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Participant: Mrs. Goheen
Interviewer: H. S. Tropp
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TROPP:

This is the 15th of August 1972 and this is a discussion with Harry Goheen in his hotel room at the Boston Sheraton Hotel. And we were going to discuss how you got into the computer field, and I think I'd like to go back to your own personal training in mathematics and sort of work our way up chronologically.

GOHEEN:

I was a student of Professor William Manning who has just died. He was ninety-two years old, I think, when he died. I was his last Ph.D. student at Stanford, just before he retired. After that I went with the— This was in what year?

GOHEEN:

In 1940. The last year I was working on my Ph.D. I was working in absentia at Reed College in Portland, Oregon. There was a project of the General Education Board of the Rockefeller Foundation— was this with Griffin?

GOHEEN:

With F. L. Griffin, on the teaching of mathematics. It was a terrific experience for me. After that I was in Wisconsin for two years, and then the war came and I star--on December 8th I started trying to get into the various branches of the service. The branch that accepted me--that I finally accepted--I don't remember exactly what the sequence was, but the one that we finally came to terms with--I came to terms with the Navy right after I finished teaching in Wisconsin in June of 1942. And I went into the Navy. I was a teacher of Midshipmen for several months, and it seemed to me that things were going from bad to worse as far as the country's war effort was concerned. And it seemed to me that I might possibly be of greater assistance as an active duty officer at sea. So I applied for active duty at sea. I was put in the Armed Guard. That was not my decision, I didn't ask for it, but they put me in the Armed Guard and I shipped out of Treasure Island, San Francisco to the Pacific. And I discovered that actually there wasn't all that much difference between what I was not doing on shore and what I was not doing in the Pacific; although my crew was a fairly well trained crew. We generally did fairly well in

the Court Director's competitions for firing. We never had to fire our guns at anything and we were never in the vicinity of any enemy forces. It was a terribly boring experience. But I had asked for it and I couldn't very well complain. In July of 1945, while I was in Manila, the Felp [?] Director gave me orders to--as soon as my ship came back into San Francisco--to report to Boston. And I joined Howard Aiken's group at Harvard University. The end of--I don't remember the exact date, but it was the end of the summer of 1945. By then the Mark I was running,

GOHEEN:

Oh, it was running, yes.

TROPP:

The Comp Lab was under construction.

GOHEEN:

The Computation Laboratory was built by then.

TROPP:

Oh, it was built.

GOHEEN:

Yes, and we were staffing it. It's rather interesting, last night when we were talking--or listening to people talk. I hadn't realized that the only person that was there for a long time before me was Bob Campbell...

TROPP:

That's right. Bob was there at the beginning.

GOHEEN:

Yeah. That actually, as far as I was concerned, Ed Berkeley, who apparently came after I did, according to what he said, I thought of him as being always there. And Grace Hopper I thought of as being always there.

TROPP:

Grace Hopper hadn't preceded you by very long. I think she came in '44.

GOHEEN:

And Dick Bloch, of course, I think he did come--I think he was there with Bob Campbell from the early days.

TROPP:

Uh, Dick came --about the time they were designing Mark II.

GOHEEN:

Well, wait a minute; Mark II was on the--was being started about the time I was there.

TROPP:

Well, Dick hadn't been there very long, then. Well, again, I think Dick came in '44 and was doing the early programs on Mark

GOHEEN:

Yes, on Mark I.

TROPP:

On Mark I, in fact he wrote the first programs.

GOHEEN:

I think that he's the first American programmer. He probably--that means that he probably was the first programmer in the world. I mean who was programming for an existing machine. Babbage programmed--no, Lady Lovelace programmed some things for the computation of Bernoulli numbers for Babbage's difference--analytical engine, but it was never built.

TROPP:

Right.

GOHEEN:

I don't know if the programs would have run, either. I'm—

TROPP:

I'd be interested in looking at them now to see how they would compare. In theory they look very much like the programs you would run for Bernoulli numbers today, they don't look significantly different in terms of the approach, but in

GOHEEN:

Yeah, I guess you can't

TROPP:

terms of actually running them, stage by stage, I'm not sure.

GOHEEN:

I guess you couldn't very well--I mean there isn't--there isn't all that much leeway in computing...

TROPP:

Right. The concept was basically sound that Lady Lovelace had of generating –

GOHEEN:

Of loops and repetitive calculations to do the same thing.

TROPP:

Right. I'd like to push back, even though this has nothing to do with the general subject, on Professor Griffin, who is one of my personal idols.

GOHEEN:

Uh huh. He died

TROPP:

Very recently.

GOHEEN:

about a year ago.

TROPP:

Yeah, very recently. But in terms of this program in mathematics education, it seems to me, if I remember his writings, he was into what we now consider the experimental teaching programs, back in the twenties.

GOHEEN:

Oh, well, when he went to Reed, I think, about the time I was born in 1915.

TROPP:

That's right. He was the first professor that they hired after they hired the President when Reed College was founded. But there are some of his writings in the twenties in the journals of mathematics education.

GOHEEN:

Yes, he certainly was very much interested in mathematical education of college undergraduates. And some of the things that people are doing now were standard operating practice with him. He tried to get each one of the mathematics majors to write a graduation paper--he called it a thesis, but, of course, it wasn't a thesis, it was a graduation paper.

TROPP:

Uh huh, like a senior...

GOHEEN:

A senior report, yeah. And some of them were quite elaborate.

TROPP:

I guess I'm asking from the standpoint of his impact on you in terms of your own career.

GOHEEN:

Oh, it was a curious impact. It developed very late. In fact it developed after I got to Iowa State. At Iowa State -- not Iowa State, Oregon State. At Oregon State there was a new Department Chairman, Professor Arvid Lonseth, who was one of the directors of the ACM from 1960 to 1964. He was a northwest--he was a council member. He also was very much interested in improving mathematics. And that was--his interest in computers was not in computers themselves, but in their impact on mathematics. And, in fact, he was a pioneer in this area, because most of us felt that mathematics could be used in computers, his was just the other way around. His attitude was the other way around.

TROPP:

All right, we'll go back and pick up Harvard and then [link] into that chain later on.

