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Interviewer: Henry Tropp
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TROPP:

This is May the 16th and I'm having a discussion with Dr. Gertrude Blanch at the Aspen Hotel in Washington, D.C. and, as you said, why don't we start with that beginning, when you first came to the Math Tables Project.

BLANCH:

Yes, Dr. Lowan had been commissioned to take charge of it. Now Dr. Lowan had done some interesting things in physics that C.E. Van Orstrand, who was in Washington, was interested in. If you remember, C.E. Van Orstrand was the one who had computed some of the basic functions like e^x and factorials to about 35 or 40 places. He was a geophysicist, I believe, but he was interested in those things, perhaps as a hobby; so when they were trying to choose someone to take charge of this Mathematical Tables Project that was coming into being he was asked and he recommended Lowan whom he had met some time ago; and Lowan, who was in New York at that time with Yeshiva University, accepted. So he was the one who was in charge of the Mathematical Tables Project.

Now Lowan was giving an evening course at Brooklyn College in relativity. I was a mathematician who at that particular time had just come back from Cornell with my degree. I had taught for one year, taking somebody's place. I didn't have a mathematical job but I had a position in a very beautiful office. They were manufacturing cameras for color photography. Since I didn't want to lose my knowledge of mathematics I decided to take Lowan's course in relativity. Well actually, of course you know how evening sessions are. Most students are very tired when they come there and they don't have time to do their homework. I had the time to, and what is more I had the basic knowledge; so of course when I handed in a paper it was a paper that was fairly well written. Lowan used to take the same bus home that I did, and one day on the bus he told me that he was very pleased with the sort of papers I handed in, even though I didn't attend every night, and I told him that I did have a Ph.D. in mathematics at that time. So that's how he knew that I was a mathematician and he told me the next night on the bus that he had been selected to head this Mathematical Tables Project and wouldn't I join it?

Now here I had a beautiful office, somewhere on East Twenty-Third Street, with paneled walls and all the rest of it. I was getting thirty dollars a week and —

TROPP:

That was a lot of money in those days:

BLANCH:

That was all I could expect - could hope for; and Lowan offered me this job in a loft on Columbus Avenue. You had to take a freight elevator to get up there - and then it was supposed to be under WPA and for me, who was interested in private enterprise, to take something in WPA, that seemed out of the question. But I came up there and I certainly wanted to get back to mathematics and I met three wonderful young people. Milton Abramowitz was there. There was a chap by the name of Kaufman who probably had a B.A. but perhaps not more, in mathematics, but he was one of those very unusual individuals who knew how to get things done and he was interested in tables. He had read a tremendous amount on it. He knew where to get what; and then there was another young man - I think his name was King - who was interested in becoming an actuary.

Now it wasn't easy to get a job in those days. The WPA was organized to give jobs to young people. A few technical positions were open to specialists. That was the category in which I was supposed to join them. In order to train those high school graduates who came who hadn't had mathematics in probably several years it was necessary to have a few mathematicians and Lowan was trying to get as many as he could. When I saw those three people I became enthusiastic too, and so I decided to resign from my job in my beautiful office and to join Lowan and go up the freight elevator on Columbus Avenue, and this is where we did the planning.

Now, you've probably read the introduction to the Table of e^x, the first table —

TROPP:

Right, that was the very first one.

BLANCH:

Now there they give you the history of how it happened. Lyman Briggs was Director of the Bureau of Standards and he had a committee. They met, and on the committee there was D.H. Lehmer.

TROPP:

Right, I know him.

BLANCH:

Well, they decided that the first thing to be worked on would probably be something that would be relatively easy to handle and they decided on e^x to many places, especially since that was needed. C.E. Van Orstrand had computed e^x to, I don't know, 35, 40 places. Ida has a copy of it; I have one home too; and so he was probably interested in a lot of decimal places and what is more, that was the area where much remained to be done. So we decided first of all to compute e^x, to make a table to - what was it? - 18 places and so on.

Now this young man Kaufman had read a lot of where things had been done. He read everything that had been done up to that time to many places and he knew enough mathematics to know what you had to do; and then, of course, there was Milton Abramowitz and Mr. King, and between them, I think, they did the most of the work in planning; how do you train high school graduates who - I will have to say that most of those were very bright high school graduates. That is, they didn't just take anybody. They took those who had done very well in their high schools in mathematics and they had to pass certain basic tests. Among them we found some very good material. Most of them were willing to learn, but we knew that we couldn't expect too much.

We had no machines. I think we had one old Monroe calculator which, of course, we kept among the so-called supervisors, those who were making the basic things from which the others would get a start; and of course the formula for e^x is fairly simple. All they had was pencil and paper and the thing was to devise a sheet so that a person could find his own mistakes if he made them. If you compute by hand you are bound to make mistakes no matter how careful you are. And so they designed a work sheet for e^x. Of course if $e^{x+h} = e^x + H$, then $e^x - H = e^{x-h}$. Thus if you subtract that H quantity from e^x, you should get a previously computed e^{x-h}. So they compared the two. They'd know if they really got back the answer e^{x-h} from what they had just added to e^{x+h}. In that way they could check them-selves at every stage. Altogether we had in the beginning, I think, about 130 people. Administrative officials found that we were using the staff to advantage.

Now in the beginning there were administrative problems. Of course I didn't have to cope with that; that was Dr. Lowan's sphere. I didn't do very much to help out with that phase of it. But he had plenty of irritations. They put in charge of the administrative staff people who— who needed favors, I suppose. They weren't very competent. Some were - they were trying their best, I suppose. So they wanted progress reports every week and so we had to give them progress reports and we were making the exponential function; it came out on the progress sheet we were doing the exponential function. Those were the things that we sometimes used to laugh over, the sort of the way things got around there. But strange — believe it or not, with all the difficulties we had, with an untrained staff, it's amazing what a great many pencils can do and within a year the book was finished and it was in the hands of the printer in 1939. I think that's the date of the first volume.

TROPP:

The - one of the things that Ida mentioned that you were involved in early, were these large sheets that were placed around the loft that enabled groups who were doing arithmetic operations to handle not only positive integral arithmetic but operations on negative numbers as well. Do you remember your — your —

BLANCH:

That I don't remember at all. Ida is much better at remembering those visual things than I am. I don't know how much influence that had. We — we had to do a little training but the youngsters had all had mathematics. The New York high schools were pretty good in those days and we had a few very competent youngsters, so that it was a matter of brushing up on what they already knew, teaching them that minus times minus is plus.

