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Computer Oral History Collection, 1969-1973, 1977

Interviewee: Alston Scott Householder

Interviewer: Richard R. Mertz

Date: July 20, 1970

Repository: Archives Center, National Museum of American History

Side 1

MERTZ:

Dr. Householder, would you like to describe your early years and experience?

HOUSEHOLDER:

Well, I guess I suddenly discovered more or less by accident that I was interested in mathematics. Not only that, but that I could make a living teaching mathematics, and I didn't discover this really until I had already graduated and was well along toward a master's degree in philosophy. So I was able to get an instructorship at Northwestern. I had taken a course in calculus in my senior year under the then chairman of the department, who was David Raymond Curtiss, and I taught there for two years, and then decided to go on to take graduate work full time, working toward a doctor's degree. I took one year off and then I went back to teaching again, at a prep school in Chicago, and doing graduate work in the summers. Then I got probably one of the last jobs that was available before the Depression hit in 1930, or just as the Depression was hitting, and stayed at, taught at Washburn College, from then until 1937. Again, I took off fall semester in 1935, and went back to the University of Chicago, and essentially finished up my thesis at that time.

MERTZ:

If I might go back just a little bit, your original degree was in Philosophy?

HOUSEHOLDER:

Yes, I, as a matter of fact, my original idea was to become a Methodist minister, and majoring in Philosophy seemed to be a reasonable thing to do.

MERTZ:

Did this involve classical languages?

HOUSEHOLDER:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

No, I didn't study any classical languages, and I always have since regretted that I didn't, but anyway, I didn't. I was interested in metaphysics and logic and actually, although my master's degree was in philosophy at Cornell, I took more mathematics courses than I did philosophy courses. I then began to discover that my real interest was in mathematics, rather than in philosophy.

MERTZ:

Was there any particular focus of interest in mathematics that intrigued you more than any other area?

HOUSEHOLDER:

Well to begin with, I thought I was interested in algebra and number theory. As it turned out, I wrote my thesis in analysis rather than in algebra, but as things have developed since then, my primary interests have come back into algebra. In fact, the aspects of numerical analysis that have interested me most are more the algebraic aspects or the algorithmic aspects than the analysis as such.

MERTZ:

Did this, would you say that there was someone, or a group of people among your teachers or fellow students that intrigued you through their teaching that might have stimulated your interest in numerical analysis or in algebra?

HOUSEHOLDER:

No, not really. I didn't develop an interest in numerical analysis until quite some time later. And that was after in fact I went to Oak Ridge, and became interested in computers. No, it was just that I had been taking courses in mathematics, and I began to realize that was where my interests lay, and although I had some good teachers, in those years I don't think anyone in particular was outstanding, in getting me interested in the subject.

MERTZ:

Would you say, this was at Cornell, did you do a thesis, a master's thesis?

HOUSEHOLDER:

I did a master's thesis. It was in philosophy, actually, well on the philosophy of Whitehead, his metaphysics of space and time, which was in a sense, mathematical. But it was a philosophy thesis.

MERTZ:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

I see. He considered I gather by some of us as a relativist and at least in some respects might have anticipated ...

HOUSEHOLDER:

Right.

MERTZ:

Which I gather what your thesis was concerned with.

HOUSEHOLDER:

Yes, not the technical aspects but the general philosophy and metaphysics behind the ideas.

MERTZ:

That was at Cornell, in 1927 on the eve of the Depression, when you were starting a job, which would enable you to teach mathematics, and pursue on a part time basis in your graduate work, or did that come later?

HOUSEHOLDER:

Well, while I was teaching at Washburn in Topeka, I spent what time I could in looking around to try to find a suitable thesis topic, and actually finally did. So that when I went back to Chicago, it was the summer of 1935, actually, and I stayed on through the fall. By that time I had already discovered a thesis topic that had interested me, and had made some progress on it, it was a subject in the calculus of variations, when I got to Chicago, I talked with Graves and Bliss, and worked it out to finish up the thesis under Bliss' direction. It was rather an unfortunate time to be there, because Bliss had the fall quarter off, but fortunately he didn't leave home, and was willing to have me come out to Flossmoor about once every two or three weeks, and confer with him there on the thesis.

MERTZ:

Did you take some courses at the same time?

HOUSEHOLDER:

Yes, I took some courses. I don't remember now what courses I was taking. Well, yes I do. I remember some of them. I took a course in Tensor Analysis which had considerable bearing on my thesis. But mostly during that time, I was working on the thesis, and trying to get it finished up. The following summer, I went up to Traverse City, Michigan, and

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

spent most of the summer boning up on the orals. At that time, the real examination came after the thesis had been accepted, and so I had to prepare for that examination, which I took at the end of the summer, and while I was at Chicago that summer, I got acquainted with some people who were working with Rashevsky in mathematical biophysics. He had just started there. And I arranged to get a Rockefeller Fellowship to come back in the summer of 1937, and work with Rashevsky, which I did. I had the Fellowship for two years, and then went on to the staff at the University, and stayed there until 1944.

MERTZ:

Had Anatole Rapoport joined the group by then?

HOUSEHOLDER:

Yes, he was at that time finishing up his thesis. He hadn't yet started working with Rashevsky. As a matter of fact, I can't remember when I first met him, but he had to do a hitch in the Army, and he didn't start working with Rashevsky until after the War, after he got out of the Army.

