Interviewee: John H. Curtiss
Interviewer: Henry S. Tropp
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TROPP:

This is a discussion with Professor John H. Curtiss in his office at the University of Miami in Coral Gables. Professor Curtiss is Professor of Mathematics here at the University of Miami. [Recorder off].

I guess the first question is really quite a general one and that is: You were in the Navy during the war and on leave of absence from Cornell as a mathematician. How did you end up at the Bureau of Standards?

CURTISS:

Dr. W. Edward Deming, who is one of my best friends in Washington, at that time was a senior adviser, statistical adviser, at the Bureau of the Budget -- was friendly with Dr. E. U. Condon. Dr. Deming, like some other great statisticians at the time and before, was himself a trained physicist. I believe Karl Pearson was a physicist and I believe also R. A. Fisher was a physicist; and naturally the physicists more or less knew each other, and so Dr. Deming felt that there should be a statistical adviser in the Bureau of Standards just as there was one in the Census Bureau -- that is, a person qualified statistically who had the ... who had publications and who might, say, be a Fellow of the Institute and the Association, just as Morris Hanson had the same qualifications for a parallel job with Census. I believe that Dr. Deming actually brought Hanson, Mr. Hanson, to the Census Bureau.

Well, he felt, from his knowledge of physical science and the lack of statistical sophistication in many physical science and engineering experiments, that the Bureau of Standards was a very fertile ground for introducing further statistics. Dr. Deming was a great promoter.

TROPP:

Well, of course I couldn't agree more, looking back on that period, the branch of mathematical statistics that evolved really wasn't used too many places and the way it was handled ...

CURTISS:

The first great introduction of -- the first energetic introduction of mathematical statistics was the quality control movement which flourished during the war. Dr. Deming played a

leading role in that. The name of Shure, Dr. Shure -- another physicist, by the way, turned statistician. Shure, at the Bell Telephone Laboratories, was the pioneer in American statistical quality control.

TROPP:

As you look back at the thirties I think we find that many of our Government bureaus, like the Weather Bureau, Census Bureau - there were no statisticians as such and yet they handled large masses of data all the time.

CURTISS:

That's true and for some years after I went to the Bureau of Standards we kept trying to indoctrinate our sister bureau, the Weather Bureau in the Commerce Department into statistical methods but they were rather obdurate.

TROPP:

You joined the Bureau, then, in -was it 1946?

CURTISS:

Yes.

TROPP:

Well, in one of the documents that I have is a report, a preliminary report, that you wrote in September of 1946 on the establishment of a ... it's called a National Mathematical Computational Center under the Bureau of Standards. Do you want to get into some of the background that led up to this proposal?

CURTISS:

Well, my concentration on purely statistical matters at the Bureau lasted only about two weeks, because at the end of a very short period of time, something like two weeks, we were informed through Morris Hanson, my statistical counterpart at the Census Bureau, that the Census Bureau had money which could be transferred before June 30th, under the Economy Act, to the Bureau of Standards for the development of automatic computing machines. Would we be interested in accepting the money? Dr. Condon was interested in accepting money from anywhere anytime, so he directed me to make overtures to the Census Bureau and indicate that we were only too happy to try to develop automatic computing machines; and that's how a broadening of our statistical effort - mathematical statistical effort at the Bureau originated, from the Census Bureau desiring us to develop automatic computing machines.

By September of that year, as I recall it, we had at least three other sources of transfer – ONR and I'm not sure whether it was by that time, but the Air Force. I think there were three sources. Let's say at least Census Bureau and ONR and Mina Rees began -- Mina Rees began to play a leading role in the mathematical activities of ONR, coming over there from OSRD where she worked as an assistant to Vannevar Bush and was particularly in the -- more, I should say an assistant to Warren Weaver, who was head of the Applied Mathematics Panel, was that called?... of OSRD and Mina became very friendly with me. She has been a very close friend for -- she was a close friend for a long time. It's only since we entered academic life that we slightly have drawn apart, since we don't see each other. And she suggested, partly probably because her clientele, that is her superiors and colleagues at the Office of Naval Research, suggested it, that there should be established somewhere a center for the development of computing machinery, since computing machinery seemed to be the thing of the future.

This sounds sort of conceited in the sense that nowadays such a center should be established and be sort of pre-empted by any one government agency would be ... seem very arrogant, because we have gotten so used to the idea that there are ... that high-speed automatic comp ... automatically sequenced computing machinery is a central part of our lives. That wasn't so then at all. The real implications of the ENIAC and of Aiken's machines were not even beginning to sink in and only certain farseeing people like Morris Hanson of the Census Bureau and certain people in the Navy who, some of them were experienced ... got their experience in naval communications where large-scale automatic machines of a certain specialized nature were in use -- only those people saw the big future and the need for government effort in automatic computing machinery.

TROPP:

Were there other candidates in the Federal Government for this national computational center, say like Aberdeen? Was that considered?

CURTISS:

No, Aberdeen had its own computational center and its own group of mathematicians and although I made a number of overtures to Colonel Simon, them the director of research at Aberdeen, we never got any cooperation from them at all. They were just jealous of us, as you would expect.

TROPP:

Well, in retrospect how do you see this decision to establish a National Computational Center at that period of time?

CURTISS:

Well, I should say again, another reason for the National Computational Center idea was that the Bureau for many years during the WPA days before the war had been sponsoring scientifically sponsoring -- the Mathematical Tables Project in New York City and ...

TROPP:

This was the one under Arnold –

CURTISS:

Dr. Arnold Lowan, and its was a WPA project. It was then supported during the war by Defense funds of one sort or another, probably through OSRD, but always under Bureau sponsorship and one thing Dr. Condon did do - and this had escaped my memory a few minutes ago in connection with that statistical advisory job - one thing he did ask me to do as part of my job was to be supervisor from at least the Washington point of view of that laboratory.

