Computer Oral History Collection, 1969-1973, 1977

Interviewee: Harry Polachek
Interviewer: Richard R. Mertz
Date: March 24, 1970
Repository: Archives Center, National Museum of American History

MERTZ:

This is an interview with Dr. Harry Polachek on the 24th of March 1970 in his office in the AEC facility in Germantown, Maryland. Dr. Polachek, would you like to describe your early education and background and the influences that you think today played a role in directing you toward mathematics as a field of your life's work.

POLACHEK:

Well, I think perhaps the two people [who] had influenced my early life and my getting in the field of mathematics were first, my father, who had an entirely different field of interest, but was keenly interested in mathematics. I remember his interest in geometry where he just picked up a Euclid book and did all the problems on his own and helped my sisters out at school. The second influence I think came at the high school level from a teacher, also in geometry, who I think had a very fine way of influencing students by challenging them with solving difficult geometric problems. And I used to get great joy out of going back home and solving these very difficult problems, bringing them back to school. I think those were early influences.

MERTZ:

Was there any particular book of mathematics or about mathematics that you encountered that interested you in the field, or did that perhaps come later?

POLACHEK:

I believe that came later. Actually geometry was the first subject that influenced me and interested me. The idea of proving theorems. I was able to do that apparently better than most people, and I took a great deal of interest in it. Those were the early times. Now when I went to college I decided to major in mathematics, and then I had many influences. Both ways.

MERTZ:

Had you decided actually before you went to college that this was what you wanted—

POLACHEK:

Yes, I think so. I think I decided when I was in high school.

MERTZ:

And at that point you hadn't really perhaps fixed on any field of mathematics but just in general, or was there a particular area of mathematics ?

POLACHEK:

I guess I did want, of course I didn't know the fields too well, but I did very well in mathematics without any necessity of remembering things, and it seemed to be a subject that had a great appeal to me, both algebra later and geometry, and these elementary subjects influenced me a great deal.

MERTZ:

Later in college, in college, were there any particular, in your undergraduate work, any particular teachers or courses or fields that you recall today that stuck out as having been particularly influential in stimulating your interest?

POLACHEK: Well, at Yeshiva University there was a Professor Jekuthiel Ginsburg who was the editor of a journal called Scripta Mathematica. He took personal interest in me and my interests and abilities in mathematics and in turn influenced me. I later also received a fellowship at Yeshiva University under Professor helped, while I was an undergraduate, helped edit the journal, and that held some influence on my mathematics career. Otherwise I can't think of any specific outstanding teachers of mathematics. I took as many subjects of mathematics as I could get a hold of, both at Yeshiva University and at night at other colleges within New York City, including Hunter College which was a girls' school at that time. And it was rather unusual for a man to be in the Hunter College, although they would allow me, in session I was the only man in the class.

MERTZ:

But they had very good math courses offered there?

POLACHEK:

Well, they had courses that I couldn't get otherwise. I usually tried to pack my program as tight as I could, so in addition to studying during the day I studied in the evening and the afternoon and so on. I took courses wherever I was able to get them. I took calculus specifically at Hunter, and then I took some courses at City College I couldn't get at Yeshiva.

MERTZ:

Was this the City College –

POLACHEK:

City College of New York City.

MERTZ:

Did you have any exposure to NYU at all?

POLACHEK:

No, I did not take any courses at NYU.

MERTZ:

By the time you graduated, got your BA in Mathematics, had you fixed then on numerical analysis?

POLACHEK:

No, no, not at all. I just fixed my attention on continuing my studies in mathematics and a desire to get a Ph.D., and I entered Columbia University and received my Masters Degree. And then I was unable to continue full time for my Ph.D., but that's when I taught, first as a fellow at Yeshiva University, and then in the high school system, for financial reasons, and I attended Columbia part time to complete my course work for a Ph.D.

MERTZ:

That raises another question. Many men of your generation were influenced by the depression years, among other things, in terms of how their career changed or shifted, and this possibly was an influence in your life in terms of your graduate work. It might have been reduced had it...

POLACHEK:

Well, I don't know. The cause of my financial problems [was] probably the fact that I lost my father at a very early age, rather than the Depression. I had to essentially earn my own way through school, so I taught and at the same time attended Columbia University, where I completed my course work for a Ph.D. But then the War started, and I had to go to Aberdeen Proving Ground and I took a position -- I didn't have to [chuckle] -- Aberdeen Proving Ground was just before the war, the year before the war, and I had completed my courses, but I had not yet completed my dissertation. I had to return later, in '46, to complete my examinations and my dissertation. **MERTZ:**Under whom did you

do your work at Columbia?

POLACHEK:

I wrote my dissertation under Professor Edward Kasner. K-A-S-N-E-R.

MERTZ:

So you were largely away then during most of the time that you were actually working on your dissertation?

POLACHEK:

That's correct. I wrote most of my dissertation while I was working at Aberdeen Proving Ground, and I think you earlier asked me if I received my interest in numerical analysis early: that wasn't in any event the case. My first exposure to numerical analysis was at Aberdeen Proving Ground in connection with the computation of firing tables. My degree was actually in pure mathematics at Columbia University.

MERTZ:

In which field?

POLACHEK:

Well, I guess they used to have three fields in those days, analysis, geometry, and algebra. I guess my field was mostly analysis and algebra, although I did write my dissertation more or less on my own at Aberdeen Proving Ground, and it had a flavor of some numerical analysis in it.

MERTZ:

So it reflected to some extent –

POLACHEK:

It reflected to some extent my experience at the Ballistics Research Laboratory at Aberdeen Proving Ground.

MERTZ:

And at that point in your life obviously computational problems became a major preoccupation.

POLACHEK:

Yes, that was my occupation when I came to Aberdeen Proving Ground my primary job was to calculate firing tables. And that's where I became acquainted with the Bush Differential Analyzer at Aberdeen Proving Ground, I think I mentioned to you on the phone.

MERTZ:

And your initial exposure then was to analog machines.

POLACHEK:

Well, to hand calculators, the kind, you know, like Marchant, Friden, and Monroe, and to the Bush Differential Analyzer.

MERTZ:

Did you ever have any occasion to go to MIT to visit the Rockefeller Differential Analyzer, which was sort of the second Bush machine?

POLACHEK:

No, I did not.

MERTZ:

And these were essentially ballistics problems, that you were running?

POLACHEK:

These were ballistics problems. They were the computation of firing tables for US guns during the War--for the US Army.

MERTZ:

Do you recall, at that time were there any modifications made to the machine itself, to, say serial changes in the machine from an engineering point of view, that facilitated the solutions to specific problems such as the computation of ballistics tables.

