INTERVIEWEE: Dr. Edward Cannon

INTERVIEWER: Dr. Richard R. Mertz

DATE: 11 May 1971

RRM: This is an interview with Dr. Edward Cannon of the National Bureau of Standards, conducted on the 11th of May, 1971 in his office at the Standard's Headquarters in Gaithersburg, Maryland. The interviewer is Richard Mertz.

[Recorder off]

Dr. Cannon, would you care to describe your early .. background and training .. that led you into mathematics and into the field of computers?

EC: Yes. Sometimes I'm not sure I know the answer to these questions, but .. looking back and thinking of my career, I - I as a boy was interested in mathematics, but I think the reason I chose mathematics as a career was the influence of my father and the teachers I had in high school. I happened to have excellent mathematics teachers. They presented it - they made the subject both challenging and interesting for me. And then in college I was fortunate in having a, a - a good mathematics teacher, in particular the head of the mathematics department at the University of Delaware .. was quite interested

in, in helping people who seemed to want to go beyond the usual courses, and he formed small groups of boys and girls together. The University was not co-educational at the time, but he had us meet in the library to study things like the law of ... sign in advanced calculus. So that was the reason I chose mathematics, I believe.

However, at -- in my youth at the University of Delaware, the only school I could afford to attend, there was no mathematics major, so I chose electrical engineering because that was the curriculum that offered the most mathematics. So I graduated as an electrical engineer, taught electrical engineering for awhile, and then returned to mathematics in 1935 and went to Johns Hopkins to obtain a degree in applied mathematics.

RRM: Where did you attend high school?

EC: At Bridgeville, Delaware.

RRM: At Bridgeville, Delaware.

EC: That was some time ago; I won't give you the date.

RRM: .. .. When you went to Johns Hopkins .. .. was there a particular -- This was for graduate work. Was it for a masters or a PhD?

EC: PhD.

RRM: I see. And what field of applied mathematics? Did they have any break-down of fields?

EC: No. I was particularly interested in differential equations, and - and what we would call the differential equations of - of physics, mathematical physics; a mathematician talks about boundary value problems, partial differential equations, systems of equations. And -- Do you want me to go on now from there?

RRM: Yes. With whom did you do your work at Johns Hopkins?

EC: I worked under Murnaghan who was then head of the department and Arl Wintner. .. They were both very good applied mathematicians. Wintner was more concerned with rigor and Murnaghan more concerned with the kind of applications that interested me, because I was fascinated by the use of mathematics in - in the solution, the formulation of solution of problems in engineering, particularly in wave motion propagation.

RRM: Excuse me, where did you teach electrical engineering? There was a brief period between --

EC: That was at the University of Delaware. I had gone to the General Electric Company as a test man and stayed there about a year and then returned

to Delaware to teach, and then went from Delaware to Johns Hopkins. .. Returned to Delaware to teach after that, teach mathematics.

RRM: Was there any particular .. reason for going to Johns Hopkins as distinct from any other school?

EC: I knew something about the faculty. I had heard something about the people there, particularly the young mathematicians whom Murnaghan had added to his staff. .. And I believe that was the influence of my teachers at the University of Delaware who knew this Mathematics School.

RRM: It was probably, along with Pennsylvania, one of the nearest major graduate schools.

EC: That's another thing. The fact that geographically it was near, and --

RRM: How long did you stay at Hopkins before you finished your PhD?

EC: I was a Hopkins four years.

RRM: .. Do you recall your dissertation topic?

EC: I've forgotten. It was in - in, in Lorentz matrices. I've forgotten the exact title. It had to do with factorization of Lorentz matrices.

Then I returned to Delaware and taught mathematics until the beginning of World War II. I

enlisted in the Naval Reserve, ... ending up in a statistical group, Bureau of Ships, in Washington, D.C., working under John Curtiss, who later came to the National Bureau of Standards, as well as Dr. Condon, then Director of the Bureau. They recognized the need to -- for greater employment in the Bureau, mixed with the planning of experiments, of - of the modern statistical techniques. That discipline was not universally known and - and applied as it is today, in physical science laboratories. It had been applied in agriculture initially. And --

RRM: Had you - during the war years had you been - when you were in the Naval Reserve, were you doing mathematics, working in mathematics-related jobs?

EC: In the Naval Reserve?

RRM: Yes.

EC: No, I was a garrison officer. And I was a line officer, one of the people who never left the eastern shore. [Laugh] I was in the Fourth Naval District. And I - I - we tested equipment on board ships which was designed to protect us against magnetic mines. Then I was in command of a station for about a year and then the war was over. We had about 25 men and 2 officers. ...

There was some mathematics, interesting mathematics, at the foundation, at the base of this garrison operation, but I was never able to obtain any training because the naval officers at the Fourth Naval District with whom I worked felt it wasn't necessary. They were unique. That was not the general opinion. One naval officer I remember particularly would not approve any officers, for example, coming to Boston for, say, a month's training at the Bureau of Ships in mathematics. I was intensely interested and got all the documents I could, but I can't say I was using mathematics insofar as certain important but small scale operations.

RRM: How did you finally wind up then at the Bureau of Ships under John Curtiss?

EC: I - I ended there, I came there, because Curtiss was looking for someone of my background, mathematician, engineer combined, for his group. And he had - he knew Wintner, my former teacher, and he heard of me. So Curtiss had the strings pulled to have me transferred here. At the time I objected to the transfer because I knew nothing about modern statistics. I had been given a local transfer to Philadelphia and - and .. was teaching radar operation

and maintenance. I was interested in that because of the mathematics, it was fascinating. So I did not want to leave that area. Curtiss [laugh] was master of my fate at that time in Washington, and he very cleverly gave me some books on modern statistics, and the same books, then I realized that this - that there was a beautiful underlying coherent mathematical theory, which I had not even suspected before from the elementary courses in statistics I had taken. So I was content to remain.

RRM: Then your activities took on definitely more mathematics?

EC: Then they were mathematical in the way of statistics, modern statistics, and followed Curtiss to the National Bureau of Standards. Curtiss -- I won't explain the reasons for his coming, and --

RRM: Excuse me - about when was this?

EC: That was around the fall of 1946, summer of 1946.

RRM: So the war was essentially over.

EC: The war was essentially over. Census Bureau was interested in the computer that Acton, Mauchly, and Pres Eckert were talking about. Actually I believe funds had been transferred from Census Bureau for the acquisition of such a computer, before I came.

And that - the reason I followed John Curtiss, was he arranged for an offer to be given to me to - for employment in the Bureau - to work in that area. He wanted a combination of a mathematician-engineer. And that was my introduction, to the Bureau.

RRM: You had, prior to this time, no actual exposure to - to perhaps call it hay calculus?

EC: I knew something about it, yes; but I don't believe - I don't believe I had even heard of the Bush differential analyzer at that time, I didn't know the distinction between an analog and digital computer.

RRM: But you had used Marchants or Fridens or something of that sort, in the Bureau of Ships?

EC: Yes, in a routine straight-forward manner, not thinking much about applications there, if at all. Doing no numerical analysis in the sense in which we do - think of it now. As a matter of fact, very few people did. ...

RRM: So in '46, and this is perhaps a couple of years before the decision was made to proceed with an interim machine which came to be known as the CIP was made. You came to the Bureau of Standards. How was it organized in the sense of ... You were under Curtiss?

EC: I was under Curtiss. .. In July of '47 there were enough of us under John Curtiss for the organization of an Applied Mathematics Division. It was then called the National Applied Mathematics Laboratory. Central Pacific.

Up to that time we had been assistants to the director. Churchill Eisenhart joined us, who was teaching at the University of Wisconsin. Bill Cohn joined us. About that time Mrs. Ida Rhodes came down to Washington from the Mathematics Tables. She had been working in Washington on the Bureau's computer program.

The Bureau's support of the computer program was - was jointly sponsored with the electronics division, I've forgotten the exact name, which at the time was headed by ... Chuck Page and finally Sam Alexander, .. and the mathematics division. The Executive Officer of the Bureau was a scientific officer, .. was contracting officer. Contracts were let by the Bureau for the development of the machine. Sam Alexander was an assistant. I believe he was called scientific officer, Alexander was his assistant and I was assistant to the contracting officer. Engineers were responsible for engineering

features and mathematicians were responsible for the last design: what commands should be built into the machine. Of course there was not a strict boundary between them. Both were interested in what the other group was doing. But the primary responsibility lay there. The administering of the contracts was assigned to the mathematics division. That is, making sure the money was available for paying the contractors; I mean, the physical administration was the responsibility of the mathematics division. That was in the early days of the Bureau's program. We had some difficulty because the machines had not been designed. They were not available off the shelf. We had some difficulty in making sure we had the right kind of monies available, monies with the corresponding appropriate authorization. ...

RRM: Well, now I believe a decision was made to go ahead and build the SEAC in the fall of 1948. .. Now there is a period of about two years from the time that .. you came to the Bureau of Standards to the time that they actually started working on the project. Was any of your activity in this National Applied Mathematics Laboratory .. .. applicable

directly or indirectly to what then came to be the responsibility both administratively and mathematically for the applications of coding for - for SEAC in this two year period before ..?