GOHEEN:

But with--one of the things he was interested in doing was to improve the teaching of

mathematics. And he wanted to reform the geometry courses. And it seemed to him that the best way to reform the geometry course was to go back to Hilbert. And I started developing my course on geometry under his guidance and leadership. But it had started with F. L. Griffin. At the time when I was in Griffin's teaching seminar I had not felt that geometry was worth a hoot. And I never really awoke to the fact that geometry was the center of mathematics until I started teaching this program at Iowa State--at Oregon State. And I had to develop it. There were a lot of things that I did that are implied in Hilbert, but they are not stated. Hilbert, for example, leaves out any discussion of the undefined terms of incidence. As far as he's concerned, he acts like Euclid. He doesn't state incidence as a primitive term. It is, of course, a primitive term. There are two kinds of incidence; incidence of lines and pairs of points, and incidence of planes and triples of points. But he doesn't state that. It's implied in his axioms.

TROPP:

Right, and it's clear that these are critical ideas that you must begin with.

GOHEEN:

Yes they are, but--and in fact when I first started to teach the material I had an awful time putting over some of Hilbert's ideas because Hilbert hadn't labeled these as primitive terms, and the students kept trying to prove them and to understand them without--it gets back to this business of intellectual honesty that we were talking about. You have--a mathematician has to say the things that are primitive terms if he uses them. Either that or he has to describe them in terms of something that is primitive.

TROPP:

Right. Let's go back, then, to the Harvard period, because I could ramble indefinitely on mathematics, as you know. In terms of your experience.

GOHEEN:

Well, at Harvard I had two babies, they were enrolled in the Harvard Nursery School, and I can say that both of my children were Harvard graduates before they went to kindergarten. The experience in the lab was multifarious. On the one hand there certainly were people who were as dedicated to mathematics and dedicated to this new discipline that had not yet been formulated as a discipline, but were dedicated to the idea of the programmed calculator.

TROPP:

Are you talking about people like

GOHEEN:

Like Aiken.

TROPP:

Campbell,

GOHEEN:

like Aiken,

TROPP:

Like Aiken, Dick Bloch.

GOHEEN:

All of them, all of them, everybody there in the laboratory was of this--there were a couple of people who, for one reason or another, had felt very badly put upon. I won't mention any names, but they left--since they got out of the Navy they just have ignored the digital computer field. And I don't blame them because they got--from their own point of view they got a very raw deal at Harvard, and there's no point in picking up old –

TROPP:

Right. As a mathematician what were your main roles there at the Harvard Lab?

GOHEEN:

To learn to program. I never did learn to program, but that was what I was supposed to be doing. And also I helped in writing the Manual of Operations for the Mark I calculator. Well, also there were some other things that we did. There was one project I was put on to compute the critical mass of a heat generating solid. Dick Bloch had done the programming on this in the summer of -- I don't remember, early summer of 1945.

TROPP:

Was this the problem of the implosion of a sphere?

GOHEEN:

Yeah, yeah. And it had taken so long to do the calculations--I think four weeks of Mark I time to do the calculations. It seemed to me, and it also seemed to other people, that this was an uneconomical way to solve the problem. So I got interested in it, and developed a means of calculating the critical mass in a matter of a few hours with a desk calculator. On a modern digital computer it would take maybe a second or two for what took four weeks on a--and this isn't the same kind of thing that John was talking about last night. It

was just an entirely different technique that I'd used. I'd reduced it to considering the temperature at the center of the sphere instead of getting the temperature everywhere on the sphere, and set up a system of--an infinite system of ordinary differential equations, which I truncated to two.

GOHEEN & TROPP:

[LAUGHTER].

TROPP:

I don't know that truncation is quite the word for that reduction.

GOHEEN:

Well, I've been interested in carrying--the paper was published in the MIT Journal, in 1950, I think it was. I've been interested in trying to validate that work, but I haven't got any students to do it, and I figure it's a student project rather than my own.

TROPP:

Now, from Harvard -

GOHEEN:

Oh, excuse me that was one thing that I worked on at Harvard. then we were computing Bessel functions. And Aiken was a genius at figuring out dirty methods--quick and dirty methods for--that would tell him whether or not he was on the right track with using recursion on the Bessel functions to get the higher order Bessel functions. And his idea of seeing what the effect of the round off was to take the numbers 1 for E0 and E1 for the first, and use the same recursion relation on them, and let them build up until you dropped off five decimals--until they build up to ten to the fifth and then, I've forgotten where that was, and to assume that the error on the recursion, then--behaved like the error on this buildup of E0 and E1. But it didn't seem quite right to me and I published a paper on the round off error in the --obtaining the Bessel functions by recursion. It was in the Bulletin of the American Mathematical Society of November 1948, I think, somewhere along there.

TROPP:

You were also comparing the results you were getting with a Japanese table.

GOHEEN:

No, no, no. Aiken compared the results.

TROPP:

Aiken compared them.

GOHEEN:

No, this was a theoretical development. It was--and looking back on it now it seems like a very trivial thing, but at the time it seemed important. It wasn't. I'm sure it wasn't. It was a dead end thing and didn't lead to any--although the same ideas that I used in it I found in many discussions of round off error. Householder's discussion of round off error in matrix calculations reads an awful lot like some of the working notes that I had for this thing.

TROPP:

Do you still have some of your documents from that period?

GOHEEN:

Well, I have--this is in the Bulletin of the American Mathematical Society. It's certainly available--in 19—

TROPP:

'48.

GOHEEN:

I think '48. I'm not sure. I also wrote a working paper--but that was later.

TROPP:

We were talking earlier about your involvement with Project Whirlwind, and was that an outgrowth of the

GOHEEN:

Well, that was later. That was later. I never was involved with Project Whirlwind, but I tried to get involved with Project Whirlwind, but I was unable to because the--I don't know exactly--there were so many currents in the industry at that time. All of the things I've talked about up till now--up till this point, happened--except what happened at Oregon State--were before the founding of the SEAC. Aiken got me a job with Dahlgren Proving Ground. And I had some friends at Dahlgren--Francis Dresh, who was going to be my immediate boss down there. And I went down to visit and take a look around and it seemed interesting, the work there, and the salary was much more than I could ever command as a professor of mathematics. They were going to hire me as a P-3, which I

think at that time got $5000 a year--a tremendous principal.

TROPP:

At least double, double what an assistant professor of mathematics got at that time.

GOHEEN:

No. Then I came back to Boston and I was talking about this with some people in the Office of Naval Research, and I don't know how they got interested in me, I think the fact that I had a Doctor's Degree was the thing. At that time it wasn't as common as it is now. And Commander Morin of the Branch Office of Naval Research hired me. And one of the talking points that he used was that this was going to be a holding operation for the National Science Foundation, which had been passed by--which, was expected to be passed by Congress, but with no budget. And so the Office of Naval Research was going to be an eleemosynary organization. And I should have been less naive. I should have realized that organizations have a way of becoming institutionalized and that they were not going to disband as soon as the National Science Foundation came about.