POLACHEK:

I don't recall any major modifications. There were always minor modifications being made to adapt the machine for the computation of trajectories, that's what it was there for, to compute trajectories which were then analyzed and used in firing tables. So that's what we did practically 100 percent of the time, compute trajectories.

MERTZ:

And during the course --I believe you were there for several years.

POLACHEK:

Well, let's see. I went to Aberdeen in, early in 1941 and I wasn't there too long. I would guess about a year or a year and a half at Aberdeen. I was still associated with Aberdeen Proving Ground and had moved to the University of Pennsylvania where Aberdeen had sort of an annex or a group working on computational tables, and I spent a little time there. I finally left I think in the fall of 1943. So my total association with the Ballistics Research Laboratory I guess was about 2 1/2 years, but part of it was spent at Philadelphia.

MERTZ:

During that time do you recall meeting among your colleagues or visitors either in Philadelphia or in Aberdeen, well, for example, one consultant, I think even at that point

POLACHEK:

Von Neumann, yes.

MERTZ:

was John von Neumann.

POLACHEK:

Yes, yes, I had an opportunity to meet von Neumann very briefly at that time; got to know him much better later in my career. And of course I met many other outstanding mathematicians and physicists and astronomers at Aberdeen Proving Ground.

MERTZ:

Would you care to mention some of the names of some of the men who –

POLACHEK:

Well, if I can remember them. Of course, I met H.H. Goldstine who, well, who is now at IBM. There was a Dr. M - we,, Dr. Morey - M-O-R-E-Y. It's not Murray. There was Dr. Leo Zippin. There was a Dr. Hubble who was a very famous astronomer at Princeton. And a number of outstanding people. I just don't recall offhand.

MERTZ:

When you were at Philadelphia did you get acquainted with the people at the Moore

School?

POLACHEK:

Yes, I met them, but I didn't work too much at the Moore School. I…there was a group of about between 50 and 100 young ladies who were human computers who were engaged in calculating firing tables and bombing tables for the Aberdeen Proving Ground, and I was associated -- They were using hand calculators, right.

MERTZ:

Do you remember primarily which machines they used.

POLACHEK:

They used a variety of machines, Fridens a great deal, Marchants, a variety of other machines. And at the same time while this was going on there was of course the development of the ENIAC going on at the University of Pennsylvania, primarily under H. H. Goldstine. And others. Let's see. He was the administrative officer. And I got to know these people although I did not work with them directly.

MERTZ:

Was there at that time an interest in applying digital techniques or more sophisticated analog techniques to the table computation problem?

POLACHEK:

Well, the primary purpose of building the ENIAC was to replace the Bush differential analyzer in the computation of trajectories and to render our computation much more efficient.

MERTZ:

But your involvement was with the group that was ongoing at the time. This was a developmental thing.

POLACHEK:

Right.

MERTZ:

Then, well that brings us up to when you left this project of the Ballistic Table computation or trajectory computation problem and moved to, to the Navy?

POLACHEK:

To the Navy, yes, to the Bureau of Ordnance. And I joined a group there called the Explosives Research Group, I guess approximately, I don't know the exact name, but it was a group that was interested in the effects of explosives rather than in the actual chemistry of it. And one of our consultants was John von Neumann who used to spend about one or two days a week with us. There was a Raymond J. Seeger, S-E-E-G-E-R, who is now with the National Science Foundation. There was a Dr. Gregory Hartmann who is now the Director of the Naval Ordnance Laboratory, has been for many years. There were a number of other famous people. I should mention Prof. George Gamow. And we had another consultant who never visited us, but his name was in the directory, his name was Albert Einstein [laugh].

MERTZ:

Did you ever have occasion to meet him?

POLACHEK:

Yes, I had been at Princeton once or twice. I had met him at Princeton on several occasions.

MERTZ:

I see. Well, what brought you to Princeton?

POLACHEK:

We were doing work on explosives research and there were a number of people who were connected with Princeton who were doing work on explosives effects research. Of course, von Neumann was one of them, because he was at the Institute of Advanced Studies. But at one time we had a conference at the Institute for Advanced Studies on explosives effects.

MERTZ:

This was shock waves or other kinds?

POLACHEK:

Well, shock waves was one of the primary studies, and that conference had many discourses on shock waves and their effects. von Neumann himself of course was very much interested and was a pioneer in the study of shock waves.

MERTZ:

Do you recall, was this during the War that this took place.

POLACHEK:

Right, this was during the War. I came to this group in 1943 and left it after the War.

MERTZ:

When they had this was this a special conference or symposium?

POLACHEK:

Oh, the symposium, I'm sorry, I thought you meant the group. The symposium was held during the War, right. It was a special conference of outstanding people in the field in the study of explosives and shock waves and so on.

MERTZ:

Do you have any recollection whether the talks, the papers presented, the discussions were ever recorded?

POLACHEK:

We, I believe --let's see. I don't recall. I think the papers were published, most of them were. But I don't know that they had proceedings. But I presented one of the little papers there, and I may have a copy somewhere.

MERTZ:

That would give me the date...

POLACHEK: Yes it probably would. I have the paper. While I was at Aberdeen Proving Ground doing work primarily on firing tables for..., Camden, and so on and so on there was another group at the Ballistics Research Laboratory primarily responsible for bombing tables. And the way these sights like Norden bombsights were controlled or the anti-aircraft things were controlled was by a cam which was cut out in accordance with computations which were derived by our numerical methods. In other words, we did the preliminary computations which went into the bombsights or the control mechanisms for antiaircraft guns.This was a part of our work.

MERTZ:

Fire control research as you may recall was very scattered. There were many groups all over the country doing different parts of the work. One of the most difficult things for the historian of this period is to try to locate first of all the different groups and secondly what documentation there is, because this was war time and the materials of war at that

time all classified, and there was no systematic way of storing or keeping these materials.

POLACHEK:

Well, I believe I can refer you to some people who are quite well acquainted with some of this. One is Professor E.J. McShane, professor of Mathematics at the University of Virginia. He wrote a book on ballistics and I guess bombing tables and firing tables. And I think he is familiar with some of the work that went on at that time. I am also quite certain that there are a number of people at Aberdeen Proving Ground still remaining who are familiar with it. A lot of the companies I remember specifically in connection with the fire control tables I guess, the Sperry Company which later joined the Sperry Rand Company. They were on Long Island. They were primarily interested in the firing tables and the cams for firing. There was the Norden Bombsight which was also developed about that time and was reputed to be extremely accurate at that time. The tables for that, I'm not absolutely certain, but probably were also computed either at Aberdeen or through work originating at Aberdeen.