EC: Yes. I was designated assistant chief of the new division and really I was assistant chief for computing. My responsibility was the computer program. Curtiss assigned that responsibility to me; that is, the mathematics responsibility of it.

RRM: Was that fairly early on?

EC: That was early. It was [in] the formation of the division.

RRM: I see.

EC: And - and .. and during that period we were - I was concerned completely with - with the computer program [and its various aspects]. First of all we had to decide how to .. arrange to have a satisfactory design, [consult with] experts in the field [on] sound designs, and the development according to those designs leading to the point where the machine would be available for procurement. Well, we arranged to have the advice of the National Research Council Committee on High Speed Computing, on the general features of the program.

The outcome of that was that the Bureau hired Dr. George Stibitz - you may have talked to him - as a consultant and he recommended the procedure to be followed, namely competition, design competition among principal suppliers, of whom there were few. There were very few. We had circulated by letter and by - by direct interview when we could meet them .. all the companies where we felt there could be an interest, including International Business Machines, ... Most of the large .. the, the computer, the - the companies which we associated with, with the desk calculator .. were not interested in this new venture because they had to recover from the, from the diversion of their activities during, during World War II.

RRM: Mhm. Excuse me. Do you recall who in the National Research [Council]? Was this Warren Weaver's group?

EC: I don't remember who the chairman of that committee was. Warren Weaver was a member, John Von Neumann was a member, .. Caldwell at MIT was a member.

RRM: Was Archibald .. ?

EC: I don't think Archibald was. I think - I think, .. I think not. I believe Aiken was a member.

RRM: Mhm.

EC: I think Von - I think Von Neumann was the chairman, I'm not so sure because I seem to remember correspondence between them.

RRM: Mhm. And .. and the Stibitz recommendation then came, .. was this in '46 or '7?

EC: I believe it was in '47.

RRM: '47? .. About the design committee?

EC: Yes. I believe early in '47.

And then we managed to .. let contracts to the Hughes Tool Company, Raytheon Corporation, and, then, I think it was Eckert-Mauchly Corporation, it was called then - for the designs. And we had the assistance of this, the NRC Committee in the evaluation of design.

RRM: Mhm. Was it the same group that --

EC: Same group.

RRM: did the design? I see. About how many - ?

EC: In addition, I - I seem to recall that Jay Forrester was in design evaluation, it appears to me. So - he was interested in the field at that time. Active. We went wherever we could to get help [laugh] - very little about it - these new machines.

RRM: Did they - did this committee then survey the number of -- About how many different design proposals did come in now with this?

EC: Well, actually only those three.

RRM: Three. Aha. Raytheon, Hughes Tool and the Eckert-Mauchly Company.

EC: Yes, as a matter of fact I believe I'm wrong there, in thinking back, I believe - I might forget - but I believe it was only two. Eckert-Mauchly and Raytheon.

RRM: And Raytheon.

EC: Hughes Tool Company had expressed some interest. I remember .. a visit there at that time, two young men. One who had recently come from General Electric Company, Sy Raymour and - and, and, Wooldridge were the two I talked to. They've - they have gone on [laugh] very far in this.

RRM: They were at that time with Hughes?

EC: They were at that time with Hughes. I forget why, why Hughes Computers did not - well, enter the competition. But the .. I'm not sure I'm correct. I believe they did not. Anyhow, I - I'm sure that the result was that two - two designs were built - were - were, were being built.

RRM: Mhm. Now, was there any --

EC: Eckert-Mauchly and Raytheon.

RRM: I was just wondering in this connection if there was any possible conflict of interest between Von Neumann's

position on this committee and the fact that Von Neumann and - and Princeton were also, I believe - eventually started to --

EC: At the time I think he was in --

RRM: Interested?

EC: Interested.

RRM: Had he already received his - his contract to go ahead?

EC: I don't know. I - I think so.

RRM: Doing his design?

EC: Well, I think there is no -- These, these were men who certainly consciously would not give into any - any, any bias - would not be biased; but it is interesting, on looking back, this is one of the difficulties we had. It was a new field, .. and, and one didn't know just what direction to go. One - we were all concerned about the high reliability though in the computer. Part of the circuit, of course, naturally - and realize it was very similar to radar circuitry, but radar - radar you can read a failure - intermittent failure - because persistent vision in that scope takes care of it for you, but not in this. So we were concerned about the engineering requirements, that controlled the logical design.

The - the, the committee, although they were very cooperative and helped us in our program, they recommended that we not go ahead at that time becau- with the computer - because they felt it was premature. We, the Bureau, should concern itself entirely with the development of components. And I remember our - a view, because we felt they were recommending that, but at least two of them, as I recollect, this was two of them, Aiken and Von Neumann, were themselves [laugh] undertaking to build a system. And --

RRM: And George Stibitz also, who wrote that.

EC: And Stibitz wrote that. Of course, but Stibitz was not a member of that committee.

RRM: Mhm.

EC: So we decided that - and of course we main-- we maintained the friendship with the committee, and they understood our position fully and helped us after we made this decision. We just decided to go counter to their recommendation and proceed to [undertake the] machine. But we had difficulty. Various .. we, we - well, there were all sorts of difficulties. You've heard about the difficulties. You have engineers and mathematicians working together to build a computer, as they had in Raytheon,

and eventually some kind of difficulty would develop between the two groups and that would slow down things. That happened at Raytheon, as I recall.

Eckert and Mauchly did not have that difficulty, but they wanted to go into business for themselves and they had one difficulty after another. Their sponsor, I've forgotten his name, he was President of American Totalization - Totalization Corporation, who was underwriting them for awhile in the beginning, and who - who was living over his younger inventive - inventor days, where the man told Alexander and the Panel, "I will underwrite them to the limit of the resources of my company." He and his, his, I guess, well - the next man in the company, [I forgot] the title, were killed in an airplane accident and no one else in the company was interested. So Eckert and Mauchly went from .. the position of not worrying about a support, to having to scrounge around for money. Now that slowed down their operation. Then there were .. that was about the time when there was great sensitivity with respect to security, and I forget this - the details, but this - they were slowed down in that respect, and eventually [got] full clearance. But some of the,

in the meanwhile .. .. there had been interest in the Government, I believe, to transfer the monies to the Bureau for procurements by - by other government agencies.

RRM: Yeah, I know there --

EC: The Air Comptroller and the Army Map Service.

RRM: Right. Wasn't there also about this time some interest expressed by .. the .. either the - the Armed Forces security agency?

EC: Yeah, they - they were --

RRM: ... Naval security group or the Army .. security agency; I'm not sure .. of the dates, of when the Armed Forces Security Agency was created, but it's about that time.

EC: Yes, they were interested and they - they were working with us. They, they finally decided -- I, I think they, they contracted themselves for a design of a machine to cover their needs. I don't believe they had the Bureau do the design.

RRM: .. Were they --

EC: I may be wrong.

RRM: But I believe they were also - weren't they - quite interested in following - having the Bureau of Standards .. .. do work for them in the way of machine design? At least in --

EC: That - that is right. I think, I think that they were closer with the electronics group than the mathematicians. But that is true. .. Dr. Condon would tell you.

Well, I remember for at least two years the - how we felt when the appropriations hearings were held, and knew that Dr. Condon would have to explain the - where we stood on obtaining these new computers, because it had been well popularized; [laugh] and - and .. being fair to the Congressmen who needed the information, and fair to the companies with which we were dealing, the technical contract and engineering difficulty which we knew [they were having] at the time; and .. and, and, and .. well not being over-optimistic because we still could not be sure that - that - that we would be successful.

RRM: And actually build a machine.

EC: Yes. And that, you know, would be - would be .. useful. .. So I remember the first time I was preparing - I had to prepare - what Dr. Condon asked for, and I -- Curtiss and I -- were brought in for the background documentation and information, sort of a chronology of what happened, and which he could -- you know.

RRM: This was in - about '47?

EC: That was '47 and '48.

RRM: Mhm. That would be very useful references for students, if it was kind of historic resume.

EC: I might be able to put my hands on it.

RRM: Mhm. That must be a very helpful thing.

EC: Yes.

This kind of difficulty then made us think about trying to build a computer ourselves to find out if we were right or wrong and one - and, and - and to aim modestly, recalling [?] many machines again, to - to test a system, but a very modest, a small system, including the kind of components that would be in the larger computers, but .. being free of the kind of difficulties, some of them at least, which had - which had beset our contractors,

RRM: Mhm.

EC: and, and finding out within a reasonably short time whether, whether we - we were unrealistic in encouraging the building of such machines.

RRM: Wha--

EC: And that's how I believe Curtiss was the prime mover in the early beginning .. and, and persuaded the Bureau .. to, to enter into this venture.

RRM: .. Do you recall, in retrospect, what some of the, the major .. pressures were, if any, on the outside to have the Bureau of Standards go ahead and, and more or less try its own luck at - in, in building an interim machine?