TROPP:

Well, they were--it was out of that group that the National Science Foundation was—

GOHEEN:

That's true. They pushed for the National Science Foundation---well; the National Science Foundation was set up about--I think about 1948 or somewhere along in there, but with no budget. And the budget for the National Science Foundation was pushed for very heavily by the Office of Naval Research, I understand, although I wasn't involved in that.

TROPP:

I have seen some of the documents on that and that's essentially the way it happened. They felt the need for –

GOHEEN:

See, the point that they made in talking to me was that this was a very important thing--to set up the National Science Foundation. I agreed, and I went to work for them. And that's when, also, I was able to help in the founding of the Society, because as a mathematician with the Federal Government I had some influence on people like Mina Rees, who might have been influenced by Von Neumann negatively. I don't know. I'm--look, you can talk to her. Perhaps you'll find out that as far as I'm concerned I was a complete nothing. You know, I mean in her mind I was a complete nothing. It's quite possible. But it seemed to me at the time that I had a chance to influence people. I talked

with Archibald after this memorable occasion of the meeting of the National Research Council when they turned this down and he became quite friendly towards the idea.

TROPP:

You might recount, then, from your viewpoint, the kinds of things we were talking about last night--essentially the atmosphere and the problems.

GOHEEN:

The atmosphere was really quite repressive. And something that people never got around to discussing, although Ed Berkeley mentioned it, was that the ACM was founded by the un-, the unessential the un-tenured professors.

TROPP:

The outs.

GOHEEN:

The people--you know when people said that there was--John Mauchly said that there was complete--as far as he was concerned; there was complete freedom of interchange of information. Well, hell, he was a director of the project that built the--was working on the EDVAC. Of course, for him there was complete--and for people like Von Neumann and Aiken--I'm sure there was never any difficulty finding out what was going on. In my opinion the ACM's main function has been, is, and will continue to be the bringing of young people into contact with established people in the profession. And that's the only excuse for its being, because there is this natural tendency for the directors of projects, the people that are funded, and the tenured professors to take the attitude, "I've got mine, Jack, and who are you?"

TROPP:

That's interesting because as I've looked at the mathematical societies I have the feeling that they're not performing this function because of the existence of the invisible universities, and that they perform more of a social type function, and a place for the young graduate student to give his paper, but they're not really serving the function of getting the people interested in a branch of mathematics in contact with the small group of people who are working at the frontiers who tend to communicate with each other, meet at various international symposia –

GOHEEN:

I think that part of the problem--I think part of the problem is exactly this--that they talk to each other rather than talking to students.

TROPP:

Right.

GOHEEN:

And part of the problem is that various universities, and foundations, and governments set up these international symposia. I think that they'd be much better to put their money into the societies, and the societies then could nurture the young folks. And when I went into the Mathematical Society in 19--I joined in 1937, I think, at the Stanford-Berkeley--I don't--I think it was called the Northern California Section--we had exactly this. I got to know Professor Lehmer, I got to know Professor Levy, and Professor--oh, Boolean Algebra fellow--axioms for Boolean--Bernstein, Professor--the head of the department at Berkeley at the time—

TROPP:

Oh yes.

GOHEEN:

I can't remember his name.

TROPP:

I think, I know it.

GOHEEN:

But he had tremendous influence on me.

TROPP:

And it seems today it doesn't perform that function.

GOHEEN:

Yeah, the society can perform that function and should. And the ACM, with all of its faults, has, I think through the influence of people like--well, from the fact that--the way it was founded. It was founded by a bunch of people who were outs, and I think this was intended to influence it so that a young man going to a meeting of the ACM has a chance to meet anybody in the field that's interested in his kind of work, and talk with him, and talk with him as an intellectual--perhaps not an equal in terms of intellectual power, but as a man who's ideas are as important as the established practitioner.

TROPP:

I was impressed by the attitude of not founding a new journal. They were more interested in the people getting together and talking to each other, rather than publishing papers in a brand new journal. The idea of communication, as you've indicated, at the meetings seems to have been the most important thrust that the founding group -

GOHEEN:

Yes. I think that the Journal of the Association has had a very good effect. I think it's--I think, despite the fact that I had my differences with the Editorial Board over my recent work on the Turing machine, I feel that the Journal has been a –

TROPP:

Well, that's today's milieu, but in the 1947 period, I think the emphasis on the other, as you've indicated, of people getting a chance to meet each other who are not at the top echelon-

GOHEEN:

Not only to meet each other, but to meet people who were top echelon people.

TROPP:

Were top echelon-

GOHEEN:

It would have been so much better if we could have got Howard Aiken, and John Von Neumann, and Howard Goldstine, and-

TROPP:

Herman Goldstine.

GOHEEN:

Herman Goldstine, and a few other people to work with us in the society in the early days. It isn't through a lack of trying. We did try to get these people, as Ed points out, and when they didn't give us the exact brush-off, this refusal to cooperate, they did give us a kind of "come back later when you're established" kind of a spiel, like Von Neumann gave to Ed Berkeley.

TROPP:

Von Neumann, probably of the three that you mentioned, was the most involved in terms

of his willingness to appear.

GOHEEN:

He was willing to appear, but as Von Neumann, not as a lecturer for the ACM. I think that that's what it amounts to. And - I don't know.

TROPP:

Let's go back, then, to the formative aspects in terms of your conversations, and anecdotes, and memories of how you -

GOHEEN:

I never--you know, I never wrote down anything, and I never kept any notebooks. I may have some of the old memoranda that I had written officially for the Office of Naval Research, I'll see if I can find them, and I'll be most happy to turn them over to you.

TROPP:

That's great.

GOHEEN:

But the ordinary day-to-day things I just didn't keep track of. Ed Berkeley's different. [Laugh]. In fact there's a kind of a joke that Ed's friends had. For a while, and maybe he still does it, I don't know, but he would ask you a question. It might be a technical question like "what in the world are Tchebychev polynomials? Because he wasn't a trained mathematician, you know. And you would answer him, and he would write down your answer, and then he would take out a date stamp and this memo pad that he had and date it and ask you to sign it. And once I said, "I'm not going to sign that, because it may be in error. I just said it off the top of my head." He said, "Well, in that case it's worthless", and he tore off the card. [laughter]

TROPP:

That may have come out of the Howard Aiken influence. I noticed everybody who worked on things had a date stamp on all their little notes and –

GOHEEN:

[Laughing] No, I think it was Ed Berkeley that had influenced them. I think that--well, he was he was the only one who kept notes, and he always dated them, and, if possible, the person that he spoke to he would get to read the thing and sign it. And thank God he was that kind of person because the record can be kept straight.