TROPP:

Well, I got the impression that you did this in a visual way, that you used different colors on your charts of addition and multiplication.

BLANCH:

That might have been there. Yes, they were using, if I remember - Ida remembers that much better than I do. We had them use a red pencil for negative values and a black pencil for positive values. In other words, when they had to subtract something, that something was in red pencil and when they had to add something, that value was in black pencil. I remember that.

TROPP:

If they had to multiply two positive or two negative or a positive and a negative number, they used different colored pencils depending —

BLANCH:

Yes, well of course what you can do by hand is some-thing that doesn't require too many digits and one of the things that the mathematicians did was to so plan the work sheet suitably. For example, consider what do you have to do to get e^{x+h} from e^x? Essentially you have to develop h over 2, h over 3, h over 4, etc., etc., so that at every stage they had division by small numbers or multiplications by no more than 2 or 3 or 5 or something else. Now this was why it was so successful. A person who had to write down 23 or 25 digits, and this is the number of digits that they worked with, couldn't very well multiply too many things by hand and one of the things the mathematicians did for them was to plan it so that at every stage the worker would have a fairly simple operation, dividing by 11, by 15, by 20, or something - a two-digit number at most, or something like that, and then putting down the result, so that he didn't have 35 digits to add all at one time. He had only a few at a time to add, the increments, so to speak.

TROPP:

Right, and the constant check —

BLANCH:

And he had a constant check. Now the key values, I think, were computed by hand. Of course we had some values to compare with. C.E. Van Orstrand had done to many places, but we relied on no one. Things were computed ab initio and the results were compared. For example, anybody could compute e^1, for example, or e^2 to 40 places without working too hard and it wouldn't take terribly long to do it. So that those basic key values were probably done by this young man whom I told you about, Mr. Kaufman. He was very good at those things. He was extremely accurate. As a matter of fact, he was the one who had done single handed, I believe, the first table that was published before e^x. It was called a Table of Powers and he had —

TROPP:

Powers of integral values?

BLANCH:

Powers of integers, 1 to 100, I believe, and he, after each sheet was done, he had it stenciled - and I don't think anybody has ever found an error in his work. His name was Mr. Kaufman; I think he's on the page of the very first value of e^x, and he helped in the planning of that volume. Eventually — about a year after I came there — he got a position with the Actuarial Bureau in Albany. That was a much better job, so he left us. The same thing happened to Mr. King a few months after that. Mr. King was also a very good mathematician. I think he had a M.A. in mathematics - very bright, quite brilliant. He did a lot of planning, not only for the first table of e^x but also for the table of the sine and cosine integrals which we were looking into at that stage. We hadn't started yet, but we were planning it and he had a great deal to do with that, reading up on it and experimenting. We had good direction from Dr. Lowan. I don't think he was a perfect person; no person is. He had his faults, but he gave us leeway. In other words, if a person was competent he could do what he wanted to do. We knew that if we wanted things done he would let us do them and if it was correct, it would be fine. So that we had the enthusiasm. Table-making in those days for me was exciting. I had gotten my degree in algebraic geometry at Cornell.

TROPP:

Whom did you study under at Cornell?

BLANCH:

Professor Snyder and Professor Hurwitz. Gillespie was there at the time. Carver was there.

TROPP:

Carver - I am sorry?

BLANCH:

Carver, C-a-r-v-e-r, a well known mathematician. When I came there he had not been doing much original work; he was a little older but he was a brilliant lecturer. Very easy to listen to a lecture of his.

TROPP:

I think Dieudonne at the moment is working on a history of algebraic geometry.

BLANCH:

Oh. Frankly, I wouldn't like to take an examination in algebraic geometry now. [Laughter]

So that I came with no background in numerical analysis and neither did the others have any back-ground in numerical analysis. We got more help from the British than we got from the Americans. D.H. Lehmer was very helpful; H.T. Davis also was. Now those were just about the only two who knew very much about computing. R.C. Archibald, of course, was a historian. Those three were extremely helpful. They were essentially at our beck and call every time we wanted help. But we got an amazing amount of help from the British. At that time the British school had emphasized numerical methods. There was Comrie, the astronomer, and, incidentally, if you want to know very much about table making, I think you still go to the astronomers even today. They computed the most accurate tables.

TROPP:

Did you have any contact with Wallace Eckert who was doing tabular computation at Columbia then?

BLANCH:

No, we didn't.

TROPP:

As he's, I think, one of the leading people in the computational field in astronomy. I think that was the period when he was doing his —

BLANCH:

You don't mean the Eckert who was with the IBM, do you?

TROPP:

Yes. Yes, Wallace Eckert. They gave him the Watson Laboratory.

BLANCH:

We met him. No, wasn't terribly helpful since he didn't need us; our fields in a sense didn't cross too much. In what he was doing, we couldn't be of help to him; I mean he didn't need our help. The people who came to us were those who needed our help. Philip Morse in MIT for example. He needed computations done, so he was extremely helpful.

TROPP:

What kind of computations, for example, was Morse interested in?

BLANCH:

Morse was interested, for example, Mathieu functions. I got started on Mathieu functions because Morse needed them and there were any number of small things and some special integrals that he came across with-in his field. He wanted them in a hurry usually. All he'd have to do was put in a telephone call or come down and give us the problem and we would do the computations.

TROPP:

When something of that sophisticated a nature came to you —

BLANCH:

That was, of course, much later —

TROPP:

It was done by —

BLANCH:

That was February of 1941. It was — at that time we had a much larger staff. Ida was there —

TROPP:

That would be done by one of the professionals rather than by the students.

BLANCH:

At that time we had a varied group. That was after the Bureau took over most of it. We had a much smaller group, but we had equipment. We even had IBM equipment in those days. That was during the war when some of our things that we had worked on were flown to Los Alamos because they were needed. Some material we happened to have had it in our files. … At that time we knew how to do things. We had expertise.

TROPP:

Let's go back to Comrie, because I'm most interested in Comrie's work.

