Computer Oral History Collection, 1969-1973, 1977

Interviewee: Mina Rees (1902-1997)
Interviewer: Henry Tropp
Date: September 14, 1972
Repository: Archives Center, National Museum of American History

TROPP:

This is a discussion with Dr. Mina Rees in her office on the 14th of September, and I am going to let Dr. Rees talk primarily about her memories of the ONR or even the wartime period on some of the topics that we broadly touched on connected with INA and some of the machines.

Suppose we start with INA. Suppose we start with conceptually why people felt that you ought to have something like that branch of the Bureau of Standards on the West Coast?

REES:

Difficult question to answer. John Curtiss was a builder and he and I had agreed that one of the prime needs if the development of computers was to be exploited adequately was for a much greater development of the necessary mathematical technique to exploit the uses of the computer. This was an idea that took a while to take hold, but we felt that the chances of getting mathematicians actively interested in developing the needed mathematical insights would be greatly enhanced if there were a place where groups of mathematicians could be brought together to exchange ideas and stimulate one another and work with the computer and see what the interplay between mathematics and the computer would be, and over a period of time we decided that this was really a good idea; I mean, if we intended to assist in the developing uses of the computers in a significant way we ought to start an Institute for Numerical Analysis.

Now the decision to have it on the West Coast was a decision that we reached after visiting a number of different institutions all over the United States which were interested in building an Institute for Numerical Analysis.

I think now rather with amusement of our feeling that the West Coast was somewhat underprivileged in —

TROPP & REES:

[Laughter]

REES:

in this kind of development, but we did go to major universities all over the United States and it was after the visits to various places and an assessment of the degree of interest and the degree of involvement that the various universities were willing to undertake that we decided that the University of California at Los Angeles had the best chance of doing a - the kind of thing that we saw as needed, and I would think that we spent at least a year making that decision.

TROPP:

Say, you had a natural candidate here at NYU in terms of the people who were already there surrounding Richard Courant.

REES:

Yes, that's true. The NYU people had a program and at that point there was not very much interest and it came considerably later in the development of the numerical analysis and with the scope of mathematical development that we felt was needed for the computer. It did go later at NYU.

TROPP:

Is it safe to say that the major germs for this came out of your wartime experience in working with groups of mathematicians who were suddenly forced to solve problems outside of their abstract areas of interest, who were forced to communicate with people in different fields, and the fact that numerical analysis as a discipline was really getting its origins in terms of looking at some of the problems during the wartime period?

REES:

I think that's a big overstatement. I think that my commitment, which has lasted till now - and is a much broader one than the one which you mentioned - toward developing the uses of science as well as pure science and not looking down the nose at the applied phases of science - and I'm talking not only in mathematics - that that was developed during the wartime. Actually, the kind of numerical analysis that we did during the war was so much less sophisticated than what we saw as needed for the computer that I, though there undoubtedly is a connection, I think that your statement is an overstatement.

During the war the thing that later became the Bureau of Standards — or maybe during the war, became the Bureau of Standards Numerical Analysis Project, under Arnold Lowan, was under my general supervision in the Applied Mathematics Panel of the National Defense Research Committee.

And as I remember it, it was during the wartime period - but I'm not sure of that, it can obviously be checked - that the transfer was made to the Bureau of Standards. I am not sure of that, but I was directly responsible for supervising that thing and what they were

doing was just building tables and they were building tables using card punch IBM equipment, and it was just a different animal entirely from anything developed later.

TROPP:

Is this the Mathematical Tables Project here in New York, was that a wartime project that you were in charge of?

REES:

Well, it was a wartime project, but I believe if I - if my memory serves me - that it was a WPA project originally.

TROPP:

It started before the war then?

REES:

Yes. This was a way to put mathematicians to work during the WPA period, and then when the war came we recognized that we needed just lots of Bessel functions mostly - though there were some other functions - that were used all over the Office of Scientific Research and Development and computers were just not available

TROPP:

Was this one of the reasons that the MARK I at Harvard spent so much of its time computing these tables and Bessel functions?