TROPP:

That essentially then really kept it going, because otherwise they would have lost the main financial support.

CURTISS:

Right. So we had that laboratory and then it was decided that we couldn't support it in rental quarters in New York any more and over their loud kicks and screams and a union boycott and various things we brought the thing to ... we brought the laboratory, lock, stock and barrel to Washington. No I'm not sure ... you'd have to get from this history, this document that's missing here, just when we brought it, but it could have been before September. If not, it was in the planning stages in September and was part of this national computational laboratory scheme.

TROPP:

Well, you mentioned ONR's interest in there. One of the documents I have is a memo from Mina Rees suggesting ONR support and it's dated October of that same year and so…

CURTISS:

You see there would have been conversations preceding that.

TROPP:

So that in a sense would be implicit in the same time frame that you just mentioned.

CURTISS:

Yes, that's right.

TROPP:

...Talk a bit about the Mathematical Tables Project because I'm just learning about it gradually. Looking back on that project, how would you evaluate it and what do you see as its impact?

CURTISS:

Well, it was the leading mathematical tables producer, manufacturer, empire, if you will, of its day. In the days before automatic computing machinery mathematical tables played a big role. The Mathematical Tables Project was far from being a pioneer. Some of the pioneering work at large-scale production of mathematical tables was done ... well, of course, it was done in the nineteenth century, but in the twentieth century by an English astronomer named Comrie, with three initials preceding. Comrie, I believe, was used as an adviser by Dr. Lyman Briggs, then Director of the Bureau of Standards, in setting up the WPA Mathematical Tables Project. See, the WPA had felt it had no expertise in this type of an activity so at the very start I think there was no vacillation here at all, no going to the Geological Survey first or something. From the very start, why, the Bureau of Standards was asked to sponsor the project and to edit the books in the sense of, oh, being sort of responsible for their format and for their accuracy and so on.

TROPP:

Looking back at the books themselves, the comments and the forewords, introductions, and comments in the literature - I have the feeling that it got quite a reputation for accuracy and ...

CURTISS:

Yes, it was ... it may be possible in retrospect to say it was the best, not only the leading but the best, the most accurate, most highly regarded mathematical tables project - that is, producer of mathematical tables anywhere in the world.

TROPP:

As I've been able to ...

CURTISS:

They started with relatively simple things. I forget just exactly what the starting point was now. I'd have to look at the bibliography again, but they got into quite complicated things such as, about the time we were moving them to Washington, they were working on extensive tables on the Mathieu functions and they had already made their reputation, part of their reputation, with large tables of the Legendre functions and many other difficulties

TROPP:

Looking at it –

CURTISS:

Incidentally, many of these tables are now summarized or perhaps completely - no, summarized would be the word - in the ... let's see, the Mathematical Handbook of the National Bureau of Standards, which I'm sure you know about, a large fat book sold by the Government Printing Office, prefaced by Condon and so on.

TROPP:

In the realm of computational techniques, I gather that Comrie was a real leader, given the lack of automatic equipment, in devising techniques to do computations. I also have the impression that the Mathematical Tables group did not really go this route. They had a lot of people and laboriously calculated these tables.

CURTISS:

It was a make-work project, you see, at first, and Dr. Briggs likes to tell of - he used to like to tell of going up there on his first trip to New York to help with the organization of the project and he entered a large sort of a loft in downtown Manhattan and there were something like 80 to 100 people there and one broken down electric calculator. That's how it started.

TROPP:

Because it's really amazing when you think back to how little they had to work with. At the beginning the first few years were all paper and pencil calculations.

CURTISS:

Well, they did have hand computers.

TROPP:

That came later though.

CURTISS:

Mm-hm. There is one thing they should be given quite a bit of credit for, although I don't think they were originators of the technique and that is they were among the very first groups actually to use automation in the sense of punch card machines as tabulators and as adders, or adding, and collating and so on. That is, the punch card machinery, the individual punch cards, the IBM punch cards of the day that is - we're talking now during the war and previous to the war - just previous to the war - were exploited pretty fully by the group.

TROPP:

The other exploitation of that of course is Wallace Eckert's astronomical computations at Columbia.

CURTISS:

That's right, but he was-- at that time, however, he was at the Naval Observatory I believe; and really, I think in this country the Naval Observatory gets pretty much the credit for first using punch card machinery in such computations.

TROPP:

In this 1946 report of yours there are a couple of comments that I ran into and you might want to react to them. On page 3 of the Appendix of that report you use the word computers and its still people - it's still not machines. Computers in the dictionary sense had been computers - I mean had been people up until this point and ... When did that shift occur, where a computer became a mechanical device instead of a person?

CURTISS:

I don't think I participated in that at all. What we felt - what we would say we were developing at the Bureau as of about then were automatically sequenced calculating machines ...

TROPP:

Right, right, and that's the phrase used in the report.

CURTISS:

--calculating engines –

TROPP:

Right, and the English used the same phraseology.

CURTISS:

I think we must have used computers as people for many, many years because – remember we had quite a large number of professional computers, human computers, in the Mathematical Tables Project, which later became as you know -- so soon as we moved it to Washington, we called it the Computing Laboratory of the Bureau of Standards.

TROPP:

In the letter to J.C. Capp that you gave me yesterday, there were a number of things that I wanted to ask you about. You used George Stibitz as a consultant ...

CURTISS:

Yes.

TROPP:

...and it mentions a number of reports and I tended - or people tended to think of Stibitz in that time period as being associated primarily with relay calculators except that I know that also at this same time he was considering and beginning design of an electronic computer for business purposes, at least that one I know of...

CURTISS:

Yes, he was, and he had Barber-Coleman Company in Ro ... in ...

