Interviewee: Harry Polachek

Interviewer: Richard R. Mertz

Date of Interview: April 23, 1970

MERTZ:

This is an interview conducted on Thursday the 23rd of April 1970. the subject of the interview is Dr. Harry Polachek, of the Atomic Energy Commission. This is a continuation of the previous interview conducted on the 21st of March 1970. The location of the interview this time is in the office of the Section of Mathematics, the Museum of History and Technology. The interviewer is Dr. Richard R. Mertz.

POLACHEK:

Those I remember as consultants, these are individual consultants, they didn't act as a committee: The prominent people. The most active was von Neumann, who actually spent about one day or perhaps two days a week sometimes working full time at the office. And the other one was Gamow, who spent several days a week full time. And the other people were, as I mentioned before, Courant, Friedrichs that's it.

MERTZ:

Einstein?

POLACHEK:

Einstein [laugh], forgot about him. ... These other people were involved to a lesser degree, perhaps Courant and Friedrichs were involved occasionally, whereas Einstein was involved on rather infrequent

occasions, never really visited us at the Navy, but a number of members of our group used to visit him on infrequent occasions in connection with answering some problems.

MERTZ:

Do you recall some of the problems that were discussed with Professor Einstein?

POLACHEK:

Well, I wasn't personally involved, and I don't really know the exact nature of the problems, but I'm certain that the theory of shock waves was discussed with him and the anomaly that had occurred with triple shock waves that couldn't be understood at that time. And also, explosion charges, that is, shaped charges, which was very difficult to understand. Now, shaped charges have numerous applications, and, of course, one application is to increase the intensity of the force that results from the explosion. Of course, one application is in connections with building an atomic bomb, which was in a sense an implosion which sort of focuses all of the forces to the center of the explosion. I'm sure these problems were discussed with him, but I don't know what others.

MERTZ:

Do you recall who were the representatives who did talk to him?

POLACHEK:

Well, Dr. Seeger was one of them who visited him on several occasions. I'm sure von Neumann probably joined him. There was a young physicist, at that time young, by the name of Finkelstein, there with us, a nuclear physicist, very competent one, and usually Dr. Seeger and Dr. Finkelstein used to go down to Princeton and talk with Einstein.

MERTZ:

Do you know where Dr. Finkelstein is now?

POLACHEK:

No. I don't. I lost track of him. I think at one time he was back at Princeton, but I lost track of him, and right now, I can't even think of his first name.

MERTZ:

Then generally von Neumann and Gamow were perhaps the two most involved in the --

POLACHEK:

Yes, they were involved on a day to day basis in a sense.

MERTZ:

Did they produce any reports as such?

POLACHEK:

Yes. Von Neumann of course produced a basic report on shock wave theory which is -- I think I still remember the number of the report, it was a research report for the Bureau of Ordnance, Research Report No. 12. I think I have a copy, by the way. As a matter of fact, it may be included in the Collected Works; I mean its contents may be included in the Collected Works. It may be exactly in the same form. I'll have to check that. It was a basic report. It's Research Report No. 12 and I have a copy of it, I remember that.

MERTZ:

That was for the Bureau of Naval Ordnance?

POLACHEK:

The Bureau of Naval Ordnance, yes. Now Gamow had come up with some reports, too, probably less formal, but I can't recall any specific one at this time.

MERTZ:

But they would have been from about the same period as the von Neumann report. It would be under the auspices of the Bureau of Ordnance?

POLACHEK:

Yes, it would be under the auspices of the Bureau of Ordnance, right.

MERTZ:

Now if we can move on to another slightly different, perhaps a related area, and that is, if I recall correctly, you worked rather closely at one time with both with von Neumann and with Gamow. Was it at the time that you were at the Bureau of Ordnance?

POLACHEK:

Yes. The Navy Bureau of Ordnance.

MERTZ:

That was 1943 to about 1947.

POLACHEK:

That's correct.

MERTZ:

at the Naval Ordnance Lab.

POLACHEK:

I was in the Bureau of Ordnance, Naval Ordnance -- let's see, let's get this -- I was at the Bureau of Ordnance from '43 to '47, so I moved to the Naval Ordnance Lab in 47.

MERTZ:

Were either of those two gentlemen consultants to the Naval Ordnance Lab or was this primarily to the Department of the Navy Bureau of Ordnance?

POLACHEK:

I think it was primarily to the Department of the Navy Bureau of Ordnance.

MERTZ:

And did their consulting continue throughout the whole period of your association with the --

POLACHEK:

Yes, it did. That's right.

MERTZ:

So, there would be some reflection in their files of the --

POLACHEK:

Yes, during those years. I could help by looking at some of the references to see exactly what other reports there were involved.

MERTZ:

That would be useful if we had some report numbers and dates. That helps particularly in some of these other archives which are not --

POLACHEK:

I realize that.

MERTZ:

Do you recall any interesting or amusing incidents that occurred during the time that you were working with Professors Gamow and von Neumann? I gather they had some fairly entertaining lunches together in which you participated?