REES:

Yes. Bessel functions were needed and they were needed extensively. The Mathematical Tables Project had printed just books and books of them actually but, yes, they were needed, and the Harvard machine would usually.

TROPP:

What do you see as the main accomplishments of the Institute of Numerical Analysis?

REES:

I think that I am not in a good position to give the answer to that, because I lost track of it in its rather early years and I don't really know.

TROPP:

Well, you know, it really was the sort of a forerunner of the idea that we still take for granted of mathematicians not only being there, and some of them permanently, but the concept of having a place that would attract people who would come for varying periods of time, and this was rather a new thing in our peacetime scene, wasn't it?

REES:

Yes. I think that was an outcome of discussions between John Curtiss and me, and one reason that we chose Southern California was that we thought that that was the place where we could get people to do that. Now the - what is it called - the Institute at Wisconsin - the Army Research - Mathematics Research Institute which had its troubles during the students' uprisings some years later built on the same concept and tried to exploit the same attractiveness at having Army work done in a university. I was strongly opposed to that at that time, and I had no foresight- I don't claim any foresight of what was going to happen later - but it just did not seem to me the right way to go about that problem, but it did seem to be the right: way to go about the development of solid mathematics.

TROPP:

It's clear now, in retrospect, that numerical analysis needed heavy funding and heavy involvement by the mathematical community as the computers were going to be able to now deal with problems involving partial differential equations. The whole problem of round-off errors, the computational problems involved, the —

REES:

Well, those are the problems that we did identify early on. I think that there are developments in these that we did not identify.

Actually, I personally did some work at the Courant Institute on that kind of thing, but those were the ones that we saw during the war and that led us to believe that there would be others.

TROPP:

There's an interesting question that I think is an area of research all by itself and it's kind of a circular thing and that is the one direction of the impact of the computer on the development of numerical analysis. But there is also the converse of that and it's kind of an open-ended question, and most people seem to think that's nil, but it's still, I think, worth asking. Is - did the evolution of areas in the branch of numerical analysis have any impact on how machines were designed and how they were built, how they were - and some that even might have not come into existence otherwise?

In your experience do you see any impact in that particular direction?

REES:

I glimpse it vaguely. I know that Von Neumann was influenced by considerations like this, but I can't be specific. You know I can vaguely recall conversations, but I can't be specific.

TROPP:

Can you recall conversations with Von Neumann on any aspects during the latter wartime period or in the immediate post-war period where you felt that he was really throwing out some major new ideas? Conceptual aspects that hadn't really been talked about before?

REES:

No, I don't think that I can recall anything that — you know, he had a decisive impact on the development of computers, but everybody knows that, and I don't recall anything specific that is different from the major thrust of his whole design of the machine.

TROPP:

When you talk about trying to encourage him to write what he was doing and what he was thinking about, you don't remember any particular areas that you were concerned with during this period?

REES:

No. I think that was not in this general area. I know completely irrelevant things about wanting to get him to put something down about the whole problem of turbulence for example, on the TV. He had some ideas.

TROPP:

Right. That's still an open-ended problem. [Laughter]

REES:

I could not get - I have - I think I may still have a draft of a paper I got out of him; but that's as far as that ever got. But Von Neumann wasn't writing; he was thinking, talking, getting other people …

TROPP:

I am trying to remember his name now, the meteorologist who went to the Institute with him to work on the weather problems, the gentleman at MIT. … I can't remember his name. I just saw him a few weeks ago. Well, we got to talking about this whole

turbulence problem - where the current level of it is because it's still not much further along.

REES:

It's an extraordinarily difficult problem, but, as I say, Von Neumann did have some things but he didn't have them finished and he wasn't writing, and he was doing so many things and talking to so many different people all the time that he just didn't get to finish that.