TROPP:

Peoria.

CURTISS:

Not Peoria...

TROPP:

I'm sorry I've been there ...

TROPP AND CURTISS:

Rockford –

CURTISS:

Illinois. I visited Barber-Colman once. They were evidently a very highly competent group in what they had been doing. They were interested - I mean their specialty was industrial controls and Stibitz - I guess Stibitz had been one of their mathematical consultants ...

TROPP:

I think he had met the President and they were in OSRD together. I think that's where the contact was. But I wonder if you remember some of his reports, some of his comments, and some of his thoughts in that time period, when he was consultant to you.

CURTISS:

No, only his definitive report on which we acted and that was that the way - by this time apparently - by the time his reports were in our hands certainly a year at least had passed from my coming to the Bureau and we had quite a bit more money from various other organizations too. I think we had the Air Force Comptroller's money then, at least promised, and the report which we accepted and acted upon was that first of all there should be contracts let for several contractors to prepare design specifications for the computer, for the kind of computer that we wanted, and prepare key - I mean build key hardware components, such as build prototype of the memory.

TROPP:

This is one of the breadbox reports that you gave me a copy of, say, from ERA, because you were merely asking - or breadboard rather-you were asking them to do breadboard work along with this preliminary design.

CURTISS:

That's right, that's the other way to say it. So Stibitz felt that not only must we have paper that is, not only must we have detailed plans for the machine, how it's going to ... what the system is, but we must have some key components built. This was unacceptable more or less to the Eckert-Mauchly firm because they felt they complied with all this and

on top of--they felt that their machine, I believe, was patentable and they didn't want too many blueprints flying around. But Raytheon accepted this. We tried to get General Motors -- I mean General Electric --interested and after some negotiations, why, they dropped it.

TROPP:

How about RCA? Were they on your list of potential builders?

CURTISS:

Yes, but they never accepted this particular arrangement. Really the only three people – the only two firms that ever accepted, and this was accepted with reservations on one part, was Eckert and Mauchly with their already fully conceptualized UNIVACS, and the Raytheon people who wanted very much to get into this game.

TROPP:

And as a result of that of course they did, with the RAYDAC machine eventually.

CURTISS:

Well, they finally did with the machine that we put at Point Mugu, you me an --

TROPP:

Yes.

CURTISS:

Yes, but while Raytheon was one continuous source of disappointment to us in those days, there were various reorganizations in the company and they got bright people in and then they let them go or the people would resign in disgust and we really never -- the Bureau itself never got any progress out of, any service out of Raytheon. It was only the Navy that finally took over that project and the cost over-rum was fantastic, something like a factor of 3.

TROPP:

The gentleman that I have talked to on that is Lou Fine who was, I guess, the chief engineer on the machine when it was finally constructed. I think he was the third in a line of chief engineers on that project.

CURTISS:

Yes, that's right, and the machine -- it was so delayed that it was obsolete when installed.

TROPP:

In that same letter to Capt you mentioned a number of machines near the end of the report that are under construction and one in there is one that I don't know anything about. I know it was never completed, or I assume it was never completed, and it's described as the Atanasoff machine being constructed at NOL in White Oak, Maryland. Remember the background of that particular project?

CURTISS:

No, I can't recall anything about it. Vaguely, I remember seeing components being designed. You don't see much when you go into an electronic laboratory. Mina Rees always says that she felt that these cathode ray oscilloscopes that you see lying around can be made, by turning the buttons, can be made to show you anything.

TROPP:

[LAUGHTER] This was far from wrong - from being right, rather.

CURTISS:

I don't remember anything about the Atanasoff machine but of course you have available to you someone to ask right there in Washington, don't you? Isn't Dr. Ray Seeger still one of the senior scientists in NOL?

TROPP:

How do you spell his name?

CURTISS:

Seeger is S-e-e-g-e-r. He's a distinguished physicist.

TROPP:

I'll have to check.

CURTISS:

I believe he came there from the University of Maryland or went back to the University of Maryland. Anyhow, he was one of the best-known physicists in Washington in the

postwar days.

TROPP:

One of the phrases I ran into in the trip report that you gave me was the usual government acronym or initial thing that I don't recognize, and it refers to an MLRF problem. Can you remember what those initials stood for?

CURTISS:

No, I don't remember that; I'd have to see the context.

TROPP:

Well, there was no context there to - you know, just a reference to this, and that's the trip report when you went to the Moore School in 1946 and apparently it was a problem that the Bureau of Census was interested in.

CURTISS:

I recall that a major incentive to the Bureau of Census to have high-speed automatic calculators developed for it was not, was not the decennial census but a survey of manufacturing - well, a survey related to the Gross National Product but it was a manufacturing and retail -- we're getting the MR -- activity. Now I don't know what the other letters stand for -- which was done on a much more frequent basis, a continuing basis practically -- they would like to have it monthly -- would have liked to have had it monthly, this particular index of activity, industrial activity.

TROPP:

They mentioned two major problems. One was this, and the other turned into a major sorting problem in terms of the foreign reports that they mentioned, and, I guess as a result of their needs, the people at Eckert-Mauchly eventually developed a sorting program to do this; because, according to that trip report, they weren't too happy about the sorting potentials of the machine then being –

CURTISS:

Mrs. Rhodes can tell you more about that, in her reminiscences, because I know that we had, as soon as we got some of those Mathematical Tables Project people crossed over into the computer development area, why, we had them working on sorting problems and collating problems as they would - as the problems would be solved by large scale automatically sequenced machines.

TROPP:

One of the companies that I don't know as much about as I should at this point is ERA and I wonder if you can remember anything, how they got going, and their eventual demise when they were taken over.

CURTISS:

Well, Engineering Research Associates was, I believe, set up by Naval Communications in Minneapolis -- is that right now, am I in the right company? Were they the ones that were taken over by Control Data?

