. They had lots of money from the Navy, but not from the ONR.

TROPP:

I just threw in another name that I'd like you to talk about who's no longer around. That's Louis Ridenour, who seems to be an important figure in a variety of areas. Do you have any recall on him that you would want to insert at this point?

REES:

Well, let's see. Louis turned up actively in Washington as the Chief Scientist to the Air Force. Now I can't recall — where was he before then?

TROPP:

I don't know. I know he ended up at Illinois.

REES:

Yes, but he came —

TROPP:

But I can't go back any further.

REES:

I think maybe he was at Pennsylvania, but that's aside from the point. He became the Chief Scientist to the Air Force and in that connection I worked with him on a number of projects. I remember one occasion specifically when he and General somebody and I went up to visit Whirlwind. Now Louis, of course, had close contacts also with Johnny. We went up to MIT and we visited not only Whirlwind, which he had some misgivings about, but there was an Air Force establishment which I think has only recently been discontinued, in Cambridge across the street from MIT. What was that called?

TROPP:

I think it was called the Cambridge Air Force Facility.

REES:

Yes. We spent a day there having them review their —

TROPP:

Because they funded a number of things that went on in different places also.

REES:

Yes, and he and I had a good deal to do with one another in connection with projects which were either jointly supported by the Air Force and ONR or were of interest to both of them.

TROPP:

That's an interesting thing that I didn't know about, that ONR got into joint support of things with other groups. How did that occur, because at that time I remember the serious rivalry between the Service branches?

REES:

There was not any real rivalry at the research support level.

Actually, I suppose the most important component of the answer to your question has to do with the work of George Danzig in the Air Force. After we had had conversations at a meeting of the Applied Mathematics Advisory Committee at the Bureau of Standards, he came over to ONR one day with one of his colleagues and told us about some mathematical results he'd gotten that he thought would be of interest in naval logistics and we had an extended discussion with him. We became convinced that his results were really very important. Indeed, their importance is now hard to overestimate. As you know, this was a result, namely linear programming, closely related to game theory. After looking into it, becoming convinced that it was really important, I made an appointment to see the Deputy Chief of Naval Operations for Logistics to tell him that I thought there was a result here that was of some interest and the Navy ought to know about it. He was quite excited about it and asked me if I would arrange what's called a presentation. Do you have presentations here?

TROPP:

No, but I know what you mean by it.

REES:

So I arranged a presentation at which all the Rear Admirals in the Navy were present because the Deputy Chief was a Vice Admiral and he had notified them this was going to happen. And I had George Dantzig and his colleagues from the Air Force present their results and explain - I explained and some other people in ONR explained - the ways in which we thought this was of significance to the Navy. And it was as a result of that - you see it really did come from a purely mathematical common interest of George Dantzig and the people on my staff - that the whole thing started. It was as a result of that, that we set up the Logistics Research Project which is, I think, still going and which I heard

recently was being very useful now because now that the budget's been cut so badly they have to reduce the amount of their purchases, and they can do it with this device.

TROPP:

That's an interesting story, because I was strictly a civilian at that point and only reading the newspapers about, you know, the various rivalries between the services for budgets and who was going to be responsible for missiles and aircraft and all this sort of thing.

REES:

But you're talking big stuff now.

TROPP:

Right. Right, but I'm interested in this point that you just made about how you had this interaction on an intellectual level.

REES:

Well, we ha it. I think this is true of all the services, and the Bureau of Standards which is also in Washington.

TROPP:

We think of Government bureaucracies as duplicating each other and nobody talks to anybody.

REES:

Well, actually we exchanged information about all projects supported and when the National Science Foundation came into existence they joined the game, and when the National Science Foundation happened they actually requested people to tell — and the other agencies did, too — to tell where else they were submitting proposals, so that you have full information about that, and you don't have double support, that kind of thing. We worked that out while I was here. But we always had meetings at which we talked about our various programs and sometimes we did work out joint support. Sometimes we gave the project to one agency rather than the other because the weight of interest would be there, but then we always got reports; they were distributed.

TROPP:

That's really interesting to me that you could be involved - I mean for example somebody could submit a proposal to you to do a research project and when you begin to see the results of this research you realize that it may be of interest to you but somebody over in

a totally different area is really looking for something like this. They're dependent upon your ability to communicate with them as to how they can apply the results of this research.

