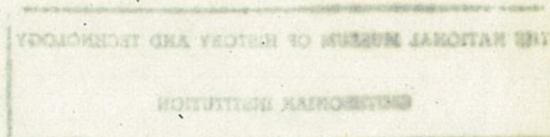


Interview with Forman Acton
Conducted by Richard Mertz
21 January 1971



RM: This is the interview with Professor Forman Acton of the Electrical Engineering Department of Princeton University, conducted on the 21 st of January 1971, in his office at the University.

Professor Acton, would you care to describe your early training and background, and what influences led you to computer technology?

FA: Well, I did my undergraduate work here at Princeton in the Class of 1943 as a Chemical Engineer. Then of course, the war was on and I was drafted. I served at Oak Ridge in Tennessee, using my chemical engineering, I'm glad to say. This was after a rather uncomfortable time in infantry basic. I was never good at bayonet drill. We had a sergeant who was very fond of people who had been to college, which he hadn't, and so I was the squad leader. It was a pretty grim experience all around, trying to show people how to do things that I didn't want to do. But I got down to Oak Ridge and I had a very interesting year there.

I realized that what I wanted to do was not really chemical engineering as it was then known. I didn't really want to get two cents more an hour out of a pot for Poppa Dupont. I was interested in theory and it was clear that there was a good bit of mathematics around that was not being communicated to engineers.

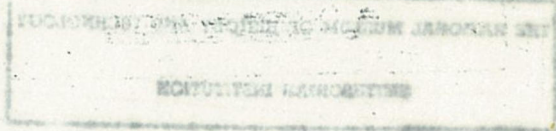
It was hidden in pure mathematics departments. The only pure math department I knew, of course, was Princeton, and that was very pure, even though it housed Sam Wilks and John Tukey in statistics, and I had taken some courses from them. I had even worked briefly in a chemical engineering statistical military-type project, called Analytic Research Group, or something like that.

Anyhow, I realized that I wanted to do what might be applied mathematics, if we can use the term rather more broadly than it was used by the British in the Nineteenth Century, where it was limited pretty much to hydromechanics and things like that.

The question arose that now that I had the GI Bill available, where should I go for graduate school. Well, I was back at Princeton and I asked Al Tucker who was then running the math department here where I could study math that had something to do with the real world, instead of doing crossword puzzles the way his group did. He suggested that I really ought to work with a mathematician named John L. Synge, who had a rather more famous uncle, John Millington Synge, who wrote "Playboy of the Western World", and things like that.

Synge was Chairman of the Math Department at Ohio State and I went directly to Ohio State to study graduate mathematics under him in January of 1946, having just gotten out of the Army. That was the

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period when everybody was coming back and I think the Ohio State population of students went from 15,000 to 20,000 over the Christmas break. I actually spent a fair amount of my time teaching calculus and some time catching up on undergraduate mathematics I had not known. As a chemical engineer, I had all the way through sophomore calculus and nothing else.

RM: You did differential equations?

FA: Yes, I had ordinary differential equations, but not as a mathematician would study. It was sort of 57 tricks for analytically solving differential equations no engineer ever sees again. It was certainly nothing numerical.

Synge wasn't happy at Ohio State and neither was I. At the end of that year he moved to Carnegie Tech as Chairman of the Math Department there, with a license to set up applied math as he knew it and I happily went along with him.

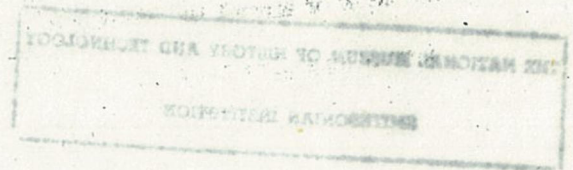
He rapidly assembled a group of about eight people. Albert Heins was one of them and Duffin, who I guess is still there, was another. There were a group of people from Brown -- plasticity, elasticity types. Synge himself taught theoretical courses in aeromechanics, aerodynamics really, relativity, and tensor calculus. It was just what I wanted, and I was very happy.

But after two years he received a call to his native Dublin where he hadn't been for I guess twenty years, to be the Professor of Mathematics at the Institute for Advanced Studies, where he still is.

So I actually did my thesis under Heins in the general area of Weiner-Hopf Integral Equations. It was not a particularly distinguished thesis, but it did get me my union card. In the course of these three years at Carnegie Tech, I realized that the mathematics that was useful for solving engineering problems was a first step. The engineers could formulate their problems, usually as partial differential equations more often than not. Mathematicians could sometimes solve these equations but more often than not they couldn't. Numerical methods was the answer, but numerical methods were being frustrated by the fact that they were very tedious. There were projects around for building computing machines, none of which had quite worked. This period, you see, when I was at Carnegie Tech, was the spring of 1946 to the spring of 1949, when I got my Doctorate.