TROPP:

Yes.

CURTISS:

OK, fine, well then, I can tell you quite a bit more about ERA. They did develop before their demise and shortly before the end of the war, I believe, a rotating drum machine which was installed in the Naval Communications Center on Massachusetts Avenue diagonally across the street from American University, from the main building, at least at that time. Their chief mathematician and partner - I think it was a partnership at first - was a distinguished gentleman, American mathematician - Dr. C.B. Tompkins, T-o-m-p-k-i-n-s, pupil of Wilder I guess at Michigan, but who had been a National Research Fellow and was thought of as one of the brightest young American research mathematicians. He spent all his war years I believe with ERA or Naval Communications, in Navy Communications but assigned to ERA.

TROPP:

This was, I know, highly classified work that they were doing during the war.

CURTISS:

It was classified. It was coding; it was ciphering; it was called a decoding machine, you know, could try millions and millions of different possible decodes, ciphers. You know what a cipher looks like - it would look like just C, D, A, B, or Japanese letters or something like that and you've got to pick out patterns, recurrent patterns. Figure out from those what was the recurrence of "is", words like that ... maybe that's not ...

TROPP:

Yeah, yeah, of course that's the way ciphers were decoded, through frequency of ...

CURTISS:

Yeah, frequencies, that's what I was groping for. Well, anyhow, Tompkins was with them and there were other partners whose name escapes me now but who had good reputations in electrical engineering, and there was at least one managerial type. The names in general, if you could go back to the list of pictures and ...

TROPP:

I have some of their early publications so it's pretty easy to get some of the names.

CURTISS:

You would find that they were rather famous - rather, at least not well -- not distinguished even, but well-known for their competence at the time. It was a blue ribbon activity absolutely. Well, as I say, they did develop the rotating drum machine. They did very little with electronics except they would have naturally had electronic arithmetical organs in their machines, I suppose, but you know, ring counters and various ways of making electronic arithmetic organs were old stuff even ...

TROPP:

Right, they were old for ENIAC, yes. That's right, they built on the Eccles-Jordan...

CURTISS:

Well, this is ERA and then of course - were they taken over by Control Data before the outfit... before they merged with Sperry Rand?

TROPP:

I'm just not sure.

CURTISS:

See, Control Data disappeared into Sperry Rand and then...

TROPP:

Well, Control Data's still going.

CURTISS:

That's the one... yeah...that's the one...

TROPP:

That's right, I guess there was a Control Data, but I guess they were taken over by Sperry Rand.

CURTISS:

But were they taken over directly? I thought there was a merger there with some other company with a name sort of like, that reminded me of Control Data.

TROPP:

I guess I can check that out, but they produced a number of machines during that period. The 1101 I guess is the one that is most familiar.

CURTISS:

Yeah, do they call that an ELECOM 1101? Do I remember that, or is that Control Data?

TROPP:

That's another machine, not Control Data, but it's...Electro Data is what you're thinking of.

CURTISS:

Yes, but the 1101 was definitely one of the early high-speed automatic computers, automatically sequenced electronic computers, with a huge drum memory, as I recall, attached to it. This was quite competitive with the early IBM efforts and with the UNIVAC. Then when the 1101's - when the merger took place with Sperry Rand, why, Sperry Rand slapped the name UNIVAC on what was essentially the 1101. Forgot all about the UNIVAC. See, the UNIVAC was a quite different beast from anything we know about nowadays, from the second generation of postwar computers. As you remember, it's serial; mercury delay line memory is the heart of it and this was abandoned and Sperry Rand concentrated on machines of the type that the ERA people pioneered in. I think the ERA machine had a parallel arithmetic organ, if I recall it, not serial. That was suitable for use with a drum, you know, and so in ways it was a high-speed machine.

TROPP:

Well, let's go back and get some of the problem that you're most familiar with and this is

the decision on the part of the Bureau to build its own machines which resulted in the East Coast and West Coast machines.

CURTISS:

Well, almost simultaneously, at the same time that we adopted the Stibitz report, over at the Harry Diamond Fuse Laboratory Dr. Condon initiated componentry research and at first there was at least - I imagine that Alexander and Slutz, the two chief engineers over there, had intended all along secretly to wangle somehow or other a complete machine from the experiment, but for a while it masqueraded under the guise of componentry research, untuned with the boss's section in which it took place. It was only much later that Alexander became the director - or the head of the section. I can't remember exactly when we came out from behind the bushes and decided to build - it was probably after about a two-year disappointment running, sort of disappointments, with Raytheon, realizing we had no alternate supplier than Eckert and Mauchly or their various corporate patrons, and this forced the Bureau into starting with SEAC.

Now the SWAC was more or less started, I believe, as I recall it, to please Huskey. There was talk that there should be at least some componentry research going on in that blue ribbon laboratory that they were setting up out there, the Institute for Numerical Analysis, and Huskey was to be a part of the Institute. He wanted to build this machine and we managed to get money for it and so this was a more or less - always was a more or less overt project in the sense that it never masqueraded under the disguise of just some componentry to check up on contractors. It was supposed to be a totally original machine. He had developed his own system analysis for it and he wanted and he knew - he brought to America the concept of the Williams tube, I believe, at least he says he did.

TROPP:

Well, but he was there in England during the period that it was evolving.

CURTISS:

Yes, and so his desire was to build some -- I believe we got money from ONR. ONR, by the way, was a part of the whole Institute for Numerical Analysis group some years there and permitted us to divert money into Huskey's scheme.

TROPP:

Well, that was, of course, a major question and I mentioned to you an independent research project that's going on the West Coast under my direction, on West Coast development and the Institute for Numerical Analysis is a thing that I would like to focus on eventually and when I think of the origins of numerical analysis and its state in the pre-electronic machine period, I guess what I really wanted to ask you is, how did you

decide to set up an Institute...

