



**Computer Oral History Collection, 1969-1973, 1977**

---

**Interviewee:** Mina Rees (1902-1997)  
**Interviewer:** Uta C. Merzbach  
**Date:** March 19, 1969  
**Repository:** Archives Center, National Museum of American History

MERZBACH:

I gather from the little bit of background we do have, that you were born in Ohio, but I don't know exactly where you did your basic school work. Did you go to school there or in New York?

REES:

No, we moved to New York when I was a baby so that I had all of my education in the New York public schools.

MERZBACH:

New York public schools. I see. And then you went on to Hunter from there and majored in mathematics?

REES:

That's right.

MERZBACH:

What made you go into mathematics?

REES:

I feel as though I should ask you that question. I think, like most of us in the good old days, I went into mathematics because I liked it very much. I had a good time. I was always good at it, of course. But, all through college I was hesitating as to whether I wanted to work in mathematics which, to me, meant being a mathematics teacher or becoming a lawyer. Every semester, I went through this agony of trying to decide whether I should switch to history, which was the only available major that seemed to be appropriate for law. Each time I went through a reassessment, and the decision stayed with mathematics. ...

MERZBACH:

You specialized in abstract algebra?

REES:

Well, I did when I went to the University of Chicago. I should mention that I had gone to Hunter High School. That is not part of the regular public school system. When I was graduated from Hunter College, I was offered a job at the College, but I had formed a firm opinion when I was an undergraduate that this was a bad mistake that the College was making, employing people who had just graduated. I felt that the standards of the College were not high enough and that people should be better educated before they ... became teachers there. So, I said I could not under any circumstances, teach at the College because I wasn't [well enough educated].

The head of the department was appalled at anything like this, so she got me a job at Hunter High School, where I taught for three years while I got a master's degree at Columbia. This was very convenient. I was five blocks away from Columbia and I really could be a full-time student at Columbia at the same time that I was teaching, because this was the easiest high school to teach in, entirely different from teaching in high school today. [The students were a] selected group of girls and very easy to teach.

Then, after I had gotten my master's degree in 1925, I was offered a position at Hunter College by the new chairman of the department, who was a Harvard man and a good mathematician — Tomlinson [Fort]. I don't know whether you know his name.

MERZBACH:

Just the name.

REES:

Are you a Harvard Ph.D.?

MERZBACH:

Yes.

REES:

Then of course, after I went to the College, I thought I should get a Ph.D. At Columbia, I had become interested in abstract algebra and had done a lot of work with a Dickson book, which was a new publication at that time.

MERZBACH:

Yes.

REES:

I decided that Dickson was the greatest man in the world and Chicago was undoubtedly the mecca of all algebraists, so without mentioning it to Chicago, I just went out in 1926 and turned up. I registered for some things I was interested in. Really, as a person responsible for graduate education, I'm appalled at the way I did this, but I got there only to discover that Dickson was no longer working in algebra. He had given his full attention to number theory a couple of years earlier, and all the people who were working with him were doing what nobody would do now. I mean a machine would do it.

They were really working to get asymptotic results — fill in information leading to asymptotic results in number theory. Nowadays you run that through a machine and, you know, there were scores of people working with him. This wasn't what I was there for, so I just informed him I wanted abstract algebra, and there just wasn't any work going on in abstract algebra at Chicago while I was there, so I'm virtually self-educated. It was the craziest arrangement.

MERZBACH:

I was wondering, was E.H. Moore still alive at that point?

REES:

Oh, absolutely. He was still giving work. I had some work with E.H. Moore, but he was working in general analysis, which never came to anything, you know. There was one of his protégés who was also on the faculty, who was working in general analysis, but this really was a side path which never came to anything at all.

Bliss was there, working on the Calculus of Variations. Saunders [MacLane] got his master's degree the year I got my doctorate. [Jimmy] McShane got his doctorate the year before I did. There were a lot of exciting people, and students were really exciting. This was a great experience.

At that point in history, I think that probably you could get a Ph.D. at Harvard, they didn't throw women out, but few women got them.

MERZBACH:

I was wondering about that as far as the possibilities. How customary was that in Chicago?

REES:

Chicago was a very interesting place in those days. I think it still performs some of these functions, but it's quite a different place now. The people who really [were interested in]

scholarship and came from the South, especially the middle of the South, just gravitated to Chicago so there were a great many Southerners, among these, women. Not many of these women were trying to get Ph.D.'s, but a great number of them were getting master's degrees, so there was a considerable female component of the student body. Now, I can remember only two or three Ph.D.'s.

I got my degree before any of the other women. I got it very quickly because I had had advanced work at Columbia. I got it in a year and one-half actually, so that I wasn't there very long. But after I left, a number of the women who had been around did get Ph.D.'s. I can think of two who actually got degrees, but it wasn't unheard of there.