One of the job offers that I had in the spring of 1949 was to join the Bureau of Standards at their West Coast UCLA establishment, the Institute for Numerical Analysis. The whole math division of the Bureau of Standards was under John Curtiss at that time. The West Coast group was under the leadership of J. Barkley Rosser, a

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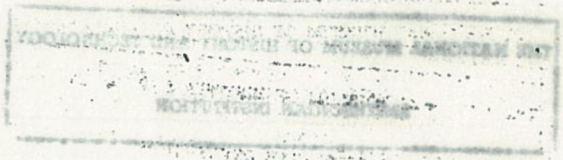


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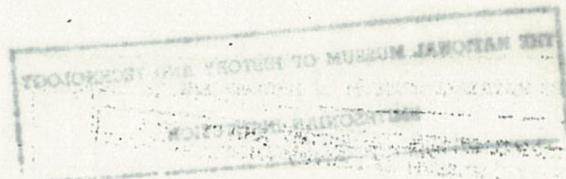
man with rather considerable experience in application of mathematics during the War. He had done rocket work and things like that.

RM: Did you know any of the people at the Bureau of Standards prior to that?

FA: I knew Churchill Eisenhart who was head of the statistics division, and still is I guess, because of course, his father was Chairman of the Math Department here and had given me my first job. As a Chem Engineer, I was in his office one day. Chem Engineering is a 5-year program here at Princeton and in my 5th year I was technically a grad student, and Dean Eisenhart was Dean of the Graduate School as well as Chairman of Math, and Tucker was actually running the Math Department.

I was in Eisenhart's office one day on a grad student matter, and not being awfully diplomatic, I asked him when he was going to start teaching math so engineers could understand it, now that he only had really engineers left in the University. This was during the War. He sat back in his chair and puffed on his pipe and smiled and said: "Well, you seem to have some ideas about it. Why don't you try it?" I said: "All right, I'll try it." He sat up and the smile disappeared. He said: "All right, you're an instructor in the Math Department beginning next semester. See Mr. Tucker for your

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assignments."

Well, next semester was two weeks away. I had never lectured in front of a class in my life and I went out of the office with my knees shaking. That's how I got into math. That's how I got into most things, sticking my neck out at inappropriate times.

RM: Were you roughly a contemporary to his son?

FA: Churchill is a little older than I am, though I would put his date by not much. I was born in 1920, but I can't account for him. He certainly is older. I didn't know him except through his father.

John Curtiss was Al Tucker's brother-in-law and I had met him through Tucker. I suspect that that's the way that I got the job offer at the Bureau of Standards, though I really don't know.

In any case, I was offered a job as a numerical analyst, something that really didn't exist at that time. There was a group being assembled out there at the Institute for Numerical Analysis whose general charge was to worry about how to use computers, once they started working, which they hadn't yet. Harry Huskey was building the SWAC machine out there.

I was there for three years. The Director the first year, as I said, was Barkley Rosser, and the second year was Fritz John,

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from NYU, and the third year was Derrick Lehmer from Berkeley, temporarily fired because of loyalty oath problems. We had a number of refugees from the University of California Loyalty Oath.

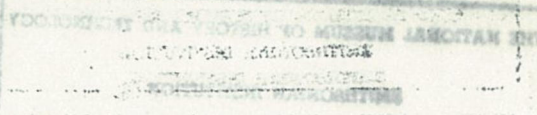
RM: You might comment on where some of these individuals are today.

FA: I honestly don't know. I think Fritz John is still at NYU, but I don't know, as I have lost touch. He was rather pure. Barkley Rosser is recovering from a gall bladder operation at his home in Madison, Wisconsin, as of a week ago. I don't encounter Derrick Lehmer very often, but I think he's in the Berkeley area. He and his wife are both sort of number theoretic types. Derrick is capable of doing very amazing things with computers too. He is the sort of fellow who uses the computer to find bigger and bigger prime numbers by very clever tricks. That was about the only thing you could do with the early computer, of course, because it had 128 words of memory for data and the program, and it didn't even have divide command. Obviously, you had problems.

RM: Was there any particular reason for the fairly frequent turn-over in personnel there?

FA: I think simply the business that these people had permanent posts from which they were on year's leaves of absences. I think the first two were that case. Barkley Rosser was at Cornell and Fritz John

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was at NYU.

RM: So they took these sort of as interim jobs.

FA: I think so. As I say, Curtiss was really running the thing. He spent six months of the year out there.

I had an office. On one side of me George Forsythe had an office and on the other side, Wolfgang Wasow had an office. Wolfgang is now at Wisconsin. Then I think about the second year I was there, Wasow was away somewhere, and W.E. Milne and his wife moved in. The first book on numerical calculus that was published in 1949 by Princeton Press was by Milne. I say first book, of course Gauss published in the field, but you know what I mean. This was the first book that was addressed primarily to a person who wanted to get answers on a desk calculator in the middle of the Twentieth Century.

RM: Was this the primary source of the work done the first period you were there? Associated with hand calculator devices rather than with a ...

FA: Well there were a number of ideas floating around, but I would say that since nobody really had a computer working, this was as I say, the fall of 1949, and nothing really began working until December 1949 or January 1950, and what was "working" anyhow?

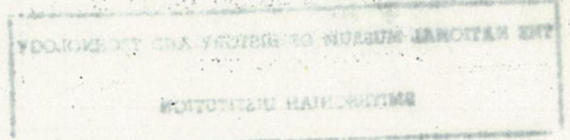
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You talk to a man about when his computer was going to be working, and he would say: "Two months from now." You'd go there in two months, and he's say: "Oh, two months from now." It got to be known as the von Neumann constant.