CURTISS:

Well, it was a joint decision, I believe, with Mina Rees and myself and Dr. Condon. We felt that Washington wasn't - Washington has never been an ideal place for a really academic effort, you know...

TROPP:

You attract really...

CURTISS:

...and so we felt that we had to go to some university campus to attract really good people, or to some attractive climate. We were actually wooed, for instance, by the Hughes Tool Company, the plant which was down near Long Beach, because at that time Woolridge -- Ramo and Woolridge were, oh, the chief scientists in that area. They were developing electronic guidance systems for the Navy and for the Armed Services in general, and they felt that it would be a nice adjunct to have a roomful of our mathematicians and some of our..well that was just a passing thought. We never gave it serious thought.

We did feel that we should place -- well, if we're going to spend all this money - by this time, by the time the Institute for Numerical Analysis concept began to jell, we had been in the computing business I think at least two years, had we not? - Is that right? - The Bureau had, and we had quite a lot of money by then, especially from the Air Force and Atomic Energy Commission, something like two to three million dollars in development money had been sort of amassed and partly spent. And the concept was this, if we're going to have such a development of automatic computing machinery, we ought to have a parallel development of how to use it, and use it effectively; and that, of course, to Mina Rees's and my mind, and to a lesser Condon's, led right to the concept of a high-class mathematical outfit. Physicists - it had to have physicists in it of course, mathematical physicists, to consider the - to extend the known knowledge, the available knowledge, on algorithms, convergence theory, and so on.

But in order to give the right atmosphere to the Institute first of all we felt that it had to be an academic atmosphere, just that there should be no authoritarianism at all. It should be a group of peers working together as a university. There should be a distinguished local director always, who would be well accepted by the mathematical community, so that would help us if nothing else in recruitment and...let's see, what else there...

TROPP:

About the selections of topics, did that...

CURTISS:

No, we left that pretty much to the Director. There was some...some influence from...well, it was mainly a matter of...the selection of topics was made mainly by the appointments we chose to make and the Directors that we chose. For instance, when Fritz John was Director naturally there was an emphasis on partial differential equations. His specialty was parabolic type partial differential equations, always has been, and naturally he would hold seminars, interest people like Forsythe, Forman Acton, others who were there in a more junior role into trying to help him with his research. So there would be a sort of a main fuzzy type of program orientation created by the Director himself. Much more than that by the appointments. For instance, Wasow was a perturbation ordinary differential equations man and he got himself interested while we had a summer...about the second summer out we had an all summer program on Monte Carlo with Will Feller out there and various other people. Erdosz took part in it most of the summer and that gave an orientation, you see, there to the program of just whom we asked to come out.

There was one person who went right on doing his...one of the senior scientists there who went right on doing the kind of work he had always done all his life and that was Arthur Szasz. Her didn't change his direction in the slightest. He kept working on the order of the coefficients of orthogonal series and so on and its order of magnitude and we felt that was good. It was a good thing. He was an excellent analyst. He was available, very, very affable and available for questions on classical analysis and not only that, he published a lot of papers and they were bylined "Institute for Numerical Analysis" instead of "the Bureau of Standards." What more could you want?

TROPP:

Right, right. Well, you were the Director for a period, and I guess...

CURTISS:

I would consider myself Acting Director. I was always Chief of the Divsion and I was always part of the Division so that was the only sense in which I would be considered Director. Yes, I remember for a while when we first began to operate, we couldn't...we didn’t immediately get the kind of chief that we wanted. I think for as long as maybe a year I acted as Director. I remember at that time Lanczos was one of our chief scientists. I don't remember exactly any particular direction that he gave to the program though. It was just that the people pursued their interests. Oh, Ostrowsky would have been there and he would have been working on a quite relevant matter - that is, the solution of nonlinear algebraic equations.

TROPP:

And Lehmer was there for a period.

CURTISS:

Well, Lehmer was later. He was always on our list but it was some time before we got him, not until the oath question broke out that we managed to lure him in. Barkley Rosser was one of our most effective Directors. I think he was there two years. He originated an oriented project and that is the project of determining - or rather evaluating, examining, and developing all possible methods for solving large linear systems, either inequalities or equalities. From that, why, there sprang an excellent report for its time from Forsythe which summarized all known methods for solving linear equations, iterative and finite. Also, from that project there developed Hestenes' work on the conjugate gradient method, which is a finite method basically. But unbeknownst to us, why, _____in Switzerland developed the same method simultaneously. When that became known we immediately invited ___ and he was one of our regulars at the Institute for years then thereafter. But that shows how the program was handled. Topics were not chosen by the Director and thrown at this or this man as an assignment. The whole thing was a matter of getting people interested and working.

TROPP:

Looking at the Los Angeles enviromnment... looking at that environment, there were a lot of things going on. You have the Rand Corporation and they're building their own machine. You have the aerospace industry installations in that environment. How do you see the interchange of flow of ideas and impact between these various groups?

CURTISS:

Well, we had numerous colloquia, as you would expect we would at the Institute. These were attended by Rand people.

It was less convenient for our people to be concerned with the Rand programs or, you know, what few activities they had that were open to the public just simply because there was so much security all the time. We knew what Rand was doing as far as machine building was concerned. It wasn't particularly interesting. George Brown at the time, early there, was their director of computing, whatever they would call him, and, mainly, what they were doing there was, they were watching the work at Los Alamos and the MANIAC work but they were not developing, as I recall, their own high-speed automatic electronic digital computers. They were...

TROPP:

Johnniac, which is a similar machine.

CURTISS:

Did they build it finally? Yes. All right, then that's a period of later on when you see... Had the JOHNIAC been completed and was working before the Rand people started on their copy?