MERTZ:

Had he done some work earlier, before he started?

HOUSEHOLDER:

With Rashevsky? I don't believe so. As I recall, I think he just did his thesis in mathematics. They naturally got acquainted with each other, both being Russians, and he developed an interest in what Rashevsky had been doing.

MERTZ:

Was Thurstone working there at the time?

HOUSEHOLDER:

Yes, Thurstone held informal gatherings at his home, once every week. I think it was on Wednesday evenings. I attended those quite regularly. In fact, during one summer when I was there, it was the last summer, the summer of 1944, just before I left the University of Chicago to go to Washington, my wife was away with the family, and his wife was away, and he invited me to stay in his home for several weeks, which I did and enjoyed tremendously. Usually around nine o'clock in the evening, he would come out with a couple of quarts of beer and we would sit around the beer and chew the rag for about an hour before going to bed.

MERTZ:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

Did you ever encounter, I was wondering, a man named Patrick Youtz who was a mathematician?

HOUSEHOLDER:

Yes, I knew him slightly, and I ran across him only once or twice later. I believe he was then working, he went to MIT it think, and was working with the Whirlwind, for a while.

MERTZ:

He later worked with the production of the Whirlwind's electrostatic storage, which was a two-valve affair,... But he at one point, I believe, was working, doing some work for Thurstone in factor analysis.

HOUSEHOLDER:

Yes, he was. I don't remember exactly what, but he was working with Thurstone.

MERTZ:

Did you do anything, I recall, in my student days, which were some years later, working out some stable polynomial approximations to some problems involving graduate students and problems they were trying to solve, they involved computation problems on Marchants and Fridens of an order of magnitude which was not too great.

HOUSEHOLDER:

Right. I didn't get involved in computations as such, with Thurstone, I did become interested in a number of the problems in factor analysis, the underlying theory, and also in what was then being called psychophysical analysis. I guess the term is still good. And Gale Young and I published two or three papers, maybe more, on problems of that general area that came out in Psychometrika and one in the American Mathematical Monthly.

MERTZ:

In the thirties or in the forties?

HOUSEHOLDER:

This was in the thirties. It was in the late thirties, and I guess it was only in the late thirties. Gale Young left Chicago at about that time and then when he came back to Chicago, he was with, what was then called, the Metallurgy Project, and I sort of became involved in other things, and less involved with factor analysis.

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

MERTZ:

You spent most of your time during the War years at the University of Chicago, with Rashevsky?

HOUSEHOLDER:

With Rashevsky, yes.

MERTZ:

They were if my memory serves me correctly, they were in a converted house weren't they, sort of a sandstone?

HOUSEHOLDER:

Right. Well at the time I was there, I think the address was 5822 Drexel. It was on the second story of this old house, and Maude Slye was on the first floor with her rats. They moved after I left, I believe, to another house, a similar house, also on Drexel, or was it on Ingleside. It was not more than a couple blocks away. I guess it was on Ingleside, and maybe a block north, and so far as I know, the group is still in that building, I don't know; of course, Rashevsky's not.

MERTZ:

But Rapoport's still there, isn't he?

HOUSEHOLDER:

Rapoport went to Michigan quite some time ago.

MERTZ:

Wasn't he in charge?

HOUSEHOLDER:

I don't think he was ever really in charge. He had a group of several people working with him, but no, as long as Rashevsky was there, Rashevsky was it. Landahl, I guess, was his chief assistant, throughout the entire period, and I think Rapoport only had a group of maybe one or two or three assistants and students.

MERTZ:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

What kinds of problems did you work on and were concerned with?

HOUSEHOLDER:

Well, my primary interest was in the central nervous system, and Landahl and I collaborated on a book called The Mathematical Biophysics of The Central Nervous System, that was finished before I left, but didn't actually come out until about a year later. While I was there, McCulloch used to come to our seminars very frequently, and then one day, this young fellow, Walter Pitts came around, and he had a paper that he had written on Manic-Depressive Psychoses, and he wanted me to look at it, and I did, and it struck me as being quite an interesting piece of work, especially for a young fellow who didn't even have a bachelor's degree yet and was almost entirely self-educated and we took him on as an assistant, and he and McCulloch got together and they developed their paper, about the "ideas immanent in nervous activity", which is essentially the basic calculus, similar to the Shannon Calculus, but much more adapted to the structure of the central nervous system and the internal structure of computers as they subsequently developed. At that time I didn't think of them in those terms. It was only later when I ...

MERTZ:

Had you developed an interest in Turing machines?

HOUSEHOLDER:

I did later. Actually, after the War was over, I first went to Washington and was working with a group of psychologists and for the Navy, under the Applied Psychology Panel, and when the War ended, some of the psychologists plus an electronics engineer, named Henry Birmingham, and I went over to the Naval Research Laboratory when I learned about Turing machines and became reacquainted with an old friend, Herman Goldstine who was then working with John Von Neumann.

MERTZ:

When did you know Goldstine?

HOUSEHOLDER:

At the University of Chicago.

MERTZ:

Chicago?

HOUSEHOLDER:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

I was there at the time he was doing his thesis.

MERTZ:

Then it was at the Research Lab that you re-established your contacts with and became re-acquainted with Goldstine, and had you known Von Neumann?