MERTZ:

I would appreciate it if you do have a copy of the paper you presented.

POLACHEK:

I believe I have a copy of the paper. There's one problem, I'm not certain that the paper was published directly with the meeting. It may have been published in advance of the meeting, but I will be glad to give you a copy of the paper and try to recall the exact meeting. ...

[Recorder off]

At that meeting at the Institute for Advanced Studies there were many famous mathematicians and scientists attending. For instance Albert Einstein attended some of the sessions and I was extremely proud that he sat in at the talk which I gave. But one little incident that was very amusing to me and very interesting historically took place during a talk which was given by Dr. Kirkwood, K-I-R-K-W-O-O-D, the physicist, and he was speaking about some phase of shock wave theory, and the meeting was held at the lobby of the Institute for Advanced Study. They put together a number of couches and these big large chairs. There were about fifty people in the room, also little fireplaces overlooking the patio and there was a blackboard in the room. And Kirkwood was speaking. He's not living any more, but he was speaking, he was giving this very technical talk. And he had a very low voice, you could hardly hear him. Of course everything was pretty quiet at the room. Everyone was sitting in their chairs or couches. One of the people in the room was Prof. Norbert Wiener the famous mathematician, --you know, he was real stout--and he picked himself a great big easy chair and sat down in the second row right in the center. And Kirkwood spoke with this low voice and everyone was bending over to hear what he was saying. Many minutes did not pass

before Norbert Wiener sort of fell asleep and before too long, another minute or so he started snoring. And he snored so loud that you couldn't hear a sound of Kirkwood's very [laughing], very erudite talk. And poor Kirkwood was going on that way, and this went on for about a half an hour. Everyone got very fidgety and nervous. And everyone was embarrassed and didn't know what to do about it. And finally the agony ended and his talk was over. As soon as the talk was over the chairman got up and said are there any questions. And immediately the snoring stopped, Professor Wiener woke up and popped up one of his hands. He had a question. It was really remarkable. Very difficult question.

MERTZ:

Did his question have bearing on...

POLACHEK:

[Laughing]. Oh, yes. It was very meaningful. Very related to the talk.

MERTZ:

Incidentally, we're collecting recordings of lectures and talks given by John von Neumann and Norbert Wiener, and others, and we did get a tape recording of a very interesting talk on the early history of computing machines in the thirties that Wiener participated in, the talk given in 1955 in New York. It's quite an interesting ..talk. That's one of the reasons I asked you if the sessions were recorded, because we're interested in trying to retrieve or locate the recordings of the men who are now dead but were active –

POLACHEK:

I'm quite certain they weren't recorded in the sense of being recorded on tape. I don't think there were Proceedings, but there may have been. I think most of the talks were published.

MERTZ:

That must have been for you a very interesting and exciting time.

POLACHEK:

It was a very exciting time in my life, yes. I was young at that time, full of energy, and the opportunity to meet so many outstanding people was very interesting.

MERTZ:

Did you ever meet Kurt Goedel?

POLACHEK:

No, I'm afraid not.

MERTZ:

He was another one of the -- he was a rather shy and retiring man and didn't particularly like to go to large meetings, I gather, still doesn't.

POLACHEK:

Well, also, this was not his interest. He's not interested in shock waves. We were interested primarily in shock waves. But also, of course, I was at Princeton on many other occasions, but I didn't stay there very long, didn't work there.

MERTZ:

When you moved over then to the Navy, to the Bureau of Ordnance, did you continue -- in terms of your involvement with computing machines, you might want to pick up the story.

POLACHEK:

Yes. When I moved to the Bureau of Ordnance my interest in machines was really kindled by one man primarily, and that is John von Neumann. And it was done in a variety of ways, but primarily by the fact that I was the computer of the group, in the sense that I had done, I did computations of the firing tables at Aberdeen Proving Ground. And whatever problems there were in the group I was the one elected to do it. So one day Professor von Neumann came to me with a very complicated problem in computation of the motion of a fluid in the area of a cylindrical explosive. And we set down the equations for that, von Neumann writing out most of the details. And I was given the job to solve this on old-fashioned IBM machines, which were available at the Bureau of Ordnance. Some of which were available at the Bureau of Ordnance; some were available at the Bureau of Ships at the Navy, and I used both these facilities to solve this problem which was really for von Neumann.

MERTZ:

Were these the CPC machines?

POLACHEK:

Well, the CPC machines were not yet available at that time. This was pre-CPC time. This was the time when the most advanced deal were the IBM tabulators and the IBM multipliers, I think they used to call them 601s or 602s or 604s. And these were IBM multipliers. They didn't have any negative signs, they just had positive numbers. And

these machines had to be wired manually. And those were the machines that I used to solve this cylindrical explosion problem for von Neumann. That was one of the problems that I solved for von Neumann.

MERTZ:

These were these ordinary ...

POLACHEK:

Yes, these ordinary IBM punch card machines that were used primarily for personnel data or accounting.

MERTZ:

This was still during the War.

POLACHEK:

This was still during the War, right.

MERTZ:

About 1944?

POLACHEK:

I would guess, right, about 1944 would be good guess because I came there in '43.

MERTZ:

When von Neumann came to you and he sat down, did he write these out? Go over the equations and write them out?

POLACHEK:

Yes. He wrote them out in very great detail and went over them with me and

MERTZ:

Do you recall –

POLACHEK:

as a matter of fact I had some of these writings very recently, and before you arrived here I was looking for them. I may still have them around someplace in his own handwriting,

but I just couldn't find them. So I'm not absolutely certain. If I can find them I'll be very happy to make them available to you. I don't think they were ever made public in any way.