REES:

Yeah. I think that may overstate the case. We were always open though. The point is, the information was always available and was distributed. I don't think we worked that hard if that project was also of interest to us, we would take it. And, of course, the main thrust of the ONR program was in the quality rather than the field. But there were things like the logistics thing - you see, the Logistics Research Project had many ramifications. A1 Tucker at Princeton became involved in pursuing the mathematical aspects of this thing and Lefshetz had some connections with it, so that we had, we developed areas of mathematics that probably wouldn't have been developed.

TROPP:

Well, of course Dantzig's is a prime example of the kind of minimax searches that were then possible in operations research analysis, game theory.

REES:

Well, a whole sophisticated piece of mathematics grew out of this thing, and I don't think that George needed us particularly to recognize that it was important mathematics, but we did support the Princeton project, you see, where Al Tucker and his colleagues developed some quite important things, and we established the Navy's Logistics Research Project. C.B. Tompkins was involved in that, too. You see this was an area that he really knew about, the mathematical area that George was working in.

TROPP:

See, as a mathematician I'm interested as I look, for example, at current research areas, and I wonder what their origins were, I have a feeling that many of them originated out of these applied project topics. Machines, for example, the current emphasis on, from a purely abstract point of view, on constructive analysis has been rejuvenated because of the machine and its implication.

REES:

Johnny has a paper on that. He talked about the feed from one field, the applied to theoretical and vice versa.

TROPP:

I don't know that paper. Do you have a reference or approximate date or journal?

REES:

I think it's included in

TROPP:

It would be in the *Collected Works*.

REES:

Newman's *World of Mathematics*.

["The Mathematician" from *Works of the Mind*, ed. by Heywood and Nef. Chicago: University of Chicago Press, Reprinted in vol. 4, pp. 2053–2063 of *The World of Mathematics*, ed. by James R. Newman. New York: Simon and Schuster. Ed.]

Take a look in there; I think there's something there.

TROPP:

I'll look in there. I own that; I'll look there, because constructive analysis strikes me as an obvious one that everybody knows about, but I wonder about things like, as you look at category theory and you look at some of the abstract work on mathematical words and some of the theorems - I mean purely abstract that I'm sure have applied areas of origin.

REES:

I think the effort to use machines for machine translation stimulated some of the work in mathematical linguistics.

TROPP:

I'm sure you're right there.

REES:

Now the extent to which that happened I don't feel I'm competent to judge.

TROPP:

Now, of course, in the whole area of what we call the history of mathematics, nobody has really done much since, say, 1800. We haven't really looked at contemporary mathematics in terms of some of the branches that have become prominent in the last decade. It's still possible to go back and search the origins if we can just find people who are interested.

REES:

The physicists are doing that, of course, and it's a very good thing to do.

TROPP:

We're not. We're not doing it. As far as I know, Ken May and a few people at Toronto are the only ones who have said we're going to concentrate on this. Now recently people like Dieudonne have become interested in the role in contemporary mathematics that history plays in seaching out important areas of future research, in terms of looking at the origins and where the really significant problems have come from.

REES:

Yes. On this subject, Lefschetz became interested in work in differential equations, which was really outside his original commitment, as a result of concern with nonlinear problems. Minorsky had this work in nonlinear theory and I talked at some length with Lefschetz, and he set up a project out of which Bellman came as a Ph.D.

TROPP:

I didn't know that. That's interesting. Bellman was the author of a paper that turned me on, and it's a paper he did in the early fifties.

REES:

Bellman is the most conspicuous person but, of course, it was another concern that we had. Nonlinear problems were really essential for any consideration at all of Navy problems. I talked to Lefschetz about it and he became interested and he really became a sponsor of a major development in this field. He had Minorsky over visiting. There was a great effort in this [field].

TROPP:

When you first joined ONR, from a philosophical point of view, how did you see the role that it was going to play in future development of mathematics or mathematical applications?

REES:

Well, I was lured into this after World War II by Jim Wakelin, whose name is still about.

TROPP:

Yes, I've met Jim.

REES:

He called on me in New York. I had known him, of course, in OS —

TROPP:

OS —

REES:

OSRD days. He told me of their plans to set up Office of Naval Research. The project as I saw it from Jim's description was to provide funding for basic research, which the war had demonstrated was needed in the United States, and which obviously needed funding in all the experimental sciences. Of course in my earlier days I had heard gripes from the chemists about how they couldn't get any money, and so on. I could see that the experimental sciences needed money. They needed equipment and supplies and obviously they needed money, but he wanted to know whether I thought mathematicians would be interested in this. My net conclusion was that I couldn't see what on earth that had to do with mathematics. That was the end of that.