HOUSEHOLDER:

No, I had not known Von Neumann before. In fact I don't think I met Von Neumann actually until later, after I went to Oak Ridge. My memory is really rather hazy. A number of things happened that I can't say for sure just when they did, but it wasn't really until I went to Oak Ridge that I became seriously interested in computing machines and realized how closely related they were to the kinds of things I had been interested in before, and in particular, the McCulloch-Pitts Calculus.

MERTZ:

Oh, I'm sorry. I anticipated you a little bit.

HOUSEHOLDER:

That's all right.

MERTZ:

If we could go back just to the time when you moved, you might want to describe perhaps a little bit about your activity with applied psychologists after you left the Rashevsky Project at Chicago, and came to Washington. Were these the statistical analysts?

HOUSEHOLDER:

Mostly statistics. The group was the primary concern of the group, was in training people to be Navy gun sight operators, to use these gadgets effectively and the particular kinds of gun sights were the Draper gyroscopic gun sights, and we made some effort to try to develop a theory of the operation of the gun sights, and while I was with the Naval Research Laboratory, I wrote a kind of training manual on the theory of the gun sight itself, the operation of a gyroscope and so on, which was put out by the Navy at that time as a restricted document. Later it was de-classified and I understand it was used, I don't know how much.

MERTZ:

Well, actually you left Chicago before the end of the War?

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

HOUSEHOLDER:

Yes, in 1944.

MERTZ:

It was in 1944, so the War was still going on when you were here in the Washington area, I gather. In the Naval Research Laboratory, this came about a year after your move to Washington?

HOUSEHOLDER:

Yes. The first year I came to Washington I think was in September or October of 1944, and this group was located at the Receiving Station, at Anacostia. We were there, the group was there, until it was disbanded, after the War ended, and then a portion of the group moved to the Naval Research Laboratory, and set up in the Fire Control Division. This Division was then under the direction of Robert Paige.

MERTZ:

Was there any particular motive for operating the move from Chicago to Washington, from the Rashevsky Project?

HOUSEHOLDER:

No. Well, my conscience had been hurting me right along that I hadn't been doing anything directly toward the War effort and when the offer came, I was asked if I'd be interested in joining the group. The psychologists felt that they needed some mathematical assistance, and so I came and interviewed for the job, and decided it looked interesting and came partly to ease my conscience and partly because it did look interesting, like a chance to really make use of some of the things I'd been doing.

MERTZ:

And from there, this moved to or at least part of the group moved to the Naval Research Laboratory. Did you continue more or less the same activity this training and the publication?

HOUSEHOLDER:

Yes, that was essentially a continuation of the kind of thing I'd been doing. In fact, I think I really started it before we went to the Naval Research Lab. With the encouragement of

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

some of the people in the Office of Naval Research, they thought there was something that should be done, and so I went ahead and finished it up.

MERTZ:

I see. I meant to ask you, parenthetically, did you, in your student days, have any interest in games of any kind?

HOUSEHOLDER:

Sports, you mean, or chess or that sort of thing? Not really. In my much younger days, I used to play some poker and some bridge. But I gave up the poker because I felt I didn't have the time to do it, and I gave up the bridge for more or less the same reason. I felt that if I were to become even a passable bridge player, I'd have to spend more effort on it. I just sort of gave up. I used to play a little tennis, handball, but that's about all.

MERTZ:

We can come up very briefly now, to about 1946, 1945, that's when you make your move to Oak Ridge, which is actually the more lasting phase of your career, did you know that at the time?

HOUSEHOLDER:

I went to Oak Ridge sort of with the expectation of staying there for a year or two, and then going someplace else, but when I got there, I became increasingly involved and enjoyed the work.

MERTZ:

Could you describe how you came to Oak Ridge?

HOUSEHOLDER:

Well, that was fairly easy. I had known Alvin Weinberg at Chicago. In fact, he was a graduate student, and wrote his thesis under the joint direction of Rashevsky and Carl Eckart, who was in the Physics Department, and then I continued the acquaintance with him. He started, after he finished with his degree, he started with the Metallurgy Project, and then he disappeared into parts unknown, and then after the War was over, I think it was around the spring of 1946, he was in Washington, and I guess, called us up. We got in touch with him. We were a little unhappy with the general situation of living in Washington, we lived in Hyattsville, actually, and there was a fair amount of commuting back and forth to the Naval Research Laboratory. Weinberg told us a little bit about the situation at Oak Ridge, it sounded rather interesting, and also a little bit about Los Alamos. So I went to both places to interview, both Los Alamos and Oak Ridge, and

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

decided I liked Oak Ridge better, and I think I'm glad I did, I think I made the right decision.

MERTZ:

Do you recall who you talked to when you went to the two places?

HOUSEHOLDER:

Well, at Oak Ridge, of course, it was Weinberg and Gale Young, who was there then, and Lothar Nordheim who was director of the Physics Division, just as a Mathematician, to see what I could contribute. At Los Alamos, I talked with Bob Richtmeyer, primarily. I met some other people. I don't now remember who they were.

MERTZ:

This was a little before Metropolis, wasn't it?

HOUSEHOLDER:

You know I can't remember seeing Metropolis at that time. I had known him at Chicago, slightly, but not much. My acquaintance with Metropolis really developed sometime later.