TROPP:

No.

CURTISS:

I've forgotten that period in there. All right...

TROPP:

That was fairly late...

CURTISS:

As of the early days of the Institute for Numerical Analysis, though, the chief preoccupation of the Rand computing people as far as automatic machinery was concerned, dealt with analog machines. They had somehow or other sequenced their program. One of the then existing types of analog machines was available shortly after the war. There were a number of rather sophisticated analog machines, you know, that were available - that were being built, some of them, in New York City by a firm whose name escapes me now. It sounds like Engineering Research Associates, but it wasn't like that – which was later absorbed in a larger firm. You're not interested, though, in tracing the history of computing through the analog stages, are you?

TROPP:

No. no, only inasmuch as it affects the computational environment I don't want to ignore it, because part of what I'm interested in is to look at the development introducing people to high-speed computing, the ability to do computing other than by hand or desk calculators.

CURTISS:

I'm sure of this, that we got a number of service jobs out of Rand. You see, we were peddling our work, our laboratory there, madly all over town. By the way, have you yet interviewed Albert Kahn who is - I think he'd be available through the Rand Corporation

where he went to after the Institute more or less dissolved. Albert was my...

TROPP:

How do you spell his last name?

CURTISS:

I'll write it out for you. His son is right here.

TROPP:

C-A-...

CURTISS:

By the way, I can probably do it with a telephone call here from his son. His son is one of our Assistant Professors. He was formerly Executive Officer. He wasn't called Associate Director. I think he was called Chief Executive - the Executive Officer of the Institute. He was in charge of selling the services that we were offering and also was in charge of personnel and he budget and things like that. He would be very worthwhile your discussing... very close to the whole activity all the entire length of time at the Institute.

TROPP:

If he is still at Rand he'll be very easy to locate.

CURTISS:

Let me try...

TROPP:

As I started to say before I turned the tape back on, I think it would be interesting to talk about people and the relative roles that they played, both at the Bureau and on the West Coast at the Institute.

CURTISS:

It was the general opinion in at least the mathematical part of the machine development effort in the Bureau in Washington, which was centered in what we called the Machine Development Laboratory where we were working on...working out paper...you know, working on systems and on programming and things like that and developing programs

for SEAC and UNIVAC - it was the general opinion there - and you may have got this from Cannon and you may not - that Ralph Slutz was primarily directly responsible for the success of the SEAC.

He was an excellent physicist with a degree from MIT, Ph.D. from MIT, and - he was close to the project all the time and was... well, a good many of the interesting ideas, such as tape machines which simply dump tape into - instead of trying to roll it up carefully – dump tape into glass compartment - ideas of that sort. That's a minor ingenious idea, but many other much more ingenious ideas seemed to develop - seemed to go back to him. Sam Alexander was his immediate supervisor and his activity was more at that time in the development of SEAC, was more of a liaison nature with industry. He was more or less responsible for trying to see what he could glean from industry and other projects, glean from other projets. He tried to keep up with reports, you know, from other projects and so on, but we felt - it was felt that aside from the fact that he had drive and ambition that his activities were less directly responsible for the SEAC. So, we want to give full credit to Dr. Slutz. I know very little else about the activities of the electronics part - that is, the hardware part of our program in Washington as far as personnel is concerned.

TROPP:

How about programming, programming part of the ___ software?

CURTISS:

The software was exclusively the province of our Mathematics Division in the Machine Development Laboratories, Section 4. The Mathematics Division was under Dr. Cannon who was chief of it for many years and perhaps his most distinguished colleague was Ida Rhodes, whom you've already talked to. She worked constantly at it. She's - you've sized her up already - very hard working, very enthusiastic, quite easy to get along with. She was very well accepted by Slutz and Alexander, much more than they accepted each other. [LAUGHTER].

TROPP:

I want to interject a question because I'm afraid it'll get away from me. How about contacts with developments abroad and there -- there seemed to be a free flow of ideas and people between England and the U.S. and also perhaps an equal amount of flow between continental Europe, at least Western Europe -- in terms of the work the Bureau was doing and the work that you were interested in and how they affected developments in those two geographic areas.

CURTISS:

Well, the developments in Europe were on a scale so much smaller, incomparably

smaller than the developments in the United States by the time that we had been in the business for two or three years and the JOHNIACs and the other machines began to come along, that I don't really think we paid much attention to them. There were - the Williams tube principle was our major inheritance from European research and development. In Germany there was - who was it? - building...

TROPP:

Walther? Alwin Walther?

CURTISS:

Walther, that's right, Walther's the name, yes, but Huskey visited him. None of the chief people in the Applied Mathematics Division except Huskey traveled to Europe at all during this period, the seven years of our existence there and Walther, according to - as I remember it, according to Harry Huskey, had little to offer for us. He was working on a very small-scale machine.

TROPP:

I'm thinking more of flow the other way and...

CURTISS:

Europe had no relations...

TROPP:

your work might have affected developments in ...

CURTISS:

It might have but we were not aware of it. Probably it did, but the intercourse with Europe was very slight except at the level of the mathematicians in the Institute for Numerical Analysis and then that was a totally different ballgame. There we had Europeans coming to us all the time.

TROPP:

I can't remember Comrie's dates exactly, but did he spend any time at the Institute?

CURTISS:

No, because I remember - I've never met him. Oh, there was one other person who did

spend a lot - I believe he was Director there for one of the years and that was D.R. Hartree.

TROPP:

Right.

CURTISS:

Hartree is a British physicist interested in computing, particularly computing tables. He made a relatively slight contribution though to our work there, both mathematically and from the point of view of engineering science. So, I believe the story of the European contacts is a very short one. There's not much to it.