BLANCH:

Comrie was extremely helpful. Every time he would come to the U.S., he visited us. I think on several occasions he came specially to see us. On one occasion he complained to Lowan, "Why did you compute such a simple function as sine x to 8 decimals with a four digit argument? Why isn't it enough to do it with two digits and so on?" And, well, we did it because we thought it would be useful. Three months after that Lowan got a letter from Comrie: "I must apologize. I had a project where I needed to do some computing with sine X and your table was — was just God's answer to what I needed."

TROPP:

[Laughter] I guess one of the things I'm interested in, and Ida and I talked about this, and that is the computational techniques. Mathematicians during the era we're talking about were not interested in computations —

BLANCH:

No, we got very little help outside of those mathematicians whom I have mentioned; maybe one or two others whom I should mention. Most of those who were interested in computing were in England.

TROPP:

And Comrie was —

BLANCH:

Comrie was one of them.

TROPP:

advising techniques to do computation when you didn't have massive equipment to help you, and I guess that's an area where if you could remember some of the computational ideas that he might have introduced or —

BLANCH:

Well, no, not formulas. Remember that the early tables that we made could be gotten essentially by power series development. .. A few, like the sine and cosine integrals, you needed the asymptotic expansions but those were the first tables in which anything outside of what you could do with pencil and paper were really needed. So that in the first two tables, the sines and cosines and the exponential functions, we had available values to verify what we had done, to make sure that our so-called key values were correct. We ourselves checked this to make sure that no error crept in that was systematic in nature.

But the computing experience yielded invaluable insights. Take the function e^x: the things that were necessary would be about the same in almost every other of that particular nature. You can determine a-priori how many places you would have to start with if you wanted to throw away those that you were not certain of, in order to end up with what you wanted to end up with. That is not a very difficult mathematical problem. Given a certain development that computers were going to use in a stepwise procedure, you knew theoretically how much you would lose in the process of developing from going, let us say, a hundred steps or a thousand steps or what have you. Well, those things were done by the mathematicians. We would determine the essentials. If we thought that we needed 15 places before we would finally cut off and make the manuscript, we would aim at least 2 or 3 more. You never tailor things too closely, and this is the first rule you learn when you start working in a computation laboratory.

TROPP:

Going back, say, to the exponential function table, to use a power expansion, say, the first 50 terms or 75 or whatever —

BLANCH:

Whatever it takes.

TROPP:

Whatever you had decided on, you had to use derivatives of that order of magnitude —

BLANCH:

Well, and the derivative of e^x is e^x, so that —

TROPP:

How did you in, say, going from that to the sine function or the cosine function or another power expansion of an algebraic function or transcendental function, you had to teach these people who had only limited mathematical training.

BLANCH:

Those people never knew - they only knew the formula they were supposed to use. In other words, they were computing - they had to know nothing much about mathematics. We had at that time - let us say we had a group of six or seven mathematicians, that's all we had. We would develop the formula and on the work sheet there would be A, A plus B, AB divided by 11, B divided by 13, etc., etc. —

TROPP:

I see. So they could be handling the fiftieth derivative of a particular function and not know —

BLANCH:

They wouldn't know what it is, but they would know what they had to do and the result would be there in every hundredth step. I don't remember whether we gave them the answer to compare against or whether we, as a precautionary measure, decided that every hundredth step we would compare ourselves. I don't remember, but I think that we trusted them with a great deal and we found only two people in the two years, the first two years when we were getting hundreds of people of all backgrounds, we found only two girls who even tried to cheat. You can't cheat in those things. It comes out almost immediately. Well, these girls were fired immediately.

TROPP:

Well, the — one of the things that Ida mentioned that you can perhaps verify is the fact that you were more concerned with accuracy than you were with volume of production.

BLANCH:

That's right, all the time. We did not press anybody and we didn't have a production schedule. We were not a factory in that sense. Of course the first couple of months were very difficult. You know how long it takes to get organized to do almost the simplest thing and here we had all those supervisors coming down, "What have you done so far?" They were worrying that we were wasting our money, so that the first few months were very hectic from the administrative viewpoint, as I remember it. But after the first year, after our first tables were published, things were different. I will say that we were very much surprised by the amount of appreciation that those whose word counted gave, especially those in England. They took the trouble to write letters to the administrators and to write letters where it counted to say how much they appreciated our work.

TROPP:

Whom besides Comrie can you cite in England that you felt played a role?

BLANCH:

Let's see. N.W. McLachlan, that was at the time I was working with the Mathieu function; Bickley in England; Sadler …

TROPP:

Bickley?

BLANCH:

Bickley. He's the one who became blind. He's done some very beautiful things.

TROPP:

B-i-c-k-l-e-y?

BLANCH:

B-i-c-k-l-e-y, yes; W.G. or G.W., one of the two.

TROPP:

Sadler's another name that I —

BLANCH:

Sadler is an astronomer. I think he's still there in Herstmonceux at the Royal Observatory if he's still alive. Those are the two that I remember. Now we had some contact with the National Physical Laboratory. That was a little bit later, I think. I met Goodwin when I was in England, but that was many years later. W.J. Olver had [been] there at the time, but that too was much later. Womersley once came over.

TROPP:

Did Turing ever visit your project during the war?

BLANCH:

No. No, I never met him and I don't think he was interested. Hamming of Bell Telephone in New Jersey came down once to visit us but that was about - after Ida was there and we had our reputation by that time. Schonberg came down from the University of Pennsylvania to talk to Lowan and he was very much interested in some of the mathematical things that we were handling at that time.

TROPP:

One of the questions I guess that without records is hard to determine is, after you had the first few tables constructed and either published or on the way to publication, what were the main demands that determined the kinds of tables you did in successive years?

BLANCH:

Uh-huh. Well, let's see now, the sine and cosine integrals, of course, were a natural outgrowth of the sines and cosines and those were the things that had been planned at the very first meeting by D.H. Lehmer and some of his other colleagues who were mentioned in that Introduction - Archibald, Lehmer. So that we, in a sense, followed that particular plan; and then Mathieu functions came as a direct result of the interest of Philip Morse at MIT; and during the war years we cut down our table making to almost zero. What we did was only the things that we had been working on and we were working on some very special things that - short computations that would take a week or two weeks or something like that.