Then the next thing that happened was that I went on my first vacation after the war. I had a letter from Alan Waterman telling me that Jim Wakelin had suggested that he ask me to become head of the mathematics branch of the Office of Naval Research. I don't know; perhaps I got that wrong: I think it was before we went away on that holiday that I got this letter. And I came down to Washington and talked with him and decided it sounded sort of interesting, but I didn't know what it really meant and then I went and talked with Warren Weaver, who of course, was the man I relied on most thoroughly, and Richard Courant. These were the two people I had been most closely associated with during the war. And they both urged me to try it and I didn't feel I had anything to lose. It sounded sort of interesting so I decided to come.

When I got here - did I tell you this before? It sounds so repetitive to me.

TROPP:

No, no.

REES:

I got here in '46, I think. Oh, I went away then for my first holiday after the war, and I had the letter from Alan Waterman asking me to report on a certain day; that's what I remembered. I came down here and got a room at the old Lafayette Hotel. I don't —

TROPP:

Think it's still in existence? No, I don't think so.

REES:

I think they tore it down. Yes, there's a big building there now. I started to work and started to try to look for an apartment, but nobody would take my name, even to put on a list. So I made a practice of staying at the Lafayette as long as they'd let me, and then when they put me out I went on a trip. They let me stay about two weeks; it was the only hotel in town that let you stay that long, so every two weeks I went on a trip. I went around the United States talking to all the leading mathematicians at the major universities, asking them what was the nature of the program that they thought would make sense. And, you know, I gradually evolved some ideas of my own. Out of these discussions I formulated a philosophy of operation which essentially focused on two things. You see, I was trying to find out, why did you give financial support to mathematics? Essentially it was time for mathematicians to do mathematics; and people - we needed more trained mathematicians. So those were the things that I was trying to buy for the people in mathematics who were doing interesting mathematics.

When I came here, Don Spencer had a project supported by ONR, the only one outside of analog machines. What I was trying to do was to interest mathematicians primarily at that point in analysis, because it seemed to me that the Navy could see the need for the development of additional analysis, including numerical analysis, and statistics, clearly a field that was underdeveloped and of interest to the Navy, and timid little steps in pure mathematics if somehow or other you could say this might be of significance to the Navy. But I was agonizing all the time, because I didn't know how far I could go in abstract algebra and things like that. The Captain who was really responsible for the Office of Naval Research - there was an Admiral who was credited with it —

TROPP:

Was this Captain Condon?

REES:

Captain Conrad.

TROPP:

Oh, Conrad. That's right.

REES:

The heads of branches met once a week and talked about their programs. Incidentally, two of the other people who were heads of programs at that early time were Manny Piore in Electronics and Roger Revelle in Oceanography. There were others, Randy Roberts

and so on, but these two and I always seemed to be getting into discussions at these meetings where we got together. Well, Captain Conrad knew I was agonizing about the problem of support in pure mathematics. I used to work quite late. I was still, you know, living at the hotel and going on trips. And I was working quite late one day and he stopped in and said, "Mina, if you want to support pure mathematics, I'll support you." And so that was when we took off in mathematics. It was one of these things that you got mostly by osmosis, but in these discussions I would mention things that I'd like to do.

So we were really supporting mathematics as mathematics, and that's what I understood I came for. But the Office of Naval Research had one component which was called the Naval Science Division or something closely approximating that, which was staffed by Naval officers, and we met once a week with the Naval officers and, of course, we got acquainted with them, had coffee with them, and they talked about their problems. And pretty soon you were exposed to an atmosphere where they'd bring somebody over from Butler who would tell you about something that was worrying him, or you'd hit something like that result that was applicable to logistics and you thought, gee, the Navy ought to use this - pretty soon you were working with a flow back and forth between Navy needs and mathematics and other fields. You were always free to go, to support mathematics just because it was good mathematics, but you were always looking for the possibility that they could use it too.

TROPP:

So you really had kind of an invisible university going?

REES:

Oh, yes. I was Dean of Graduate Studies of the university. The Navy decided after - I don't know what year this was - that it wanted some of its Naval officers to get Ph.D.'s, and I was designated to receive these guys when they came from active duty, to indoctrinate them in the ways of a graduate school, and send them forth to their university. They would spend, I don't know, a month or so in my bailiwick. The first time they came in they would say, "Yes sir," and then they got used to the idea.

TROPP:

But your little environment was kind of like an invisible university,

REES:

Oh, there's no question there.

TROPP:

of corridor kind of discussions as to what people were doing and how they could be used and what the problems were.

REES:

It really was a great intellectual environment. When I went to be Dean at Hunter in '53, I remember that the thing that I liked best there was that it wasn't all science. I could talk with humanists, social scientists — the only social science, if it is that, that was in the Office of Naval Research was the psychology branch, and I had lots to do with the psychologists, because I didn't believe they were doing anything serious.