TROPP:

Let's jump ahead since we're going to be limited on time to the formation of the Courant Institute. How did you see that going from an informal kind of gathering of mathematicians around Courant into the formation of an Institute that was very different than the one in Los Angeles?

REES:

I think that that question implies there is some sort of relationship between them and I don't think there really is. Courant had built the Institute at Goettingen and I think he never had any doubts that he was going to have an Institute, but of course when he first came, NYU was underfinanced - as it is indeed now. He was given a home without any real support. He was the most extraordinary entrepreneur and succeeded in getting support from various sources, and during World War II when he had one of the mathematics groups under the aegis of the Applied Mathematics Panel, he did very significant work; but unlike the other groups, his group focused on solutions of problems that were needed, but also on the development of theory that was needed. So that during World War II his group became extremely powerful in the study of continuum problems. The shock-wave book that he and Friedrichs published was a very important manifestation of the development of theory and then Stoker became very active there so that you had a very powerful group of mathematicians interested in the kinds of mathematics that were of interest to those who wanted to apply them.

They don't like to call themselves applied mathematicians. They are mathematicians, but their work is applicable, and when I went to the Office of Naval Research to establish a basic - support a basic mathematics in the United States with naval support - the NYU group was clearly one that had great power and that the Navy was interested in encouraging so that the Navy over the years gave that group increasing support and Courant with his extraordinary abilities managed to attract to the group very, very powerful mathematicians and to build a very powerful educational environment.

Now, I don't even remember the exact date when the Courant Institute was born. I know that when I came back to New York in 1953 I became a member of an Advisory Group that Courant established and I believe that it was at that point that the group became known as the Courant Institute. But I am not absolutely sure.

TROPP:

They had been funded by ONR earlier?

REES:

They had been funded by ONR and — they were one of the first groups funded by ONR — and as the years went by, additional funding agencies in Washington supported them. The Air Force became interested in supporting some of their work and the Army, I believe, too. So that they diversified their support, but always with Federal funds and when the National Science Foundation was born they began funding the Courant Institute so that the, the — I was talking with one of the senior men there yesterday and he told me they fund themselves completely still, except for the maintenance of the building, which as you know is the - Hall, which was funded by the Sloane Foundation. I guess I should say primarily by the Sloane Foundation, I believe there are some other funds in there, government funds as far as that goes. They have a budget of their own of five million dollars of which two hundred thousand comes from the University. I don't know if that's the kind of thing that ought to be injected, but in any case they are still funded almost exclusively by outside sources.

TROPP:

Were you still with ONR when the decision was made to have UCLA take over the Institute for Numerical Analysis?

REES:

Take over is the wrong word.

TROPP:

Or become a part of.

REES:

Establish. This didn't exist.

TROPP:

No, I'm sorry, in the —

REES:

Oh, you mean when taken over from the Bureau of Standards?

TROPP:

From the Bureau of Standards.

REES:

I think I had left ONR at that time. I am not sure when it was, but I'm almost sure it was still part of the Bureau of Standards and still supported by the Office of Naval Research.

TROPP:

Some of the other broad questions are related to both the Bureau and ONR and additives, but I guess what I 'm looking for is the, kind of the environment in which some of these decisions were made. For example, the decision for the Bureau of Standards to take on some of these functions. How were decisions made to build their own machines?

REES:

To take on some of what functions?

TROPP:

Well, the functions of the serving as a source for computational problems, which at that time nobody really knew what they were going to be. They did in a sense, build their own computer, so people could bring problems to them.

REES:

Well, largely - well, first of all Ed Condon, who was Director of the Bureau, had established the mathematics division and got John Curtiss to go there as its Director. Now there was a policy decision, which I assume, and John can tell you better than I, was the Director's decision, of building a resource, a mathematical resource was needed.

When I went to Washington, John immediately approached me to try to get the Navy to accept some of the responsibility for supporting mathematical activity there, and it was because he was seeking ONR financial support that he and I talked particularly about programs.