TROPP:

Special projects for a particular branch.

BLANCH:

Special projects for particular groups. Some of them were very complex. Some of them were not easy. I remember one that involved a function whose derivative became infinite within the range of interest. One of the physicists - I think it was a very famous one - had differentiated in an unacceptable fashion, and was puzzled by the out-come. In other words, he forgot that the function becomes infinite in the region. He was getting something; he was getting a positive answer and he didn't see why we didn't get that positive answer. Willy Horenstein (one of our mathematicians) was handling that project at the time, so Willy Horenstein wrote the letter and pointed out that when there is an infinite derivative, direct differentiation in the ordinary way was invalid. That cleared up the case.

TROPP:

The theorems just don't work in that region. Right.

BLANCH:

And he pointed out why it is that you will necessarily have a negative region on one side and a positive region on the other and the two will eventually cancel one another.

TROPP:

That — that's an interesting aspect of computation that I remember running into, oh, it must have been 15 or 20 years now, and that's the techniques for dealing with non-convergent series which may be convergent or treated in a convergent manner over a short region; and there are very nice mathematical techniques designed for handling that.

BLANCH:

Oh yes, asymptotic expansions for example.

TROPP:

And they're almost, at least at that time they were hard to find in the literature. There are only one or two sources I remember being able to go to to find out how to treat these and some of the early programs I remember at places like, oh, the big aircraft companies for example, where they were doing large computations on the very early beginnings.

BLANCH:

And they had very little time.

TROPP:

They ran into all kinds of problems because of dealing with - trying to apply conventional methods to non - to divergent series and in regions where they went all over the place.
BLANCH:

Yes. Well, of course, I knew nothing about Mathieu functions when we started, but you learn when you have to do things and one of the best ways of learning something about a mathematical function is to try to compute it.

TROPP:

Some of the things that Ida suggested that I ask you about you've already covered to some extent, but I'll ask you some of the others. She said that you kept a journal of the various requests for work at the Math Tables Project. Do you still have that journal? Do you have any idea where it might be?

BLANCH:

I don't even remember. I threw out all the papers after several years. I had quite a bit of an accumulation of this, that, and the other thing; I don't have a thing now.

TROPP:

That's unfortunate. I'm sorry that you didn't save them. Do you know where some of the correspondence might be?

[Interruption on a loudspeaker] BLANCH:

I'm sure it isn't in existence any more. It was probably thrown out long ago. No. I don't know whether there was any journal. Well, for example, if we would get letters from a prominent mathematician saying that this would be of some interest, naturally we kept a file of those things and we would get around to eventually computing if money was available. Now, during the war years our project was very handsomely supported. We were not getting munificent salaries, but they were comparable to what others were getting in the field. We had a beautiful office downtown overlooking the East River. It was one of those very narrow buildings right opposite City Hall, or one block away at 150 Nassau Street on the nineteenth floor. It was a gorgeous building - well, cold in the winter and all the rest of it, but by and large it was very nice. When they had a strike we had to walk up nineteen floors. But we did that too - that was one Sunday, Schonberg was visiting in New York and we all climbed nineteen floors to the office to talk mathematics.

TROPP:

In the — in the early period when you first got started, how were the finances handled?

BLANCH:

Well, we were paid by the WPA. We got WPA checks.

TROPP:

Directly from some central administrative office?

BLANCH:

Yes, yes. We were assigned to an approved project, which was sponsored by Briggs. He was the technical sponsor of the project. He would come down once a week or once a month to make sure that things were going on. He'd have a conference with Lowan. He's an awfully nice person, by the way - and any difficulties that Lowan had Briggs would try to straighten out. There would be difficulties that Lowan couldn't resolve; very often a word from Briggs went a long way, and so we got those things we needed most. It was very difficult to get jobs in those days, so people stayed for a long time, and some of those stayed with us till the very end. The very best we kept. The Bureau took over a staff of about 20 or 25 computers in addition to a technical staff of about 10, I believe.

TROPP:

Were you involved when the Math Tables Project was finally moved to the Bureau here in Washington?

BLANCH:

Yes, at that time I was asked to go to the West Coast to the Institute for Numerical Analysis to start the laboratory there and I was tickled pink to go. I had never been to the West Coast, so for me California was a revelation. Ida went to Washington as you know. Lowan refused to go along. He had a job with Yeshiva University. He had ties here that he didn't want to break and he and Curtiss were not on the very best terms for one reason or another.

Now I stayed in California up to the McCarthy era when the Institute folded. I got a job with the Electrodata Corporation in Pasadena. I had done some work on Mathieu functions for someone at Wright Field - Henry Fettis.

TROPP:

Would you spell that name for me?

BLANCH:

Henry Fettis, F-e-t-t-i-s. He's — he's still at Wright Patterson Air Force Base; he just retired. He has done some very nice things in table making, by the way.

TROPP:

I should talk to him.

BLANCH:

Yes, I think you'd find it rewarding. Of course, on Mathieu functions I was probably the only one who had any experience with them, and I helped Fettis out in some things that had given him a lot of trouble. He couldn't get anybody to help him with [it]. So apparently when they realized that I might be available, the one in charge of the mathematics group at Wright-Patterson Air Force Base was Dr. Millsaps, a real Southern gentleman who got his degree with Von Karman in mathematics. He succeeded in getting under his wing a very famous thermodynamicist who had come from Germany, Guderley. I think he's one of the greatest thermodynamicists alive.

TROPP:

You'll have to spell that name for me.

BLANCH:

Carl Gottfried Guderley, G-u-d-e-r-l-e-y. He's done some very fine things. He always said he was not a mathematician, but for someone who was not a mathematician he knew more mathematics than I know of.

TROPP:

Pretty hard to do theoretical thermodynamics with-out an incredible amount of mathematics.

BLANCH:

Yes, yes. Well, so Millsaps tried to get me to come to Wright Field. Every time he came to California he would invite me to have dinner with him. He would come with Karl Pohlhausen, who used to be head of the Siemens Laboratory in Germany. Millsaps could get almost anybody he wanted to. How he did it, I don't know.

TROPP:

Pohlhausen?