MERTZ:

Perhaps we might just backtrack a little bit. You were married in 1926, and have two children, a daughter and a son, or perhaps by ages, it should be the other way around, a son and a daughter?

HOUSEHOLDER:

A daughter and son.

MERTZ:

Daughter and son. Do you want to describe when they appeared on the horizon?

HOUSEHOLDER:

Well, I don't know. They came along during the '30's and we of course after that made certain re-organizations in life, but I guess I don't really know much what to say about them.

MERTZ:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

Well, had they conceivably been a factor in your move?

HOUSEHOLDER:

Well, as a matter of fact, they were a factor. I guess monetary considerations did have something to do with it. I shouldn't say I was entirely disinterested or altruistic in moving, or patriotic, in moving to Washington in the first place. I was doing really two jobs, in addition to my normal full time job at the University of Chicago with Rashevsky, I taught at the Northern Illinois College of Optometry. I gave two courses, of course, well there were several different courses I gave at different times, elementary physics, and freshman algebra, ordinary freshman college algebra, and geometric optics. And that substantially augmented the budget, which was quite necessary because the stipend from the Fellowship and the stipend when I went onto the staff at the University of Chicago was rather modest, about 200 dollars a month, and ...

MERTZ:

You might want to mention where you lived when you first were at the University of Chicago?

HOUSEHOLDER:

Well, when we first went to Chicago, we had an apartment on Drexel for a year, and then we moved farther south, I can't even remember.

MERTZ:

What side of the midway was it on?

HOUSEHOLDER:

South of the midway, and east. It was 6700 and something, around the park, actually. We lived there for a year, but we felt the children needed a different kind of environment, so we moved out to Glen Ellyn, and rented a house out there and stayed there until, for the rest of the time I was at the University.

MERTZ:

You commuted in?

HOUSEHOLDER:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

I commuted in. It was quite a bind. It took well over an hour each way. I used to get up and take a train at around 6:00, in order to get to the College of Optometry in time for an 8:00 class each morning. I gave my class there, and then I went out to the University.

MERTZ:

Was the College of Optometry in the Loop?

HOUSEHOLDER:

No, this was 4200 and something. It seems to me it was also on Drexel, but anyway, it was about that area.

MERTZ:

Then from there, you went out to ...

HOUSEHOLDER:

Yes, as I recall, I took the Cottage Grove Streetcar out, took the Elevated out to there and got off at, what it is stop where it goes around, I can't remember any longer.

MERTZ:

Oh, yes, I know what you mean. It's sort of the end of the line ... 63rd?

HOUSEHOLDER:

No, the Elevated comes directly South until it comes to about 42nd Street, and then it makes a jog, and there's one of the main stops on that jog.

MERTZ:

Yes, I know.

HOUSEHOLDER:

That's were I got off the Elevated, and the College was not very far from there. And then as I recall, I took the Streetcar from there on out to the University.

MERTZ:

Was Eckhart Hall ...

HOUSEHOLDER:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

Eckhart Hall was finished ... let's see ... I started at the University of Chicago in the summer of 1928, and Eckhart Hall had just been finished, and I was there. They were holding classes for the first time that summer, in Eckhart Hall. Prior to that, they had been in Ryerson, of course.

MERTZ:

And when did they throw the Mathematics Department out of the Hall?

HOUSEHOLDER:

Well they never really threw us out altogether. I don't remember for sure. I think it was perhaps around in 1941. The Library was closed off, and certain parts of it were closed, completely closed off, yes.

MERTZ:

Then mathematics was then taught elsewhere too, at the University?

HOUSEHOLDER:

Yes, it was scattered around somewhat. I don't remember exactly where. Of course, I was at that time with Rashevsky, and ...

MERTZ:

Was Ernest Preston Lane ...

HOUSEHOLDER:

Lane? E.P. Lane? No, he wasn't chairman at the time. I don't think he was. I had a couple of courses under Lane. I liked him very much.

MERTZ:

He's still retired? Living in Southwest Virginia?

HOUSEHOLDER:

Yes, the last time I heard, but it's been quite a long time since I heard anything, he apparently gave up mathematics entirely, and hasn't been back to any meetings, or anything of this sort.

MERTZ:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

The last time I heard, was from Lester Ford. He was in retirement at the University of Virginia in Charlottesville.

HOUSEHOLDER:

Oh, I guess you know Ford is dead.

MERTZ:

No, as a matter of fact.

HOUSEHOLDER:

Well, I really didn't have much of a feeling whether Oak Ridge or Los Alamos was better, from a living standpoint. I remember one thing that rather threw me away from Los Alamos, that somehow I hated to be that far away from civilization. And also the fact that Weinberg and Young were old friends and I didn't know anyone at Los Alamos, except Bob Richtmeyer, whom I met only there, and hadn't known before. And I felt from that point of view that Oak Ridge might be a little more congenial, a little easier to get into.

MERTZ:

And so then you appear in Oak Ridge in 1946, as a mathematician in the Physics ...

HOUSEHOLDER:

Physics Division.

MERTZ:

Physics Division. Could you describe briefly the area of mathematics--mathematical area you were concerned with when you arrived there?