TROPP:

I gather there was some rivalry between the SEAC and SWAC?

CURTISS:

Oh yes, there was a race to see who would get the machine going first and Huskey got it going, except it wouldn't work. He got it going first and we had a farcical dedication of the Institute building and of the SWAC and all it could do was multiply 2 by 2 wrong and get 3.

TROPP:

[Laughter]. That's only the first approximation.

CURTISS:

Dr. Condon was very much put out by it. He'd invited Louis Ridenour who had always been a friend and rival of his in the physics world and Ridenour was to give the inaugural address - I mean the dedicatory address, and there were a number of other distinguished people. The Chief of the ONR was there, of course, with Mina Rees and others, and it was quite a distinguished group of people but a very disappointed group because the darn machine wouldn't work! It wouldn't work for nearly a year after that with any kind of regularity, you know, with any kind of reliability.

TROPP:

You might talk a bit about Condon. I told you how impressed I was with some of the early documents of his that I've read, one of the speeches that I have a copy of. I gather he was one of the prime movers for establishing the Mathematical Center, Comptational

Center.

CURTISS:

Well, I suppose so. He brought me in - this is going over material we've gone over earlier today. He brought me in primarily at Dr. Deming's recommendation to be his statistical advisor like Morris Hanson in turn, to go around to the labs and try to straighten out their ideas as to experimentation, but then very shortly and very quickly why we... and then of course I was also told to be head of the computation laboratory - of the Mathematical Tables Project and then from that developed very rapidly this entry of ours into the field of high-speed computing, or rather development of high speed computing machinery.

TROPP:

Was he instrumental in that shift?

CURTISS:

Well sure, but he was an opportunist; he took what came. He didn't actually go out and try to raise the money for this. It just dropped into his lap, but the moment it dropped he knew what to do with it, you see. That is, as I told you before, it was he who participated in the decision, once we had a lot of money, to enter into the activity of trying to figure out how to use the machines as well as how to build them.

TROPP:

What was the story that brought about the demise of the Institute on the West Coast? In the literature it's pretty well sloughed off as being a victim of the early Eisenhower budget... shortly after he became President.

CURTISS:

In order to get an idea of what happened to the Institute you have to recall the history of the first few months of the Eisenhower administration and particularly the Department of Commerce under, let's see, a fastener manufacturer named Sinclair Weeks. Sinclair Weeks was a businessman with very little experience in science, from Massachusetts but of conservative Republican persuasion and Eisenhower, as you may recall, came in with two concepts in mind, one to clean up the Communists in government and the other one was to make the government smaller and more responsive and more economical, more responsive to business. At the same time - at the time the Bureau was under somewhat heavy criticism from the...Eisenhower...early...from Sinclair Weeks and early Eisenhower administration due to the testing which occurred...which was done, by the way, according to experiments designed in my division, of a battery additive named ADX2, was it, or

ADX5, something like that. This additive had been sold on the basis of testimonials. Now

I won't waste your tape here by going too far into this thing here but it brought an awful lot of newspaper criticism and criticism from Sinclair Weeks, of the Bureau, saying...he went before Congress...Congress or a Congressional committee over the radio - guess it was radio at the time - and said that the Bureau was simply unresponsive to the needs of business, because it tested those additives and found them worthless.

TROPP:

[Laughter].

CURTISS:

Although it sounds funny now, it wasn't funny then.

TROPP:

I know, we have similar things still going on, so it really isn't funny.

CURTISS:

And so it was decided, I guess, on high, up in the Commerce Department, to dismember the Bureau, so at this time - the Institute wasn't the only victim, far from it - it was decided that the Institute would be given to UCLA, so that was where the Institute was absorbed. It became a laboratory of UCLA. The Corona laboratories were given to one of the parts of the Department of Defense, I forget which, I think the Navy. The proximity fuse activity and all the electronics activity north of Van Ness Street in fact the entire – yeah, I guess all of the activity north of Van Ness Street which included some materials testing too, was handed over to the Army, to the Army arm of the Defense establishment...

TROPP:

This is north of Van Ness - I think of San Francisco. Is that...?

CURTISS:

No, this was the street which divided the Bureau in two.

TROPP:

Oh, in its original location?

CURTISS:

Yes, the original... But that in particular was the electronics activity in which the SEAC had been built. So this was given to the Army as part of, oh, Army electronics laboratory or something like that. So that's how we lost the Institute; it was just...

The mathematics division there had been subject to a number of security hearings so that contributed to our black - that gave us a black mark and this was part of the problem certainly, but it wouldn't have helped even if the security problem had never been raised. It wouldn't have done any good because the Institute was just part of the dismemberment program, that was all, period. They weren't going to have things like, branches out in California allowed for the Bureau of Standards.

By this time, by the way, Astin has been in, you know, at least a year. Condon got out while the getting was good. He did not retire under a clout at all. He went to Director of Research at a large salary for Corning Glass Works, you may remember, and there however he did get into trouble because the Navy revoked his clearance - capriciously everyone believes; he was useless as their Director of Research for a while. He landed on his feet as a Professor of Mathematics at Washington - Professor of Physics at Washington University after a period in which he was unemployed and was advertising in Science as a consultant, not in physics matters. A little box in Science.

TROPP:

...go back and look at some of those early issues.

CURTISS:

Not early, this would be 1954 or sometime around there and now I believe he's a Professor at...he changed to Boulder, Colorado, which he always loved. Incidentally, he was instrumental in setting up the Radio Propagation - in removing the Radio Propagation Laboratory from ... that's, you know, that's our ionosphere and... you know what we do...

TROPP:

Right.

CURTISS:

...to Boulder, Colorado, where he got money for new buildings which they've been in ever since, close to the University of Colorado at Boulder.