BLANCH:

Pohlhausen. That is, Dr. Millsaps could. He had that Southern charm or what have you, and you never — you rarely said no to him. He's now either in Florida or somewhere in Colorado. Anyhow, I realized that the firm I was with would not be in business very long before it would be sold to some-body, and Millsaps had asked me three times. I didn't want to leave California, but then I decided maybe I'd better, and I took the job. In many ways it was the best thing I ever did.

TROPP:

While you were with Electrodata, had they built the Electrodata computer?

BLANCH:

They were building a magnetic - no, they didn't have magnetic cores in those days.

TROPP:

No, no, I think that was a drum machine.

BLANCH:

Yes, yes, a drum machine, but it was very reliable and we were doing some of the things that they asked us to do in connection with that machine. Brock, Paul Brock was there. You talked to him? He's in Bennington, Vermont.

TROPP:

No.

BLANCH:

He was in charge of that; a very nice boss to work for, but I realized that there was no future for me, and six months after I left, the whole outfit was sold to the Burroughs Corporation. In a sense, I left at the right time and I was never happier anywhere - I was never as happy in any other place as I was at Wright Field. I had complete freedom to do exactly what I wanted the way I wanted to do it and, I think, my best work was done there.

TROPP:

Do you want to describe some of that work? It would be interesting because that's — we're talking about roughly 1953, '5—4.

BLANCH:

Well now that's — 1954. I came to Wright Field in 1954 and I retired in 1967.

TROPP:

And what are some of the more interesting things that you remember doing there?

BLANCH:

Primarily I was working on Mathieu functions. We did Mathieu functions in a complex plane and it is published. We didn't do very much routine computing there. Guderley used to have some very interesting special problems in connection with some thermodynamic theory. We'd help with those. People from here, there, and other laboratories at Wright Field would come for advice, and if we had the knowledge we were very glad to share it. We would have lectures on modern mathematics and so on. We all participated in them. It was a college atmosphere. They had a graduate institute - oh, what do they call it - they've changed the name a dozen times maybe. They teach aerodynamics primarily to those officers who needed training. Admission isn't open to anybody except the military, but some of us used to teach there, subjects like higher mathematics. Recently they've opened Wright State University on Wright-Patterson grounds. That is, the Air Force donated the ground.

TROPP:

That's very recent.

BLANCH:

Very recent. The Air Force gave several acres or whatever it is. In addition, the University of Dayton was another institution of learning. In the summer [Loudspeaker interruption] One of the things that I had originated was summer programs. During the summer we would have jobs for about six people. That is, in our small laboratory that I was in charge of; and we would invite interested mathematicians to come and spend the summer with us and do as they liked; and some of the most interesting things were the things they were doing. There were lectures, and if they had any special things that required computing, I would try to help out. They had a very strong statistical group and still have at Wright Field. In statistics you have to know a lot of mathematics. Dr. Harter would ask my help on one or two occasions and I would help him out in the hard spots.

TROPP:

I'd like to go back and talk about the Institute for Numerical Analysis, because it fits right in with some other studies that the project is involved in. When you went to Los Angeles, who was the Director of the Institute at that time?

BLANCH:

The first Director, I think, was Hartree of England.

TROPP:

Right. Well, was Hartree there when you arrived?

BLANCH:

He was there - I think so. In other words, let's see I think it was Hartree. He had been invited to be there for six months or so. Now, after him let's see, who was it? It was Rosser, I think, came. Lehmer was there for a time. Now whether Lehmer was there before Rosser or not, I don't remember any more.

TROPP:

Yeah. I've talked to Lehmer about this period.

BLANCH:

You spoke to Lehmer, yes.

TROPP:

And I guess one of the things I'm interested in is, as I look back at the — the short life of the Institute, is that they, in different years, had different areas of concentration, and much of that emphasis in different time periods depended on the person who was in charge, and as a result attracted people of similar interests. Do you remember how the decisions were made as to "what we're going to be concentrating on during this next period?"

BLANCH:

I don't think that it worked this way. One of the nice things about the Institute was there was no firm direction of concentration. That's why mathematics could be done there. You could have Paul Erdös there and it didn't make any difference that nobody else knew as much about number theory as he and Lehmer did. We could go our own way and do other things. For example, you had various people who were available. There was no attempt to sort of get them to do something for an essential output. It was recognized that mathematics is an individual thing, so the best things are done where you have no direction and I think it was — a great many very beautiful things went down the drain with the McCarthy era. Lanczos was there at the time. Of course, eventually the whole idea was taken over at the Madison, Wisconsin Center under Langer. He had learned some of the mistakes that the Institute made, or Condon made, by accepting Army money, Navy money, without having a foundation of its own. However, in many things, I think one of the most fertile periods for mathematical learning was the period of the Institute for Numerical Analysis. It was taken as a matter of principle, so to speak, that you do not direct mathematicians.

It is only in research and development when you're almost close to the end of something that you want to accomplish physically, that you have to have directions to get to a goal, but when it comes to mathematics, to inventive mathematics, the less direction you give, the better results you're going to have. If you're going to have anyone work under a certain head who is interested in — in getting everybody stretched in his direction, you're not going to have anything of very much value.

TROPP:

It's interesting that in terms of mathematical research, that remained the philosophy for a long time but is recently no longer that way.

BLANCH:

Well —

TROPP:

It's very difficult now to get research funds for non-goal or non- —

BLANCH:

Well, this is the tragedy of things, that research is being administered by technicians; it's being administered by accountants. This is the tragedy of our present situation, that things are decided by accountants and they're killing the goose that laid the golden eggs, so to speak. Inventive genius is a very peculiar thing and you may get it in spite of everything. So there are not too many people who will ever do anything to set the world afire. It may be that those few people are going to do what they're going to do, what God intended them to do, whether or not you give them any help; but that is hoping for too much. I think that the period of most fertile invention will be the period when you give encouragement and reward. You don't have to have three hundred thousand dollars or a million dollars to attract a mathematician.

TROPP:

But the fifties, I think, were a good example of that kind of environment.

BLANCH:

Yes, I think so.

TROPP:

Except for the short political period of McCarthy.

BLANCH:

In the McCarthy period we lost Lanczos, of course.

TROPP:

Lost a lot of people.

BLANCH:

Yes, we lost Erdös, and I think very few have done as much to fertilize American mathematics as Erdös has done.