EC: I don't think there were pressures from the outside. I think it was all - an internal desire --

RRM: Generated right here?

EC: Generated inside. I think we were certainly encouraged by - for example, the Air Comptroller's office supported us.

RRM: Is this George Dantzig's office?

EC: That was George Dantzig's group, yes. .. I think they were the major support in this. We were encouraged, but I don't believe there was any specific pressure. I think we had the general pressure, you know, let's get going [laugh] ... [without] these delays.

RRM: I have the impression - correct me if this is erroneous - I have the impression from having talked to a number of .. different individuals who were active at this time that .. that there was a general mounting of pressure in terms of .. problems that required extensive calculations and for which hand

machines were just totally inadequate; but yet these calculations needed to be done. For example, the Atomic Energy Commission.

EC: Yea.

RRM: There was a mounting degree of genuine need that they felt they simply had to have some - some breakthrough.

EC: I think that was true. However, this was not universally felt. Let me tell you an interesting little story. I remember when Sam Alexander and I were in Dr. - in Howard Aiken's office, and Howard really scolded us because we were misleading the public, you know. There were - these big machines would never be needed. He said, "Why don't you turn to making something that could be put on a desk that the average Joe can use, because there won't be enough problems in the whole country to keep more than - to keep one of these machines busy." Now that was - now I think he was exaggerating, he might [laugh] agree. I was fond of Howard. I think he probably was - perhaps he was - he was just baiting us. But - but I think there was some - I believe that represented to a certain degree his opinion. So, .. and there, there were people, there was a body of opinions,

which .. of, of that kind - which felt that these - that these machines were - were really not needed.

There was, on the contrary, a - a, a large .. body of opinion by people who really had the problem, who, as you say, in reactor design, and those where the computational problems were just beyond the, the, the capability of the existing machine-human being combination; the desk calculator. And - of course, even the ENIAC had .. had been demonstrated as far as certain things like firing tables to be accurate.

RRM: Right. Now, as you say, that is another - another view -- another [viewpoint].

EC: But we had made a survey in the - in the beginning. This is one thing Curtiss had - had me do. Well, I participated in it. I shouldn't say that I did it, the Division did it. With the help of - of the Office of Naval Research, and the Air Comptroller and the Census and .. we had surveyed the needs for the computer and convinced ourself that there were these problems that really could not be handled.

RRM: I was going to ask you about - in this regard with .. in connection with the Census Bureau, my impression is that there was a rather close relationship between the Bureau of Standards and the Census Bureau in - in this era, of the [advent]

EC: Oh, yea. Oh, yes.

RRM: of the computer as a potential user with, with demonstrated needs for such a machine. .. Well, I don't know whether the word "pressure" is appropriate, but .. at least making the - their needs felt in the Bureau of Standards as, in addition to --

EC: Well, I think you're right in using pressure, if we define - if we make sure we understand where it came from. It was not an administrative pressure coming down from the higher levels of administration, it was pressure coming from all of us who - who were close to the problems at the operating level who knew that we had to have such equipment or eventually our jobs could not be done.

RRM: Mhm.

EC: And - and - and, and - and, and these were, of course, quite senior people. But it was not - it was - it was large-scale sort of pressure coming from the organized user, professional peoples, rather than coming from the administration,

RRM: From the hierarchy --

EC: hierarchy, as from policy or anything like that. You're right. In that sense there was this pressure which was building because we - we were intrigued

by the potential of this machine and we didn't know what we would do without it. We, meaning the - more the users, like George Dantzig, like Morris Hansen, who was the person at the Bureau of Census, and - and the Army Map Service was interested too, and - and, and they had begun to bid [inaudible] computer [inaudible].

RRM: Do you recall any of the individuals who were in this period interested and active in the Army Map Service?

EC: .. [Pause] John O'Keefe is the - there was another one who - whom I - whom I, I can picture and whose name I've forgotten. But John O'Keefe was there then as a mathematician. [Cass? Background voice]

RRM: And he was?

EC: And he is now with NASA.

RRM: Yes, I see. [Pause] And in the Navy group .. .. ?

EC: In the Navy group at the time was Mina Rou- Mina Rees --

RRM: Oh yes.

EC: and - and Charlie Smith was there then.

RRM: C.V.L. Smith?

EC: C.V.L. Smith. And Gene Smith. Those were the - the - yes. Charlie and Gene were in what was the Computer Branch, or Section, I forget which; and Mina headed the --

RRM: Mathematics?

EC: mathematics activity [inaudible].

RRM: Right. .. One thing that I'm always interested in is learning new names of people who were interested

EC: Yes.

RRM: and active in that time. .. I - In George Dantzig's group, do you - do you recall any others who were also of --

EC: Well, of course, Marshall Wood,

RRM: Yes.

EC: and his assistant. And Geoge Dantzig. And then .. I think Walter Jacobs was there at that time. Maybe not that time, maybe a little later [inaudible] I can remember. Now we ca- do you want me to go ahead?

RRM: Please do, please do.

EC: .. We knew of the .. EDVAC program, at the University of Pennsylvania, and we knew of two people who had been at the University, I believe both of them had worked on UNIVAC .. at that time, with UNIVAC development and both of them had been interested in the development of computers at that time, and both had [experience]. Harry Huskey, who had also been in Europe for a little while. And - .. I cannot - I can't remember his name, it may come back to me in

a moment. .. And we arranged to have them come to the Bureau to help us build the design of SEAC.

RRM: There was Samuel Lubkin.

EC: Samuel Lubkin is the name, I just remember that.

RRM: Aha.

EC: And --

RRM: He was with Reeves at one point. I don't know where he was - where he came from.

EC: I don't know where he came to us from.

RRM: Yea.

EC: It's true. But - and Harry. Harry - I believe Harry came first. He worked with Alexander and his people. And at that time we - we, the section that we had - we formed the section at the UCLA campus, and that was flourishing. And .. Harry went out to join that section and Lubkin came up - I think it was about that time.

RRM: Excuse me. Could we go back just a little bit to this formation of the UCLA section which I gather was to become the Institute for Numerical Analysis.

EC: That's right.

RRM: How did that come into being? How did it fit in with - with the Bureau's activity?

EC: It came to be because we - we .. realized the - the,

the extensive, because of the aircraft, the extensive computational activity needed at the West Coast. And .. and we felt that it would be highly advantageous -- we were still thinking of a National Applied Mathematics Laboratories -- advantageous to have an annex at the West Coast. And - and we - we - I remember, Curtiss went to the West Coast, and I, .. the director sent me to the West Coast, talked to people at Berkeley, at Stanford, sounding out their interest in having a section. And at the time Curtiss, who's a very fascinating person, felt that what should be done was research in what he called numerical analysis. In the beginning this was called an Institute for Numerical Analysis, to develop the kinds of techniques that would be suitable for application on these computers, which would not necessarily be recognizable to the old-fashioned computing expert.

RRM: Mhm.

EC: And that was the reason. And it was felt that there would be advantages in having this located in a university because it would attract university people, those on leaves of absence, on sabbaticals, and there'd be some advantages in the contract operations within the university. And that was the reason this was established.

RRM: And UCLA seemed to be the most promising of the places visited for that -- a - at the time?

EC: Yes, because of interest. I - I forget just what factors we ... considered but, of course, the main thing, one of the main factors was the interest of the faculty and the university administration, the kind of arrange- contractual arrangement that could be made, and - and the, the .. concentration of computing activity ... there.

RRM: I was going to make - correct me if I'm mistaken in this impression, but, at this point, among mathematics faculties there wasn't all that much interest in numerical analysis, at least in many departments.

EC: That is right. Yes, very few.

RRM: So, conceivably, his survey might have unearthed the - the fact that perhaps at UCLA there was -- there was one or two men on the faculty who were a bit unusual in that they were interested. Do you happen to recall any of the individuals at UCLA who were on the faculty at the time? Who were --? Was C.B. Tompkins?

EC: No, he was with Engineering Research Associates.

RRM: Yea, I see.

EC: .. Well, I remember talking to Sokolnikoff,

RRM: Oh, yes? Aha.

EC: he was interested,

RRM: Sure.

EC: had great stature in mathematics. .. I think I talked to Taylor, and Curtiss also went there in person and then Taylor and Sokolnikoff. .. I'm not sure Hestenes was there at that time. He certainly was extremely interested.

RRM: I'm sorry, I didn't get his --

EC: Magnus Hestenes. I don't know if he was there at that time or not. I suspect he was.

RRM: Aha. Aha. Let's see - is that H-e-

EC: H-e-s-t-e-n-e-s. No guarantee this is correct.

RRM: Right. .. I see. And, .. but they were - they were regular mathematics faculty,

EC: Yes.

RRM: and .. certainly gave some support to Dr. Taylor. Conceivably there'd be a very direct interest in this, more so than you might find in a traditional mathematics department.