MERTZ:

This is another thing that I am doing in connection with von Neumann and the IAS computer, and that is attempt to locate and identify all the documentary materials that relate to him, in addition to this published series of reports which he and Goldstine and Burks and –

POLACHEK:

I may be able to locate those equations which he wrote in his own handwriting. Now, other computations I did with him were of course the computations related to -- with von Neumann and with Dr. Seeger -- were computations related to shock waves and their interaction and their reflection. This is the work that I did perhaps the most original work in personally. I later wrote some papers on the refraction of shock waves, and one of the effects that was studied with von Neumann's aid and the aid of other scientists was this question of shock waves and how they interact with each other. One principal anomaly was what is called now the Mach effect. This was the anomaly that when two shock waves interact, or when a shock wave reflects from a wall, the returning shock wave is not always a regular shock wave. When the glancing angle becomes too large the reflecting shock wave takes on the form of a triple wave. There are actually one wave -- three waves involved and a discontinuity. And when we tried to calculate the regularly reflected shock waves, we found that our calculations agreed very well, that is, the theoretical calculations agreed very well with the observed values for - both for the angle and the strength of the shock waves. These experimental values were obtained by using shock tubes-and that's another relationship with Princeton, by the way. There was a Dr. Blakeney, who was the head of the Physics Department later, who had one of the early shock tubes at Princeton. Now on the other hand, in the case of Mach reflection the theoretical computations that were carried out did not agree with the experimental values, and it was a great challenge to both Dr. von Neumann and some of his collaborators, some of the people at Princeton like V. Bargmann, B-A-R-G-M-A-N-N, Valentine Bargmann, to explain this anomaly. This Mach reflection was important in a practical connection. It was theorized by von Neumann that -- well, not only by von Neumann, but it was easy to see from the theory of the reflection of shock waves that the reflected wave is sometimes stronger than the incident wave and that strength increases as you get close to the Mach reflection. So that theoretically you should get a best, maximum blast effect from an air blast like an atomic bomb, like that of an ordinary bomb or an atomic bomb exploded in air at the point where the Mach wave occurs. So that before the explosions at Nagasaki and Hiroshima von Neumann calculated [what] the exact location of the bomb should be in order to get the maximum effect. In other words, the bomb was blasted not close to the surface of the earth but at a certain distance from the earth. I have a little story about that... Von Neumann attended a meeting just before this first bomb blast, let's see, Hiroshima came first. There was a meeting. By the way, von Neumann worked with

the Manhattan project part time. While he was our consultant, and he used to go away for a little while. No one was supposed to know where he'd go, but of course we knew and we'd say he went to Shangri-La. Then he'd come back and discuss with Professor Gamow and some of these others about the possibilities of an atomic bomb being developed. At any rate, to come back to the story about the blast at Hiroshima: It was exploded a certain distance from the ground because of this Mach effect, because von Neumann calculated that that effect would be greatest if it's exploded at a specific distance. Now he did that calculation by hand with pencil and paper on his way from Princeton to Washington, and he found--I forget the exact number of feet, but something of the order of perhaps a thousand feet the blast effect would be greatest. He gave me the exact number. And he went to this meeting, he had a high level meeting, and he said to me very casually, "Harry, will you please compute this number very quickly, because I'm going to give this number to the people who are going to use it at the Pentagon and I want to make certain that my calculations are correct." I went through a very careful calculation by using a desk calculator, I worked on it for about four or five hours. And sure enough his number, which he obtained mostly in his head and partially with pencil and paper in probably a matter of a half hour, was absolutely correct.

MERTZ:

Which raises -- Did he happen to leave the work he had done? ... with you? On this computation?

POLACHEK:

I did the computation at my desk at the Navy Department, but he left. I mean I did it on my own. He asked me to check –

MERTZ:

Did he leave any of his results?

POLACHEK:

Oh, you mean -- he left his notes, yes; which, of course -- it was not very easy to understand what exactly they were.

MERTZ:

I was going to ask you. Do you recall, when you worked with him what kind of person was he to work with?

POLACHEK:

Very fine person. Very nice person, getting on with - very personable. Very friendly. And one would never believe it possible that one man would be such a great man and yet

at the same time so modest and so humble.

MERTZ:

Was he very unassuming?

POLACHEK:

Yes, very unassuming. We called him Johnny, of course, rather than Von Neumann.

MERTZ:

Yes, I take it everyone called him that.

POLACHEK:

Yes, everyone called him Johnny.

MERTZ:

But he was also very, quite incisive on his problems?

POLACHEK:

His mind was really unusual and quite wondrous to watch. He just loved to do problems, to think, to do puzzles, he used to enjoy that. His thinking, of course, and his work had great depth to it. Most people who have great -- His thinking was very deep and incisive and normally one associates a person who thinks deeply with someone who thinks slowly and methodically and carefully in his own room, but that was not the case with von Neumann. He thought very quickly and had an uncanny ability to relate for instance one field to another. I'd like to give an example of that in just a moment. But he remembered most of the things that he knew so that when he read something--he very seldom used many books or any books, he just sat down at a desk, and in a little note wrote out all the equations that were necessary. To give that example as to how he related one field to another, we had a well-known astronomer working for us at that time during the War with that same group. His name was Keenan, K-E-E-N-A-N. He was working on an optical problem, which he couldn't solve for a number of weeks. There were some difficulties about which he just couldn't reach a conclusion. He asked various people and worked on it himself for a long time, and we suggested that he ask von Neumann. Of course he was a very shy man and hesitated to do that. Finally, after a long period of time he decided he will ask von Neumann. And von Neumann looked at the problem. It didn't take long, just a few minutes, and said "well, for goodness sakes, that same problem occurs in a different field, in the electromagnetic field."..Let's see, now, this is an entirely different field, ...which field -- and sat down and wrote out the difficulty, wrote out the equation, wrote out why there is difficulty, and explained what the difficulty was in just a matter of just about ten to fifteen, twenty minutes.

MERTZ:

I had heard stories about him reviewing looking over someone else's work and just as if he were reading a newspaper, finding errors or, or –

POLACHEK:

Yes. Well, this was a matter of relating two entirely different fields which had essentially the same difficulty, but most people...well, there were very few people or there were no people that could really make that connection, know all these fields quite so well to make that jump.

MERTZ:

He also apparently had quite a sense of humor I gather

POLACHEK:

Yes, I think so.

MERTZ:

The way people talked about him. He was not a humorless man, ny a long shot.

POLACHEK:

Yes, I guess so. No, he wasn't. We used to have a great deal of fun, I should say, have a good time, especially when he and Professor Gamow used to join us for lunch, and they would have discussions about various non-mathematical subjects or mathematical subjects. Of course, Professor Gamow had a sense of humor, too, very much so, played all sorts of practical jokes.

MERTZ:

Did this period near the end of the War when you were with the Bureau of Ordnance of the Navy, was this the period that you were most closely connected with von Neumann?

POLACHEK:

That's correct. That's the principal period in which I was connected with von Neumann. The other connection was -- well, occasional connection -- by visiting Princeton, or in connection later in building the computer called the NORC computer. N-O-R-C. At that time he was a consultant to IBM, and I got to talk to him. And then of course on various visits I used to see him from time to time.

MERTZ:

Well, if you do find anything in the way of manuscript material that relates to him I certainly would appreciate knowing about it.

POLACHEK:

I'd be glad to make it available if I can find it.

MERTZ:

Then, the influence of von Neumann was quite considerable in your own involvement with computing machines as such.