TROPP:

There's another number theorist that I was thinking of who I think was close, maybe just personally, to Erdös, was Leo Moser. Was he involved in the Institute?

BLANCH:

No. I don't remember him. I don't think he ever came out to the Coast. I don't remember - maybe he was there for a meeting or so - I don't remember having seen him.

TROPP:

I'm going to turn this … [turns recorder off]

END OF SIDE I SIDE II BLANCH:

We had quite a number of .. computing specialists in the laboratory. We didn't have high-speed computers. The ENIAC - the SEAC wasn't available - no, no —

TROPP:

The SWAC.

BLANCH:

The SWAC wasn't available - the SWAC never worked anyway. Anyhow, it wasn't available then, so that we essentially had no high-speed machines. We had IBM equipment, CPC, and so on. We had people who knew how to use it - and desk calculators. Most of those computations that they wanted were perimental in nature, so in a sense if they had fast machines it would have been very nice, but not essential. What most of the staff were doing was experimental in nature, something that required a few hours' work or maybe a few days' work. Lanczos would have many such problems. Rosser would have very many. He was working on certain modifications of the gamma functions that he needed to a lot of places. Rosser always needed a lot of places, so we'd get those for him.

TROPP:

[Laughter]

BLANCH:

Well, those were the things that were done in the computing laboratory, something that anybody asked. We didn't ask how important it was. It would be done and we'd find time. There'd be somebody who would do it, whether it was a numerical integration or differentiation or — or what have you. We would plan it.

TROPP:

Did you have problems coming to you, say, from the surrounding air frame industry? Did you have problems, say, in wind tunnel data reduction? Or atomic energy problems?

BLANCH:

Those were too simple. Data reduction is too simple a thing. In other words, we were never bothered with those routine computations. In the first place we weren't set up for anything that was mass production and they had equipment they could do their data reduction. They had problems. For example, Cecil Hastings at Rand Corporation was doing his approximations in those times and he used to spend hours with Lanczos on .. Chebyshev polynomials, so that he would get help that way. That much I remember. We would get some very peculiar things to compute that very few people could handle. Some were statistical in nature, probability theory, that required a great deal of expertise in planning and they were not routine so that you had to nurse it as it was being done. In other words, a mathematician and someone who could compute. Lanczos used to handle a desk calculator very beautifully and still does. He always had one on his desk. He never learned how to program for a machine. Machines scared him, but he would use a desk calculator as well as I could.

TROPP:

[Laughter] I guess what I'm interested in, in talking to you about that whole West Coast environment while you were at the Institute of Numerical Analysis, you were essentially a national computational bureau, among other things, for that geographical area and - or is that an untrue statement?

BLANCH:

No. We were there to do the very special things that the pure mathematicians or whoever wanted anything that needed numerical analysis, we would be there to help them. We had the equipment; we had the personnel to do it. We were not there to do anything that required mass production for the aircraft industry. The aircraft industry would come to the lectures. We'd have one or two. Davis, I believe, one chap by the name of Davis was there and a few others.

TROPP:

Is this the Davis who's done a book on the lore of large numbers?

BLANCH:

I don't think so.

TROPP:

Another Davis.

BLANCH:

No, I think it's another Davis. He was working for one of the aircraft industries. They would come for seminars, anything was in their field and there'd be a lecture on matrices, for example, they were interested in, they would come to the lecture. Sometimes they'd get nothing out of it, but most of the time they'd get something.

TROPP:

You said Rademacher was there.

BLANCH:

Rademacher was there for one summer. Oh, you name it, anyone - Comrie came there - no, well, of course Hartree was there. Quite a number of English mathematicians came there. Some French mathematicians came. What's the name, oh, one of the great French — begins with a C … Wait a minute now, some Hungarian mathematicians too were there. I think the cream of the crop came at one time or another. Collatz of Germany came there once I believe, at least once and then of course Erdös spent, I think two summers there.

TROPP:

As you look back at the evolution of what was then called numerical analysis — now I think it has another term — how would you say that that short period at the Institute affected the development of this as a branch of mathematics?

BLANCH:

I don't think it did. They were building the SWAC at the time and they were interested in large-scale computing. That was the period in which numerical computations in a sense didn't progress; it was a sort of an interim period. Things were being planned for the large machines. You no longer thought in terms of a desk calculator, and anything that could be done with a desk calculator nobody would even be interested in. It was a period when things were planned for the computers that are going to be and naturally you can do things in a large computer that you couldn't dream of doing in hundreds of years with the sort of equipment we had in the beginning, so that — but the period there, as I remember it, was a period in which they came with various very specialized problems that frankly could not be handled by a large computer even if you had it. It needed a lot of experimenting, a lot of examination. If they'd had a large computer in addition, that would have been fine, something that I had at Wright Field at the time I left.

TROPP:

When you joined Wright Field in the early fifties, was there a computational center developed or were you in the process of setting one up?

BLANCH:

Well, of course they had a magnetic drum machine that was built by General Electric and never worked; so that when I came, one of the first things I did, or well, maybe others had started it but I complained out loud that that machine was no good, and I hurt a couple of administrators - I ruffled some feathers. It didn't take long and then we got a much better machine and then the head of the laboratory changed. We got somebody who knew something about computing after that and we had a very nice setup at Wright Field by the time I left. I wasn't in charge of it. I was working with Dr. Guderley's group. This was one of the nice things about it; anything I wanted there was no question of how much it cost. I never saw a bill in my life.

TROPP:

[Laughter]

BLANCH:

In other words, if I wanted it, it was done and the financial part of it would be settled somewhere in some office. We were not timed that you had so much to spend. We had a computer there. Of course, if they had some problems that had military value, it would be a day or two and we would wait. A mathematician is not that much in a hurry. But there was no question any time — of course I didn't need very much time except for the things I wanted. There were a few others there who were doing experimental physics, I know, and I wouldn't vouch for the value of what they've done - it was valuable to them, but I don't think it was valuable to the world as a whole. It probably educated them, what they were doing, taking reams and reams of computer output. Nobody ever questioned it.