TROPP:

Right.

GOHEEN:

For example, when someone raised the question about "well, Von Neumann didn't really oppose this" --well, Berkeley had "I wrote the letter to Von Neumann in December of 1945, and this is what he said," and he could pull the letter out of his file [laughing].

TROPP:

Yes, I'm sure, I'm sure he could. That's exactly what he could do.

GOHEEN:

And I think that the Association was most fortunate in having as secretary in the early years, Ed Berkeley.

TROPP:

I think that it was interesting that it came out last night that the direct formation was essentially Ed's rebellion to the establishment.

GOHEEN:

In a way, yes. In a way. It wasn't a rebellion. You know, I talked with Bob Campbell after the meeting last night, and he said--Bob says, "When I started out talking I'd forgotten just what it was like to be in the Lab in 1945 or earlier. I had forgotten it. And when Ed was talking the whole tight feeling came back to me. And that's why I supported him so strongly, because it was an emotional support as well as an intellectual remembrance. During the discussion I sat next to Grace Harper in the audience.

TROPP:

And Grace's experience was totally different.

GOHEEN:

Well, she was on the inside, you see. The difference between Grace and us was that she was the Commander's right hand girl. And he was lucky to have her for a right hand girl, because she was most skillful in writing and she worked hard, and she was a clever mathematician, and completely dedicated both to computing machinery and to Howard Aiken. I don't know whether she's dedicated to Howard Aiken anymore. I think she's transferred it somewhat to the Pentagon.

TROPP:

Well, she still carries this dedication. And she didn't, for some reason in my short conversation with her last night, she didn't feel the tightness that you felt, and Bob felt, apparently others.

GOHEEN:

Well, everyone felt...I don't think there was an exception...

TROPP:

Yes, Grace...

GOHEEN:

...felt the same way I'm sure.

TROPP:

Right. And Grace felt like she had a fully open world to her and that Howard Aiken sent her to all the installations...

GOHEEN:

Well, he never sent us...

TROPP:

Apparently she was the only one who had that freedom, not only of movement but of moving at his...

GOHEEN:

His level...

TROPP:

...his level. I guess it boils down to again the kind of world that Howard Aiken lived in. That sort of black/white world that I mentioned.

GOHEEN:

Yeah. That there are the--it wasn't a case of we weren't the good guys, we were not the...

TROPP:

No, I didn't mean that black/white...

GOHEEN:

We were the not so worthwhile people, as in the Jewish wife of Brecht there is a line in which the Jewish wife is rehearsing a speech that she's going to make with her Nazi husband, who is going to send her away--just about to send her away when he comes home, and she says, "Perhaps it's my own fault. I used to agree with you, darling, when you would say that there were two kinds of people--the worthwhile people and the not so worthwhile people. The difference was that the worthwhile people got insulin when they had diabetes, and the not so worthwhile people didn't. Now I've been denominated one of the not so worthwhile people. It's my own fault."

TROPP:

Wow. Yeah.

GOHEEN:

And I think that that was at the basis of the way we felt. We felt that we were not so worthwhile in the eyes of people who were making arbitrary decisions about what's worthwhile. And we were sure that we were worthwhile. Some of us because we felt that we, personally, were worthwhile, and others of us, I'm sure Ed would be in this other category, because we felt that all human beings were worthwhile.

TROPP:

I think that that's an important view.

GOHEEN:

And the interesting thing about it is that IBM corporation, which many of the kids nowadays consider to be the bulwark of the establishment, and I suppose it is in a way, and I suppose it was then, but because of the fact that they had that personal feud with Howard Aiken [laughing] they supported the ACM also.

TROPP:

Was that--is that possibly, were--is there any break between that and the things that I read in Eric Weiss's paper about the need to protect the people from IBM. Did you have any reaction to that?

GOHEEN:

No. I never felt that.

TROPP:

No, IBM wasn't a threat in 1947.

GOHEEN:

It wasn't a threat to us. In fact it was a supportive organization. We laughed at Howard--at T.J. Watsons Think and "Hail to the Chief" song and the rest of it. And we laughed about this, but it was a joke.

TROPP:

But IBM was no threat in '47 in terms...

GOHEEN:

There may have been people that felt so. I didn't. In fact IBM helped Ed Block and me form the SIAM organization a couple of years later at--when I was at the University of Pennsylvania. Ed Block was with Philco, I think, and IBM helped Ed Block and me and a few other people form the SIAM organization.

TROPP:

Franz Alt was

GOHEEN:

He was also an active

TROPP:

...part of that group. Let's go back to the ACM thing, as you remember the chronology and the sequence of events and some of the anecdotes connected with that very first meeting in New York. Was it at Columbia?

GOHEEN:

It was at Columbia, yeah. I think it might have been in the Electrical Engineering Hall, but I'm not sure. Maybe it was in the Physics Laboratory. But it was a rather small room. The fewer than a hundred people that were there jammed it, as I remember.

TROPP:

Was it a--this sort of an informal gathering or did you have...

GOHEEN:

No, it was--we didn't have papers, but we had direct discussion about the forming of the constitution and the charter of the organization.

TROPP:

Somebody said that, they couldn't remember precisely, they thought that Sharpless might have given a paper at that meeting and that maybe—

GOHEEN:

He spoke, he spoke, but .. I don't know. Ed Berkeley would know, because it would be a dated memo, [chuckle] but I'm not sure. It seemed to me the most important thing there was to iron out just why we were having an organization.

TROPP:

Do you remember some of the discussion?

GOHEEN:

Well, there were people from IBM that felt that this was a ploy; I don't know, I can't remember their names. But, I'm sure there were people there that felt that this was a ploy of some of Aiken's friends to form another self-adulation society for the benefit of non-IBM people, and they were opposed to that. And it took considerable maneuvering on the part of--not maneuvering because it was true--considerable assurance--reassurance on the part of Ed Berkeley to placate these people. In fact, there was a--we had an election for who'd be the Secretary, and I think, as I remember, it was pretty nip and tuck whether it would be Ed or an IBM man. I don't remember who the IBM man was; but fortunately for the Society, and I think for the IBM Corporation, too, because I think it would have been--I think that there might have been a danger that it could become an adulation society for the IBM Corporation. I think there was that possibility, because they were so strong and well knit and organized. And they didn't know that we weren't strong and well-knit and organized.