Columbia is more enthusiastic about women now than it was then, but Columbia really didn't want women in it's student body [in mathematics when I was there]. They were very nice to me when I was working for my master's degree. That was decent for a woman. Anyway, they didn't have great Dickson, even though I didn't know he was not working in algebra.

MERZBACH:

But he did work ...

REES:

I worked with him, yes, and I saw him occasionally, and we became fast friends, but he simply was not giving any courses.

MERZBACH:

Well, then what happened?

REES:

Well, then I went back to Hunter where I became a professor with a Ph.D. I got my degree in 1931, and I moved through the ranks at Hunter.

MERZBACH:

What kind of courses did you teach?

REES:

It varied. Hunter was all over the lot. They'd had a fire. [The College was forced to operate in three rented] buildings, and one building had nothing but freshmen. [At the] new campus [in the Bronx, there were only] freshmen and sophomores ... and in [a] building ... near the old campus, [there were] juniors and seniors. From one year to the

next, I moved from one building to the other, and I taught the whole range of things. In those days, we didn't have calculus in the freshman year, so I was teaching only geometry, and calculus a good deal to sophomores. The next year work was somewhat different. I did some experimental courses. I enjoyed teaching, and I was a very good teacher and I had a wonderful time with the students.

It was ten years later that Pearl Harbor occurred, and then I became uncomfortable the way most people did. I wanted to get into something that was relevant to the war. Then in 1942, the National Defense Research Committee was established as part of the Office of Scientific Research and Development. In 1943, the Applied Mathematics Panel was established. This whole effort, [as] I'm sure you know, was an attempt to enlist the assistance of civilian scientists outside the military to help the military ... with weaponry and specific work oriented toward winning the war, which was somewhat in contrast to what was going on in England where they had this activity, but also an ongoing activity to try to educate people for the future. We [did not] carry on a research program, I mean a basic research program. We committed ourselves to applied research [focused on getting usable results quickly]. ...

Well, the Applied Mathematics Panel was established in recognition of the fact that many of the components of the Office of Scientific Research and Development needed mathematical assistance that was not available to them within their own shop. Many of them had very good mathematicians doing things immediate. [For example,] I think it was the Explosives Division that had Abe Taub. Aberdeen had a lot of first class mathematicians [too, but] it wasn't possible for each component to have a range of mathematicians who could take care of all the kinds of problems that came up.

After [OSRD] had been in operation for a year, they decided they needed a sort of stable of mathematicians who would be available to all of them and to the military. Warren Weaver was asked to become [the head of this effort] and they called [him] Chief of the [Applied Mathematics] Panel. Panel was a name used to distinguish between a group of people available [of all kinds of problems] and a Division which had an assigned job, like Undersea Warfare, Explosives and so on. The other panel was a Psychology Panel.

When this organization was established, Warren Weaver invited me to become what was called a technical aide. This was a government employee who represented the government in dealing with what we called contractors. I think this was the beginning of the notion of the university contractors. We had university organizations all over the country who had contracts with the Applied Mathematics Panel to do mathematical jobs that were assigned to them. The Panel itself consisted of five or six eminent mathematicians, and they were [among] the great names in mathematics. I was secretary of this Panel, which gave me a central opportunity to understand about all the problems that came in either to Warren Weaver or to me. The Panel then discussed the nature of the problem, tried to isolate the mathematical character, and determine whether mathematics could do anything because, of course, often you were asked to perform a miracle which wasn't quite within our capacity.

Then, if it looked as though we could contribute something, we decided which of our various stables of mathematicians were most likely to be productive, and then asked them to take that problem. So all over the United States, we had mathematicians working on assigned problems for various parts of the military organization, and for the other parts of the Office of Scientific Research and Development.

So I was in [a] central position with regard to the military problems that required, or hopefully could be handled with some mathematical assistance. Of course, as you well know, the mathematical nature of a problem is abstracted from its origin, so we would often have problems coming from one group of users which were the same mathematical problems that came in from others, so we served as a communication link, even between parts of the Navy. I spent a good deal of my time telling the Bureau of Ships what the Bureau of Ordnance was doing, and things like that. It was a very intellectually interesting job. Of course, you became terrifically involved in the urgency, the need to solve these problems, and to get the weaponry out and so on.

MERZBACH:

One question concerning the personnel. How did, for example, the members of the Panel come together? How did you get to be there?

REES:

Well, the Panel itself was composed of a very small number of people who had been selected by Van Bush, Dr. Conant, and Dr. Weaver. Dr. Bush was Director of [OSRD], Dr. Conant was Director of the National Defense Research Committee and Dr. Weaver was the Chief of the Applied Mathematics Panel.