Somebody hired that person and you had a reason for hiring them. For a military establishment - for any university it was research oriented all the way. Of course it was one of the few research laboratories that the Air Force had. It had the one at Wright Field, the one in Cambridge, and the one at Holoman Air Force Base, but if they were supported in that fashion, I can't think of any better arrangement. It was geared for research. Those people at the head were given authority to do the things that they knew how to do and most of the time the military head of the organization had a Ph.D. in one of the fields like physics.

TROPP:

Well, one of the interesting questions in the early period, before machines became so common, and this is, I think, a rather nebulous direction - and that is: I wonder what the impact might have been of people like yourself who had computational needs on the design or the structure or the ability of the computers that were being designed.

BLANCH:

We were not consulted. If you ask me or ask a person like Ida, we'll tell you a dozen things that a mathematician would want in a computer that they don't do. Computers are built for industry. The mathematician, I would say in one of the books I am writing - I could name 25 things that I would want in a computer that would be very simple to put in, which would be of tremendous value to a mathematician, but nobody builds that. Nobody's consulted. I wasn't consulted when they built a computer! So the mathematician makes use of what he has. Somebody upstairs who's an accountant will decide what machine he wants. We have no voice.

TROPP:

I was thinking primarily of an earlier period, and I had raised this question at one time with Alston Householder, and he is really kind of thinking about it, so I really don't have an answer yet. It's clear that the computer had an impact on numerical analysis in terms of what it enabled you to do, is a very simple answer. I guess I'm curious about the reverse impact and you seem to indicate that it was very little.

BLANCH:

We made suggestions but our suggestions were very seldom wanted. For example, one of the things - if for example you want to conserve as much round-off error as possible. In any computer that you have nowadays you have one register that can accept the result of A times B. [Interruption by loudspeaker] If you want to compute A times B plus C you must transfer to another place in the memory, then you have to bring it back and add C to it, so you have doubly rounded. Now if they build a computer with - if you want to build up, let us say, a matrix of order a thousand. How many more decimal digits would you need at the end to take care of all possible carry-over, assuming that the numbers are the same size? You've allowed for double precision; allow 3 more decimal digits, 8 more binary digits. Any engineer can build that. Did they do it? Oh no. Did anybody suggest it? I suggested it. Nobody listened to me.

TROPP:

I think some of the early machines had the ability to do double precision but they weren't dealing with those kinds of problems.

BLANCH:

Oh no, they all do double precision. In other words, if you add A times B the whole thing would be extended. Of course the crazy 360 - no mathematician wants it. Now the CDC is a pretty good machine, mathematically, but of course I understand they don't have the software to go with it, but what the IBM has is the software, but the 360 is not a mathematician's machine. None of the things they built is for mathematicians. The 7094 wasn't so bad; you can do quite a bit with the 7094, but nothing is built for the mathematician. They built things for the census bureaus; they built things for industry. Industry doesn't need the sophisticated things that a mathematician needs and they wouldn't take a mathematician's advice - why should they? It would cost money [break in tape]

TROPP:

Plus the fact that mathematicians probably as a group are among your smallest users.

BLANCH:

Yes, that's right, so the mathematician uses the things and he's grateful for what he gets. Without them it would be much worse.

TROPP:

You mentioned that you're writing some books. Do you want to talk a little bit about what —

BLANCH:

No, no, it may never be published. It may never be finished.

TROPP:

Oh, that's right, but I'm interested in what you're interested in writing about.

BLANCH:

I'm writing a book on numerical analysis that is not intended for college freshmen. It is intended for somebody who has had a lot of computing experience and has wondered sometimes what things are important in numerical processes and what are not. It is partly theoretical. If it's ever finished maybe a few people like yourself might be interested in reading it. I won't have too many customers. Maybe Academic Press will publish it. They asked me twice and unfortunately I haven't finished it yet. At the rate I'm going it'll take another five years. There's nothing soon so

TROPP:

Well, in terms of some of the things that are important in computation, could you characterize a few of the major aspects that you would consider critical, as you look at numerical analysis and the problems of computation.

BLANCH:

Well, the fact that beautiful theory doesn't always carry over, that you're always plagued with the fact that you have finite resources and every time you think of what you can do with N terms you have to remember that in addition to that there will be rounding errors and that you can't afford to neglect them. There's a way of knowing what it is. It isn't enough to have — for me it isn't enough to have statistical upper bounds. I want an upper bound on an error. It is true for nine purposes out of ten the statistical upper bound will be good enough and if you're going to use the results for statistical purposes they certainly are good enough and should be, because if you have an occasional error and you know its probability, that's enough. And Tukey, I think, at one meeting that I attended some several years ago, he was proposing that very thing. He was resentful of the fact that the mathematician is taking too much time to get out a perfect table that he can guarantee is as error-free as you can make it. He says a statistician doesn't need it and doesn't care for it.

Well, of course, my answer is, that's fine. I think he has a point. If you're going to say in the introduction, no matter what it is, that this is good to a probability of ten percent or five percent or what have you, fine, so long as you say so. But if you're going to give a thing and the reader will assume that it is as perfect as you can make it and it's only a preliminary manuscript, then, of course, you've told a lie. In other words, there's nothing holy about having things accurate to any given precision, so long as you say so. If you say so, "This is a preliminary manuscript done on this computer which usually has been very reliable. This is output from the machine and that's all we have done to it," that's fine, so long as it's written in bold type —

TROPP:

[not clear]

BLANCH:

— so long as it's given in bold type so the reader knows what he's getting, I think it's perfectly fine. It would be foolish to waste too much effort on a thing that isn't needed accurately, and for a statistical purpose very often that would be enough.

TROPP:

What you're suggesting, then, is in the search for some upper bound, whether it's the least or — or just some kind of an upper bound, that you're then able to say that given this upper bound whether I have only three-digit accuracy, that is absolutely precise.

BLANCH:

Yes. In other words, there's no such thing as any-thing being error free. In other words, it has sometimes happened that a type font fell out, for example, and tables that were otherwise accurate and given to the printer accurately came out inaccurate to that extent, but such an occurrence is so very rare. Most of the time if there is such an error you can spot it before publication, and, all things being human, it is possible that there would be some errors sometime, but nobody has yet found an error in any of the British places. I think somebody found one in the very last place in one of the tables.

TROPP:

How about in the tables of the Math Tables Project?

BLANCH:

Very few errors found, almost none.