EC: And we scoured the countryside nearby to find some kind of building which could be moved. And I think we had a surplus government building from the, I believe, it was a hospital somewhere and had it

moved there, .. and placed on the campus. ...

RRM: And .. what was, in Curtiss's view this: an adjunct of the Washington effort or was it going to be a fairly autonomous kind of thing?

EC: Well, it wasn't - it was fairly autonomous, in this sense, that - that it was largely a contract operation with the university, but not entirely, because some of our staff were detailed out there; our Washington-based staff who eventually became Los Angeles based, but they were still members of the division. But .. it was really considered a section in the beginning and operated as - as - as a section of the Mathematics Division.

RRM: And do you - who was the first person in charge of the group - the group out there?

EC: .. Curtiss went out for a few months to - to head the new section. And I believe the, the - the next person was Barkley Rosser. I'm not sure, but I believe he was the next person. Next was Derrick Lehmer; he was a number theorist interested in computing.

Now about this time we had .. commitments to build .. fairly firm commitments with government agencies to build more computers than we could - we

could procure it seemed, under the money that'd become available. No one - it's sort of amusing to look back - no one knew, including - including the - the combatants -- the people who'd become manufacturers, for example, including Eckert and Mauchly, no one knew exactly what this would cost. So, we - we had money for computers and - which was sort of outside the framework of our arrangements with Eckert and Mauchly. We found that they had won this competition we were talking about, the contract ... [for] computers, ... the UNIVAC, .. but outside that we had money for two computers, and, and we talked to people including Engineering Research Associates. That's where I met C.B. Tompkins, who later became a very close friend of mine, and who - who went up to the University of California and [inaudible] numerical analysis research out - through there.

RRM: Excuse me, I'd like to reverse the tape.

END OF SIDE I

SIDE II

RRM: We were just discussing the founding of the INA - Institute of Numerical Analysis.

EC: Yes. And I was mentioning that we were in the predicament of needing, of being commissioned [laugh] to take other computers and not having enough money. We were expected to purchase two and as it turned out we consulted with Engineering Research Associates and Raytheon and General Electric Company and found out there was not enough money for a single component. And then - they, with the consent and [laugh] advice during this operation of our sponsoring agencies .. we decided to try and build another computer.

We had Harry Huskey, a talent who was interested in the design of a computer, who - who was a mathematician interested in the computer, who had been active in, an active participant in the development - well, for awhile in the development of the ENIAC ... and the EDVAC, and then the Teddington computer. So the mathematics division decided to build a computer which [laugh] was one of the biggest mistakes, well - in this sense, that we were still in the era where you find a group of mathematicians heading computer development and you find engineers. This pulse

circuitry was wonderful from a mathematician's standpoint, the mathematics theory was rather nice and simple, and you - but the mathematicians - I think we - we - we - we - among mathematicians might well think that the design - the engineering design was a lot simpler than it was. I mean [laugh] you would not weigh for instance when you think of a Taylor series expansion, you need the higher frequency response.

So our main difficulty was in - in, in achieving engineering reliability, circuitry reliability. .. Harry Huskey almost single-handedly, I guess entirely single-handedly, got together the group and - who, who designed and built the empirical system design - he, I am sure was completely responsible for the logical design of what was called SWAC, the Standard Western Automatic Computer, in contrast with SEAC, the Standard Eastern Automatic Computer.

RRM: Wasn't their logical design quite different?

EC: The logical design was quite different. SEAC was serial, began with an acoustic memory. .. SWAC was parallel, began with a Williams type electrostatic memory. And --

RRM: Didn't this in a way represent sort of reflections

of perhaps two of the main streams in computer development at that time? What could be called the UNIVAC-ENIAC-EDVAC line and the Princeton serial line?

EC: That is right. Yes. That is right. I think Huskey might have been influenced, rather than by the Princeton program, the work in England, particularly Williams in Manchester. He thought of him as the inventor of the electrostatic memory - it's called the Williams memory.

RRM: Did the decision to go ahead and make and build the SWAC, did that follow sometime after SEAC, after the decision was made to build the interim computer?

EC: Yes. I forget the exact timing. It was a little bit after SEAC, but not - not too far after. And they - they came into operation about the same time, SWAC about a year later. However, .. I must be completely honest and say that when they, they came into operation, that is you had a certain amount of -- SWAC was not - not the computer SEAC was. [Laugh] SEAC was operable from the beginning. From the very beginning they could use it for output. SWAC was very temperamental from the very beginning, and actually it was a good thing for me perhaps, because

I, because of my engineering background, I was sent out to see what I could do to help tidy up the circuitry and get some productivity out of SWAC. At that time we were lucky if we got a half hour or one hour of computing a week.

And I went out for a couple of weeks to see what could be done and eventually stayed out for over a year. It was an interesting part of my career for me because I went back to my engineering days and a combination of mathematics and engineering and I arranged to work during the night sessions with one or two engineers and we would go around looking for difficulties and, and tidying up the circuitry.

RRM: What year was this, approximately?

EC: This was in about 19- hm. One of the highpoints in my career [laugh] - this was around 1950, I believe.

RRM: Was it after .. SEAC had been officially commissioned and was running? Which was about April of 1950.

EC: Yes, and it really was after SWAC had been mounted.

RRM: Then it might have been '51.

EC: I think it probably was. '51, '52. Yes.

RRM: I meant to ask you in that regard, .. were they having any problems with the Williams tubes at this time?

EC: That was one of our major difficulties, but not the only one. I mean, that was, that was, that was the visible one.

RRM: Yes.

EC: We ran test after test, as I remember we had what was called a leapfrog test, test for spillover from one memory slot to another, and we had an advisory committee from the mathematicial division come out, while I was there. I said, "Great," I said, "we can pass the leapfrog test." And Curtiss was very despondent, thinking we should drop the SWAC. And we resisted that because you see -- But that was the visible, but not the only one. Really, ... it's hard to describe what .. was going on except that, as I've said before, a very good mathematician-engineer could .. do a wonderful job in designing a machine and, and, and building it, except that he would not know enough really about the engineering need to - to -- He would not be conservative enough in the engineering. That was Huskey's difficulty, our difficulty. And I had to really - as a matter of fact I would do this kind of thing. We - we had been operating, and their engineers were leaving because they felt, they didn't see any chance for

improvement, and that was the reason I went out. Now, from the rearline [?], I thought they were right.

We were operating from the beginning of the announcement of the SWAC as though this machine is fundamentally soundly designed, just some minor adjustments, so we'll .. you know, as you do when you - when you know you have a sound machine and there is some temporary difficulty so you try to push a problem through. And then later you will take up the engineering. That was our normal mode of operation and that was not satisfactory.

So I would .. look at the log. When I - when I, when I realized what was going on. We were, for example, interchanging chassis to remove instability to try to push a problem through. Well, that's all right if you need to finish that problem, but then that is not a way of remedying the [laugh] difficulty with the circuits. So I decided and, and followed this procedure, to look at the log, and I decided I should work at night to increase their span of effort .. on a temporary[?] basis. So I put myself on night shift and then we - I remember Benny Ambrosio and I worked together as a team, just the two of us.

RRM: Ambro --?

EC: Ambrosio; he was one of the engineers.

RRM: Mhm. Is he still out there?

EC: He is there. I think he works with the Magnavox Company.

RRM: Aha.

EC: And I would look at the log and find out what changes of circuitry .. changes of chassis had been made and put them back where the fault occurred. And then we would proceed to try to find out what caused the fault. And you - you'd find out that things like this had occurred; that, that you had a differentiating circuit, electronic circuit which had in effect differentiated. .. And it would be a little shakey and it would, would - the result would not be as smooth as you'd expect it to be, it would peak. But that would not - that would go through the next chassis because the next chassis was wonderful. You know, very steady. [Laugh] Then two or three up the line this would be magnified, however. So it would trip a chassis erroneously. So that, these were in a counter, for example. The counter where the machine went to look for the next instructions, you know.

RRM: It had a sort of cascading effect?

EC: Yes. So - it would be amplified so that the one word - the one that would finally fail .. really could be perfectly serial. But then those would be interchanged so you had no chance to find where the basic trouble was. And that was the sort of thing we did. And, but really the, the central difficulty was with the memory. And we had, I think by the time I left, I was there approximately a year, well, we had come up to [pause] what was not really a satisfying, but I guess an acceptable level of operation. It seemed to me that we would have, perhaps, a third of the time productive computing, a third to a half. ... not really, not satisfying, but --

RRM: At least enough that significant ...

EC: Yea, and really productive and tremendously significent results were being obtained. But then before I left .. .. Huskey hired another engineer from General Electric Company, Thorenson, who came out, and --

RRM: T-h-o-r-s-?

EC: T-h-o-r-e-n-s-o-n. I'll give you that name. And he was very good. He gave him the assignment of

actually evaulating the electronic circuitry associated with the Williams tube memory. And he redesigned that and his work resulted in a marked increase in the life of that circuitry. After incorporating his new design .. into the machine, the machine worked really quite satisfactorily, compared to SEAC or - or any other machine. In the meanwhile we had picked up - and these had all been done with a combination of Huskey and his group, and Ambrosio and Canning and I - we had picked up things like cold solder joints and the basic circ-- Who would think of looking into these racks of 60-cycle power circuits for difficulties. We would sit by the hour and look .. and look - look for a transient and try to find out what caused it.