HOUSEHOLDER:

Well, mathematically, the problems were ordinary and partial differential equations, and I became involved in some problems that involved matrices, which I liked. The general setting was in Reactor Criticality Problems. They had a small computing group, a group of girls mainly, under the direction of Bob Coveyou, who had a section in the Physics Division that did computing, hand computing, for the, mainly, for the physicists and, it seemed as though it would make sense to expand that somewhat, and so I was put in charge of that group, and it was made into a mathematical and computing section of the Physics Division. And then a year after that, since a number of our customers came from

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

other divisions besides just the Physics Division, we were set up as what was called the Mathematics Panel. It was really an independent division, separate division, but being very small, only about fifteen people, "division" seemed like a rather imposing title, for such a small group, although Panel was not a very good name either really. But anyway, by that time I had become convinced that the Laboratory needed to have its own computing machine and ...

MERTZ:

This was in 1946 or 1947?

HOUSEHOLDER:

Well, I'm really skipping over two years. In 1947, the Section was formed, and in 1948 it became separate, independent.

MERTZ:

Autonomous.

HOUSEHOLDER:

Autonomous.

MERTZ:

Now you might want to mention, in this regard, was it when you had the hand machine, the hand calculating group, under your responsibility, that you became more aware of or involved in actual computational problems that they were faced with. These were, I take it, the Marchants and Fridens?

HOUSEHOLDER:

Right.

MERTZ:

There was even more of an intensified awareness on your part, of the existence of this problem. Now how, could you describe a little bit more, about how you became aware of the computer technology, in this period of 1946, 1947, 1948?

HOUSEHOLDER:

In January of 1947, Aiken had his first symposium in which he unveiled the Mark I, and I attended that symposium along with Henry Garabedian, who was also there at the time.

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

And it was then I began to become really aware of what was going on and what was developing and I started reading and thinking about the whole situation and felt that this was definitely the coming thing and that the laboratory of the type of the Oak Ridge National Laboratory was certainly going to need one of those computers.

MERTZ:

Had you been made aware of the work of the ENIAC of the Moore School or some of the work or some of their plans, Von Neumann, plans to ...

HOUSEHOLDER:

Yes, of course I learned more about that at the Aiken meeting, but I had already known something about it. Morrie E. Rose, one of the theoretical physicists there came up with some rather elaborate problems that were clearly beyond Friden and Marchant, and I don't remember when it was, but it was around that period, perhaps, I don't know, in the fall of 1947 or early 1948, I visited Aberdeen to check up on the possibility of using the ENIAC for doing some of this. I think perhaps I also went to Harvard and talked to some of Aiken's people about the possibility of doing it on the Mark I, and we never did run anything on the ENIAC, Dick Clippinger was then in charge. I would ask him questions about how much trouble it was to do the programming and, how long it would take to learn to do the programming and what size problems one should consider. He said, as I recall, something like a matter of three or four months to do the programming for the problem, and one shouldn't attempt to put a problem on the ENIAC unless it would involve several years of hand computation. That sort of discouraged me a little bit.

[End of Side I]

[Start Side II]

MERTZ:

Dr. Householder, did you know about this course that was being offered at the Moore School, at the University of Pennsylvania, derived from the experience of ENIAC and ... talking about and looking forward to EDVAC?

HOUSEHOLDER:

Right. I learned about that while I was at the Naval Research Laboratory and had been very much interested in attending it myself, but I was then beginning to think about going elsewhere so I didn't feel as though I was entitled to push the matter and try to get approval from the Naval Research Laboratory to go, and so I didn't attend it. After I got to Oak Ridge, I did arrange to get at least copies of some of the course notes put into the library there, and that, I guess, in a way, was even more of a beginning for me, to get

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

interested in this the ... well, I suppose I wouldn't have been interested in perhaps, in attending Aiken's Symposium if it hadn't been for what I learned about that.

MERTZ:

Do you recall, in particular, some of the things that you did read, from the Moore School? The summer course that intrigued you?

HOUSEHOLDER:

I'm afraid that I don't specifically. I was beginning to see the connection between the logical design of the computer and the notions about the logical design of the central nervous system. That was the thing, from a theoretical point of view, that interested me much more than anything else.

MERTZ:

Did you by any chance know of or come across C.V.L. Smith?

HOUSEHOLDER:

Oh yes.

MERTZ:

I believe he was with the Office of Naval Research, approximately this time, 1946, '47?

HOUSEHOLDER:

Well, actually I didn't get acquainted with Charlie Smith until a little bit later when he was working with Raytheon. When we first started looking around at the possibilities for getting a machine for the Laboratory, the proposed Raytheon machine was one of those that we thought about and I remember going with one or two of our electronics engineers to visit Raytheon. I don't remember what year that was, it must have been 1948 or 1949.

MERTZ:

Well, there was some time between the, sort of, initial efforts to use the high speed electronic digital machines, the solution of the problem, and the actual use. These machines were not all that available, and indeed the ones that were available, were not necessarily the amenable to use the problems that ... ENIAC was for example. Were you then on the committee that helped decide and recommend the kind of machine that was ...?

HOUSEHOLDER:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

At the Laboratory. Yes, that was essentially my job. With the assistance and advice of mainly the electronics engineers at the Laboratory. And while I made quite a few trips here and there around the country, to visit various establishments, Mauchly and Eckert, and we went up, on one or two occasions, to visit Herman Goldstine at the Institute for Advanced Studies.