TROPP:

[Chuckle] That's still a matter of opinion.

REES:

But in any case, your time was spent very significantly on substantive questions. It was a very, very interesting experience.

TROPP:

It sounds exciting.

You raised an area that rather curiously has not come up in my discussions with people, and that's the area of mathematical statistics. I can still remember conversations with people like Jerzy Neyman about specific projects that he was on. I remember one very vividly, I think it was with the Health Department here in Washington in which he did a statistical study on epidemic. This would have been about ten or twelve years ago, and he described this in some detail and later wrote a paper describing it. I was really fascinated with the way he had analyzed the spread of an epidemic and the percentage of a population you had to inoculate in order to minimize the impact, with the knowledge that the epidemic had to run its course. You couldn't stop it once it started; it was going to run its course. And, because I'm not a statistician, I'm trying to think of the state of — the mathematical state of statistics in this immediate postwar period and the role that it played in these developments that we've been talking about.

REES:

Mathematical statistics had barely begun in this country before World War II.

TROPP:

That's right.

REES:

We had three statistics centers in the Applied Mathematics Panel. Jerzy Neyman had one; Sam Wilks at Princeton had one; and Allen Wallis, who's an economist, had one. Actually, Hotelling was head of the project but he served primarily as a consultant on difficult statistical questions.

TROPP:

Where was that third one?

REES:

That third one was at Columbia. It included Allen Wallis and Milton Friedman, the economist.

TROPP:

That's right. Milton Friedman, I know.

REES:

They were the day-to-day leaders of it. Now they were much more concerned with applied statistics, but it was out of that project that Abraham Wald's sampling techniques came. They actually were used in connection with the atomic bomb's fuse, the most trying episode on the project and top secret. But Sam Wilks, of course, had a program in mathematical statistics at Princeton and Jerzy Neyman had a program, and still had a program, in mathematical statistics at Berkeley. It was clear in World War II that statistics is a very, very important field of applications, so that ONR made a practice of supporting good statisticians any place they were. We had a project at Cornell that included Kac and probability.

TROPP:

Iowa State would have been another promising site.

REES:

We had a project at Iowa State. Mood and Brown were there at the time. Obviously we had one at Berkeley and one at Princeton.

TROPP:

I think it would be interesting to trace the growth of the level of the sophistication of mathematical statistics in terms of the support and attitudes towards it in that post-World War II period.

REES:

Well, of course, Columbia had been up and down on statistics and I wouldn't be able to tell you at the moment whether it thinks it's up or down. Herb Robbins has been there and left there and come back and so on.

TROPP:

I'm thinking in terms of that immediate five or seven years after the war.

REES:

Immediately after the war we were very active in encouraging people to go into, to be serious about it and so the people at places I talked about just now all had programs that were very good, including Columbia.

TROPP:

You know it's rather interesting, we think of the machine applications in terms of solving large systems of equations or systems of linear differential equations or partial differential equations, doing approximations, but I think back to the days when the statisticians were about the only ones who used the old Marchants and stuff to do just the hard arithmetic they had to, to get their correlation coefficients and all the, the —

REES:

Of course they still are doing this.

TROPP:

Right, and I wondered if there was any pressure from them as a group to get machines in order to do some of their work.

REES:

I think I cannot identify any such pressure. I think that so far as I know, and I wouldn't say that I am really informed now, they had never felt that they needed sophisticated new mathematical techniques. They're still doing correlation coefficients —

TROPP:

Even modeling? I think of the —

REES:

The modeling, after all when they model they're not using statistics. They're using other mathematics and then you get into the other question, and that part is different.

TROPP:

Right. In terms of the early simulation, some of the early modeling, do you remember any particularly exciting early projects?

REES:

Hm. That's one I think I'd have to give some thought to because, of course, this was an important field and I'm sure there was something, but I can't remember it.

TROPP:

I've read something - I can't remember whether it was Captain Conrad's writings or Condon's writings.

REES:

Ed Condon?

TROPP:

Ed Condon, on the sort of philosophical ideas underlying the formation of ONR. I think I've told you before, I find them fascinating. They ought to be republished, they're really —

REES:

That sounds as thought it would have to be Captain Conrad. Where did you read them?

TROPP:

I'll show them to you in the office after our talk. Jim Wakelin has given me some of his material from that period and I'll show them to you.