POLACHEK:

That is correct. I got to learn how to use machines. There was our first IBM punch card machines, and later, of course, I solved some problems on the MARK I calculator. I solved a problem on the MARK I calculator.

[End of Side 1]

[Start Side 2]

POLACHEK:

I was involved for a very brief period with the MARK I calculator trying to solve a problem related to shock waves, and stayed at Harvard for a matter of a week or so. Now, as soon as better machines became available, of course, we tried to use those machines to solve our problems. One of the problems of interest at that time was the shape charge effect, one of the problems in explosives was the shaped charge effect. So we tried to calculate what would happen if a charge is of a certain, if a charge has a certain shape, and how that would increase the forces in a certain region and perhaps cause, let's say, piercing of armour and things of that sort. That was one of the problems being solved. There were a number of other problems, but, going back now, as larger machines became available we tried to use those to solve our problem, and of course the first one that became available was Mark I, and it was very much related to the Navy because, of course, Aiken was a Commander in the Navy, and the Mark I, the Navy helped develop the MARK I calculator, primarily through the Bureau of Ships although also through the Bureau of Ordnance. And then after the MARK I -- Well, getting back to the MARK I. Von Neumann used the MARK I, to the best of my recollection to solve a very important in the design of the first A-bomb, the problem is the problem of implosion. The bomb is exploded by an implosion process. I believe that's not classified any more.

MERTZ:

Not [unclear] this point.

POLACHEK:

You know about that. So at any rate he did some calculations on what effect -- well, these were theoretical calculations, and he used the MARK I to find out what forces one obtains by taking, let's say, a spherical explosive and imploding it, that is, starting its explosion from the outside rim and going inward, and see what forces and pressures could be attained in that way. And the results were obtained theoretically, of course. These were approximate. And he did try to use the MARK I for that purpose. I understand that similar calculations were carried out on ordinary punched card machines at Los Alamos, and I think the leading light there was Professor Bethe, and I think that Bargmann, V. Bargmann worked with Bethe on that, but I think Bethe did the theoretical work.

MERTZ:

I think that that's right, and we are interviewing the man who is, I believe the head of the IBM punched card computing at Los Alamos at this time, in connection actually with the later development of MANIAC, but –

POLACHEK:

But I think that von Neumann did a similar calculation on the MARK I calculator.

MERTZ:

I didn't know that he actually used the MARK I.

POLACHEK:

OK. Now later from the MARK I calculator we went on to use other computers, and my personal experience was with the use of the IBM SSEC, Selective Sequence Electronic Calculator, which was located in New York at 590 Madison Avenue.

MERTZ:

[What problem] did you try to solve...?

POLACHEK:

There I solved a problem of a refraction of a shock wave, that is, what happens to a shock wave as it enters from one gas to another. And I did a very elaborate calculation on the computer.

MERTZ:

Boundary -- ?

POLACHEK:

Yes, a boundary problem, and call it [?] refraction. That is, if a shock wave originates in one gas and then hits a boundary, let's say the first gas is air and then it hits neon, what happens to it? Well, sometimes the shock wave is transferred, but sometimes the shock wave is reflected. Sometimes the very refraction wave is reflected. And sometimes there is a transition between the shock wave and the refraction wave.

MERTZ:

I meant to ask you another question in connection with the Mach effect, and that is, didn't this also have a great deal indirectly to do with aerodynamic design theory as it was developed in World War II? I believe at one point they reached a situation where aeronautical design engineers simply did not have an adequate theory to design aircraft because of the limiting conditions that their theory had previously had over ...

POLACHEK:

Well, it certainly enters in that, and, of course, engineers find ways to solve problems practically and get around problems. But one way the Mach wave does occur on airfoils or on aircraft especially when you get close to the velocity of sound, but also the Mach wave occurs in the nozzle of a rocket, and the Mach wave occurs also in the nozzle of a wind tunnel, which caused a great deal of problems. Now when you design a wind tunnel, if it's a subsonic wind tunnel, of course, very much subsonic, you have no problem. If it's supersonic you usually [end up with] no problem. But if you get close to sound you run into a nozzle which could get stuck, because there is a Mach wave just standing in the nozzle and you can't operate the nozzle. In other words, the question of building the wind tunnel also runs into this problem of the Mach wave.

MERTZ:

This particular boundary condition, which was, you know, in a sense a discontinuity, represents an area that's being added –

POLACHEK:

Causes difficulties, anyway.

MERTZ:

Apparently, theoretically, if I recall correctly there was no really fairly well worked out theory.

POLACHEK:

That's right, there was no well worked out theory, that's correct. Because the simple theory they tried to apply to it didn't work. There have been theories worked out later for certain special cases, but I don't really think the entire problem has ever been solved yet.

MERTZ:

Even now. So you then moved to the SSEC. What -- your view of these machines that you did run problems on -- did you find some more amenable to the kinds of problems you were running than others? I think we talked about the - Mark I ---

POLACHEK:

MARK I and the SSEC. Those were early machines. And the Mark I, of course, by modern standards was a very slow machine. It took about one second to do a multiplication. We thought that was very fast at the time. The SSEC did about 10,000 calculations a second. That's my recollection. I may be wrong on that. But anyway, we thought it was extremely fast. But unfortunately errors occurred all the time, and if the machine ran for one second without making an error that was unusual. I remember staying up one whole night solving a problem, and whenever we had a half a second errorless running time we used to congratulate each other and say "Wow, we did five thousand calculations [laugh] without making an error."

MERTZ:

Now, I was wondering, with the Aiken, with the MARK I, this is not really a problem.

POLACHEK:

With the MARK I it's not a problem, but, of course, the MARK I is a mechanical machine and it's very slow. The early electronic computers did have errors, but they were automatically checked. For instance, the SSEC had an automatic checking circuit and any time it made an error it automatically indicated that it made an error. So we did know an error occurred, and so we'd have to reset the program and start again from some starting point. Of course, we'd try to get a program to run that we'd have it run a certain time and start it at a good starting point so we don't lose all our computations.

MERTZ:

I was going to say, did you then sort of modularize your program, break down your programs?

POLACHEK:

That's right, modularize would be a good word. Break down our program into small

units, and then we'd start at a point where we hadn't lost too much time.

MERTZ:

Roughly when was this? Was this also toward the end of the War or was this after the War was over?

POLACHEK:

It probably was -- yeah, that was after the War. Sorry, no probably. That was after the War, the SSEC was after the War. I guess I should go back now to say that we used the SSEC, but that was actually when I joined the Naval Ordnance Laboratory, that was after the War.