POLACHEK:

Yes. Do you want me to take time? Yes, there were interesting times and many lunches. Of course, some of the discussions were quite serious and revolved about the development of an atomic bomb. That was before we knew that there was any success in this undertaking. One of the discussions between Gamow and some of the others, perhaps von Neumann and Finkelstein, who was a nuclear physicist, was, if a bomb was developed, would it have enough energy in it so that if it were exploded in the water it would produce enough heat and energy to perhaps set off an explosion of all the oceans, blow up the whole earth. And that was discussed in a rather serious vein, to some extent, because the forces weren't quite known at that time.

Now, you asked me for amusing incidents. Gamow had a great sense of humor; he was a practical joker and always had some amusing incidents going, perhaps too trivial for history.

MERTZ:

Well, if they involved himself and von Neumann they might be on sort of the lighter side of history. It might be interesting for posterity.

POLACHEK:

Oh. I don't know. The incident I'm thinking of involved myself and I don't know if that is important, really. But I do recollect a discussion between von Neumann and Gamow before the War was over, World War II, at the luncheon table, and that was rather interesting. Because at that time Russia was our ally and we were looking forward to a victory on our part and Russia's part and hadn't really seriously looked ahead beyond the War. But both Gamow and von Neumann agreed that after the War was over we were facing a challenge from Russian Communists and that a war would take place. Both predicted that a war would take place between the United States and Russia. The only difference of opinion was how many years would pass before this war starts. And I think there was a bet between Gamow and von Neumann, one predicted about five years and one about ten years. I think they were both incorrect but I think it was quite remarkable that they foresaw this confrontation that's going to take place.

MERTZ:

They were pessimistic about the prospects.

POLACHEK:

Yes. I think you have to set yourself back in history at a time when, you know, we were winning the war and Russia was our ally and most people just didn't give any thoughts at all to what would happen after the war. They were both not only pessimistic, but they realized -- it shows their keen insight into communism and world affairs -- realized that there would be a very, very difficult confrontation between the United States and Russia.

MERTZ:

Do you recall ever discussing any of the possibilities of peaceful applications of nuclear energy, such as

controlled thermonuclear or other energy sources.

POLACHEK:

I don't recall any specific conversation. I'm certain that this was one of their topics of discussion from time to time. I don't recall anything specific.

MERTZ:

The more serious side was in connection with the future course of--

POLACHEK:

history.

MERTZ:

history.

POLACHEK:

Of course, there were many discussions about the feasibility of developing a bomb, that goes without saying.

MERTZ:

If I recall correctly, historically, there were also discussions at the time, perhaps almost in the middle forties, '45 and '46, of the feasibility of the hydrogen bomb as well as…

POLACHEK:

Right. Actually, when the discussions went on, all considerations for building a bomb were discussed, either using fusion or fission, as a matter of fact, the first time I personally found out, in spite of all these discussions, the first time I personally found out that it was uranium that was used, and it was a fission bomb rather than a fusion bomb, was after the bomb was exploded and the Smyth Report was written.

And so, I was very keenly interested by the fact that it was a fission bomb. But all possible ways of building a bomb were discussed and with their feasibilities.

MERTZ:

If we could balance the serious side. I'm sure both Professors von Neumann and Gamow were, along with many others, quite concerned about the future prospects for peace and the implications of the atomic bomb in this context. You mentioned that Professor Gamow had a rather practical sense of humor. Do you recall any particular case where it showed itself off?

POLACHEK:

Well, [chuckle] there's one little story that comes to my mind that I really think is perhaps too trivial. He was really playful in a sense, and I think perhaps I did mention it last time. Well, an example of his kind of practical jokes was, he came to the office one time and he said he had discovered a new explosive. He took out a vial from his pocket with some black stuff like powder, and immediately went on to pour some of that out on the table right in the center of the room. Everybody just, horrified, jumped backward and out of the room, and --oh, before he did that he also took a box of matches out his pocket. He went right to the powder ready to light it up, and then everybody jumped. And ran out of the room. It turned out to be some pepper that he brought along.

He used to joke a great deal about the secrecy. I recall at one time he complained that he didn't have c clearance for secret material because he was not a citizen, and he had written a memorandum which related to this focusing effect of explosives, he had written it at home, and when he came to the office, the people at the Navy read it and they thought it was very good and stamped it "Secret," and beyond that he couldn't read it himself because he was not cleared for "Secret."

He kept on talking about it.

MERTZ:

I think he was somewhat kidding about that because he did have a number of clearances.

POLACHEK:

Probably did have a number of clearances, but he was kidding about the secrecy, too. Yeah, I think he was -- well, I'm sure he was kidding. I don't remember exactly what his clearance--it may have been top secret.

MERTZ:

When he had lunch with Professor von Neumann, do you recall any particular topics that they found mutually amusing that they liked to discuss...

POLACHEK:

Well, they would talk about anything, topics of the day, what goes on in world affairs, the newspaper, almost anything that would amuse anyone else they would engage in discussions of. Von Neumann particularly liked riddles and puzzles, and Gamow used to bring them to try to trap von Neumann, and von Neumann used to solve them very rapidly, and they used to challenge each other that way.

RRM:

Were they generally logical problems?

POLACHEK:

Yes, logical problems. Sometimes, of course, they were problems that really didn't have a good logical answer, they were just jokes or traps or something like that. They would challenge each other with those kinds of problems, too. Sure.

MERTZ:

Do you happen to know if von Neumann played chess?

POLACHEK:

No, I don't.