REES:

Yes, well that sounds like —

TROPP:

These are immediate post-World War II periods and the germs for a National Science Foundation are in those writings, the early —

REES:

Oh, no question. Of course, the Navy had the Office of Research and Inventions during the war, which was the liaison office between the Navy and OSRD. Captain Conrad and Jim Wakelin and so one were in this. When Van Bush wrote *Science: The Endless Frontier*, everybody thought in terms of a National Science Foundation. Of course, Truman vetoed the bill and so on and the Navy, which had become convinced in ORI of the importance of supporting basic research, decided that they weren't going to take any chances and they plunged in and established the Office of Naval Research. They really were thinking of it as a holding operation for the National Science Foundation.

TROPP:

Right, as I saw it looked like an embryonic National Science Foundation.

REES:

There's no question they were thinking of that. The same thing happened to the whole office, I think, that happened to me. Once you got into the game of working in the field and living with the Navy, you found that there was an inevitable relationship and so you just responded to it. I was amazed at my loyalty to the Navy. I really became very much interested. Of course I was appalled initially at the idea of working for a military establishment. Of course I never expected to stay very long, and most people didn't expect to stay very long.

TROPP:

You still had the kind of wartime attitude even though the war was over.

REES:

Yes, that's rigTROPP:.

TROPP:

This was duration plus kind of thing.

REES:

But the thing that you mentioned earlier was something that took, that gripped you. You see, it was so interesting. You were dealing with such significant ideas that it sort of won you over.

TROPP:

I guess it would be great to digress here and talk about Bush, as you remember him, reflections, any anecdotes, conversations.

REES:

There must be endless anecdotes about Bush, but I don't think I am a good source for them. Bush was - and is - I haven't seen him very recently but - just didn't deal in trivia. Very interesting guy. He had ideas and they were always - of course a number of the people who were involved with OSRD were like that. He was sort of intellectually fearless. He was perfectly prepared to cope with anything and to take people on in discussion or argument. I didn't know him as well as I knew Warren Weaver, whom I knew very well. They seemed like the same kind of person to me, and I guess I'm extrapolating from my judgment about Warren. What I'm saying sounds as though you're talking about an abstract ideal, you see, but I know that in the case of Warren, he also was intellectually fearless and prepared to cope and, you know, was just —

TROPP:

New ideas were the stimulus [to] both.

REES:

He was perfectly prepared to say he didn't understand something, but he was also prepared to try to understand it and these two whom I knew - I knew Warren well, of course, and still do; and Bush moderately well. I never knew Conant very well, but I think that he probably operated in much the same way. But these were great men, they were great guys, and there were others in OSRD. How they managed to get so many great men in that operation is hard to imagine.

TROPP:

During that period wasn't Alan Waterman kind of Bush's representative in Europe, or at least in England?

REES:

Asia too.

TROPP:

Asia too?

REES:

Late in the war he went to Asia. But Alan was an entirely different kind of person from the ones we are talking about. Alan approached things much more softly. These people didn't have a soft hand at all.

TROPP:

But they left their imprint.

REES:

Oh, I think there's no question at all.

TROPP:

I think we're very fortunate today because of the imprint of their philosophy and attitudes in things that we've done since World War II in the intellectual areas in terms of basic research that were just not part and parcel of the scene before then.

REES:

That's right. There was a real discontinuity.

TROPP:

I feel like right now we could use three more like that, to kind of rejuvenate that feeling and attitude, because we seem to have gone away from it.

REES:

Well, the world has changed, of course.

TROPP:

Oh, yes, and you learn to live in a different environment, but you sort of long for the days when basic research almost for its own sake was an important piece of our day to day intellectual existence.

REES:

I think it still is. There are a lot of points to be made in this connection.

TROPP:

I think the emphasis has been more on, how can we apply or concentrate our energies on things that will have a near term impact?

REES:

At this point, I haven't been on the National Science Board for two years, and so I'm not full of statistics. My impression is that we're still putting as much support, money, into basic research as we ever did. We're also putting money into applied research. Now the difficulty with this speculation is that it may be right and it may still be true that we're supporting it at a lower level because the inflationary factor cuts down on the support. But there still is very substantial support of basic research.

TROPP:

Of course, one of the things that's changed the picture is that during the period you're talking about when you had the need for more and more trained people - you needed more Ph.D.'s in all of these areas - and certain funding had to go into producing those.

REES:

That's right.

TROPP:

We had no choice. Today we suddenly find ourselves faced with a surplus. Maybe it's a temporary surplus, but it's there.

REES:

Yes, well that's what I meant when I said the world has changed.

TROPP:

Right.

REES:

You do respond to the facts in the situation.