GOHEEN & TROPP:

[Laugh].

GOHEEN:

I mean, they didn't realize that we were a bunch of outs, I'm sure. But their attitude was: These are some of Aiken's boys trying to take over.

TROPP:

Perhaps this is jumping ahead, but you mentioned the formation of SIAM, I know they're celebrating their 20th anniversary in the fall at a meeting in Austin, and this might be a good opportunity to recount how SIAM came about. HG: Well, again it would be my opinion. I got acquainted with Ed Block when I was at the Moore School--no, I can't remember where I got acquainted with Ed Block. It must have been earlier, it must have been while I was –

TROPP:

Ed was a student at Harvard.

GOHEEN:

Yes, that was when I was at the Office of Naval Research that I got acquainted with Ed Block.

TROPP:

Because Ed was doing his Ph.D. at Harvard, in fact –

GOHEEN:

When did he get his Ph.D.? That would date it.

TROPP:

It has to be in the forties, because he mentioned talking to Norbert Wiener about the possibility of doing his dissertation under Wiener. So I'm guessing that it was sometime in the

GOHEEN:

Oh well, you really can't

TROPP:

middle or late-

GOHEEN:

Well, that's hard to--you see I left the Office of Naval Research in 1947, at the time when

TROPP:

Ed would have been at Harvard prior to then. I don't know quite –

GOHEEN:

I see. Well then I must have met him at Harvard, then, prior to that. And I kept in touch with him when I was at Delaware. I went to the University of Delaware. I left the Office of Naval Research, because I was distressed about the developments in the Government leading--well, the removal of price control and the setting up of the hegemony of the National Association of Manufacturers over the organizations of the Government, and the passage of the--what do they call it--the Attorney General's subversive list, and that sort of thing. It kind of--many people blamed McCarthy, for example, for the things that are really to be blamed on Harry Truman, and –

TROPP:

McCarthy was really just taking advantage of the time.

GOHEEN:

That's right, and he was much later. You see, this was 1947.

TROPP:

It really isn't until '49 that McCarthy made –

GOHEEN:

McCarthy never became even anything--I don't think he was elected to Congress until '48. I don't know. I may be wrong on this, but it was certainly--nobody ever heard of the Junior Senator from Wisconsin in 1947, and I still remember the feeling of repression. We used to get secret documents circulated through the Office, and you had to sign for them and instead of just having the stuff circulated you'd have documents.

TROPP:

About the time the ACM was founded, then.

GOHEEN:

About the time--just after the ACM was founded the--this Truman executive order really began to have an effect on the morale of the Civil Service.

TROPP:

So you left and went to work at the University of Delaware.

GOHEEN:

Yes. I would have gone to--I would have gone to Ypsilanti State Normal at that time. I was so disillusioned. There was this organization that was supposed to be the holding organization for the National Science Foundation, and it was acting like a two-bit spy agency. And, well, some of the things--I suppose that they should be made part of the record, and if you can get a hold of the records of the branch office of the Naval Research in New York City you can find out. I was asked whether or not Natasha Artin was a communist, as if this meant something to somebody. And I, you see I had some courses from Martin in the summer of 1939 at Stanford. And I was asked "is Natasha Artin a communist?" without even--I said "No, of course, she isn't". Her folks were refugees from the communist government in Russia. And I had to go down and talk to the Captain in the branch office of Naval Research, and he showed me some--a security report and that's when I really flipped my lid, because this security report was the same kind of thing that people associate with the Junior Senator from Wisconsin. It was--when--I still remember the language because it was so shocking: "When Professor Artin and his wife were at Notre Dame they had parties at which politics were discussed." And I said, "Who signed that?" And they wouldn't show me the signature. So, the ACM was founded in the nick of time in my opinion. [Laugh].

TROPP:

Well, you went to Delaware and?

GOHEEN:

Yeah. I didn't like Delaware because it was such a racist place. The year I was there the NAACP had brought a class action against the University of Delaware for refusing to let [black] students go there in areas that the black school did not teach. I don't know what the areas were, but the NAACP won that case as I remember. And I suppose that I should have joined the NAACP right then and fought at Delaware, but it seemed to me a losing proposition, so I—

TROPP:

A century of tradition is pretty hard to break.

GOHEEN:

Well, it wasn't quite a century--it was from 1890 on. The--at…then I went to Syracuse, and I enjoyed Syracuse.

TROPP:

Was Kibbe there then?

GOHEEN:

Kibbe was the head of the department. Well, not the head of the department then. Kibbe later became the head of the department, and I think he's the head of the department now.

TROPP:

He's one of the Administrative Vice Presidents now, I think, just recently.

GOHEEN:

He was rewarded for his actions. I shouldn't say this because it's probably--this is for the record what I'm saying, so perhaps I'd better be very careful what I say.

TROPP:

[Laugh].

GOHEEN:

When I went to Syracuse, I wanted to teach a course in computing machinery, and they told me

[End of Side 1]

[Start Side 2]

GOHEEN:

..."Well, people aren't really interested in that, you can teach such a course later." I taught a course in hydrodynamics, which I knew nothing about, but I learned about hydrodynamics that first summer I went there and I got quite interested in it. And I wrote up some of the papers that I mentioned earlier. The--it was when I was at Syracuse that I published that thing on the calculations for spherical symmetric heat generation. The atmosphere was wonderful for a mathematician at Syracuse when I was there, but along about 1950 the kids that weren't born in 1932 weren't coming to school, and they had a financial bind. And the Junior Senator from Wisconsin was raving over the land, and most universities were striving to get Government money to see them over a rough period, and they were trying to get people to work on various projects. I remember a meeting of the Mathematics Department with the Dean of Science, I've forgotten his name, in which he talked about the In chon landing which had taken place about that time. And he pointed out that this was because of the application of the great Von Neumann's theory of games. I didn't hear very much about that later after the retreat from the Yalu.

TROPP:

[Laugh]. Reverse game theory.

GOHEEN:

Yeah. The In chon landing was one of the really disastrous decisions of the war--one of the disastrous decisions of the cold war, because it kept--there was a chance for the war to end on the 39th parallel in Korea before that time, and it dragged on until 1953. I don't know how many thousands of people were killed. The war in Viet Nam was not the first disaster that the United States has been engaged in.

TROPP:

Unfortunately, it may not be the last.