I'm not sure I can remember all of them. I remember that the Deputy Chief was Thornton Fry, who was head of the Applied Mathematics Division (or whatever it was called) of the Bell Telephone Laboratories which, of course, has become magnified. He was really a pioneer in the notion of mathematics in an industrial setting. He and Warren Weaver had a kind of understanding of the link between mathematics and the user that the other people were learning. They had worked in the applied aspects of mathematics.

[Others were] Griffith Evans, who was head of the department at Berkeley, was one of the members of the Panel, Marston Morse, who [was] at the Institute for Advanced Study, [and Veblan who] was head of the Aberdeen mathematics effort. There were a couple of others, but these had been selected really because of their vast reputation as mathematicians.

It was characteristic of [many of] the university groups that they had many, many more people who had never done anything applied than people who had done applied work. Of course, very few really good mathematicians had worked in any applied fields. That was rather an outgrowth of the war, so that the basis for selection of the Panel, which was in

existence before I got there, was, I am sure, the outstanding achievement and reputation of the men as mathematicians and their desire to be involved in the war effort.

I was invited because Richard Courant was one of the members of the Panel and I had had a lot of association with him in the New York community. You asked me what I taught. Among other things, I taught students, and one of the students who was most successful at the New York [University] Institute of Mathematics was a student who had [studied] everything with me because she had been in these various buildings, and I had moved around from one building to another. So that she had only, I think, one course with anybody else and this made a great impression [on] Courant, [who suggested me,] I think partially as a result of that, but partially through our meetings at the American Mathematical Society. I was active in the Society as he was, and I think it was he, more than Warren Weaver, who knew me, and suggested that I be invited.

MERZBACH:

This basic problem of the theoretically oriented men now being put into this, not just position of judging applied work, but applied work with such urgency. Did you have any particular observations at the time? Were there any particular problems that came up? How did these men take to the very practical and immediate problem solving?

REES:

Of course we were all terrifically motivated, and I know how I suffered for six months [before joining the Panel,] trying to figure out how could I [could] do something to contribute something to the war effort. I'm sure that most of these people went through this too. It's a dreadfully frustrating sensation [when] you think you ought to be able to do something, and you don't know how to get in there and do it.

The groups were organized around different concepts. [At Columbia and Northwestern, the assigned applied mathematics problems were chiefly concerned with air warfare, including Operations Research applied to Air Warfare. Operations Research, of course, was new during that period of time, and there was a special program at the Applied Mathematics Group at Columbia to train mathematicians to work in operations research abroad.] Actually, we did send a number of people over to work with the 8th Air Force, which was in England - with their Operations Research group that was there. The American Operations Research became effective along with the English Operations Research.

At Princeton, also, was one of the major statistical groups, under Sam Wilks. At Berkeley, there was also a group of statisticians under Jerzy Neyman. [These spent much effort on bombing problems. At Columbia there was also a statistical group under Allen Wallis, who is now President of the University of Rochester. In that group there were superb statisticians of the applied variety who dealt with many kinds of problems including sampling and industrial techniques more generally. Sequential Analysis was

born there.] The people [in] these groups were recruited by the core people. Sam Wilks got his kind of statistician, Jerzy Neyman got his kind of statistician, so that you had, first of all, a kind of natural grouping of people who came were the people who wanted to put an effort into war work, so that you started off with a commitment, which I still think is quite essential for successful applied mathematics. You have to have a desire to use the mathematical resources that you have at your disposal, to enlighten problems. And all of us, I think, had that, and we all wanted to use our resources.

One of the things I found interesting was the way Adrian Albert, for example, would solve a problem in contra-distinction to the way Saunders MacLane would solve the same problem. Adrian always brought the full machinery of matrices to bear. Later, when he and I became involved in a high security operation of applied nature the same thing was true. We had some analysts trying to work on this problem, and Adrian always brought the full machinery of whatever kind of analysis they were specialized in. I think this was one of the most interesting things mathematically: that the same problem would be attacked typically, not surprisingly, but typically, by the resources that [a] particular mathematician had most completely under control, and often solved by two quite different machineries. But I think really the basic answer to your question, if I can remember it after this long discourse, is that people wanted to solve problems.

Hassler Whitney seems a most improbable applied mathematician, I think you will agree, but he became a great, great man in air warfare. He really wanted to do these things. I went with him to Port Washington to [try out] the training machines they had to train air gunners. He was the most successful novice at air gunnery that you ever saw, but he also improved the training of the gunners, because he really understood what was going wrong with the machinery there.

Now there may have been some gross failures, and these are the things I would forget. I certainly remember the startling successes. Saunders MacLane became absolutely the most effective man with, particularly, Navy officers. Now if you know Saunders MacLane, that [may not] sound likely, but he wanted to sell [his solutions to their problems]. The Navy officers found that they could talk to him. He really was able to interpret their problems into forms that he could make some contributions to. ....