TROPP:

I was going to ask you about your checking procedure when you talked about that earlier, because not only did you have checks built in for the people doing computations, but those of you who were supervisors, as I understand it, at the end of the day checked the things that were turned in; and then, of course, you had to prepare manuscript and eventually had to see these through publication. How did you - what kind of procedures did you use to —

BLANCH:

Well now, for a function like e^x, differencing is your best method. That doesn't catch a systematic error, but those were very carefully allowed for. Now in typing you wouldn't have a systematic error, so those — those errors would come out in differencing very well and in one of the manuscripts that we made well, the coefficients for the Mathieu function for example. They're not singular; every coefficient is an entire function, but they grow like e^{nx} or in some such fashion, so that if you want to difference them you can't. Well, we found from theoretical studies that if you multiplied them through by certain constants and then differenced them, they should be smooth. This is what we did. In other words, we put in almost as much work in checking the final manuscript as it did to compute the coefficients themselves.

TROPP:

That's interesting. You did a smoothing operation on the manuscript itself.

BLANCH:

Yes, and then wait to see that what we had on the manuscript - in other words - and by the way we didn't have the machine output that you have now. It was actually typed by a typist, so that we had to be doubly careful. Now if you have something in a modern computer the probability of it having a typing error is very small, but when we had an ordinary typist, an accurate typist, but somebody who does make mistakes once in a while, whether she made a transposition of the digits or something else was very probable. So we did have to devise things of that sort. Now that is one thing, for example, I remember took quite a bit of study and I determined the sort of factor you would need to get a smooth function. So what we did was to take the complete manuscript, the typed manuscript that was going to go to the printer; we multiplied every number through by the constants that could be pre-determined; and the resulting thing, if the typing was correct, should have differenced very well. And I think we did spot one error in a value and we changed that.

TROPP:

Still talking about the Math Tables Project, you mentioned a number of people who were involved during the early years and some who were involved later on. Who were some of the people that stand out in your mind as the project grew larger and became more extensive?

BLANCH:

Well now, for example the early ones, those who were mentioned on the Tables' Introduction probably, there would be Jack Caderman, Herbert Salzer. In the very early stages before Ida came was a woman by the name of Mrs.Perseley.

TROPP:

How do you spell that?

BLANCH:

P-e-r-s-e-l-e-y. She needed - she didn't stay with us too long, I think for about a year, and then her husband got a job and she didn't need to work any more. She had a B.A. in mathematics, not more, but she was very competent. Now on the West Coast, and by the way, she might be worthwhile talking to - there was Rosalind Siegel. She got married and she's now Mrs. Lipkis.

TROPP:

Can you spell her last name?

BLANCH:

L-i-p-k-i-s. She's now working for Ramo-Wooldridge there and she's in charge of the computing outfit out there. A very competent person. I think she has an M.A. but that's all, but for someone with only an M.A. she has profound knowledge of numerical processes. Now she was my right hand on the West Coast. She came at the time I came there to start the laboratory and she remained there after I left, I think, for a little while

TROPP:

That's a name I hadn't had before.

BLANCH:

Rosalind Lipkis. She now lives in Los Angeles. She's working for Ramo-Wooldridge. Now what part of it I don't know, but I can give you her address; I'd be glad to. So you write to her and she can give you a lot of information about what they're doing now at the aircraft industry in the way of mathematical computations.

TROPP:

Did you ever have an opportunity to go to England and visit any of the British table-making or computation groups?

BLANCH:

I think I - that was around 1953, I think, I went to one of the conference and I saw the Physical Laboratory, what they called the National Physical Laboratory. I met Goodwin, I met Olver there. I met Fox and then — of course, Sadler was there at Herstmonceux. I took a trip to Herstmonceux to the Observatory - a beautiful little city there, village maybe, very charming. I met J.C. Miller at Cambridge. He came over to the United States on several occasions and he visited with the project. He's still at Cambridge.

TROPP:

Did you ever have any contact during the war years with people like Norbert Wiener or Von Neumann?

BLANCH:

Von Neumann, yes. Not I directly; Rosser did. Von Neumann came down for several weeks or days and he was there quite often on the West Coast. Norbert Wiener, no. .. Maybe he came too as an invited lecturer once. I don't remember him, but - I knew Norbert Wiener, of course, fairly well. He was a Cornell graduate or else he taught there.

TROPP:

I think he taught there after [inaudible].

BLANCH:

And at one time there was a dinner in honor of Professor Snyder and Norbert Wiener was at the same table with me. He was the one vegetarian at the entire table. But he wasn't particularly interested in the computing area.

TROPP:

While you were still in New York, did you have any contact with, say, the people with Courant and his group?

BLANCH:

Who?

TROPP:

Courant.

BLANCH:

Courant, yes. Yes. Courant used to come down quite often and we used to - we had quite a bit of contact by the way. Professor Isaacson at NYU is a graduate of the Mathematical Tables Project. He was with the project for a little while.

TROPP:

That's another new name.

BLANCH:

Eugene Isaacson.

TROPP:

I'll get all of these addresses after we're through with the tape.

BLANCH:

Yes. He was — he was with the project at the time when Lanczos was visited with us.

TROPP:

Well, Lanczos visited you while you were in New York and then later he joined the staff at the Institute.

BLANCH:

In California. Before he went to Ireland he was at the Bureau in California.

TROPP:

I've just seen a copy of that, that nice little paper that you did for his Festschrift.

BLANCH:

Well, I didn't see it. Of course, mostly Ida wrote it, because we wrote it together. She is a little bit more effusive than I am, so when I toned it down she wasn't terribly happy about it.

TROPP:

I think she left it that way because she needed you to temper her effusiveness.

BLANCH:

She - of course she writes beautifully, and Ida's the best speaker that I know of. You can — I remember her coming to give a lecture once on the West Coast, on computers and I think she had the audience rolling down the aisles; and you should have heard the whistles after she finished.

TROPP:

I can believe it.

BLANCH:

No, she was absolutely - well, she has a very good sense of humor and it comes to her very quickly. Well, she's a very unusual woman. If there are saints on earth she's one of them. Saints may be difficult to live with but —

TROPP:

It's nice to have a few around.

BLANCH:

It's nice to have a few around, yes.

TROPP:

Thank you.

[End of Interview]