MERTZ:

Did you, I believe you did, participate in the -- there was a series of symposia sponsored by IBM, that Cuthbert Hurd edited.

POLACHEK:

Right.

MERTZ:

Actually this is a sequence of publications that goes way back to 1940, but it stopped at an earlier period before it started again.

POLACHEK:

Right, yes.

MERTZ:

But about '47, either the fall of '46 or early '47, there was this series that dealt with applications and I think they had a series of ... papers. Von Neumann presented one of the papers in this sequence of symposia.

POLACHEK:

Right. I did participate and I did present a paper, and I believe that paper was in connection with the calculations of the refraction of shock waves on the SSEC. I'd have to look back to see exactly where that is, what issue.

MERTZ:

You have the series?

POLACHEK:

We have it. [laugh]. You'll have to remind me. Do you have Von Neumann's talk on the NORC when the NORC was dedicated? N-O-R-C. The NORC was a calculator that was built also by IBM and von Neumann I believe gave a talk. You probably have that.

MERTZ:

Was it published?

POLACHEK:

I believe it was.

MERTZ:

Well, I don't have it. But –

POLACHEK:

It may even be in here but I haven't looked at it. OK. Well, it was my recollection that von Neumann gave a talk at the dedication of the NORC calculator, which was held in New York City. I believe Watson attended that meeting, too. A special meeting held after the completion of the NORC. I believe that was recorded.

MERTZ:

In those days, do you recall, in the late forties, was there much interest generated in the mathematics community as such in computational problems, or did this tend to be a group who, because of their previous experience in computational problems, was a little bit separate from the general main stream of mathematics?

POLACHEK:

I think the latter was generally true, although there were a few fundamental papers that were written prior to that time on computational methods. One of them was a paper by Courant, Lewy, and Friedrichs, which was written about 1923, which related to computational methods. Norbert Wiener also had written a paper about computational methods, although I never quite understood it. [Laugh].

MERTZ:

Are you referring to one on the Fourier transform?

POLACHEK:

No, this was a question of stability in differential equations, and Norbert Wiener's paper, I don't know -- there was a controversy between Norbert Wiener and Courant about priorities there -- but Norbert Wiener's paper probably occurred, probably was written in this MIT -- you know, MIT has a journal in applied mathematics; it used to have anyway. I forgot -- what's the name of it? I forgot the exact name of it.

MERTZ:

There is one called -- well, it's not the Journal of the Franklin Institute?

POLACHEK:

No, not the Franklin Institute.

MERTZ: It was mainly MIT?

POLACHEK:

Yeah. I'd have to look at it. I'm certain there are references to it. I can look at a reference.

MERTZ:

Did you have much exposure to the Courant group?

POLACHEK:

Yes. Well, you say the Courant group. I should have mentioned, I will forget many things, I'm sure, one of our consultants at the Bureau of Ordnance was Professor Courant, another one, of course was Friedrichs, Professor Friedrichs, and I got to know Courant and Friedrichs very well as a result of that. And, of course, Friedrichs was a fluid dynamicist and Courant, of course, wrote his book on shock waves and shock waves had supersonic flow, which I guess I have here on my desk. And he tried to incorporate in that book everything that is known in shock waves and included some of the calculations that I did on triple shock waves. As a matter of fact, he calls them the P waves, or the Polachek waves.

MERTZ:

Aha.

POLACHEK:

I discovered a new family of triple shock waves [laugh] which no one ever knew existed.

MERTZ:

Did you go up to his Institute and spend much time, or did they as consultants come to you?

POLACHEK:

I think primarily Courant used to visit us. Yes, I used to go up to New York, but I -- hardly to, to do what? Either to see Friedrichs or someone there, but I didn't have much discussion with the Institute. I did have discussions with a computational group under Dr. Arnold Lowan who was responsible for calculating a large number of tables, Bureau of Standards tables, that originally started as a WPA project that they calculated tables of e^x, sine x, cosine x. There were a large number of tables. I think you are familiar with them.

MERTZ:

Yes. [Pause].

POLACHEK:

My next position was at the Naval Ordnance Laboratory, and I was in charge there of a mathematics group which also had as its primary function carrying out computations. At that time commercial computers were not available, so we started out by using standard IBM punched card equipment, including the CPC. That's when the CPC got into operation.

MERTZ:

Was this in the late forties?

POLACHEK:

OK. That was in 1946, I believe, or '47, when I -- was it forty—

MERTZ:

Was this when you were in the Naval Ordnance Lab?

POLACHEK:

Yes, Naval Ordnance Lab.

MERTZ:

Then it was around '47.

POLACHEK:

'47. OK. In 1947 I transferred to the Naval Ordnance Laboratory.

MERTZ:

And what kinds of problems did you run?

POLACHEK:

We, well, we ran all types of problems that arose at the Naval Ordnance Laboratory for various groups. The primary interest of our own group was ballistics -- well, not ballistics, but hydro ballistics, and we had wind tunnels and also underwater entry problems and things of that sort.

MERTZ:

Did you by any chance in this time in the late forties have any contact with any of the groups, I heard of at least three, which were interested in antisubmarine warfare and the use of probabilistic, well, computing machines to take the data, well, for plotting and tracking submarines?

POLACHEK:

Oh, for tracking submarines.

MERTZ:

This is very much related to air problems too, the multiple submarines, the air defense problem, which also came a little later, but I believe the ASW problem was earlier.

POLACHEK:

No, the anti-submarine warfare problem, I was not involved in the anti-submarine warfare problem directly. We may have done some computations on our computing machinery -- I didn't know all the computations that were run -- but I was not connected directly there. However, I did get into the anti-submarine problem much later in my career at the David Taylor Model Basin, or what is now called Naval Ships Research and Development Center.

MERTZ:

...I got you off. Let's go back.

POLACHEK:

OK. OK. I was involved in wind tunnel and shock waves again, and computations, and applied mathematics in general.

MERTZ: And this was done largely, the machine that you initially worked on with these problems was the CPC? **POLACHEK:** The machines that we worked on were a group of IBM punched card machines, including the CPC as one of them, but we had a whole group of machines. **MERTZ:** At about this time, as you may recall, the Navy, the Office of Naval Research, was set up in the late forties, after the War, and was

supporting a variety of computer research and development projects. Was there any associations between your group and the ONR group or their activities? They were sponsoring, for example, they participated in part in the IAS computer, the Whirlwind computer.

POLACHEK:

No, we were not in any way involved in the development of computers. We had a very small mathematics project with the Office of Naval Research as applied mathematics.

MERTZ:

I see. Were these computational problems essentially?