MERTZ:

HOUSEHOLDER:

Oh yes. The Whirlwind, and also of course, to Harvard to talk to Aiken and some of his people. One of the things that we considered was arranging with General Electric to make a copy of the Mark, I forget whether it was the Mark III or Mark IV, Mark IV I guess it was, and we considered that quite seriously. Flanders by that time had gone to, had started at Argonne, gone from Los Alamos to Argonne and he was in a way a little bit ahead, in that he had made arrangements for their electronics group to make a copy of the Von Neumann machine, and of course, as it eventually worked out, we made a deal with Argonne to send some of the Oak Ridge engineers to Argonne to try to improve the design, not just to make a copy, but to improve the design; we spent, I believe it was six months, in working on the improved design, and then to build the machine under the direction of Chuan Chu, who was the electrical engineer in charge at Argonne.

MERTZ:

Do you recall when you made your visits ... around what did you like about the evaluations of the various machines, you visited and what struck you as being their strong points or weak points as far as using them for the laboratory at Oak Ridge?

HOUSEHOLDER:

Well, I'm sure I did write reports. I have no idea whether any are still in existence or not. We felt that the Raytheon machine was a little too expensive, they were asking all of \$375,000, as I remember, for Raytheon. Reeves Instrument was considering making a digital machine, using delay line, and we talked quite seriously with them for a while. We felt that their logical design in general was better than any of the other commercially built machines.

MERTZ:

Was this Lubkin by any chance?

HOUSEHOLDER:

Yes, Sam Lubkin was involved.

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

MERTZ:

Had he briefly been taken on by Reeves?

HOUSEHOLDER:

I believe that's right.

MERTZ:

I don't know whether he stayed there very long.

HOUSEHOLDER:

I don't think he did, but I really don't remember what became of him.

MERTZ:

At that point, I believe Sam started on the analog.

HOUSEHOLDER:

Yes.

MERTZ:

Do you happen to recall, hopefully they haven't destroyed or left your reports at the time, they would be of interest as a contemporary documentation by an outside visitor to some of these computing facilities about which we're interested in. What essential factors were inclined in your decisions which you made in regard to the computer?

HOUSEHOLDER:

Well, I think I can tell you in a nutshell. As I said, I felt the Raytheon machine was too expensive, and although we rather liked the logical design, we also liked the logical design of the machine Reeves was proposing to build, as I remember, it was called the REEVAC. With regard to Mauchly and Eckart, we felt that they were trying to do all things for all people, and really the UNIVAC was going to be less designed for scientific computing, well at least that they were trying to do too much in the way of commercial applications and that it would not be a suitable machine for Oak Ridge, and those were about the only possibilities from commercial firms that were building their own machines. We thought, as I said, of asking General Electric to make a copy of the Mark IV, but we decided the Mark IV was too cumbersome a machine, and was definitely not in the new generation of machines.

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

MERTZ:

Was it an internally stored program by that time, something that was taken for granted as part of the requirements that you'd want in a machine?

HOUSEHOLDER:

I don't believe the Mark IV was ...

MERTZ:

I was thinking about one of the considerations ...

HOUSEHOLDER:

Oh right, yes. That was one of the considerations.

MERTZ:

And another, how about access and speed?

HOUSEHOLDER:

It was slow, and of course, Aiken's argument was you aren't going to get reliability with much higher speed, it's just not practical. Well, we didn't quite agree with him at that point. Another possibility that we considered was of course, by that time the Bureau of Standards was well along on the SEAC, and we thought about trying to work out a deal either to have the Bureau of Standards build a copy of the SEAC, or perhaps assemble some engineers at Oak Ridge to build a copy of the SEAC, but P.R. Bell who was our chief engineering consultant at the Laboratory, did not like the engineering of the SEAC too well, and I guess I shouldn't try to quote him on it or summarize his reasons. His dis-recommendation on the idea of having G.E. do a copy of the Mark IV was also substantial. So we gave that up and the possibility of making a deal with Argonne became increasingly real and attractive and there was just no question in our minds that an Argonne type machine was the kind of machine that we wanted if we could get it. So then, as I remember, it was in the fall of 1950, we actually worked out a deal with Argonne to do just this.

MERTZ:

Were there any special problems that came up in connection with the similar mathematical requirements for this machine? Did you anticipate any particular problems in regard to a ...

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

HOUSEHOLDER:

Well, of course at that time a thousand words was a large memory, and nothing much larger than that seemed feasible, and we certainly wanted a thousand words if we could possibly get it. For some of the problems, some of the mathematical problems that we knew we were going to be faced with were in the actual size of problems and we did need a large memory, for large systems of ordinary and partial differential equations. And then, one of the problems that M.E. Rose came up with, first the L-Shell Calculation and then the N-Shell Calculation, we did actually do the L-Shell Calculations on the Mark I. We sent some of our people to Harvard to set that up and run it through and then we made a deal with the Bureau of Standards and did at least some of the N-Shell, I don't remember whether we did it all on the ENIAC.

MERTZ:

Was this the SEAC?

HOUSEHOLDER:

Oh sorry. On the SEAC, right. And the memory limitations there were rather serious. It involved some rather tricky, as I remember, some rather tricky programming to fit the problem on to that machine.

MERTZ:

That's limitations?