RRM: Mechanical failures?

EC: Yes. And - and, and tried ... I believe that we really assembled that 60-second power circuit [inaudible] I wonder - there might be some difficulty - [inaudible] find cold solder joints. This was, of course, stiff bus bar type heavy conductor and you could not solder it in place, so the panels were moved out a few inches from the final position for soldering and then pushed into place. [Laugh] In the meanwhile

we were worried about stability of the power and we had gone - I had gone to Minneapolis to look at a motor generator set and, we brought that back, and had done a lot of things, but the real significant crucial improvment was how to improve the main memory and then SWAC was completely satisfactory and operated alm- well, until just a few years ago at the University and ran at top paces.

RRM: I meant to ask you, well, a, a couple of other -- Who were some of the others who were engineers working with .. Harry Huskey?

EC: .. [Pause] Ben Lacey.

RRM: L-a-c-e--?

EC: L-a-c-e-y. He had designed the [pause] voltage regulator.

RRM: Mhm.

EC: Went into business for himself to - formed a little company to design voltage regulators, and I understand did quite well.

RRM: Is he still in the Los Angeles area?

EC: I swear I don't know. I haven't seen him for years.

RRM: Mhm.

EC: Then --

RRM: The circuitry itself was primarily Huskey's work?

EC: The logic circuitry.

RRM: The logic circuitry. Aha. Well, wh- what about the --

EC: A - and some of the - I - I believe that Huskey did the memory - I'm certainly not sure of that; but Lacey designed the voltage regulators.

RRM: Mhm.

EC: .. .. Then, Ben Larson, .. I don't - he had the West Coast - now I don't know where he is. .. And those are the only names I can recall now.

RRM: And he was part of the engineers that worked with Huskey.

EC: Yes. They were a good group. That was the miracle[?] with them. They - they knew nothing about digital computers before they joined us.

RRM: I gather there was very little you could say could be transferred in terms of the - the knowledge from SEAC itself, because it was a rather different logical machine.

EC: It was. Then .. .. we - we had, Ralph Slutz had joined the Bureau and really he played, I would say, a role - well, he played a primary role in the completion of the SEAC. He was the ... practical engineer, and he was under Alexander's general

administrative supervision. He was the technical man on the job.

Ralph came out once or twice and - and gave general advice. .. It seems to me someone else came out. Alexander was completely preoccupied with SEAC and he - he - I couldn't get him interested in coming out. His people - but his boss then, I, I'd forgotten: Gill Reed was Alexander's boss and - and Gill Reed - is not alive now, of course; but he was such a wonderful fellow. I had not known him before. And he - he was a source of great comfort to me. He said,"Anything at all we can do, you know, on the home ground, the Bureau, Washington Bureau, in our division, let me know. But as you said, there was not very much in parallel computers going."

Now we did have a lot of advice on the memory circuitry. .. As a matter of fact, Curtiss, who was good at this sort of thing, arranged to have some Britishers who came, I remember Sam Williams was one of them, [inaudible] Bowen was one. I forget who the third one was, there were three of them. They came out to evaluate the technical design and I remember this was after I'd been out there a year

or something. It seemed to be a little [inaudible]. We were a little beyond that one-half [inaudible].

Well, in the beginning there just had been so many sources of difficulty that you couldn't sort them out. And, as I say, the basic power circuitry was unstable, and - and - and - and, and, and the tolerances were too low. For example, this business I mentioned about the - the counter circuitry, ... all the changing of the value of the capacity, pulling out something and putting in a, you know, resistor capacity.

RRM: The changes were a bit more conservative.

EC: More conservative, yeah. But when you were looking at the machine and all these possible sources it was awfully difficult. No kidding. Sometimes what looked like a memory's difficulty, sometimes [inaudible].

RRM: I meant to ask you about component reliability. If I'm not mistaken the Bureau of Standards did undertake some studies in some areas, I know in ... diodes, for example, to do long-term life stability tests and reliability tests, I think largely stimulated by the need for very reliable components.

EC: I think those studies were underway at the time.

RRM: Was there any - do you happen to recall whether ..

the choice of particular vacuum tubes, and the like had some unfortunate effect in terms of the over-all reliability of SWAC and they changed or modified the tubes that were being used?

EC: Well, I - I - there was - we felt possibly we had not chosen the best cathode ray tubes.

RRM: Yeah.

EC: But - and I forget which, we experimented with different ones, but this again was the second order of magnitude compared to the basic circuitry.

Meanwhile, to come back to this British group, we had very sound recommendations from, from - ... I can't remember his name. Williams, ... and he ...

RRM: It isn't Gill, ... Stanley Gill?

EC: Bowen .. said we'd never be able to make that memory work, we should use the British type. What he was concerned with, I think dot-dash - you see, dash to represent say a one, and the dot zero. We should use the circle dot. We'd never make it work. Well, we knew that Eckert-Mauchly had been experimenting with the circle dot of dot business. This is not very interesting in the context of this interview, except this is the sort of thing that happens to

you when you're involved with contacts. We were told that our technique, that this design technique, was - was wrong. But - and we experimented with it briefly, with no improvement. And then, I think it was about that time, but maybe even before, but I believe it was about this time that Thorenson began working on the basic circuit and certainly the machine was quite [inaudible] quite proud [of the new] machine, and the fact that we had built [it] out there.

Our main difficulty was that it took us longer, because we - we put the machine on the air, the logical design was wonderful, the machine would count [laugh] which is something, but would not do enough to - for effective application to the important problems. So then we had to tidy up the circuitry. But I'll tell you that was a wonderful .. I - I guess I shouldn't complain because that was the reason for my getting close to electrical engineering, electronics, for a while and I enjoyed it very much and stayed out there for a year and then came back.

RRM: If we could backtrack for just a little bit. In this period when the decision had been made to create

the Institute for Numerical Analysis, to do some .. .. fundamental mathematical analysis relating to, hopefully improved utilization of these machines, .. what in retrospect would you, do you feel were some of the more significant contributions made by various people who were associated in those years with the Institute for Numerical Analysis, which later did in fact contribute to - to applications of the machine?

EC: Well, I respond to this with some reluctance because I'm sure I will do a lot of people injustice by forgetting. I can remember a few. For example,

RRM: Well, whom do you recall? Let's stick to --

EC: for example, I - George Forsythe, was very competent in the field. .. .. He began, I think, his career in numerical analysis at the INA and he - and he was quite productive at the time in the development of techniques for matrix, linear systems and matrices, various eigenvalues, of course. Magnus Hestenes at the University, whom I mentioned before. Stiefel from Switzerland came out and was with us for awhile there and worked with Hestenes. Together, they - they - they made various advances in the development techniques for --

RRM: Mhm.

EC: matrices and solutions of systems of equations.

RRM: Those are essentially eigenvalue problems.

EC: Yea. There is a name associated with the technique that they - they - that they developed. The technique, I believe, is associated really with Rutishauser, who was over in Switzerland and an associate of Stiefel's. And I forget whether now it's called the accelerated gradient or the linear gradient technique, which again is something I advised in, but anyhow this was a major development.

RRM: That emerged from INA.

EC: Yea.

RRM: Yes.

EC: I would like to come back to the Bureau before I forget it; you probably may have heard this; you may not. Really, there were tremendously significant applications of the SEAC, some of which we had a lot to do with mathematicians; some of which we had very little to do with except in the computer field. For example, the AEC preempted the SEAC, you may have heard this. [Laugh] And we couldn't get very close to it for a while.

But the most significant application was the

development of the linear programming technique by using the SEAC to obtain problems and to test things as we went along. George Dantzig, if he had was - he had the problem, he was developing the technique, he was - he had - he was persistent, he - he had [inaudible] he got one thousand and forced the result really was [inaudible]. Of course someone else would have developed it eventually, but he contributed to the program. And, we had - the SEAC was used a great portion of the time by Dantzig and he made people available to work with him. Translating, for example. ...

RRM: I meant to ask you about Dantzig's group. Who else worked with Dantzig on linear problems?

EC: Oh, another name I'd forgotten before and the name I associate closely with George Dantzig, and just the two of them it seemed to me were - were - were the main strength of this activity, were Dantzig himself and Alex Orden.

RRM: Alex Arden?

EC: Orden. O-r-d-e-n.

RRM: O-r-d-e-n?

EC: O-r-d-e- ... And then - now in the meanwhile Franz Alt had come to join the division. He'd come from

the Naval - excuse me, from Aberdeen Proving Grounds. .. He certainly was interested in this development - he, he, however, well - well, he headed our computation laboratory. The Bureau had been faced into the operation - I, I mean the SEAC by the Applied Mathematics Division and we operated it. And we had the accepted Computation Laboratory and we operated the SEAC, and then in turn its successors like the 7 - IBM 704, and the 7094 until the organization, well, of the Bureau's present Center for Computer Science and Technology at this time properly, I believe, although some of my people don't agree with me. The Computer Laboratory or the Computation Laboratory, was moved to that .. organization. [inaudible] So we - we were operating the computer and at the time that they moved to Office of Air Comptroller and, of course, we recommended it, Air Comptroller.