MERTZ:

I know, this former professor of mine who is now chairman of the Department, I think, at Chicago, Irving Kaplansky, went to the Institute for a while, was a Fellow there, and one of his favorite things was game theory.

POLACHEK:

Oh yes.

MERTZ:

and he worked out different kinds of problems with chess. But I don't know, he never told me whether he ever played with Von Neumann; I don't know.

POLACHEK:

I don't recall von Neumann ever mentioning, but, of course, in his book of game theory I'm sure he thinks about chess as a game. I'm certain that he knew how to play chess, but I don't get the feeling that he engaged in it very much.

MERTZ:

If we could just cover one other area of activity. You mentioned that later in the forties, around 1947, you did run some problems on the CPC. It was one of the series of machines that you did have some experience on. In fact, you touched on a number of the major machines that -- I don't believe you ever

ran any problems on ENIAC, is that correct?

POLACHEK:

No, I didn't personally run any.

MERTZ:

But the Bush differential analyzer?

POLACHEK:

Bush differential analyzer, Mark I, IBM Selective Sequence Electronic Calculator.

MERTZ:

And you helped participate in the design of the NORC.

POLACHEK:

Of the NORC, yes.

MERTZ:

Did you actually run problems on it?

POLACHEK:

Ah, let's see. Personally, I did not. Our laboratory did. I was in charge of a group later and we did use the NORC computer.

MERTZ:

After you went to the David Taylor.

POLACHEK:

After I went to David Taylor.

MERTZ:

Ah. Thank you.

And do you recall the problems that were run on the CPC? Oh, first of all, were these problems ever published? The results?

POLACHEK:

Ah, let me see. On the CPC, well, we had a CPC up when I was at the Naval Ordnance Laboratory, and for a number of years, I was in charge of the computation group. And there were a number of problems solved on the CPC, that were of interest to the Navy, but I believe, and I'm not absolutely certain, that one of the papers that I wrote jointly with Seeger and Lutford, I believe that that computation of a one dimensional shock wave moving and resulting in one dimensional interaction in shock waves was done on a CPC at the Naval Ordnance Laboratory. This paper is listed in this list of papers that I gave you, and if my eyes were a little better, I'd find it and give you the number. Oh, here it is, it's number 6 on this list. That would identify it.

MERTZ:

Could you give me a brief rundown of the machines that were available at that time when you were working at the Bureau of Ordnance...

POLACHEK:

Are you going back to Bureau of Ordnance days?

MERTZ:

Oh, well, let's see, this is actually after that, isn't it?

POLACHEK:

This is at the Naval Ordnance Laboratory.

MERTZ:

If I remember I believe they had several machines there.

POLACHEK:

At the Naval Ordnance?

MERTZ:

At the Naval Ordnance Laboratory.

POLACHEK:

At the Naval Ordnance Laboratory, they had mostly, at that time, when I was there, they had mostly IBM computers, and the most advanced one they had was a CPC. They started out with a multiplier 604, whatever it was.

MERTZ:

And the CPC was the most advanced --

POLACHEK:

The most advanced computer during my days. Yes, now I left there -- let's see if I can remember it now '52, I guess.

MERTZ:

Yes, when you went to the David Taylor--

POLACHEK:

Yes, when I went to the David Taylor Model Basin.

MERTZ:

That is part of the story I would like, if possible, for you to expand somewhat on. And that is, you mentioned also there is an article by Admiral Reik.

[Recorder off].

POLACHEK:

We'll now discuss my work at the Naval Ordnance Laboratory, which .. began in 1947 and ended in 1952, and during that period I was involved, either personally or my group were involved, in the solution of a variety of problems that were of interest to the Navy at that time, and my involvement may have been, ranged a great deal from very heavy involvement to very little involvement. But the problems ranged the gamut of all the Navy interests, including problems in degaussing, which was one of the problems at that time; wind tunnel -- I was associated with a ballistics research, sorry, hydro ballistics aeroballistics research group, and the problems involved wind tunnel problems a great deal. They designed wind tunnels and the interpretation of wind tunnel results. That is, the testing in a wind tunnel not of aircraft but of missiles or projectiles.

MERTZ:

I think you did touch the last time a little bit on some of the problems of the anomalies, specifically of the problems around Mach I of the wind tunnel nozzles.

POLACHEK:

Yes, there are some especially difficult problems at Mach I which we got involved with to some extent because they involved shock waves. There were also problems at that time for very high velocities, and there were hypersonic tunnels brought into being, and as a matter of fact the hypersonic tunnel was

constructed in my group. I wasn't personally involved in the construction nor experimental phases of it.

MERTZ:

Did this increase the number of computational problems? I'm thinking of the order of magnitude of the number of simultaneous differential equations that have to be solved.

POLACHEK:

Well, some of the problems I had very little involvement with, with the exception of aiding people in getting them solved by computers. These problems came from various sources in the Naval Ordnance Laboratory which did a lot of work in the design, all sorts of designing of all sorts of Naval weapons, fuses, and all sorts of things. The range of problems was very large.

MERTZ:

One of the things that I'd be interested in your views of, and that is, as computer technology developed in these years, the scope of the possible, in terms of the numbers of equations, the numbers of parameters being introduced into the problem, increased.

POLACHEK:

That's correct.