HOUSEHOLDER:

We would have preferred using the SWAC, as I recall, it would have been more suitable, I can't remember exactly why, but I think it was partly in terms of memory, but the SWAC didn't become reliable quite as quickly as the SEAC did, and so we had almost no choice. Meanwhile, we had been doing other kinds of problems, using just ordinary IBM machines. We didn't have any multiplying punches at the Oak Ridge National Laboratory, but at the Y-12 Laboratory, and at K-25, they did, and they put in CPC's as soon as they were available, and during the interim period, before the ORACLE became completed, we sent some of our people first to the Y-12 area to run problems on the CPC's over there, and then some problems developed, and we transferred them over to the K-25 area, and continued to do quite a lot of work in that way, until the ORACLE reached its final stage.

MERTZ:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

One other question. At that time there was a symposium that was being sponsored by IBM, a series of meetings, of computational problems, I believe, where serious complications was heard came out of this.

HOUSEHOLDER:

1947, 1948?

MERTZ:

Yes.

HOUSEHOLDER:

There were two of them in 1949. I think there was one in 1948.

MERTZ:

Yes, it was in this period. Some very early work done in applications. Do you recall participating in these?

HOUSEHOLDER:

I went to one of those, in 1949, August, it seems to me, I don't remember. I gave a paper fitting exponential decay curves. They were important at all the atomic laboratories, and I thought I had some ideas on how to do it, but they didn't work out so well. I gave a paper on it and it came out in one of the Proceedings volumes.

MERTZ:

To get back a little bit, in this connection with Von Neumann, was it in this time that you got to know Von Neumann a little more or ...

HOUSEHOLDER:

Yes, it was during this period. We put on a little ... we were foolhardy enough to put on a sort of training session on programming. I think it was in 1950, or possibly 1951.

MERTZ:

At Oak Ridge?

HOUSEHOLDER:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

At Oak Ridge. It was well before the ORACLE was completed, but our people were beginning to learn something about programming, and so on, and so we organized I think it was, a two week series, and advertised it among the southeast colleges and universities and we had, I don't remember, I guess about 25 or 30 people attending and Von Neumann was one of the speakers, I believe he gave the keynote speech at the opening of the session.

MERTZ:

This was around 1950 or 1951?

HOUSEHOLDER:

Somewhere around there.

MERTZ:

Do you recall whether any of those were recorded? Wire recorded?

HOUSEHOLDER:

I don't think so. We did write up at least some of the lectures and put them out in mimeographed form and distributed them to the people in attendance.

MERTZ:

One of the things we're interested in particularly is Retrospective Oral History, and that is collecting recordings and talks given by someone, somewhere.

HOUSEHOLDER:

Yes, well, we did not make any tape recordings of any kind.

MERTZ:

But there are possibly in existence some of the notes?

HOUSEHOLDER:

There very well may be. It may be that the Laboratory's central files still has copies of those. I can check when I get back.

MERTZ:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

We're very much interested in the very early stages of training. There was a book that came out in 1950 ...

HOUSEHOLDER:

In 1950? Oh, Wilkes, yes.

MERTZ:

And there were a couple of early courses given, training programs given in 1951 was the first one given by, sort of a one-semester course then they gave a special summer session course. Aside from being the keynote address speaker, did Von Neumann play a more active role? Essentially, until ORACLE was running, which was about when?

HOUSEHOLDER:

It started, it was put into operation at Argonne in the summer of 1953, and we sent some of our people to Argonne to run some preliminary problems and one of the first programs that was run on it was the Givens Eigen value Program. Givens was then a consultant at the Laboratory and he had Virginia Carlock, who was one of our programmers, set up the program. Anyway, during that summer, several of our people spent a period of time there and ran a number of things on the ORACLE. But it wasn't really finished, and they continued working on it through the fall, and it was shipped down to Oak Ridge, and it went into operation in early February of 1954.

MERTZ:

So it wasn't until the early spring of 1954 that you had a digital computing capability?

HOUSEHOLDER:

That's very true.

MERTZ:

Prior to that time, had you relied on CPC or ...?

HOUSEHOLDER:

CPC was the best we had, or borrowing time on SEAC, etc.

MERTZ:

What about the others such as SEAC or with the advent of now, sort of a new house, did this generate problems in itself?

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

HOUSEHOLDER:

Oh, definitely. Before the machine arrived, I used to have nightmares worrying about whether there would be enough problems to keep such a voracious monster fed, you know? But I didn't need to worry, as soon as it got there, people began to get acquainted with what it could do. The thing is, that until a machine of that sort is available, people don't really think about it, but when it's there and they begin to hear about what it can do, and think about how it might be used for their problems, then all of a sudden, there's a whole slug of them. And so ... I don't think ... I couldn't really trace the growth rate, but in almost no time at all, we had filled up one eight hour shift, and were ready to go to two.

MERTZ:

It started to go up exponentially ...

HOUSEHOLDER:

Right.

MERTZ:

At this time, your group had started to grow, as this grew, as the demand by users at the time for programming. Was there a central programming facility then which fell under your purview for the whole Laboratory?

HOUSEHOLDER:

Yes, well we always ran an open shop, it was never a closed shop operation, and we'd run training sessions. One of our people would give a series of lectures on programming, and the, say the, chemists and physicists and other people would learn to do their own programming, or else they hired programmers of their own. But we had the central programming group and we were always there to take on whatever jobs were brought to us.