TROPP:

Yeah, I was in...I think in that laboratory while it was still in Washington, where John Mauchly got his exposure back in the thirties. Or was its geomagnetism?

CURTISS:

I get the impression Mauchly might have been associated with the Naval Observatory.

TROPP:

That's right, it was the Naval...I'm sorry, I'm wrong. It was the Truesdell magnetism group at Naval Observatory. That's where his father had been associated.

CURTISS:

Oh yes. We didn't lose the Boulder laboratories fortunately. They're still part of the Bureau of Standards and they're ticking out their atomic clock on five wavelengths - four wavelengths on my short-wave radio.

Well, yes, the Applied Mathematics Laboratories, or, as we grandiosely called it, National Applied Mathematics Laboratories of the Bureau of Standards, were finally organized in, and appeared on the organization chart of the Bureau, in about 1948. Was it not '48 that we announced it in Science and so on?

TROPP:

There's an organization chart of that period.

CURTISS:

And the parts consisted then of the Institute for Numerical Analysis on the campus of UCLA, which we've talked about quite a bit before; and the Computation Laboratory in Washington, D.C., which was the old Mathematical Tables Project which had been moved from New York and which was under the direction of Milton Abramowitz, Dr. Milton Abramowitz, an extremely competent mathematician in computational science.

TROPP:

I think he had worked with Lowan on that Math Tables Project.

CURTISS:

Yes, he was Lowan's second-in-command most of the time and then Gertrude Blanch was

sort of a specialist - I believe we called her a consultant. And then Miss Irene Stegun should be mentioned there...

TROPP:

Spell her last name on the tape.

CURTISS:

That is spelled S-T-E-G-U-N. We also had as a consultant to the laboratory, although she would prefer to forget this, Mrs. Olga Tausky Todd. The Todds were brought from...we seduced them from England and brought them over here, not to work in the Institute for Numerical Analysis - though it was agreed that they would work there in the summer sessions - but they would be...well, it was first of all Jack Todd was made Director of the Computation Laboratory over Milton Abramowitz. I forget just how that occurred. It was later that Milton Abramowitz took over, but I guess we needed a Director. The original Director, Arnold Lowan, refused very vociferously ever to move to Washington, so rather than promote from within I believe we asked Jack Todd to be the Director, and his wife Olga had the post of Consultant to the Chief of the Division - that was me - on numerical analysis. So, the Todds were leading personalities on the Washington scene, although they tell people now that they always were associated with the Institute for Numerical Analysis. Later, much later, they were permanently, I believe, stationed out there, but I'm not sure whether that was not only in the last year or so of their work for the Bureau.

The third division - the third section in the mathematics division...was the Statistical Engineering Laboratory, so-called. This originated with just the appointment of Dr. Churchill Eisenhart, the son of a well-known Dean Eisenhart at Princeton. Churchill had gotten his Ph.D. at the University of London, studying with a well-known English statistician of the time, and was primarily of a scholarly bent rather than a research man; but he proved to be very useful to the Bureau and right now he's finally ended up not in any one section or division but as a ... one of the, well, the highly paid little group of consultants to the Director, who have sort of a free portfolio, do anything they want.

Churchill, when the Applied Mathematics Division was organized in a formal way, he was placed... he was assigned to be Chief of the Statistical Engineering Laboratory. I believe it's a title he selected himself - I mean the title of the laboratory is one that he selected himself and he soon managed to get some quite strong people in there. One of the most distinguished of his scientists was a mathematician named Dr. Lukacs. That's spelled L-u-ka-c-s with an accent somewhere towards the end - Hungarian –

TROPP:

Lukacs?

CURTISS:

That's right, a Hungarian-- who bought a house in nearby Maryland and became associated with the Bureau for many years. Then he moved over to Catholic University during the McCarthy era when foreigners in the Bureau were under some attack. He also well, there were a number of other quite competent people he brought in. The Statistical Engineering Laboratory was supposed to be...to play the advisory role to other sections of the Bureau - which I was supposed to play all by myself when Dr. Condon first brought me in, you saw. My role changed very rapidly, as I told you a number of times. So this type of advisory service devolved upon the Statistical Engineering Laboratory and they were very well liked. Eisenhart had a ...well he was a very affable person and very seldom lost his cool. It's quite difficult, you know, to do statistical - I mean to occupy a statistical advisory role to scientists who think they're rather distinguished, or engineers who have something of a reputation, and who are totally unable to formulate the problem that they're working on as a statistical problem. At the time that the Statistical Engineering Laboratory started, most of the scientists in the Bureau of Standards, and engineers, still had a very deterministic point of view towards presenting their results, you know. Well, after some five to seven years of work by the Statistical Laboratory, quietly and very gently collaborating and sort of socializing even outside of the Laboratory - outside of working hours with many of the more distinguished scientists in the Bureau whom the Bureau had totally adopted - I can say without any hesitation that even the old line - the tough old liners in the Bureau had totally adopted the statistical point of view as far as presenting their results.

TROPP:

It's fantastic.

CURTISS:

...[this section is not very clear]...you know, the psychologists have always gone overboard and are always running analyses of variance and covariance, as you know, and... on material which may or may not be probabilistically compatible, so, if anything, the Statistical Engineering Laboratory oversold itself and don't think...[not clear].

The first division - first section was this section we talked about earlier today several times, the Machine Development Laboratory. That was devoted to software. Dr. Cannon as Assistant Chief of the Division was also Section Chief of that all the times as long a I was there and one of his - I don't remember all of his more senior personnel, but Ida Rhodes always had been practically his right-hand person and that's about the story of the organization there and I think that's what you wanted to know about the Statistical Engineering Laboratory.

TROPP:

Right, and I'm going to thank you. With our background noise this is a good time - and your time commitment -- this is a good time to turn it off.

END OF INTERVIEW