GOHEEN:

I rather think that it will. I rather think that it will.

TROPP:

I only hope that you're right.

GOHEEN:

I hope so. We'll see what happens in the future. Because of this, they were letting people go at Syracuse, and I got a job at Washington State University. Oh yes, the last quarter--the last semester I was there--I've forgotten whether we were on the quarter or the semester system, but the last period that I was there at Syracuse I taught a course in--I think the first course, really, in Computer Science in the country.

TROPP:

This would be in 1950?

GOHEEN:

No, in 194- --I guess it was 1950, yeah. It's just that--if it wasn't the first one, it was one of the first ones. I had several students.

TROPP:

Do you remember in rough outline what you covered?

GOHEEN:

Yeah, what I covered was the nature of computation, and the nature of a stored program

calculation, and the DO loop, and--I don't have my course notes from it.

TROPP:

All right, but was it--sort of a combination of

GOHEEN:

Numerical analysis,

TROPP:

Numerical analysis,

GOHEEN:

classical numerical analysis, and the use of computers, actually, to solve problems.

TROPP:

Did you cover a kind of the state of the art--there still weren't very many machines around.

GOHEEN:

No, I didn't cover very much on the state of the art because, as you say, I think at the time there was the Sequence Controlled Calculator--there was no stored program calculator.

TROPP:

Well, there was one, but it was in England. The EDSAC, I think, was operative in '49.

GOHEEN:

No, not till '50 because it was the invention of the Williams Tube that made it really operative.

TROPP:

That's right.

GOHEEN:

Oh no, they did have the--they had the delay lines. They had the mercury delay lines. Maybe it was '49.

TROPP:

Right. I think EDSAC ran its first program sometime in '49.

GOHEEN:

Possibly so, but it was all hush, hush and we didn't know anything about it.

TROPP:

Right. So there really wasn't any machine to talk about then.

GOHEEN:

Yeah, so I didn't see much point in describing a machine.

TROPP:

But the concept was around, but no machines were available to implement them.

GOHEEN:

Yeah, yeah. I'd read Allen Turing's papers--I mean the papers in the Proceedings of the London Mathematical Society, but I didn't understand them. I didn't understand them at all. When Turing wrote for non-mathematicians he wrote with outstanding clarity. When he wrote for mathematicians he wrote with a turgidity which is hard to believe. I think because he--when he was writing for non-mathematicians he explained why he was writing. When he was writing for mathematicians he never gave any reason for anything.

TROPP:

He assumed.

GOHEEN:

No, I don't know whether he assumed, but he just didn't level with the mathematicians why the thing was interesting. His paper that I mentioned earlier to you, the paper of 19--. Well, the paper in the Annals of Mathematics on the word cancellation problem for semi groups which is the--one of the jewels of mathematical literature in computer science--there is no indication in it why anybody would be interested in building this most wretchedly complicated automaton. And I think it was--I think really there he was expounding upon his experience as working for the--Her Majesty's Post Office in the security, or the diplomatic service. I'm sure that that machine was used as a cryptanalysis machine, and encoding machine.

TROPP:

Right, which would come out of his wartime experiences.

GOHEEN:

Yeah. [Knock on door.]

GOHEEN:

Oh, come in. We're just about—

TROPP:

Come right in, come right in, Mrs. Goheen. And listen to our discussion, too.

GOHEEN:

Maybe before you let this tape go you should let my wife hear it.

TROPP:

Well, you will get a transcript of this tape

TROPP:

Yeah, oh good.

TROPP:

before anybody else outside of this little particular circle has a chance to either hear it or read it.

GOHEEN:

OK.

TROPP:

In a sense this is privileged information and in case you have—

GOHEEN:

Oh no, I have no desire to make it privileged information.

TROPP:

Well, I think until you've had a chance to decide in what form you want other people to—

GOHEEN:

I don't want to change anything I've said. The only thing I might want to change is the grammar and inflection.

TROPP:

Right, but this is your privilege and various people have different feelings about..

GOHEEN:

Well, every time you open that up--that door opens, and you don't have it tight. Well, let's see, that

TROPP:

You left Syracuse in

GOHEEN:

1950, and we went to the Moore School and I was working on a project with--at the Moore School on designing telescopic systems. That was the first project that they had had at the laboratory, and they--at the Harvard Laboratory--and they hadn't really done very much with it.

TROPP:

Was this the lens design?

GOHEEN:

Yeah, and

TROPP:

This was one of the very first, if not the first program, one of the first two or three.

GOHEEN:

Yeah, well actually it was just a ray tracing program that they worked on at Harvard. We wanted to develop it into something more than a ray tracing program, we wanted to make it a tool and design. And setting up the error criteria for the quality of the image, astigmatism, spherical aberration, color, coma, I think that's all. Color we took in three different wave lengths and so we had about seven errors that we wanted to minimize.

That was the idea. And the minimization of these errors, we set up a system of simultaneous linear equations, a great huge system, and now obviously we couldn't reduce the errors to zero, because the systems were pretty well designed in the beginning so that if you get a minimum in most of these--most of these, most of the coordination's [?] were pretty nearly zero, but we did work out some techniques for getting Chebyshev type solutions to these—

TROPP:

What kinds of machines were you using?

GOHEEN:

Well, we're getting around to that. We were using a Card Programmed Calculator—

TROPP:

The CPC, correct?

GOHEEN:

Yeah, and for the ray tracing we used the 602A, and that was fun because we programmed the—

TROPP:

That's the automatic multiplier, isn't it?

GOHEEN:

No, it's a kind of--it's a plug machine. The 604--

TROPP:

604 is what I was thinking about.

GOHEEN:

Yeah. In my briefcase there are rafts of things about it and I'd rather keep them. And some of this stuff was published in the Journal of the Optical Society by a fellow that joined the project after I left it. He's now a big shot in the--Saul Rosen.

TROPP:

Oh yes.

GOHEEN:

Saul Rosen.

TROPP:

Saul's at Purdue.

GOHEEN:

I think he's at Purdue. He published a paper on it, and I think it was the only paper that came out of the project, but then, on the other hand, it was a very small project of, I think, $100,000. One paper for $100,000 is--considering the way the Government throws its money around--is quite a thing. And we had several reports that we issued. Some of them are machine surveys on machines that might possibly be used for this project. Our conclusion was that they'd just better hurry up and build a good stored program calculator. Somebody better, because it's--I don't know whether they're doing ray tracing and telescope design

TROPP:

I don't believe they are.