MERTZ:

Sir, you did quite a service function for possible users. Did you get many customers?

HOUSEHOLDER:

Oh yes, an increasing number. Well, the two things went on parallel. Our group grew and more problems came to us to program, but at the same time, other divisions, especially the reactor division for example, took on their own programmers and trained their own programmers.

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

MERTZ:

In regards to programming, the development of programming for this particular machine, did you fairly early in develop some libraries of sub-routines?

HOUSEHOLDER:

Yes, well, we started right away, to try to build up a library of sub-routines. We put one of the programmer's in charge of collecting and testing these. But we always felt that we weren't doing as much of this as we should. There was always the pressure of people coming in with a problem that they wanted done yesterday, you know, and so we could never quite find it possible to take one or two or three people and put them full time on developing sub-routines, which, in retrospect now, I feel that we should have done regardless, but we didn't. So the sub-routines library to some extent developed a little bit haphazardly. People were encouraged to contribute to the sub-routine library any particular sub-routines that they might develop in connection with any particular problems they might be on, but that's about the way it went.

MERTZ:

But there was, hopefully, a library set up, so that there was a reduction of the redundancy of programs?

HOUSEHOLDER:

Right.

MERTZ:

How many specific numbers of people did this involve? You say you imagine that the Mathematics Panel had about fifteen people?

HOUSEHOLDER:

We started out with about fifteen people. I really don't remember exactly how rapidly the growth did take place. I guess by the time the ORACLE arrived, there were probably thirty or maybe forty people, and for a period of time, it was rather static, it didn't develop very rapidly. Weinberg was always rather anti-computer, and he was never quite convinced that these things were here to stay, and he was always a little reluctant to authorize too many people for this, and it was only in about the last three or four years that the division, then called a division, grew quite rapidly, and I guess the peak was around 120. That was a little over a year ago.

MERTZ:

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

Now one question. Did you notice at any point in all of this, a kind of disjunction between say mathematics, people who were formally trained in mathematics and programmers as such? Or was there an isomorphic relationship between the trained mathematician and the person who was not?

HOUSEHOLDER:

I guess I'm not really sure what you're asking. The people we hired as programmers, by and large, were mathematics majors. People who had just graduated, or maybe not just graduated, but did have a major, an undergraduate major, in mathematics.

MERTZ:

They were undergraduate BA's, BS degrees.

HOUSEHOLDER:

Right. The lowest level job essentially, the lowest level professional job was a job for a college graduate who had an undergraduate major in mathematics, and who was willing to learn programming. Now there were a few exceptions and the number of exceptions increased as time went on, but we never took on anyone with less than a bachelor's for the job, but we did take on a number of people whose majors were in other areas, physics or even chemistry.

MERTZ:

Now how about the area of say, more mathematicians than programmers, that is, more interested in mathematical analysis than in programming. How did that change in composition?

HOUSEHOLDER:

The people who were farther along, mathematicians who were either farther along towards the degree, or who actually had the degree, almost all of them we encouraged to learn something about programming. In fact, for a long period of time, all of them did, and most of their efforts was in directing the programming work of the junior people and although we started out as a group doing mathematical consulting, as well as doing computing, for a period of time, there was almost no one doing any real mathematical consulting, it was essentially all either programming consulting or programming supervision, and it was only starting three or four years ago that we actually hired some people who were not involved primarily in programming but were doing mathematical consulting. Now these are the mathematicians themselves. There was also a statistical group, who did statistics. But that was different, that was separate.

Computer Oral History Collection, 1969-1973, 1977

Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

MERTZ:

You might want to remark on how the capability changed in terms of machines, and how does the current or reasonably current computer capability of Oak Ridge stand in the spring of 1954?

HOUSEHOLDER:

Well, of course in the spring of 1954, the ORACLE was essentially then the best machine in existence, and remained so for at least a few months. At least we thought it was. The IBM machines, the 701 and so on, were just beginning to come out, I think in fact the first 701 may have gone into operation a little before the ORACLE did, and there may have been one or two others. We never did quite consider the early UNIVACs as being in the same class, essentially for the reason I've indicated, but then of course the 700's were improving, the 704 and so on, and by about 1956, the ORACLE was already beginning to obsolesce, and there was need for more computing capacity, computing power, but the Laboratory was having some budgetary problems, and it didn't seem possible for the Laboratory to buy or even rent another machine, so a joint deal was worked out with the Gaseous Diffusion Plant, and they put in I think a 704 which was jointly used by the Laboratory and by them, and their capacities increased, and those at the Laboratory did not for a period of time. In fact, the next real step at the Laboratory was the acquisition of a 1604 A, which I believe was in 1962.

MERTZ:

That was a different generation altogether.

HOUSEHOLDER:

A different generation altogether. Of course now the Laboratory has a 360/75 and a 360/91, and it's already looking at bigger things.

MERTZ:

Has there been an increase in the amount of direct user involvement in the Laboratory on programming in terms of time-sharing?

HOUSEHOLDER:

I wouldn't say there's been a relative increase. There's been an absolute increase. But I would guess, that relatively, it's remained fairly constant.

MERTZ:

Thank you very much Dr. Householder.

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Alston Householder Interview, July 20, 1970, Archives Center, National Museum of American History

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