One of the things I remember with great satisfaction is what I believe, again I - I think I'd like to believe this, I've not attempted to verify this - is perhaps one of the first significant business applications of the computer under a kind of embryonic rudimentary remote control. That was

our presenting a solution to the Armed Services Procurement Agency, I think it was called, for determination of the optimal contracts for procurement by the government of clothing, essentially clothing. This operation was in New York City and we - we felt, when those people came to talk to us ... I was a George Washington University on loan to the University for about two years to head the Logistics Research project. But I knew of the Bureau's strength in linear programming at George Washington University.

These - the representatives of this Armed Services Procurement Agency, I think it's called, were charged by law to make the contract at lowest cost to the government, which is somewhat difficult because you almost always would find no one manufacturer or no two manufacturers together who could supply what you needed. So you had to run around. I think they called it cut and try. Garment - manufacturers, and, and, and the manufacturers had different cost scales, so much per garment if within this range; more so much. A little less, and so on. Each - they would have different requirements for, say what the government should furnish them in their

warehouses, you'd have to pay the cost of shipment from warehouse to - to [laugh] send back to manufacturing points. So you had to put all these costs together. This was being done by hand desk calculators and one had the feeling that the contracts were reasonably good, but there was always the possibility that the manufacturer would - would - would demonstrate by an example that you had not let the contract at lowest cost. You couldn't be sure.

So this seemed like a natural for linear programming. So the representatives of the Armed Services Procurement Agency were finally brought to the National Bureau of Standards where at the time Allen Faulkner, who is now with General Electric Company, .. was our expert on linear programming. And - and the problem was programmed, .. and solved, and the program developed and the data and - and solutions were supplied on a regular routine basis to the Armed Services Procurement Agency until they were able to make use of - I believe, obtained their own computer. The data was telephoned down to the Bureau. This was the remote control aspect [laugh] and the results telephoned back. Also tape shipped back. But that was, I believe, one of the earliest business applications.

RRM: Do you recall about when this was?

EC: .. I'd have to get that date for you. But that was fairly early, say '52, '53, or '54.

RRM: Mhm. You mentioned that you had been more or less placed on loan to GW computer facility. That was an early - a, a - a fairly early computer installation.

EC: That's right. To come back, I don't know if we mentioned in the interview, but we were talking .. before the interview about my friend Tommy Tompkins. And you just missed an interview with him because of his incumbent back. Tommy .. had .. well, been the prime mover in the establishment of what was called the Logistics Research Project at George Washington University .. .. under the support and sponsorship of the Office of Naval Research. And .. C.B. Tompkins, Tommy if you were a friend, was the first, what we'd call the principal investigator of the project. Now, he did a remarkable job in organizing this .. and, and - I, I wish - I'm sorry you could not interview him because I don't - I don't have the information I wish I had now. I know that a computer called the Logistics Computer especially tailored to logistics-type applications in the Department of Defense .. was designed, and I'm sure

Tompkins and some people, that I don't know their names - of, at the Logistics Research Project played a very primary role in the design - prominent role in the design. But it was, it - it was procured from Engineering Research Associates. And it was installed .. well, I was -- I followed Tommy - after he had been there a few years, and then, I believe he went back to the University of California at Los Angeles, and I followed him. And .. I, there was - you understand, I did not stay permanently ... and it was an interesting experience for me. But then we placed the Logistics Computer into operation. I think it was just about the time I left. ... It was used for a period of time in the Logistics Research Project which was headed after I left by Bill Marlow. And, if you - and, and Bill Marlow may - may know the - the early history of the Logistics Research Computer. It had quite an interesting .. design, I've forgotten what - what uniquely characterized it.

RRM: Mhm. But they did .. run linear programming - problems?

EC: Yes, I believe so.

RRM: Mhm.

EC: And it was installed in the - in the building and operated by the, what we called "The Project."

RRM: Yes, I believe it was in the building right next door to the library b uilding in the university. It's now turned over to the university.

EC: It was on 22nd and G, I think.

RRM: It's now called Steward Hall or something. But it was at that time closed, I think; it was a classified project. It wasn't open to the University as such.

EC: Well - yes, in a sense. It wasn't that the work was classified, military classification. It was more true that the University was not interested in the computer. This was still fairly early, 1952, I think.

I remember that - I have forgotten his name. Well, the Dean of engineering was interested in - in - in - in training at the University. The mathematics department was not as interested. I remember meetings we held, meetings which I attended, where - where the engineering department, the mathematics department, and - and a few of us talked over what could be done. We were interested in helping out because I was afraid we'd [inaudible] we knew the importance for that time - well, from the beginning we felt we knew

the importance of this development, we thought our judgment -- But I remember this, the discussions finally fell down or were stopped temporarily by this kind of judgment.

This was my main interest, you know, applied mathematics. We were great potential of this as a tool to do mathematics, mathematics we could not do before. But the mathematics department was where the foot dragging was, and the judgment made was, "Well, you - unless you can guarantee a minimum attendance to us, of course, then we can't guarantee to give the courses." We said, "When you're beginning a new curriculum like that, you - you have to - you have to have this .. this guarantee for a few years and then you can drop back, but otherwise there is no - in the beginning you can't expect all these courses to be, you know, to be --"

RRM: Instantly popular.

EC: That's right, to be popular, and so forth. You see, much earlier in their discussion, at that time there were very few schools where there were courses in what we now call modern computing and - and numerical analysis and where you could obtain degrees in, in computing, and I think one of the first was Harvard

and they - under the guidance of Howard Aiken.

RRM: And I don't think that was properly speaking in the mathematics department at Harvard.

EC: That's right. That's right. It was a computer laboratory course. I think Howard Aiken was a mathematician but it was done - this was the second - I think you are right - not in mathematics, I believe in the School of Engineering.

RRM: Which to this day one finds that some of the curricula relate in a university directly to computing one finds in courses in the engineering school.

EC: That's true. There you are.

RRM: If we could go back now to - to some continuity in terms of your career. .. You then - you had gone - essentially .. shortly after the SEAC was functioning you went up to the West Coast .. to work .. .. on the SWAC machine, where you did so for a little over a year, returned back to - to Washington.

EC: Then I came back and very shortly went to George Washington University for two years.

RRM: I see. You were there for --

EC: For about two years.

RRM: Two years. I see.

EC: And then came back to the Bureau [pause] about the

time Curtiss left. I think just after Curtiss, who had gone ... the division. Herbert Stein and myself and, in the meanwhile, or about that time, the administration of the Institute of Numerical Analysis was term- was assumed by the University. And that t- that section of the Bureau, that's a section of the Bureau, Analysis. The main reason was that we all felt that Mr. Charles Wilson, who had been in to see me as Secretary of Defense and had certainly indicated, made a noise and indicated that he doubted the value of research and development. Particularly, he felt that the Department of Defense, we believed, that he felt strongly should not support that kind of activity by other government agencies. We felt that if the Bureau continued this section, we might lose the source of funding for it, so we turned it over to the University.

And I don't know whether that had something to do with Curtiss leaving or not, but anyhow he had left by the time I returned and for awhile, Franz Alt, whom I had mentioned, had come in, was the Acting Chief. As a matter of fact, this had happened before I returned.

RRM: There was at that time a major up - something of

a major upheaval in the Bureau of Standards itself. Hadn't the Director resigned, and there been some --

EC: Yes, there was the Badger accident - the famous Badger accident and he had - and -- Dr. Condon had resigned to go to Corning Glass and Dr. Allyn Astin had replaced him. And this again was a new administration. It could happen under any administration, not just because this happened to be a Republican Administration.

RRM: Yes.

EC: There was a critical evaluation of the National Bureau of Standards in the light of the difficulty we'd had in this Badger attitude and Allyn Astin resigned. And then he was asked to return to his position and a wonderful working relationship developed, which I could see at my level, between Dr. Astin and the Secretary of Commerce. I thought this was a tribute to the character of both of them.

Well, then in - in about a year I was made Chief of the - of the Division of the ...

RRM: I see. Alt had been the Acting Chief.

EC: Alt had been the Acting Chief. And he was, he, he - he continued his position as Deputy Chief. I think his title was Deputy Chief, I'm not sure. For

awhile there had been two. While I was out at California, I think, and aware of people at universities, Curtiss had arranged to have two, I think he called us deputy chiefs. I had been Deputy Chief for Computing throughout and he elevated Franz Alt to the well deserved position of Deputy Chief, I believe, for Research. And he had a different type of computing activity. So then Alt continued in that capacity. And he was a great source of strength for me. As to ... the division before he left he - I forgot just when - .. well, he, he left first to - to - to join another organization in the Bureau, the Office of Standard ... and then from there to the Institute of Physics, the American Institute of Physics. He is a - I think ... talking about.