MERTZ:

And also, the limitations imposed by such things as memory and the scaling of your programming to the fact that you have to cope with a very limited memory, and this imposes all sorts of limitations on how extensively you can introduce elements into a problem.

During this time when you moved, well, up, the CPC was the most sophisticated machine at that time --

POLACHEK:

Available to us. There were other computers.

MERTZ:

Could you run problems on any of the other machines around?

POLACHEK:

Yes, I think I mentioned to you that we did use other machines outside of the Naval Ordnance Laboratory.

MERTZ:

The SSEC.

POLACHEK:

Like the SSEC, right. Yes, that's an example of a machine that we used.

MERTZ:

Given the fact that there were no -- this is pre-compiler, pre-

POLACHEK:

That was pre-compiler, yes.

MERTZ:

with the whole era. From the point of view of doing the programming, what kinds of things did you look for in terms of machine, computer machine, technology, to help improve, oh, either the accuracy of the solutions that you'd get from the particular problems, or the number of parameters you could introduce into the particular problem?

POLACHEK:

I think the answer is pretty obvious. First of all, greater speeds; next, larger memories. And I guess the third characteristic we looked for was faster and larger input-output devices. Those were essentially the three devices. Now, of course, the fourth element would be the range of instructions and the number of instructions we had available. That was not really a very important factor because we could construct programs, that is, subroutines, with whatever we had, and we had the basic instructions, so it was just a matter of more work to construct more subroutines.

Of course, at that time we didn't foresee the very complex subroutines that would become available as time goes on, and the really large size that computers would become in future years. So, I think we'd have to go a few years later before we can start talking about that aspect.

MERTZ:

None of the problems -- correct me if I'm wrong in this fundamentally, none of the problems that you were concerned with were, so to speak, real time problem, in the sense --

POLACHEK:

That is correct. I wasn't concerned at that time with any real time problems. Not in that period.

MERTZ:

So, the need for some of the technology that would later be developed would not be felt as much.

POLACHEK:

Well, it was felt, but, of course, we were happy to do it with whatever was available, which was a tremendous improvement over what was available just five years earlier. But we did feel the limitations very much, because we had to simplify our models to get any solutions. One example, of course, is this

shock wave problem that I mentioned a while ago, this paper Number 6 was a one-dimensional shock wave problem, it was one-dimensional because the limitations of the machine didn't allow us to solve two or three-dimensional problems.

So, this is just an example of going to -- we tried solving cylindrical problems on old IBM machines and we ran into difficulties there and our solutions were only in the very broadest sense in any way accurate.

MERTZ:

Did you in your own career mark any turning point in terms of the kinds of machines you used which opened up more or less new horizons in the sense that the models didn't have to be so simplified. I was thinking of the UNIVAC possibly or...

POLACHEK:

I see. Well, I think the developments were so fast and dramatic and much faster than I really hoped for ... that there were many stages where new horizons were opened up. UNIVAC I was an opening of a new horizon in my mind. I think I already discussed -- now, that's a later period, when I came to David Taylor Model Basin. I discussed my elation when I found out that one of the very first problems solved, which involved the computation of half a million numbers which were going to be used throughout the United States for many many years, and there wasn't a single error in them, so far as we could tell there were no errors. And this opened up an entirely new vista.

MERTZ:

This touches on another problems too, and that is the whole problem of error and reliability of machinery.

POLACHEK:

Reliability is a question, too. Right. I didn't mention that.

MERTZ:

Which in the earlier period made it necessary to rerun and to structure your programming in such a way so that you would have to use almost an iterative process.

POLACHEK:

That's correct. When I mentioned these factors, or these characteristics that we were looking for, we were looking towards, in computers, I think I should have mentioned reliability as one of the very important factors, because reliability is a very troublesome area.

MERTZ:

Was there...this was something that while it happened fast it's hard to pin down in time, or with a particular generation of machines. Was there a particular time that you felt yourself that many of the constraints that had been imposed by the machines' capabilities on the kinds of models and the kind of problems that could be introduced were to a large extent removed or no longer of great concern?

POLACHEK:

As you realize now my experience doesn't sweep across all computers developed and used, but I did mention that UNIVAC I opened up an entirely new world in the field of computing to me, and, as far as I can see, to our group which was one of the first very active groups in the Navy for the use of computers for naval applications as brought out in this article by Admiral Reik, which you mentioned before.

Now we mentioned UNIVAC I. But then of course the next step in that was - the next step in the

development, perhaps we can call it, that to me personally opened up a new horizon happened to be the LARC, but there were other computers of similar type which opened up new horizons. Next to the UNIVAC that I was involved with personally was the LARC, L-A-R-C, which gave us capability to solve nuclear reactor problems in three dimensions plus time plus different energy levels.

You asked before about the solution of simultaneous equations. This gave us a feasibility to solve problems in many thousands of simultaneous equations, several hundreds of thousands simultaneous equations in a sense. They were very simple equations but there were several hundred thousand equations.

MERTZ:

This again had something to do with the speed, the computer speed.

POLACHEK:

Well, yes. It had to do with an increase in the order of magnitude of the speed of the machine. I mentioned the LARC, but there are other, let's see, in that same category, there'd be IBM 7090. It increased the speed by orders of magnitude, not necessarily one order of magnitude. And it increased the memory capacity by orders of magnitude from the UNIVACS, so it opened up entirely new areas.