TROPP:

Right. And it's clear there's no need to double the number of Ph.D.'s in a given field when you have more than enough, but in 1950 or 1955 it was clear that we were going to have to substantially increase the rate of production.

REES:

That's right.

TROPP:

of people capable of doing basic research.

REES:

And you see, going back to the concern with numerical analysis and indeed with the building of machines, one result of financing research in the field of numerical analysis and supporting the building of machines was that a lot of people got Ph.D.'s in numerical analysis and carried on, or in connection in engineering schools, got them in connection with building machines, so that you have an entirely different attitude in the engineering schools now and have a new field which most engineering schools are training people to participate in.

TROPP:

I think the whole growth of what we now call engineering science is a result of this.

REES:

Well, I think there are some other components operating there. Of course one thing that happened during World War II was that the new fields like electronic engineering and some others to a lesser extent drew on people with Ph.D.'s in physics rather than on engineers to build fields, so that you had a number of physicists trained - people trained as physicists - who went into engineering and made the field much more science oriented than it had been before. Then engineering science took over.

TROPP:

Of course with the kind of world we're living in, the idea that an engineer should be a broader individual than just the very limited kind of, say, civil engineer and 900 hours of just civil engineering courses. The fact that he's living in a different environment has also —

REES:

Yes, well, there are a number of forces operating, but certainly what happened during World War II had its impact on schools of engineering as well as on the universities.

TROPP:

In terms of the early machine I think by 1955, and I can check the paper that you've given me, there were something on the order of 40 or so computers that were either built and useable or in the process of design and building, some of which came to fruition, some of which didn't. And the majority of these were as a result of service-oriented needs or contracts or support, in terms of ONR, in terms of the Air Force, in terms of the Navy itself. Had there not been this interest, do you think the machine would have developed in the way that it did or would it have just taken a little bit longer?

REES:

Incidentally, that paper won't give you the kind of information that you refer to because I was focusing, if I remember correctly, on the machines that were commercially available, because I think part of the answer to your question is that the way in which the developments went was very largely determined by the commercial availability of machines.

TROPP:

Ultimately, yes.

REES:

And when the machines began to be put into everything, you had insurance companies developing their techniques, and banks developing their techniques, and there was a big flurry in my world when the first airplane company ordered a machine for the control of reservations. These things had to happen when the machines were available to those users. Now some of the things that we've been talking about would have developed in connection with little machines, available just to the services and so on. I haven't followed the development of machines on naval vessels and, you know, control among the whole [lot].

TROPP:

A whole new field now is to have naval vessels designed by machines.

REES:

Yes. Well, of course, we were talking about that kind of thing way back then. I don't know anything about what may have happened.

TROPP:

But that's fairly recent.

REES:

But that was in the talk stage. We dreamed of these things.

TROPP:

You dreamed of a machine being able to design a cruiser, a battleship?

REES:

Yes. One thing that is rather fun is to get a record of your dreams back there and check them against what happened. We were pretty good, I think.

TROPP:

I think so. As I look at the predictions, I think people who thought they were making wild predictions turned out to be very conservative.

REES:

Yes. Of course, Perry Crawford used to be convinced that that would happen. I told him he was nuts! I'm modest.

TROPP:

I think Perry still thinks big.

REES:

Yes. See, he was associated with the dreams for project Whirlwind, and he and Forrester were very stimulating to one another. They both thought bigger and bigger and bigger all the time.

TROPP:

His M.A. thesis I find kind of a far-out thing in its day.

REES:

Oh, really? What is it about?

TROPP:

I can't remember the title right now, but it's leading into areas of machine that I didn't think people were really worried about seriously for another five or seven years. It seemed to be way out. It's a 1942 thesis. I've forgotten the title at the moment, but I remember when I heard of it I couldn't believe it, so I asked Perry for a copy, that somebody had actually written a thesis on this topic as early as 1942.

REES:

Perry was never particularly tied down by limitations of theoretical possibility or the state of the art or anything like that. He let his imagination wander. At one point I remember he came in to ask me about, what was this business about logic and machines? He heard about this after he decided what could be done.

TROPP:

I seems that that started off as kind of a West Coast implementation, the use of Boolean algebra to design a total machine.

REES:

Yeah, I guess so, but I guess you never can quite write off Johnny, because remember Johnny was a logician and he certainly did do some designing of machines. He may not have established a school as the West Coast people did, but he certainly used logic in the process. So did Barkley Rosser. See, Barkley had a good deal of an impact on that.

TROPP:

Is Barkley Rosser somebody I should talk to?

REES:

Yes, he was an important person.