Now John Curtiss was a real builder and he was the one who saw the possibility of making an extremely significant mathematics division if it had active working computers. He was the one who got the Census Bureau to ask the Bureau of Standards to take responsibility. He was the one who got me involved in working with him in trying to get the development of computers. And he decided that the thing wasn't going fast enough, and that the thing to do — he and his colleagues, and I was in on the discussions, decided

that they should take what was known and put it together and get something operated and SEAC was built. [Accompanied by table pounding]

It was just because he was so impatient with the way things were going.

TROPP:

And he was a mover.

REES:

He was a dynamic guy, that's all. And out of our many discussions, of course I was involved on behalf of the Navy, because the Navy was supporting a great many of these - through the Office of Naval Research supporting a great many of these developments, and I was also involved in wanting to get something going and I think he and I in joint discussions probably developed the attitude that you just had to have the mathematics going as well as the machine.

I think it was mostly attributable at a whole energy of this and John was an extremely able guy.

TROPP:

One other general question. This is more closely related to the Navy, is where and how were decisions made to fund various machine projects like yours?

REES:

Well, that again gives the wrong impression; there was no machine project like this.

During World War II there was an Institution at Sands Point which was devoted - Sands Point, New York - which was devoted to training operations using machinery, training of pilots, private pilots using machinery for training.

TROPP:

Link trainer.

REES:

Yes.

Now that thing, that group was called the "Special Devices Center" and that was under the auspices of some other part of the Office of Naval Research. I guess it was put into

the Office of Naval Research when the Office of Naval Research was established, cleaning up the organization chart.

They had, I can't remember the name of it, another machine that was ultimately finished and sent out to Point Magoo. Do you know the name of that?

TROPP:

No, but I know the machine you are talking about.

REES:

And Whirlwind. These were Special Devices efforts, and they had Perry Crawford, and they dreamed the good dreams that I dreamed and were not nearly so much concerned as I was with getting something that looked as though it would work, that had a chance of coming to fruition. And they had Jay Forrester on the Whirlwind Project and Jay, as you know, thinks big thoughts, so that the Whirlwind Project kept requiring additional millions of dollars and after a while the Admiral decided to ask me to take a look at these things. They were not part of my original responsibilities.

TROPP:

Oh, I see. This came later.

REES:

When I took a look at them I was appalled; and I took many looks at them with the aid of many competent people.

Everybody agreed that Jay Forrester probably was a genius and certainly that he was doing many very exciting things. Whether the Whirlwind would ever operate is something we weren't so sure about, but given Jay Forrester's extraordinary abilities, the policy decision was not to eliminate the project even though it was extraordinarily expensive and even though I never would have gone that route.

But, of course, it did develop some extraordinarily interesting results. On the other hand, its costs were fantastic and I was always of the opinion that we were putting too much money into it.

TROPP:

How do you feel about it in retrospect?

REES:

I had a friend once who was the Executive something or other of Cook County's Planning Operation, and one time when I was in Washington I said to him, "It is fantastic the amount of money you put into this highway system and the amount of graft involved," and he said, "Maybe you're right, but if we hadn't done it we wouldn't have this superb highway system." I don't think there was graft in the Whirlwind Project, but there was a terrifically large amount of money wasted, we wouldn't have had some of the very best things that came out if we had not put that much money in it.

TROPP:

I was talking to Bob Everett about this kind of thing and he was trying to describe the difference in atmosphere of the wartime and postwar era and today, and one of the things that he described in terms of building a new project, something that had not been done before, was the freedom that engineers have to have to make mistakes to try wrong approaches which you can no longer do today. Today you tell somebody it will take three million dollars and two years to do a given thing and they'll ask you why it will cost so much and take so long, and you say, "Well, I know in advance some of the things I am going to try out are not going to work." And the man says, "Well, suppose all the things you try work the first time, what will it cost and how long will it take?" "Well," you say, "a million dollars a year." And he says, "Fine, eliminate all the things that won't work and the money." I think Whirlwind was that kind of thing.