MERTZ:

Right. Did this have any direct effect on the way in which you treated some of the problems that you had earlier been interested in, for example, shock wave theory and applications?

POLACHEK:

Well, ..not tha -- yes, it did in my thinking. I'm not certain I carried my problems to a final conclusion, but it gave me the possibility to attack and to try to solve problems in three dimensions, shock wave

problems in three dimensions, whereas before we were limited to one or two dimensions, and to set up the equations in a much more realistic form, so that they would really characterize the problem much more accurately.

MERTZ:

This carries over also in the problems of ballistics, certainly, or, well, such things as missile guidance, for example.

POLACHEK:

Missile guidance, it carries over, yes. It carries over in many fields. And then these machines were available; at that time, of course, I was at the David Taylor Model Basin. I had started thinking much more in terms of real time problems, and the solution of tactical problems, strategic problems of the Navy in real time, and the use of computers in analyzing sonar data, or detecting submarines, and things of that sort.

MERTZ:

Did this have any, also, applications so far as ship design simulation was concerned, because of the number of say degrees of freedom that you can introduce into problems for ship design theory.

POLACHEK:

Yes, the answer is yes; but let me make clear for the record that I'm now speaking, addressing myself to the period now 1952 to 1965, when I was at David Taylor Model Basin, after I left Naval Ordnance Lab.

MERTZ:

What computers did they have there, aside from the UNIVAC?

POLACHEK:

Well, at the David Taylor Model Basin they had a UNIVAC at first and then an IBM 704, IBM 709, IBM 7090 and a LARC. And we also, of course, were always free to go out and use computers available outside. But the LARC, when we received it, was about the most advanced computer of its kind at that time, at the time we installed it. So, we did have about the most advanced computers available to us.

MERTZ:

Does Admiral Reik's article cover all of the different types of hardware that you had available or does he keep primarily to the UNIVAC?

POLACHEK:

No, he discusses the LARC. It was written at the time when the LARC was not yet installed, so he discusses the fact that the LARC is going to be installed and its characteristics and what we hoped to do, the kind of problems we hoped to solve with it.

But getting back specifically to your question about ship design, when I first came to David Taylor Model Basin it had occurred to me that ship design could possibly be aided by the use of computers, and this was the period when I was searching out problem areas in which computers could be useful to the Navy, and what I did was, I and one or two others of our key mathematicians, set up a meeting with the director of the hydrodynamics laboratory at that time that was responsible for designing the ships, that was a Dr. Todd, T-O-D-D. Very shortly after I came to David Taylor, perhaps within a few months, we had a lengthy discussion with him about the feasibilities. And we embarked on a program to develop methods for using computers in the design of ships. There were many aspects to it, very interesting

aspects.

MERTZ:

Did they include also simulation problems?

POLACHEK:

Simulation in what sense? Simulating how the ship would react..

MERTZ:

I'm trying to see if there were any analogies, for example, to the kinds of problems that, for example, the Air Force and other groups were interested in, in air simulation.

POLACHEK:

Well, they were interested in simulating what the airplane will actually do and the only analogy to that was in the development of nuclear submarines, but it did not involve the digital computers. There was some thought given to the use of digital computers, but there were analog computers at David Taylor Model Basin that were specifically purchased to simulate the action of a submarine, for instance, the dive characteristics, one would actually simulate the submarine as if one were actually diving and getting out of the water and one could sit in a seat and operate this device and the computer, this is an analog computer which would react according to the equations of motion of the submarine.

MERTZ:

What type of analog computer was used?

POLACHEK:

Well ..

MERTZ:

A REAC?

POLACHEK:

A REAC mostly, yes. Mostly REAC computers. But I want to say that I wasn't, this was not a part of the laboratory I was involved with. This was located in the hydrodynamics laboratory.

MERTZ:

Directly attached.

POLACHEK:

It was directly attached to the REAC analog equipment, and they did the simulation there.

MERTZ:

Was there any consideration given to combining in various ways and forms analog and digital machines?

POLACHEK:

Well, there were considerations of that, but we never felt, at least during the time I was there, that it would help in the solution of this particular problem.

MERTZ:

So, they pretty much stayed in their simulation studies to the analog?

POLACHEK:

In their simulation studies, no, that isn't quite correct. They stayed to a large extent with the analog equipment, but when they wanted to solve more complex problems, they did simulation on our digital computers separately from the analog computers, but that was not on a real time basis. Now it was done

in the computer, and the question was how would a submarine react to certain maneuvers and the solution to that problem was too large for the analog computers to do that, they would do that on the digital computers, so there were two separate computations, they weren't tied together.

MERTZ:

I see. Was there any difference in terms of the order of accuracy between the simulation that was done on the digital machines…was…

POLACHEK:

Yes, well, yeah. It was mostly on the faster computers; I think some of it may have been done on the UNIVAC, but, I think, primarily, when the fastest computers came into being some of that was done. To answer your question about the accuracy, I think it did give them greater accuracy, but, more important than that, it gave them the capability to solve much more difficult problems and more sensitive, to carry out more sensitive maneuvers and see what the result would be, and take into account many more equations, which really means more surfaces on the submarine, that is, control surfaces, I guess I should say.