POLACHEK:

No, the mathematics was more theoretical, numerical analysis.

MERTZ:

Then what next area of machines did you get involved in?

POLACHEK:

While I was at the Naval Ordnance Laboratory, the Naval Ordnance -- we proposed that the Navy buy a large computer, develop and buy a large computer, and we finally settled on the NORC, N-O-R-C, Naval Ordnance Research Calculator. And IBM was developing that, and I became involved in that, in the development of that computer. It was originally supposed to go to the Naval Ordnance Research .. Naval Ordnance Laboratory.

MERTZ:

Is this the N-R-L, meaning –

POLACHEK:

Naval Ordnance Laboratory, not NRL, NOL, N-O-L, at White Oak, Maryland. OK. As time went on there was competition between the Naval Ordnance Laboratory and the Navy Proving Ground at Dahlgren for that same computer, NORC, although we had essentially been responsible for developing it and for proposing it. The Naval Proving Ground at Dahlgren claimed that they were the principal computer facility for the Navy and therefore they should get the Naval Ordnance Research Calculator. Eventually they did get it, and the computer never went to the Naval Ordnance Laboratory. I was involved in its development and had many meetings at IBM, and also von Neumann got involved in some of it, in developing that particular computer. OK. And since the

computer never went to the Naval Ordnance Laboratory even before the decision was made I had made a decision to go to, well it was then called David Taylor Model Basin in 1952, which was acquiring one of the early UNIVAC Is, the UNIVAC I number 6; right. And it was also getting the responsibility for doing computations for the Navy Bureau of Ships. So I was asked to form a new laboratory at David Taylor Model Basin in computing and applied mathematics, with the UNIVAC being the principal computing facility for that mathematical laboratory.

MERTZ:

Could I just backtrack once a little bit. Do you recall what areas of the NORC that von Neumann contributed, and in what areas you were directly involved, in, in terms of development of the NORC computer?

POLACHEK:

Von Neumann was at that time a consultant to IBM and he attended some of the meetings at which we discussed the various possible, well, now they call it architecture for the computer, how to arrange the computer and what the input-output equipment should be, and so forth. He attended our meetings. The computer was being developed in New York City at the Watson Laboratory near Columbia University, and I was involved first in proposing it and secondly in these various meetings which were held in order to determine the actual characteristics of the NORC computer.

MERTZ:

This was a parallel machine?

POLACHEK:

Well, parallel only to a certain extent. It was a very fast machine for its day; but, let's see, parallel in what sense?

MERTZ:

Was it asynchronous?

POLACHEK:

No, it was not asynchronous, it was synchronous.

MERTZ:

And its storage? I was wondering about storage. This was in the early fifties?

POLACHEK:

This was in the late forties and the early fifties, yes. Let's see, Its storage was drum, I think, mainly.

MERTZ:

ERA ...?

POLACHEK:

No, it was IBM.

MERTZ:

IBM.

POLACHEK:

IBM drums. But, let's see.

MERTZ:

Did they have any internal storage like a Williams tube?

POLACHEK:

Let's see. It also had a Williams tube sort of storage, yes.

MERTZ:

So in that respect it had similarities to the IAS computer.

POLACHEK:

It had similarities to the IAS computer. And one of the questions, of course, was the amount of checking it should include, and another question was whether it should be binary or decimal. And there was a great debate going on at that time whether this machine of the future was going to be binary or decimal. The decision was made to make the NORC a decimal rather than binary. And this was one of the questions that was debated with von Neumann on a number of occasions.

MERTZ:

What was his, von Neumann's, position?

POLACHEK:

I think von Neumann's position was mainly in favor of binary. The argument against binary was the difficulty of people to get accustomed to using binary notations. But he felt that this could be overcome.

MERTZ:

There were I believe at that time converters where you could just simply take ... representations of numbers and just simply mechanically convert them.

POLACHEK:

I think you must remember that at that time machines were not as reliable as they are today, nor did we ever realize that they could be as reliable as they are now. And we constantly had to go into the machine in order to, for instance, check out our programs, to actually see the numbers as they are in the machine, and we thought it would be much easier if the numbers were decimal and we could easily spot errors and things of that sort. So this was a consideration at that time. As it turned out the binary machines won out in the long run and rightfully so.

MERTZ:

Was your primary activity or concern with the machine on error checking so far as running programs through it?

POLACHEK:

That was one concern, yes. Of course, the reliability of the machine was a great consideration. The question of how much checking it should have, and what input-output it should have, and how fast that should be, compared to the operations, was a major consideration. And also as you say checking, the checking of the programs as they went through the computer, which was a primary consideration at the time.

MERTZ:

Were there any other specific concerns that von Neumann was involved in with the NORC that you recall, other than the binary-decimal problem? How about floating point/fixed point arithmetic?

POLACHEK:

I think floating point/fixed point was being discussed, but to the best of my recollection it was not one of the major points of discussion. Anyway, von Neumann was involved in much more than just the binary decimal question. He was involved in the entire architecture of the machine and how it should be constructed, what input-output equipment it should have, what speeds they should have, as compared to the operating

speeds, and things of that sort.

MERTZ:

This was pre-compiler days.

POLACHEK:

It was primarily pre-compiler days, yes, that's right.

MERTZ:

One of the problems that machines of this generation had was in data output and data display. Were there any specific problems in that regard that you recall von Neumann made -- did he make any written contributions to this project or were they mostly ideas in discussions?

POLACHEK:

I don't know of any written contributions. If he made them, he made to IBM, not at these meetings.

MERTZ:

And do you recall any of the very first programs run on the NORC?

POLACHEK:

Well, as you know, I never got to run NORC. NORC went to the Naval Proving Ground, and I think some of the problems solved on the NORC at the Naval Proving Ground were trajectory problems again, very complicated trajectory problems with 6 degrees of motion and things of that sort, very complex trajectory problems, and that was the main preoccupation of the laboratory out there, and, of course, it solved many other problems like flow problems. I know the flow around a missile was solved on the computer.

MERTZ:

If you had had a chance to run the machine what problems had you lined up to run?

POLACHEK:

Well, my personal interests were still in shock waves, explosions, and fluid dynamics problems. However, if the Naval Ordnance Laboratory got the machine we would run all kinds of problems that are related to Navy applications, any application.

MERTZ:

Now I gather from what you've said that the David Taylor Model Basin offered you a sort of a new chance to set up a group to work with the UNIVAC, and you might want to describe briefly how you set it up and the kinds of problems you did run on the UNIVAC. Was the machine running when you came there?