REES:

I always took that position with regard to the total program for research, that you had to have room for mistakes and I certainly took that position with regard to engineers' projects as well as others, but I think that LARK was the name of the; I recommended that that be discontinued. It was not discontinued. I was taken to a, to save the person who flunked, it got patched together, it was just - there is a difference, you don't take somebody who has no competence and say, "Go try things and make as many mistakes as possible." Nor do you encourage people to move without making some assessment of the likelihood of — It isn't just a random operation, it's an operation that has some guidance and I think there was too much that was dreamy in Whirlwind, but I wouldn't know, and that was the problem, nobody knew how to eliminate the unsuccessful tries.

TROPP:

It is difficult even today and it is one of the things that I am working on, that is to go back and look at Whirlwind and try to identify what its major contributions were, not only as a working machine, but what it contributed to subsequent developments. Core Memory is something everybody knows about, but as you indicated, there were two other groups who probably would have gotten there had there never been a Whirlwind.

There were other things that Whirlwind contributed and I am interested in looking back now and trying to identify what they were.

REES:

I think that is very useful. Certainly they contributed Jay Forrester, and he's nontrivial himself. [Laughter]

TROPP:

That's right. But Jay Forrester was around, would there have been a Project Whirlwind?

REES:

I don't know, though I saw Jay Forrester developing over the period of Project Whirlwind and he had to have something, it might not have been Project Whirlwind, but he had to have something to bring out this extraordinary gift that he has for leaping barriers.

TROPP:

He is still doing it today and is doing some very exciting things.

REES:

I think I was probably somewhat wrong but — and maybe the only way to get Jay Forrester to operate was to give him this fantastic thing, but I was very unhappy about it. I thought it was too expensive.

TROPP:

What were some of the other machines other than the LARK and Whirlwind that you were involved with from the funding viewpoint of ONR?

REES:

Well, of course, the Bureau of Standards machine.

TROPP:

Both the East and West Coast machine?

REES:

Yes, and ERA. And the thing I remember most clearly about ERA was its work on the magnetic drum which was quite distinguished and that it probably would have got the core memory too, indeed may have got it about the same time as the Whirlwind.

TROPP:

So there was also the group at RCA who worked on similar lines?

REES:

Yes, that's right. Well, of course, they were in the core memory thing quite early. Of course the tube, the amount of wasted energy on the tube that went on at MIT as well as at RCA and ERA was considerable.

TROPP:

What are some of the things that strike you from today's vantage point as being some of the major milestones that you saw during the era in the development of computers and the atmosphere in which they operated and developed?

REES:

Well, I don't think that I have anything to contribute. It is obvious that SEAC was a very important move, but we were all working at getting a memory and I think that there was lots more hope for the tube than there was for the core. The core was a great break-through as far as the problem —

TROPP:

Right, that's at least a major milestone.

REES:

The establishing of INA.

TROPP:

I think the whole idea surrounding the establishment of something like ONR which philosophically was very close to what NSF eventually became.

REES:

Yes, but of course it was not by any means limited to the computer development.

TROPP:

Right, but in that era I think that was a significant development.

REES:

There is no question, you see - there were five branches in the mathematics division of which the computer branch was only one.

TROPP:

I guess as a mathematician I'm asking the broader question, I think as a mathematician and scientist, I see ONR as a significant development.

REES:

Yes, of course it is an extremely difficult one now to assess. You had student attitudes about the support of research and Senate attitude about support of research by the military, the support of basic research by the military. At that time it seemed just so important that government funds should flow into the sciences and make it possible for the United States to move forward, as indeed it did.

TROPP:

When I read Condon's original documents about his conception of ONR, it has a non-military flavor. There is no question about that.

REES:

Well, there was no question that it did have a non-military flavor. On the other hand, ONR had as one of its major subdivisions, the military subdivision and the function of those offices was to watch what was happening in science and see that the Navy benefited from any developments that were relevant, and the fact is that when the National Science Foundation was established I was concerned about the ways in which they would exploit the findings that might be of interest in application. In the Navy you had a natural way to do it. If you saw something coming up that was of interest you just stopped and saw your naval colleague and said, "Look, this is interesting, let's to go see the Admiral," and something happened about it.