MERTZ:

What -- I better turn this over.

[**End of Side 1]**

[Start Side 2]

POLACHEK:

Well, perhaps this is repetitive, but one of the most important areas of problems that became tractable with the very fast computers like the LARC was problems involving the solution of partial differential equations and perhaps in three dimensions in space and one in time. And furthermore, the solution of eigenvalue problems related to those differential equations, or the solution of differential integral equations perhaps that are involved in solutions of transport theory. These are problems that occur in the design of a nuclear reactor. And we have done at David Taylor quite a bit of work in that specific area. There are many facets to difficulties in these problems. One of them of course is the question of stability. How do you solve a very large system like that and assure yourself that the solution is valid? And very many difficulties had arisen before that as a result of the instability of your finite difference equations which result from the differential equations. And a great deal more had to be learned about stability, which is a mathematical exercise.

But also, a great deal of power was necessary in the computer, that is, memory and speed, in order to be able to solve equations with very complex boundary conditions involving, as I said before, perhaps 250,000, well, what eventually became linear equations; but these were linear equations derived from the differential equations. This is one area, remember, a very important area.

MERTZ:

The development of the stability problem actually grew out of the increase in the capabilities of the machines itself, didn't it?

POLACHEK:

No, not really. Well, not really. The history of the stability problem goes back to -- well, I was involved with -- well, there are many facets to that, too, but I was involved in it very, with Gamow and von Neumann, very closely, and perhaps we'll get them back into the picture, by about, well, let's see, by about 1944 or so, by about 1945 I was asked by Gamow to solve a problem that had to do with storing explosives, and the problem was the following: When you store explosives in a hot location these explosives could explode. What do I want to say? Automatically, without ..

MERTZ:

detonators.

POLACHEK:

detonators, yes, is the word. And, of course, cause a great deal of havoc and damage. So, the storage of explosives was a problem that we were involved in, and Professor Gamow set up the equations. Now those equations involved the solution of essentially what could be said a heat conduction equation plus an energy term which has an exponential term. And I set about, he asked me to solve it, of course, it couldn't be solved analytically, so I went about solving it numerically. And to get my numerical methods I went back to a, what was considered then a classical paper, by a mathematician called Richardson which was published in 1910 in the Proceedings of the London Academy of Sciences. And that was, of course, The Bible on how to solve finite difference equations.

And when I proceeded to solve that problem, and this was not using very advanced computers, but using IBM computers, it blew up after about ten steps. The temperatures went completely haywire, went negative, went positive, they went all the way to very high numbers which were absolutely impossible,

ridiculous, and, of course, I realized what was happening because I was using what I thought was a valid method used by Richardson and published in a paper which was accepted by everyone. So, I ran into the problem of instability right there, and we discussed it with von Neumann. Of course, Gamow was mostly interested in the results rather than the techniques, and we found that we had a problem in stability, and that actually Richardson's method, the one that he used in his article, was absolutely unstable. That he had only solved this problem about ten steps and compared it with the real answer, the correct answer and they compared, but if he had gone about ten more steps, he himself would have found the answer completely invalid. So, the stability problem could occur without too many steps, but, of course, they are much more severe if you have a thousand steps or fifty thousand steps.

MERTZ:

That was what I was, in a way, referring to,

POLACHEK:

Yes, I guess so.

MERTZ:

because of the capability of the machine which enables you to pursue all…the different order of magnitude of different steps.

POLACHEK:

Of different steps. And that, of course, makes the stability problem much more sensitive and much more important. But the stability problem was still there, even for shorter problems.

MERTZ:

And did this become an increasing element of concern, then?

POLACHEK:

It became an increasing element of concern, but, on the other hand, there was an increasing amount of work being done in many places. von Neumann was again one of the pioneers in that area. But going back, I think I did mention this article of Courant, Lewy, and Friedrichs, which dealt with this problem of stability. But a great deal more work had to be done before some of these problems could be solved from the point of view of stability.

MERTZ:

So that became another element in the..

POLACHEK:

In the advancement of numerical methods and how to use computers properly and so on.

MERTZ:

Looking back over your own career, up to, say 1965, the time you joined the Atomic Energy Commission, what do you feel, well, first of all, were the most interesting machines that you worked on and the most interesting problems that you worked on the machines? I think you've touched on them, I could make a good guess, but I'd rather have your own view.

POLACHEK:

On computers, well, of course, I have my own personal interests and therefore - that's what you're asking about. I worked problems in shock wave reflection, and they interested me a great deal, and shock wave refraction, and shock waves in water, and underwater explosions. And then later in the solution of partial differential equations, related to the solution of many kind of problems, but particularly the design of reactors, and perhaps these were the most interesting to me.

MERTZ:

Was there much contact between your group and the Atomic Energy Commission in your work on the--

POLACHEK:

Well, there was contact between us and the group that was headed by Admiral Rickover, who wears a Navy hat and an AEC hat, and that was the primary contact. There was also contact between our group and the AEC laboratories who were working in the field of nuclear reactor design, particularly the Bettis plant at Westinghouse, and the Caput plant of GE. We had a great deal of contact with them all the time.