RRM: He had .. he had been -- how did the work break down in the - under Curtiss between you and Alt's activities?

EC: Up until the time Curtiss had left he had been completely involved in an extensive computer program which was more than enough to keep me busy.

RRM: Mhm.

EC: The development of computers and the application.

One activity we had, we formed a section called the Machine Development Laboratory which I headed until Ida Rhodes came down and I made her Chief of that. The purpose of that was to assess the value of logical designs - proposed logical designs. We'd act as though you had a machine of that kind next door and we'd be preparing problems for it. What kind of difficulties that we'd have. And very many significant recommendations came from that.

RRM: Is this when you returned from GW?

EC: Yes. .. Then .. then we had the Computation Laboratory. And Alt was, was, I think probably mainly concerned with the operation of the Computation Laboratory, which was time consuming and ... provided the kind of ... we could provide. He certainly was - he knew computing and - and programming was - was not difficult for Alt. It was so easy for him. [Laugh] He felt that it was - it was - he would - he would say, "You know, anyone could learn to program in six weeks." Which - well, programming was different in those days. Because the machine had about eight to ten commands and, and quite a difference between programming of those days as far as the machine requirements, you didn't have tremendous

... until later, and the programmer today, I mean, this is a highly special profession and it's quite demanding. But it was not as simple, to most people, including me, as it was to Alt. But he - so he - he was a source of strength in any evaluation of computers and - and the computation and trying to assess the - the - to evaluate proposed computers for, for our installation - like the IBM 704, obviously contrasting with the UNIVAC - I remember his computing - his program. The reason I mention that, I remember we used to hear so much a little bit later about simulators where you could have one machine imitate another machine. Alt did that in two days. We'd simulate the 704 on the - on the SEAC. He didn't think that was significant enough to make known. [Laugh] Talk about ... But there was enough work for both of us. Very interesting. But looking back my, my - my - I was more pre-occupied and concerned with what we'd call administration, some technical work, too, but technical administration, of the mathematicians' part of the Bureau, Computer Bureau, the computer's computer programming. Alt was more the professional science administrator in the Mathematics Division,

concerned with the technical operations of the Division with emphasis on the computer.

RRM: Now the Bureau of Standards in this era also, if I'm not mistaken, also had played something of a very important role in training, .. people who became active in other government agencies in the - in the computational techniques. And perhaps you might like to comment on that at the beginning of the next reel.

END OF TAPE I, SIDE II

TAPE II SIDE I

EC: Now

RRM: We were on the subject of education.

EC: Yes. We did contribute to education, both the, the engineers at the Bureau and the mathematicians. .. Mathematics Division, for example, had courses, I believe, under the support of the Office of Naval Research, mainly, summer - summer courses conducted twice for university people who, who, who were involved in the operation of computer facilities and concerned with -- I believe that we had a least 20 to 25 people each summer. This was done with advice of the advisory panel which we had, had throughout these years, consisting of representatives of government agencies and representatives of universities.

RRM: Do you recall the name of this panel?

EC: It was called the Applied, the National Applied Mathematics Laboratory Advisory Council. [Laugh] Long name. We still have advisory panels, much smaller. They are concerned - We call them evaluation panels now. They are concerned with technical evaluations. The early advisory council committee was concerned with assisting us administratively and, and you know, in contracts, sources

of supply and, and helping us in arranging funds.

RRM: Do you recall any of those individuals who were active on this committee?

EC: I'm sorry, just one or two. For example, Leonard Eastis, Marshall Wood, Ezzard Glazer was the Bureau of the Budget representative for one year from HEW.

RRM: How do you spell this? He used to be with the budget but he ...

EC: He has been in the Budget Bureau and then he went with Patent Office. Now he's at Health, Education and Welfare. .. Now, I'm thinking of the early advisory panels and we had, had people like Dr. Rossiter, Bill Norris of MIT, .. Dr. Cullen. As a matter of fact, he was on - I think he was on the advisory in the early days because I remember his coming out to look at the SWAC. And I remember he asked questions and he ended up in a very short time sitting at the console operating the SWAC himself.

RRM: He must have been at the University of Chicago then.

EC: .. I remember getting advice from Von Neumann, but I don't thing he was ever a member of our Advisory Council.

Well, I think only twice did we hold those .. summer schools. They, they - they were important, we enjoyed them, but they, they were a drain on our resources. I mean, conducting them, planning them and preparing problems. Most - not all the teaching, but most of it was done by members of our staff.

As a result of that and the people who worked with us in Washington, and - a - and, and at the ~~west coast~~ W C at INA, people scattered all over this country but really in - in Europe, for example, like in Switzerland, Hartree was with us for a while and he was one of the pioneers of ~~co~~ the past, a physicist ______, who were interested in computing --

RRM: Did Hartree give a lecture or did he attend one?

EC: He did not attend either but he has been with us. As a result of these courses and people who have been associated with us both at INA and Washington, we have people we call alumni all over. [Laugh]

Now at the same time, well, we Reed had conducted courses in programming, ~~I wrote~~ Ida Rhodes taught a couple of courses in programming.

RRM: That was more -

EC: That was earlier. I believe that was the only

course Reed taught. We have taught .. courses in programming at various times. For example, Ida Rhodes taught a course in programming to the students at Gallaudet College. One of the best programmers we ever had was one of her students. I forget when and forget his name.

At the same time the electronics division in Alexandria now was developing other computers and it was, it was assisting in engineering education of computers. And I believe, I believe it taught one of the courses in programming. .. .. I've forgotten just - which is more comparable I think with the courses that Ida Rhodes had been giving in the summer schools.

RRM: I take it these summer schools were more in applications? .. Or -

EC: They were the underlying theory, numerical analysis developments, bring them up-to-date.

RRM: A bit more mathematical then?

EC: More mathematical, yes. They were in numerical analysis rather than computer mathematics or programming.

RRM: They presupposed a greater degree of training.

EC: That's right.

RRM: In mathematics.

EC: That's right. These were all - these were all mature mathematicians. ~~In fact I~~ Although. This certainly isn't the only index we can - we can use, isn't the best one, often but I think they all had advanced degrees, almost all of them had PhD's in mathematics, who had, who had indicated an interest in or who were already in the field of numerical analysis and its applications.

RRM: I see.

EC: So, ~~so~~ they were all university trained

RRM: Mhm.

EC: ... and most of them in mathematics

RRM: About how large were these groups?

EC: About 25.

RRM: This was then over two - two summers.

EC: Yes. I think - I think there was a gap of two or three years.

RRM: Mhm.

EC: Now, I think, the Bureau has - well, has been very much interested in education. We have not - we have also been aware of the fact that our resources are limited; and of course this is a part of our mission, it is not our primary purpose. As mathematicians our

primary purpose, of course, is to help people in government as mathematicians ~~the Bureau of Standards through its mathematics programs~~. That's one reason we were so intensely interested in development of computers. Though I was interested in electronics because of background that wasn't my main motivation. We wanted this tool and we felt, particularly in the light of the way things were being done elsewhere. And almost anywhere you had mathematicians and engineers together, ~~xxxxxxx~~ one place the mathematicians were charged, the other place engineers were being charged, another place ... I think almost all the mathematicians, this was before we foresaw, were interested in developing this tool and we hadn't thought of the main unit in the, in the computer development apparently. I had no thought, some people thought we should have kept the computer laboratory forever, I had no desire to operate a computer.

RRM: It was a tool, a means to an end.

EC: A means to an end. ~~They badgered us~~ to put the SEAC in operation. One of the things that intrigues me about the use of computers is that there seems to be a lot of the same enthusiasm and fervor among the people who are using the computers,

scientists and engineers and people who are programming, as there was in the early days when people began to talk, and when were they going to be able to get the computer. That tool has some great potential, you can do fantastic multiple things with it. It brings out the best of all those who use it.

RRM: One question I wanted to ask about your comments on Samuel Lubpin. He, I believe was only briefly associated with the SEAC project. And he was there when you were there.

EC: Yes, but he stayed long enough to, to - to outline the logical design and really to train, although - well to enable an assistant to train himself. I don't know to indoctrinate Al Leiner who worked with Lubpin on the design. Lubkin was concerned also with the engineering.

RRM: He was not with the engineering group was he?

EC: No, we decided ... At that time Chuck Page was head of the division and we decided we needed Lubkin, he had been, he was a sort of combination mathematician-engineer in his experience, although we decided to let him join whichever division he chose. I don't know the basis of this. It was

not personal or anything because he knew all of us well. He said he would prefer the mathematics division. In that sense the mathematicians began the design of the SEAC. Lubkin began - he had been working with the EDVAC group.

RRM: Did all Leiner work with the mathematics group too?