When we saw, for example, that the logistics problems of the Navy could be handled with results that George Dantzig had and his machines, we saw that the Deputy Chief of the office — oh, what was he called then? — Vice Admiral in Charge of Logistics, and told him and quickly a logistics effort was developed in the Navy. You know, this always happened when the Navy had problems in communications and the various points when the military had problems about the defense of the North American continent. They came to us and we talked with the relevant mathematicians and the various things tied in. And when the National Science Foundation was born, I said how are they going to use these results?

Now they are talking about how to get results and the immediate application.

But again, as I said, looking at Condon's early writings on the formation of ONR in which he also talks about the eventual creation of the National Science Foundation, it is clear that he is concerned about a couple of things. One of which is the thing that you talked about in terms of INA and that is the research atmosphere in which people can communicate with each other as opposed to the departmentalized research that went on during the war in the high security projects, and he is concerned very much that scientists are able to hold dialogues.

REES:

Well, of course they - I don't know what the motivation of your comment is.

That was characteristic. The Navy was supporting basic research. It was encouraging universities to act like universities.

TROPP:

Right, but it was really contrary to the wartime atmosphere in which this group was working and that group was working, but they weren't supposed to know who was doing what.

REES:

Well, but in the wartime atmosphere they weren't doing basic research. They were doing applied research, research applied to military problems, all of which were classified.

I think perhaps I should make it explicit what I've not said explicitly, that it was because of the recognition in the predecessor office of the Navy - it was not the Office of Naval Research during the war, it was the Office of Research and —

TROPP:

Scientific Research —

REES:

Scientific —

TROPP:

OSRD?

REES:

No, no, no, the Navy had.

TROPP:

Oh, I'm sorry.

REES:

The coordinator of research in the Navy which —

TROPP:

Was that the Naval Research Laboratory?

REES:

No, the Naval Research Laboratory was part of ONR. It became a part of ONR just as the Special Devices Section did. No, this was ORI - the Office of Research and something else, and there was a Coordinator of Research and something else, who served as the Navy, the prime Navy liaison of the Office of Scientific Research and Development, and it was here - and I cannot remember the name of the Admiral, but he recognized that the basic research which was being used during the war by the scientists in OSRD had largely been carried out in Europe and that there was relatively little emphasis on basic research in the United States, and it was he and his advisors who urged the Navy in the absence of the National Science Foundation, which Vannevar Bush had urgently recommended, not to lose time and get in and get government money into the universities to encourage the scientists to move strongly in the areas of basic research.

It was a decision that we had to be strong in science, not a request for applications which was the occasion for the founding of ONR and it really was a, it was a move to substitute, I mean to provide for the need which Vannevar Bush had identified …

I think it was the success of ONR, not only in the support of basic research, but as part of the Navy, that made ONR continue later on.

The Navy found that this was a very handy thing to have around the house, because it meant that the officers - first of all, I was doing graduate studies in the ONR incidentally, and they instituted this program of bringing young officers in and sending them off to universities to get Ph.D's. The officers came in to - to get proclamation from me, they were [laughter drowns out completion of sentence] They always, on their first visit, called me, "Yes Sir" and "No Sir." But the Navy discovered, and the other services did too — the Army and Air Force established corresponding offices — it was a very happy thing to have these scientists around and kept the offices in touch with the developments of the scientists.

The original plan was to support basic research

TROPP:

Well, if I'm going to let you meet your one o'clock deadline I guess I would first like to thank you for clearing up some fuzzy thinking on my part, and in terms of some of these ideas.

It is clear that I am just beginning to probe some of these areas. I hope to get into the ERA period that you suggested, and I will try to run down some names. But I do want to thank you for your time.

[Let me turn this off.]