MERTZ:

Ah, I see. Were there any problems that were run for your group, or of interest to your group, by other computational facilities, or that your group ran on other…

POLACHEK:

No, it was almost the reverse. That is, we set up the programs and ran the problems that would -- were required in the design of reactors, primarily for naval vessels but also for power reactors. So, we were servicing those other groups, and we exchanged programs with these other groups that I was talking about.

MERTZ:

I see. So, people would come and see you.

POLACHEK:

They would come to us to work problems. They would actually use our computers. And they would help to formulate the problems, because they had the physicists that mostly were at their establishments,

so, they would formulate the problems.

MERTZ:

So, your machines actually ran much more than purely Navy problems.

POLACHEK:

Yes, they ran power reactor problems, too.

MERTZ:

That brings us in very general terms, without too much detail, to the time you joined the Atomic Energy Commission in 1965. Was there a particular challenge or interest that the AEC offered you over and above the David Taylor?

POLACHEK:

Well, the interest that I had in the AEC was primarily getting free contact with some of the AEC, people in some of the AEC laboratories, and some of the university people that had grants with AEC, and I knew many of these people from my previous work. And I thought that I would free myself from some of the administrative work running a large laboratory and computing facility.

MERTZ:

That is something we didn't touch on and that is how this grew. You mentioned that when you arrived there on the scene at David Taylor there was you and nothing else, no building, no machine, or the machine had been ordered, and no staff, and that in less than a year it was operating full time with the UNIVAC. That brings us up, sort of, to 19 ... 53. And then the computational facility there, I take it, grew.

POLACHEK:

Yes, during the period I was there, from its inception to 1965, the facility grew, as I said, to primarily two large computers at any one time. At the time I left there was a LARC there and the IBM 7090. And the number of people grew to between 125 and 150 that were involved, and, in addition to that, a splinter group was formed, which was originally one of the divisions in our laboratory, which formed the nucleus for an entirely new operation in the Navy at the Navy Yard, called NAVCOSAC, and their primary purpose was to use computers for tactical and strategic applications.

They then grew faster than we did, and they probably have about 600 people or more in their employ.

MERTZ:

I see. So, by this time it had become quite a large-scale going organization.

POLACHEK:

Yes, it had.

MERTZ:

And there was an increasing amount of time that you had to devote to administering.

HP:

That's correct. So perhaps that was one of the other considerations that I had in leaving that and going to a position which would give me contact with some technical people and give me a little more free time to do some thinking of my own.

MERTZ:

You mentioned one thing that I thought was quite interesting, because I think the situation changed over the years. That was at the time you were setting up the original staff at the David Taylor Model Basin

you managed to find a number of very good mathematicians who were available, and I would say that maybe 5 or 6 years later you might have found that perhaps the situation had changed.

POLACHEK:

Yes, that's very correct. The market for good mathematicians, programmers, computer operators, what have you, became very tight in a few years after I established the laboratory. I had been very fortunate to get some good people on board, but we had to train, and after about 1954, 55, we practically had to train most of the people. Of course, we were still able to get a few good young PhDs. But we had to train most of our operators. Well, the servicemen, we serviced our own computers, I mentioned before, the maintenance men ...

MERTZ:

Did that also apply to the LARC as well?

POLACHEK:

Right. It applied to the LARC as well. We maintained the LARC. Because it was a one-of-a-kind computer, and there just weren't crews available to maintain LARCs.

MERTZ:

And what did you, I was wondering, you mentioned there was no great problem in finding qualified people when you were originally setting up the project, then you gradually moved to training your own staff, bringing them in at levels below the PhD level, and then training them internally. Did this change at all? ... Your perspective on prospective employees then was scaled somewhat differently from when you originally set up?

POLACHEK:

Yes, it did. And it varied from time to time. And as the pendulum swung, and, as you know, there was a very severe shortage of good people, and we tried to get the best we could. And then, of course, more recently perhaps, the market perhaps has swung the other way and the number of people available has, is perhaps swinging the other way, I don't know. Until the [end of] the time I was there, 1965, I think it was still quite difficult to get outstanding people. But we didn't find that a real impediment, because we had already staffed our laboratory with a large number of excellent people who served as a nucleus to train others. So, we were able to run on our past laurels, in a sense.

MERTZ:

Would you care to mention some of the, the people?

POLACHEK:

The individuals. Yes, if I can name all. ... One was a Dr. Wrench, W-R-E-N-C-H, and I think we mentioned him before. And Dr. Shanks, who was at Naval Ordnance Laboratory with me, he came along with me. Another one was a Dr. Thalheimer, whom I mentioned before.

Ah, let's see. When we first started, we took pride in the fact that we had three women mathematicians, three women PhDs in mathematics. Those were Dr. Ruth Davis, Dr. Elizabeth Cutwell, and Dr. Jean Porter, that was her married name, originally Jean Boyer when she came with us. Those were three very good women mathematicians.

We had as consultant Professor Murnaghan of Johns Hopkins, past chairman of the Mathematics Department of John Hopkins University. And we had a number of others whom I just don't recall at the moment, but I could get a complete list of them, who were the basic mathematicians?

But also, I guess I should add, we were also able to get some excellent computer engineers. Among them was a Mr. Giles, G-I-L-E-S, worked on computers before. Mr. Goldstein, who is now at the Office of Naval Research. A number of younger people who later -- well, a Mr. Alexander Rosenberg who is now, heads up a large group at the NASA Goddard laboratory, yes.