POLACHEK:

No. When I came there, there was nothing there. No machine, no building. There was just myself and one other person that I employed that went ahead of me to David Taylor. The problem was to get the group going, and get the machine going, get a building up, and then attack as large a number of practical, useful problems for the Navy, in this case the Bureau of Ships, as was feasible. And I got to David Taylor about the end of December 1952, in December 1952, and went about to do all these things simultaneously. I had great success in doing this, I think primarily because at that time computers were not as popular as today. The number of people, good people who were available at that time in the field, in the related fields of mathematics. Of course there were very few who were actually trained in computers, but in the related field of mathematics it was essentially plentiful in comparison to what it was later. And I was able to gather a very excellent group in a very short period.

MERTZ:

Would you care to mention some of your colleagues in that group who you felt were particularly –

POLACHEK:

Some of the key colleagues in the mathematics were Dr. Wrench, Dr. Shanks, Dr. Theilheimer, T-H-E-I-L-H-E-I-M-E-R. In the programming field Betty Holberton was really outstanding. She worked with UNIVAC for a very long time and helped design the UNIVAC. She came with us, and her husband, John Holberton, who worked on the ENIAC, came with us. In the engineering field we got people who had some engineering experience in other fields, who were excellent. There was a Mr. Giles, G-I-L-E-S, who was our chief computer, a Mr. Rosenberg, Mr. Alexander Rosenberg, who is now with NASA, heads up one of their computer groups. And a host of younger people, and a number of other, I guess, more senior people in these various fields. The fields included applied mathematics, programming, operation of computers, and maintenance of computers. We had to maintain our own UNIVAC. There were no maintenance of UNIVACs at that time to be had commercially.

MERTZ:

Who was the chief engineer?

POLACHEK:

The first chief engineer was a man by the name of Giles, G-I-L-E-S, and he worked with a group of engineers who got rough training at Philadelphia, where they built the UNIVAC, and they got a number of months of training and actually helped put our machine together and got training that way, so when our machine arrived they had had quite a bit of experience with the computer.

MERTZ:

About when did the computer start actually working on problems?

POLACHEK:

OK. Let's see. I believe that in May 1953, which was the following year, I came in December '52, in May 1953, I believe, the computer arrived. The building was built and the computer arrived and the people were on board, about 30 or 40, a total of about 30 or 40 of us were on board, in the space of four or five months. It was really remarkable. And about September of that year the computer actually got in operation. And we thought that it would be in one shift operation for a while and then we'd go to two shifts. But the problems piled up so quickly that before the end of the year we were operating around the clock full time, and UNIVAC was –

MERTZ:

So within a year of the time you –

POLACHEK:

In less than a year of the time that I arrived, the computer was in full operation.

MERTZ:

Running around –

POLACHEK:

Running around the clock, building built, the people on board, the people learned how to operate it, and maintain it, the programmers programming, and the -- well, and we had solved some very interesting and important problems for the Navy. One of the interesting little problems, one of the first ones, was an actuarial problem. At that time Congress had passed a bill for annuity benefits to relatives of naval, of officers, of -- no officers, of armed personnel, just at that time. Let's see, we had about, I forget how many, but a very large number of options, perhaps fifty. About half a million numbers had to be calculated in a period of about two weeks. And the gentleman in charge of the Bureau of Ships undertook to do the job knowing we had a UNIVAC without even consulting with me. [Chuckle]. And believe it or not, we had the job completed. We had it all calculated on

the UNIVAC and typed out on this old Unityper which types one letter at a time, and copied and mailed to the various people involved. I had nightmares at that time that we would have hundreds of letters from dissatisfied armed personnel who would find errors in these tables, because these numbers were never seen by human eye. But I have never received a single letter questioning any of the number and these tables are used very widely.

MERTZ:

Had they started working on the UNIVAC prior to the time you came up in Philadelphia?

POLACHEK:

Yes. You see, that was UNIVAC Number 6, and I think they had one perhaps delivered about that time.

MERTZ:

So you knew that UNIVAC was slated to arrive when you went to the unit.

POLACHEK:

Yes. I went there and I knew that it was already purchased to arrive. Yes. On the other hand I never dreamt that any computer, UNIVAC or otherwise, would ever be so reliable that it would be able to calculate a half million numbers without an error.

MERTZ:

I was going to ask you about that. Did you find that in terms of actual machine time or running time versus breakdown time that this was a highly successful machine for its generation?

POLACHEK:

Yes. It was highly successful. It was operating reliably perhaps something of the order of 90 percent.

MERTZ:

That often. That was unusual for that generation of computer.

POLACHEK:

Yes. Right. And it also had a mercury delay line memory, perhaps close to 90 percent. It improved as time went on. Perhaps it started a little lower than 90 percent.

MERTZ:

And do you recall, in the very early part, were there some other interesting, mathematically significant problems that were run by your group on the machine?

POLACHEK:

Well, there were many problems that were run on the UNIVAC in its early days. I wouldn't consider them mathematically significant, but I think they were significant in the sense of utilizing computers for Navy applications. For instance, one of the early problems that we attacked when I came on board with the few people that I had was the problem of inventory control for the Navy. And, oh, I made many visits to the Spare Parts Control Center, SPCC, of the Navy, which is located in Mechanicsburg, Pennsylvania. And at that time, before the computer era, they used to do all of the operations of maintaining and keeping spare parts for the Navy either by hand or by old fashioned card punched machines. We had convinced them that we can handle this on the computer. And we had a whole lot of convincing to do, because they didn't think that computers can ever do things like that. They said it was just too subjective. After about a year's work we finally convinced them to set it up on a computer.

MERTZ:

These were shelf life inventories ...?

POLACHEK:

Shelf life inventories of components that are used by the Navy. There are something on the order of about half a million different items located in about 25 different locations, Navy locations, in the United States and throughout the world, and one of the problems was the purchasing of these items and to transfer items from one location to another if one runs short, and so on.

MERTZ:

One of the classic problems –

POLACHEK:

One of the classic problems [for which] computers are used nowadays. The other problems? Well, there were a large variety of Navy problems that were solved, and perhaps the best thing is to refer you to a paper which was written, not by myself but by Admiral Wright, W-R-I-G-H-T, who was later the Navy Director of David Taylor Model Basin, about our laboratory and what he called in the article something like Naval mathematics, and describes the work the laboratory did to solve Navy problems by computer.

MERTZ:

Do you recall where this article appeared?

POLACHEK:

I'll give you a copy of it.

MERTZ:

Oh, thank you. I see we're running out of tape.

POLACHEK:

OK.

MERTZ:

Thank you very much Dr. Polachek for this interview.

End of Interview