EC: No, Al Leiner was an engineer. So Lubkin stayed, I forget how long, then he left. Sam was a wonderful person, very quick. He had a little difficulty with - with engineers because he was so quick that he, he couldn't understand just why anyone with whom he was talking wasn't equally quick. He sort of frightened people. He arranged that Alexander, Lubkin, and Cannon would have sessions where no holds were barred. We would get at the source of this difficulty. Mathematician Lubkin and the engineers. We remained great friends throughout. Very good. He was well-intentioned. It was just that he frightened people and he couldn't understand why it would be necessary to demonstrate the value of the procedure.

This wasn't the reason, however, for Lubkin's leaving. He was interested in the potential of this new device and he wanted to find a company which he did. And, by the time he left, Leiner had

developed to the point where he could take over entirely. So Leiner was involved with SEAC in the initial design, not the problem design. Then there was a great deal of give and take and compromise and mutual interaction between logical design and what was found to be engineering feasible or desirable. Sometimes the design was creating the push; sometimes the engineering was creating the push. If we can do this so easy, why don't you want this in addition. So Leiner was quite capable and then - was the head of the logical design of the Bureau throughout the design of other computers.

RRM: Mhm. Where is he now?

EC: He, I think, is with IBM Company.

RRM: He was in a sense replaced or supplemented by Ralph Slutz?

EC: No. Ralph Slutz was here at that time. Ralph Slutz had come in, I don't know just when he came. He was at Princeton and he came to the Bureau. He interested also or about that time I don't know whether he came before Lubkin left. Slutz really headed the design and development of the SEAC, as far as the administrative I mean, he was the technical director, responsible for design.

He replaced Lubkin in a sense. Lubkin was primary concerned with the logical design and Slutz was concerned also with these other contracts and component development. Alexander was at a higher - our administrative level, technically he was a little closer to the SEAC design than I was. Slutz, I would say, his role was comparable to Huskey's role.

RRM: I was trying to place Leiner --

EC: Leiner worked under Slutz. Leiner was really the logical design of the thing, but had nothing to do with engineering design. Slutz did and Slutz was responsible for the over-all project.

RRM: He, he was the chief engineer.

EC: I would say that was his role.

RRM: Would you - oh, one thing it seems that emerges from this generation of machines, the SEAC era, you've already touched on it a couple of times. There are some traditional tensions that seem to emerge between the two groups, the engineers who were building the machine and the mathematicians who wanted to use it. These are almost perfectly understandable ones, because the mathematicians very often have to begin before there is any machine. Working with theoretical programs,

EC: That's right.

RRM: theoretical controls, somewhat more sophisticated applications. Needless to say over the years of effort put in you like to see it working.

EC: Yes, and you don't like to give it up for a day or a week, while the engineers pull it apart, rearrange the components while they experiment. And - and from the standpoint that's true. From the standpoint of the engineers, the engineers quite largely and quite rightly solved that design which, say the mathematicians like Cannon created. SEAC was really not the best we could do so they wanted to improve it. So how could they if the mathematicians were using it all the time.

RRM: In a sense you are trying to optimize two conflicting sides.

EC: That's right. Well I think in all, with a few exceptions. I remember I think in some cases they groups fell apart, the engineers were went, then not mathematicians maybe left the company a few went with Raytheon, but then other mathematicians came in. You know personality difficulties developed too over these circumstances - sort of and of course the tendency - there is one.

Very rough draft

In most places as at the Bureau, we all remained friends. We would have arguments and at times maybe go up to a referee.

And I can remember the engineers talking about some of their people like Ida Rhodes. She was one of the most powerful programmers we had. She would forget instructions engineers would give her. You know, "This, maybe, this - Look tonight, Ida, this tape device isn't so reliable so don't run that. Try to run that tape alternately. We suspect that's were the trouble is." Of course, Ida would forget all these things. And then - then Ida would complain about the unreliable set. [Laugh] But we all remained good friends. We can chuckle now and Sam and I made good friends although I wasn't best interested at time full of convinced. I think that was true almost everywhere.

RRM: With that generation of machines, correct me if this doesn't - didn't apply at that time to, to SEAC, since the engineers were really building a one of a kind machine in this generation we were, for all intents and purposes, the priority if it came really came down to a choice and it was raised, tended more to be in favor of the engineers, the

ones that had to get the machine built and running as - In other words, if there were battles perhaps they would win a few more than the mathematicians. I don't know, .. this at least seems to be the impression I have with other machines in this era.

EC: I think that's possibly true and I think it's a good thing it was true. Looking back to an extreme case, I went to the West Coast because the - the - the engineers could not really put their hands on the SWAC because the mathematicians were using it around the clock throughout the week to try to obtain a gainful. They could not. Yet the engineers were not given the opportunity to really have the machine in the sense that they needed it not just for a few hours but maybe to be able to pull it apart a little and try to do different kinds of tests and, and, and to experiment with it. This was an extreme case. And we recognized the fact.

RRM: In a way it's quite a compliment to you, you as a mathematician to help out the engineers in a situation like that.

EC: Well, it was very interesting because I had the job of, of, of making the engineers more visible without losing Harry Huskey's interest. Harry

Huskey was convinced he was doing the right thing, and who could be certain? And yet we knew only we we were not making enough progress in proving the machine and good engineers were leaving. We felt something had to be done. Again Harry and I remained good friends and sometimes he was not convinced of what I had to do was right and sometimes
it just has to
I was not convinced of what he said. We remained good friends.

It was partly the fact that we were such beautiful people and also partly the fact that we were involved with something bigger than we were and we realized it. I think it was a combination of the two. I think that this is sort of typical of what was happening in other installations, too. But you are right,

It was necessary to have a machine and to give the engineers enough time to develop a working machine.

and- and - and this was apparently -some difficulty in thinking back

RRM:

EC:

RRM: If you could do an over-all assessment of - of what you feel have been the more significant contributions of the Bureau of Standards in the era from 1946 up to say '53 or '54 to computer technology.

EC: I think the most significant contribution from the mathematics standpoint was its - its acceleration of the development of numerical analysis. ..

Through the purpose

With the assistance of outstanding mathematicians in the not only in this country but

Our early

The operation of SEAC in our organizing and the computation laboratory which I believe sometimes

and our application of the machine on the West Coast in solution of problems which were really important and difficult and needed to be solved. But -

and our - our participation

but we were concerned

Of course I must not leave the census type applications. I think from the standpoint of the government, the Bureau of the Census Department together, that this was an extremely significant contribution, the - the development of techniques and utilization of large scale computer techniques.

From the standpoint of - of, of hardware development I think that the, the SEAC and the SWAC both sort of helped people. I mean, in those days we needed success and we had success eventually, and so on. But we were all working together so each success sort of strengthened all the other groups as far as the possibility of management to continue working and their assurance that what they were trying was not impossible to do.

So from that standpoint I think it was important.

Also, the SWAC particularly was extremely valuable in its applications at the University of California, from the standpoint of education in this sense: it was operated for some years on a round the clock basis by graduate students and anyone from the university who had need for this kind of machine. ..

I think - I, I, I think that's about the sum of the contributions of the early life. We were early in the game. I think we accelerated the development of the field. My feeling is that we, we hadn't been in the fifties, as far as numerical analysis is concerned at least maybe a year. I've said this at the Bureau considering cost and everything else, how do we put a dollar value on the contribution? I would say certainly that certainly in that field, we accelerated the development of the field, at least a year.

Who knows. If we had not been beating the bushes and talking to people like IBM, I have no way of telling how long it would have taken before they entered the field of computers. If we and other people, like Von Neumann at Princeton, Ed Berkeley
Forrester and people at the University of Illinois had not been active and talking about this, I - I don't know long it would have been before industry got into. But to say one year ... - we gave one year. Then the cost benefit analysis is fantastic.

I can't say that we - we - I can't point to any one technical thing and say We've mentioned circuit which was key in the development of the computer. Of course one thing that was

very important was the magnetic core. Jay Forrester But really not in this period in which we're talking.

RRM: Do you feel that the contributions of the Bureau of Standards as a stimulator of activity in the field .. were particularly also important historically when one considers that this was a time before the training war, after World War II, when there was generally a retrenchment in expenditures for research and development programs, by the government, by the services. There was to a certain degree less money after World War II and this was before the impetus of Korea.

EC: That's right. I'd never thought of that one. I think we and the government agencies who were associated with this, Air Force, Army Map, National Security Agency, I believe we were responsible for keeping alive this development and - and, and as I said we ran into some difficulty, our director had difficulty with Congressional committees explain why there wasn't any progress, haven't you developed this, why isn't it in operation now.

So I think the small group of government agencies, and universities made a tremendous contribution)

more than we will ever realize, when we consider the output of attitude of the public, the publicity people of manufacturers.

I believe this was a good example of the kind of development which was so gigantic at the start with respect to cost in time and money. That, that, that, the only way of doing it is through government sources. This is not the sort of thing that any one in industry ordinarally can be expected to do.

RRM: In this particular instance actually the government did do it, support academic activities, some industrial research, and ERA then in-house work. Thank you so very much, Dr. Cannon.

EC: Thank you for keeping me on the track of it, which you have.

END OF INTERVIEW