TROPP:

Who would you suggest as some of the other people in this whole environment that you considered very exciting, that you would recommend that I talk to.

REES:

I guess we've probably mentioned everybody. You've talked to the Lehmer's, I gather.

TROPP:

I just saw them. He's a Visiting Professor at Northern Illinois currently, and showed me his current number-theoretic research. His wife had left the room so I didn't talk to her. She was off doing something, but I did get a chance to talk to him.

REES:

Did you talk to Peter Lax at all?

TROPP:

Peter was at the meeting and I went to talk to him, but fortunately he's nearby, so I can get to him.

REES:

Yes, because he's an extremely interesting person in this development. Friedrichs, of course, is a very important theoretical part of the development, but Peter has not only done important mathematics, but he has actually worked on the development of things for the machine and with the machine, so that you've got somebody here who understands this.

TROPP:

Knowing that he's at NYU and having met him for the first time, I do plan to see him. He just looks like a very nice person to talk to, very interesting.

REES:

He's a delightful man but - we were on this subject earlier about the pure mathematics and how it relates to the machine developments, but he's the most significant mathematician at the Courant Institute who has indeed really worked, particularly on the Atomic Energy Commission's [machine].

TROPP:

He just doesn't look old enough to go back that far. I looked at him and I thought, he can't be that young!

REES:

Of course he isn't as old as Friedrichs, but he's one of this generation who has worked in both fields. I think he's the most interesting person at NYU. If others there have done it, I don't know about it.

TROPP:

I guess another thing, and I'm going to really push your recall at the moment, when you talked about your early residence in Washington and going off on trips to talk to prominent mathematicians in major departments, who were some of the people that you felt helped you the most? Who were some of the key people that you talked to, that you felt would be of interest?

REES:

I remember particularly Griffith Evans. Now I don't know whether that name means anything to you. … He was Head of the Department at Berkeley. In fact, that whole building is named after him.

TROPP:

That's right, Evans Hall is named for him.

REES:

Yes, and he was particularly important because he, like Marston Morse and Veblen and Birkhoff - the older Birkhoff, were the establishment, you know the pure establishment, no question at all what they said mathematicians listened to. So he was an extremely important person in that scheme of things. While I was there I also talked to Neyman whom I knew well - well, I knew both of these people well from the OSRD days. Allendoerfer, I can't remember where he was at the time. I thought of him because I was mentally traveling back —

TROPP:

Allendoerfer, many years ago when I was talking to him at the University of Washington, mentioned an involvement during the war, operations research analysis of something related to the Battle of Midway. I often meant to go back to Carlin and now talk to him in detail, about how they plotted that battle on a single piece of graph paper.

REES:

Well, you know, one of the things we did during the war was to identify and train mathematicians to go work with the Air Force on operations research. A lot of people passed through our hands in that connection. Now I don't remember where he was immediately after World War II. I'm not sure; I think he had gone to Washington.

TROPP:

I think so, but I'm not sure either. He's been there so long it's hard to identify him with any place else.

REES:

Let's see, at that point I did not talk with anybody at UCLA. Stanford: Al Bowker was there and I had known him during the war too. And, let's see, who was there? Oh, Polya and Szego.

TROPP:

Polya and Szego. Yes. How about the University of Chicago at that point?

REES:

Oh yes, Adrian Albert and Saunders MacLane, definitely. Was Saunders there then? I'm not sure.

TROPP:

I think so.

REES:

Gus Hedlund at Yale. Garrett Birkhoff at Harvard; and some other people at Harvard. At this point I can't remember who was at Harvard in those early days. You know that's changed quite a lot. Some of the people at Michigan; I can't remember the name of the most obvious of them.

TROPP:

Michigan would have been a center at that point.

REES:

Yes, and I can't remember.

TROPP:

The time element, because I know people about a decade later.

REES:

Yes, well that's the trouble. People have changed and you've been in touch with them and it's hard to remember who were there, but Michigan was one of my stops. Virginia was one of my stops. Whyburn was there, and I think Jimmy McShane was there too.

TROPP:

Had he come there by then?

REES:

I think so. I'm not absolutely sure, but Jimmy McShane is somebody I talked to. Now whether he was there or at Tulane I don't know, but you know, you think of the big universities and I just — Wisconsin, Langer was at Wisconsin.

TROPP:

That's right.

REES:

And I just went to all of them, and by the time I really got organized they were all ready to play. Don Spencer was at Stanford, I think, at that moment. He moved back and forth, but since he was one who had a contract, you see, I had talked to him.