GOHEEN:

with the stored program calculator, but it's an obvious application. I had a Masters student who was interested in telescopes at Salem. What was his name, Molly, the fellow in Salem?

MRS. GOHEEN:

Nick.

GOHEEN:

Nick, what was his last name?

MRS. GOHEEN:

A Russian name.

GOHEEN:

He's a-- he's from Lithuania, I think, or Latvia. I first met him because he spoke such beautiful Russian--he came in to talk to me in my office and during my itineraries I decided that Russian was an important language to pick up, and I became quite fluent in

Russian. What was Nick's name?

MRS. GOHEEN:

I can't think of it.

GOHEEN:

Oh, but it's important, this is a prime matter of a permanent record, and he has a software company. Oh, what is Nick's name?

TROPP:

Well, perhaps it will come later, we can insert it.

GOHEEN:

Yeah, I'd like to insert it. As I say, he teaches programming at [?] University and he did a very nice Master's project on using the stored program computer to do this. But actually he--we don't even think of it now as being a--using a stored program computer--it's--he did a thesis in programming. But that's the only place that I've seen any application of the ideas. I gave him all the notebooks from the project that I had and the quarterly reports and he worked out this very nice piece of work.

TROPP:

Well, it's clear from your discussions of your time at Harvard, any time until this period, that you have a strong interest in applied mathematics, and

GOHEEN:

Well, I have a strong interest in

TROPP:

that naturally leads into the SIAM kind of orientation.

GOHEEN:

Yes it does, but my interest has been mainly in computing machines. I think that computer science is taught incorrectly now, as being something sort of abstract and Ding an Sich sort of thing, and not something that's really responsible--responsive to the questions of mankind. I think it should be taught as something that grows out of the questions of mankind. All the time that I was at Harvard, and all the time until I understood what Turing was talking about, I kept thinking of trying to make it--Liepins, Nick Liepins!

TROPP:

How do you spell that?

GOHEEN:

L-I-E-P-I-N-S.

TROPP:

Thank you.

GOHEEN:

All the time that I, that I kept trying to conceive of a machine that would be completely self-checking. Now it wasn't until I'd really understood Turing's proof that you couldn't have such a machine, that I really appreciated it--the power of Turing's ideas. It isn't because God doesn't want you to have control of the universe, it's because you can't have control over the universe, and it has nothing to do with any new case of Almighty God, or any, any--it's just in the nature of the thing.

TROPP:

The thing's a recursive extension, an analog of what Godel-

GOHEEN:

Yeah, that's exactly. In fact I don't--I get after the Godel theorem in terms of the stopping problem. And my whole approach to recursive function theory is through the Turing machine. I gave a course in recursive function theory which was kind of a disaster last year. I tried to follow Rosza Peter's treatment. I had taught a seminar out of Rosza Peter in the book that was published by the Academy of Sciences of Hungary oh, about ten years ago, Rekursive Funktionentheorie. And in German it was a very easy, readable book and my kids enjoyed it in the seminar, and I enjoyed it. And then I thought, aha, it's been translated, and so we got the translation, full of misprints, and just cumbersome expressions and so on. And whereas the Hungarian and German original had cost $4, this translation into English brought out by, I've forgotten which of the prestigious presses in America, cost $11 or $12. And it was completely un-understandable. This kind of thing is related to what I said for the reasons that the Association for Computing Machinery got started. We got started because we were concerned about the dissemination and free interchange of information. When you pay fifteen or twenty or twenty-five, or thirty, or thirty-five dollars for a book, you don't have free interchange of information. You know, information is not free, if it costs so much to get it. As Anatole France said "the Laws of France in their majestic impartiality forbid the rich man as well as the poor to sleep under a bridge." Well, it doesn't do much good to have no restrictions on publication if the

book then sells for a price that a man can't afford to buy. I've felt very strongly about this. I don't blame the publishing companies for it. They have to do it, things being what they are. But I do blame my colleagues that rip them off. I mean my colleagues who get tremendous royalties for their books. And that's one of the--I think that's one of the things that jacks up the cost of the book. If professors would realize that the time they spent on translating that book was time being a professor, and they already got paid for that, and just donate the book to the public

TROPP:

In a way, it's more like a labor of love.

GOHEEN:

That's right.

TROPP:

and doing the translation that's that tedious—

GOHEEN:

That's right. You don't charge your wife for making love to her.

TROPP:

I hope not. [laughter]. I don't know—

MRS. GOHEEN:

On the contrary. [General laughter]

TROPP:

I don't know whether I can get paid.

GOHEEN:

So the--I think that the whole thing is wrong. This goes back to this business of the scene and the Jewish wife. There are worthwhile people and not so worthwhile people, and the difference is that when the not so worthwhile people get diabetes they don't get insulin.

TROPP:

To go back to this last subject, the formation of SIAM, again what were the pressures that led up to people like Ed Block, and yourself, and—

GOHEEN:

Well, I did it because--because it was pointed out to me that it was in applied mathematics the same kind of organization that the ACM was in the theory of computing machines. At first many people felt that it was an overlapping society. Ed was very careful to claim that it was not going to be an overlapping society, and it turned out to be--although they did publish some papers on the theory of computing machines in their early days. Clippinger had a big long series of articles in the first issues of the SIAM Journal. By and large they did publish books--stuff on applied mathematics that wouldn't have been published in the regular journals that certainly would be out of line to be published in the ACM journals.

TROPP:

In the case of SIAM, as a mathematician I can feel this much closer, because of the knowledge of my colleagues in mathematics, the pure mathematicians who refuse to have anything to do with applied mathematics, and this sharp division that existed for so many years on campuses.

GOHEEN:

It's--I think that it's--I hope that it's being wiped out.

TROPP:

I think it's pretty well wiped out now, but I think in the period you're talking about--if we go back to Harvard, for example, the period that you were there in decades earlier, I think if you look at the mathematics department you might find one mathematician who was interested in applied problems.

GOHEEN:

Well, there were more than that at Harvard. Young Birkhoff, for example.

TROPP:

Well, of course, his father was the one I was thinking of in, say, the thirties, when Donald Menzel was concerned with certain astronomical problems; he indicated that Birkhoff was the only one in the math department who would talk to him. And I think by then there was a tradition beginning to build up at least within—

GOHEEN:

Possibly so, but I think that--I think that the pure mathematicians and some of the applied

mathematicians made more of this dichotomy.

TROPP:

That could, that could well be.