MERTZ:

And this nucleus, plus or minus a few people, remained more or less continuously on?

POLACHEK:

Yes, they did. They found the work sufficiently challenging, and most of them did remain. As a matter of fact, many of them are still there, although I've left.

MERTZ:

So that they provided enough of what you might call, of course together with yourself, mathematical sophistication, so that even though the staff grew and did not maintain that level initially, that was initially established, it wasn't necessary, because they could perform what you might call the mathematician's role. Some of the other junior people were more in less sophisticated mathematically involved problems.

POLACHEK:

That's correct. I was speaking of the mathematicians. I failed to mention a few very important people, not in the mathematics field. Of course, there are many aspects to using computers for the solution of practical everyday problems, and mathematical analysis is one of them. I mentioned mathematicians perhaps because I'm one. But I was able to -- I mentioned the engineers, or some of the engineers, not all of them. I should mention some of the outstanding programmers we were able to attract. One of

them I should mention specifically is Mrs. Elizabeth Holberton, who was one of the original designers of the UNIVAC. She designed the first sorting routine for UNIVAC and actually helped set up the set of instructions for the UNIVAC, decided upon what should be the instructions for the UNIVAC. She designed, helped design those instructions. She, with others, was able to train some of the younger people. And, of course, the operators and some of these other people who were required we were able to train. So, we had a complete staff going.

MERTZ:

Yes, well, it sounds [as though] with over a hundred people you had quite a concentration of computing expertise.

POLACHEK:

Yes.

MERTZ:

Not rivaled by too many groups in that period.

POLACHEK:

Well, we've set up [laugh].

MERTZ:

I was thinking in terms of the staff.

POLACHEK:

Yes. There were many, of course, that I haven't mentioned, but it was really more expertise than this. I'll have to think a little more, go back, but we had a number of other very good people I haven't mentioned.

MERTZ:

Did you have any group under you that did work of a more -- well, that were not necessarily responsive to specific problems that were raised by the Navy or by the situation, but would study experimental approaches...

POLACHEK:

Yes, when we set up the laboratory, we set up a number of groups, and one group had the specific responsibility to do long range thinking and to do research in applied mathematics and possible applications. And it had various names as the laboratory expanded, first we called it the theory division, I believe. But it had changed its [name]. So, there was a group that was thinking ahead all the time.

MERTZ:

Do you recall any of the people who were [in that]?

POLACHEK:

Well, the first head of that, I think, was Dr. Wrench, and the second one Dr. Thalheimer. They were the mathematicians, some of them that I mentioned were part of the theory group, when we used to bring them in, we used to bring them to bear on our problems only when it was necessary. Otherwise they would be sitting and doing their own thinking.

MERTZ:

Did you do any training for any other group? You mentioned one group that broke off from your group and later became even larger than yours. Did you do any training for any other facilities?

POLACHEK:

Yes, when we first started, we did some training orientation for some officers and civilian employees of

the Bureau of Ships and any of its other laboratories to indoctrinate them on the potential applications of computers. And we gave a number of courses in the general use of computers, and, specifically, in programming computers, too. And we had started work in various places, had generated work by starting people thinking on their problems. I think I did mention this example and perhaps discussed it in some detail, of the SPCC, Ships Parts Control Center, which was located in Mechanicsburg, Pennsylvania, which had the responsibility for control of spare parts for the entire Navy. They were doing that by hand and by using old-fashioned IBM tabulating equipment. And we had developed the idea of using high speed computers with them. And finally, they had taken it over themselves and set up their own computers and probably had a larger group than we had, too.

MERTZ:

To get back to one earlier part of the story, and that is the NORC. After the David Taylor facility had developed, was there much more communication between the activities of the NORC people at Dahlgren and...

POLACHEK:

Yeah, we had used the NORC for the solution of problems, when we had the UNIVAC I, and when our facilities were not up to par to solve certain problems. And then there were just communications from time to time.

MERTZ:

What would you say were some of the advantages to using the NORC over the UNIVAC?

POLACHEK:

Oh well, the NORC was a much faster computer, it was more advanced than the UNIVAC, I forget the

exact -- well, both in speed and memory size and variety. And the NORC had drums, the UNIVAC didn't have any drums. It was a more advanced computer.

MERTZ:

When, do you recall, was it operating...

POLACHEK:

It was operating at, you meant the time, when did it begin operating? Well, it was built by IBM for the Navy, and it began operating sometime after 1952, sometime after I left. I don't recall right now what year, but we can..

MERTZ:

Do you recall any of the problems that you did run on the NORC?

POLACHEK:

Well, the problems that we ran on the NORC were primarily the nuclear reactor problems that we couldn't solve on UNIVAC I. We later went to the LARC or the 7090.

MERTZ:

Yeah. So, then you didn't have to depend upon the NORC.

POLACHEK:

Yes, we used the NORC as a stop

MERTZ:

Only as a stop-gap measure.

POLACHEK:

As a stop-gap measure, right.

MERTZ:

Well, thank you very much.

POLACHEK:

You're very welcome.

MERTZ:

This concludes the second interview with Dr. Polachek.

[End of Interview]

INDEX