Adrian Albert, who was [an] abstract algebraist before this time, really had had some interest in one variety of applied problems. It was an important interest actually. He had made a contribution toward cryptography, cryptanalysis, but he became interested in a great variety of applied problems. Of course, he has gone back to being a pure mathematician.

MERZBACH:

Yes. That's the other thing. As far as I know, most of these people, once it was over, did go back to the abstract non-applied type of work.

REES:

Well, that isn't completely true. Herman Goldstine, who was a Calculus of Variations man, was completely separated from that. He was in uniform at Aberdeen during the war, and at the end of the war, Von Neumann asked him to join him. He became identified with the machine, of course, conspicuously so.

MERZBACH:

So much so that I didn't think of him as part of the ...

REES:

Well, he was at Chicago when I was, you see.

MERZBACH:

I see. Yes, that's right. That's the other.

REES:

Yes. Now I think, given a moment or two, I can think of others that are perhaps not quite so conspicuous as that.

As I mentioned, after the war, I came to Washington to head up the mathematics component of the new Office of Naval Research, and I think probably we have to get into that. When I was in that role, I was working with many components of the Navy in their applied problems. There was a whole group of people who worked on [a] secret project of the Navy during the summer and they were drawn from pure mathematics. They all gave three solid months of hard work during the summer on this applied Navy problem, and this happened for years. Al Tucker, who had been working in topology of various kinds, became interested in Theory of Games. When Dantzig began working on the various problems of logistics in the Air Force, we wanted to get some people to work on this for the Navy. Al Tucker set up a group at Princeton [that] did quite a lot of work. [They developed important results in] Game Theory, and [Linear Programming.] [Tucker] became completely committed [to] this particular kind of application, and has done [important] work in [it].

Lefshetz set up a project. I don't know just what a project means, but I mean he gave a substantial amount of his time, and developed a number of students, including Richard Bellman, in nonlinear differential equations, and a great deal of the Minorsky type analysis so that we had a number of Ph.D.'s out of Princeton under Lefshetz in this particular kind of applied mathematics.

Well, I've mentioned a number, but there are many others around the country.

MERZBACH:

Well, this is extremely interesting, because really, the idea that the break between the pure and applied ... it now looks as though the union, the sort of coming together is taking place now as a result largely of interest in computers and such, but in fact it seems as though it was through.

REES:

You see, I don't really know too well what went on in what was called applied mathematics before World War II. Warren Weaver had been identified with this notion out at Wisconsin, and Thornton Fry came out of that same background, and was the one who established the concept at Bell Labs, of having the Applied Mathematics Division.

Now the thing they were talking about was much more restricted when they said applied mathematics than what we mean now. I think what we mean now is essentially anything where you use mathematics in [attacking] a real problem, and [this] calls on all branches of mathematics. There just is nothing that is excluded. I think that does go back to World War II.

It's true that at the end of the war, I was pestering Johnny Von Neumann to get down some of the information about computers that he'd talked about but [had] never [written], and I never succeeded in doing that. I did manage to get some things written when Herman Goldstine and he collaborated, but you had to get things through collaboration at that point. Johnny wasn't writing anything himself. He would talk to people who would write things down, but it was clear that we were going to have some computers and we had moved toward lots more computing. We did have the Aiken machine and the Stibitz machine, so computing was not completely out of the picture at that point.

Actually, when I came to Washington they had already established a mathematics branch in what became the Office of Naval Research, before it was established under that name. The only projects they had there were analog computing projects, except for one in conformable mapping. They had discovered Don Spencer and knew he was a good mathematician so they had [a project with] him. They didn't have a program, but there was that project.

Most of these analog computer projects were [not] of [great] significance, but we very quickly got into projects that were [focused on] digital computers. There always was the point of view that computing was needed to move into significant applications and, of course, we had a lot of hand computing during the war, [and the Applied Mathematics Panel included the Mathematical Tables Project,] so that it perhaps distorts it a little if we act as though there [had been] no computing involved. There was always computing involved in any applied mathematics. It just didn't have the advantage of big machines.

But I do think that the whole recognition that there is no specific mathematical training for an applied mathematician, and that he needs to be a good mathematician and interested in problems, came out of the experience in World War II.

MERZBACH:

It seems to be going back historically, because I find that in the early part of the 19th century.

REES:

Oh, yes. Of course, the kind of problems to which mathematics was applied (or I guess the English would say were applied) usually called on analysis, and the British were powerful analysts and they operated effectively. Now the kind of mathematics that we call on in applied problems are all over the lot, so I don't know how valid the comparison is. The real difficulty that we suffered, I think, before World War II, [arose from the nature of the problems to which mathematics was applied. These needed analysis. Now the problems need a great variety of mathematical techniques.]

**[End of Interview]**