GOHEEN:

As I was talking to someone last night, I've forgotten whom--it might have been you, about a friend of mine that worked in Galois field theory--real pure, finite fields, and when I started discussing with him the application of this for design of experiments and statistics, he was most interested. I mean it isn't--I don't think that it's--the pure mathematician is uninterested in applications, but he's interested in a very narrow subject, and if this subject can be applied, oh, he feels real good about it.

TROPP:

I think we, in the applied ..., we tend to overlook that. In this whole subject of numerical analysis I'm interested in looking at the development of numerical analysis and its impact on the development of computational equipment and conversely, because I think there's a heavy interrelationship there.

GOHEEN:

I think it's much more conversely. Although, there was some impact on--

TROPP:

Well, the converse part is easy to see in terms of the ability to now do very complicated applied mathematics that was known about theoretically since the days of Gauss and possibly before. But I also think there is some in the other direction, but it's going to take some very careful...

GOHEEN:

I think that its effect in the other direction is probably more deleterious because most of the early numerical analysis was set up to avoid computation, because it had to be, because if you did it by hand you can't afford to spend all that time. As an example of this kind of thing, there are techniques used in the design of the logarithmic and exponential unit in Mark I that were discussed by a fellow in England or part of the British--

TROPP:

Brooks?

GOHEEN:

No, no, much earlier than Brooks.

TROPP:

...?

GOHEEN:

No, no much earlier than Hartree or Comrie. This is back in the nineteenth century. Some ways of computing logarithms were most cumbersome, tremendous number of decimal places, and very slow. And as a result of the incorporation of these techniques of numerical analysis, which were designed for hand computation, into the Mark I, I think that the logarithmic unit on the Mark I was used only in this problem that I mentioned, of heat generation.

TROPP:

Of course, as I look at the overall architecture of the Mark I, and this is my viewpoint many years later, I see the Mark I essentially from that point of view as a machine that was designed to sequentially do what a mathematician or a computational--

GOHEEN:

Right, and I think that this is a very bad influence on the machines. If there had been a slightly--slightly different attitude that you compute your logarithm by means of Chebyshev approximation or Taylor expansion, I think there would have been a better state of affairs. I think that the effect was negative.

TROPP:

So you think that the deleterious effect might be a more overwhelming one than a positive effect in terms of the impact of numerical analysis on the architectural capacity of the machines.

GOHEEN:

Yeah. Although I have to say that Wallace Givens' paper on the computations of--the reduction of a matrix to a three line form--three diagonal form--is a tremendous piece of work, and I'm sure that a lot of the interest in eigen values of high order was due to his paper. A lot of the development of machines was by matrix calculations.

TROPP:

I notice that Wallace Givens will be the Chairman of a panel of individuals that will

discuss the history of numerical analysis at this meeting.

GOHEEN:

I think that this paper really was a remarkable piece of work.

TROPP:

He's a remarkable individual. As I mentioned he was a student of Veblen's in Princeton during the thirties.

GOHEEN:

Yeah, I'm--I was really quite surprised that he got interested in numerical analysis. And I think that this was the effect of the Federal Government. I know that the reason that I got interested in computing machines, clear and simple, because I was assigned to Harvard by the Bureau of Personnel. If I hadn't been, who knows what I would have done. I would have been a teacher, I think, probably--I don't know, maybe not even a very good teacher.

TROPP:

I don't know, you'd already been exposed to Griffin before the war. I think that might have had an effect.

GOHEEN:

That might have had some effect.

TROPP:

Well, before we run out of tape and I keep you and your wife from doing things you have to do, are there any general comments that you'd like to place on the record at this point? Any thoughts?

GOHEEN:

Yes, I think that--or it's a pious wish--I hope that the Association for Computing Machinery will keep in the path that the founders laid down, for providing a forum for young people to meet with the establishment.

TROPP:

I think it's very interesting as I meet people like yourself who were the pioneers in the field, Grace Hopper is also a prime example of your continued interests in the young people. In certain surrounding areas--

GOHEEN:

Despite the fact that we have philosophical divergence's, in practical affairs Grace is not very much different from Ed Berkeley. From a practical point.

TROPP:

Practical point. As I say, Grace today surrounds herself with people that she's training.

MRS. GOHEEN:

One of your students.

GOHEEN:

Yes, she has one of my students from Oregon State who's working with her rather closely. What was his name? Calla—

MRS. GOHEEN:

Callahan.

GOHEEN:

Callahan.

TROPP:

And, as I say, I find this common among the individuals that I've met, they are still concerned with this theme that you talk about. And despite other interests and philosophical viewpoints, ..

GOHEEN:

I think it's important. The only excuse for the existence of science is man. Science without man is just about as dead as life without a woman.

TROPP:

That would be pretty disastrous. Well, I watch the, you know, the shifts of the mathematical--I think more of the Mathematical Association rather than the Society, and the attempts now to kind of bring it back to what it was at the end of the nineteenth century, when people like Halsted, and Young, and others were involved in this early formative period. And it was, I think, in much the same spirit that you talked about in the formation of ACM to be a place where people could communicate other than writing

letters to each other, a vehicle for people to get together and see what's going on, and try to bring mathematics, which was in a pretty sorry state in the U.S. at that point--

GOHEEN:

Yes. Do you know who the first Ph.D. in America--no, the first PhD in mathematics at Harvard was Byerly. Have you ever read any of his books?

TROPP:

No, but I know the name.

GOHEEN:

Yeah, he was such a formalist. He was the first person to introduce me to applied mathematics.

TROPP:

I was going to say, the state of the art of--the mathematical art at the turn of the century was pretty grim. I know that Halsted was a student of Sylvester at Johns Hopkins in the later 1870's. And without somebody like Sylvester there really wasn't a Ph.D. program worthy of the day.

GOHEEN:

Well, there was Peirce at Harvard, and there was Gibbs at Yale.

TROPP:

Yeah, I'm thinking in terms of just mathematics, rather than physics, or sort of the ground between the two, in terms of what was going on in Europe and on the Continent. I think you could probably at the turn of the century list a couple of dozen names that you would consider important mathematicians, but that would be about it. And these were primarily concentrated in just a few places. If you wanted to study mathematics you really had to leave the country. Rather than leave the country this group of individuals sort of formed a society to try to spread what they knew.

GOHEEN:

And, of course, the money available in Chicago made it possible to attract a real mathematical school around Eliakim Hastings Moore. Well.

TROPP:

OK. I thank you very, very much for your time and--

GOHEEN:

It's been a pleasure.

[End of Tape]