TROPP:

I guess it would be interesting to get into the archives of INA and perhaps - Lehmer was going to show me some of his papers - to see the correspondence between people - not the formal documents, but the correspondence between people as they communicate their interests and willingness to do certain things, or unwillingness to get involved.

REES:

John Curtiss is a man who thinks big - not quite like Perry Crawford. He really went after the best people and he and I talked a good deal. Since we were both in Washington we had a good deal of opportunity to talk and plan together, but it was his show and we were supporting it, so I had an interest and he had an interest.

TROPP:

Is there any chance that your official - not official in the sense of formal - but that your correspondence during the ONR period is still in existence someplace, buried in some archive?

REES:

Well, I think it's very likely that it's in an archive because, you know, when you leave Washington, you leave it.

TROPP:

That would be a fascinating thing to read through in terms of your travels, talking to people, and eventually formulating a working operation.

REES:

I'm not one who keeps diaries and things. I've just discovered that I failed to write a letter I should have in connection with this trip. My correspondence isn't very good because I minimize it. John's correspondence, if you can get at that at the Bureau of Standards, which is much more likely I would think than the Navy - John always talked and wrote a lot and I didn't.

TROPP:

That would shed a lot of light on the attitudes of the mathematical community at that point in time.

REES:

Well, the thing about John is that he dreamed of how - for example, I'm sure he's the one who identified Barkley Rosser as a possibility. He knew him well, of course, but he thought of him as a very able mathematician and he was reasonably sure that we could lure him into this field. He would, you know, be unabashed at approaching the most improbable people.

TROPP:

How about foreign mathematicians?

REES:

That we worked on, of course. As we said at lunch, with regard to the development of computers, we were conscientious. I went to Europe and visited projects there. I don't think John did that, but I told you I enlisted UN support to try to get the French to work a little harder at math and more successfully. On the actual building of machines I think we were pretty conscientious, went to Germany —

TROPP:

In the Scandinavian countries where there seem to have been some early machines that are not too well documented in our literature, who were some of the key people that you were —

REES:

I did not go to Scandinavia and I was not aware of important developments there.

TROPP:

But in England you were seeing all the major —

REES:

In England I saw all the major people, and in Germany.

TROPP:

How about Canada?

REES:

Canada I didn't have contact with. That was a strange omission, but I just didn't have any contacts, you know, except with casual people.

TROPP:

Because there was an interest at the University of Toronto. That's written up in these Newsletters, their machine that they called UTEC, which they ultimately abandoned when they bought the Ferranti, but they had an interesting research approach that they never saw through to the end.

REES:

I'm trying to remember, I did go to Ottawa one time, and I think it must have been in connection with a machine; but I can't remember what it was about.

TROPP:

Of course, in terms of the atomic energy relationship they had a similar problem, because they had their atomic energy needs out at Chalk River. So they had the same kind of machine problems on a smaller scale that we were faced with here.

REES:

Well, I saw Canadians in that connection, but in terms of machine developments, I don't know about. Now in terms of mathematics, the Office of Naval Research had an office in London. The London office kept in touch with developments there so when I did go I worked out of the London office and I knew where I was going.

TROPP:

There are a couple of documents that I have seen that do survey developments in Europe and in England, that come out of the London office, and I have been referred to others that are in existence that I haven't seen as yet. But I guess if I visited London I migTROPP: have access to some of those documents.

REES:

They did a very systematic job. You could really keep track of what was going on. Incidentally, Joe Weyl —

TROPP:

How do you spell that?

REES:

W-e-y-l, who became Head of the Mathematics Branch of the Mathematical Science Division when I became Division Chief - he was a member of my staff early on, and incidentally he's a son of Herman Weyl - he at one point was in the London Office. He became Chief Scientist at the Navy before he left the Navy, so he knows more about ONR than anybody. He's at Hunter College …

TROPP:

Oh, great, then he's close by.

REES:

Yes. He spent at least a year and I think maybe two years in the London office and if you want to get information about the London office I think you can get everything from him.

TROPP:

Very good.

REES:

When he left the Navy he went over to become Assistant to the President of the National Academy of Sciences and in that connection he wrote a report on some sort of information exchange. He's very sophisticated about the whole development of computers. He wasn't as involved with them initially as I was, but he knows more about them than I do.

TROPP:

He's at Hunter at the moment?

REES:

Yes.

TROPP:

Very good. I'll make it a point to see him. For some reason I hadn't associated his name with Herman. I don't know why; the spelling is identical.

REES:

Yes.

TROPP:

Well, this has been really great. I may be back bothering you some more as I think of more questions or get more information.

**[End of Interview]**