RM: The JOHNNIAC was up there at Mitre, wasn't it?

FA: It was being built, but wasn't as far along as the SWAC. The SWAC, I think you can honestly say, began working just about the same time as the von Neumann machine at Princeton. It's very hard to say when a machine works, simply because for a long time our machine would add and subtract and store very well, but it wouldn't multiply. There are a number of calculations you can do with just adding and subtracting and storing and we did them, so it was working. But not all the commands in the machine were implemented and of course, as soon as you implemented them all, you had ambitions to add a little more equipment, and add some more commands, more memory, more storage, something. Then the machine would go down for anywhere from one to three months while you added these things and retuned everything.

RM: Roughly, when was it that you were initially concerned with adapting problems in numerical analysis to the SWAC machine, i.e., in anticipation of its operation? You might wish to comment on the type of categories of problems.



FA: I don't think that there was any, what you might call, systematic program outside of the area of prime numbers, which was also Barkley Rosser's interest. It was quite clear this was about the only thing that you could do usefully with the machine for some time. There was certainly no directed research. It was a free research group and we had a sort of stable nucleus that was there for the three years that I was there. During the summer we had fairly extensive programs, and lots of people were brought in, and each summer there was sort of some theme that tied it together. There were Stochastic processes one summer with Marc Kac and Dick Feynmann, the physicist, one of the smartest pupils I ever had. (Don't tell him I said that.) The only thing I ever taught him was how to program the SWAC, but I only had to give the lecture once. I gave it three times for everybody else.

I got involved in a couple of things at about that time. We had a woman out there named Dr. Gertrude Blanche. She may still be around the Bureau for all I know, though she is probably retired, or close to it. She was in charge of a handful of, I guess ten girls, who worked on desk calculators and did service computation for the Bureau and for us, and checked things, and computed some systematic tables. She was a product of the WPA Math Tables Project

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in New York under Lowan, etc. As you probably know, that group developed a great esprit de corps that valued extreme care in getting the right answer in the 10th decimal place. It's a rather specialized, aesthetic drive, but that group seemed to have developed it to a remarkable degree and with very useful results.

Well Gertrude got herself into the position of teaching a numerical analysis course for two semesters at UCLA. One of the deals was that although we worked for the Bureau of Standards, we would teach such a course for free if UCLA wanted it, which of course, they did. Gertrude was in her second year I think, of teaching it, and the crisis arose back in Washington. She got through the first semester and she had to go back to Washington for the second semester. About three weeks before the second semester, I was told that I could pick up and teach the second semester of the course, not having even taken the first semester.

RM: When was this?

FA: I honestly can't tell you. I suspect that it was January 1950 . It might have been January 1951, but I don't really think so. In any case, I taught it the rest of the time I was out there.

RM: Was this a course primarily for engineers?

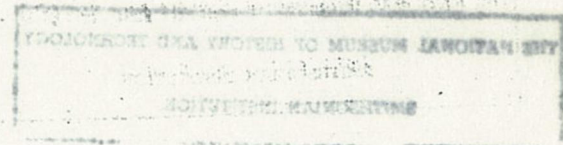
FA: It was numerical methods and out of Milne's book. I learned

that book in an awful hurry and did almost nothing the rest of that year, except work out problems and find out how these things went. I'm not a person who can simply read a theory and feel confident that it works. I am mainly in the numerical methods game.

It was a large class of 60, and they were sort of a sea of faces. I only remember a couple of people, but there was one fairly bright aggressive argumenative chap who sat near the back, named Curtis Benster, and he was a student of Russian, but he wandered into this because he wanted to find out what it was all about. He was the type that gives you trouble even from the back of a class of 60. Nonetheless he did reasonably well in the course, and some years later, you'll find that the book by Kantorovich and Krylov, "The Approximate Methods of Higher Analysis", is translated by Curtis Benster. So I claim him as one of my first successes, though it was a reluctant one on my part at the time.

I consider myself primarily a teacher, and I'm not awfully active in research work. I pick up other people's research and pass it on to people who can use it, translate it, and make it into something applicable more often than I originate.

The next thing I did for this first summer program, summer of 1950, was that Curtiss asked me to pick up a couple of graduate



students that I thought looked as if they might profit from a summer internship type of thing. I went around and found we had money for about three. I looked around and didn't have much luck getting three but I did get two. One of them was a chap at Harvard that was already working at their computer center, named Kenneth Iverson. He's the author of the APL language now, and that's what he's best known for. The other was Tom Kurtz who was a statistics graduate student here at Princeton. He's now Director of the Computer Center at Dartmouth, and the author of the language known as Basic, which pretty much came out of conversations with Tom and me over a number of years when he worked for me later here at Princeton. But that's not in the era that you're primarily concerned with.

I don't remember whether that was the summer that Kac and Feynmann and Stochastic processes were there, or whether that was the summer after, but I have the feeling it was that first summer, the summer of 1950. I could check these things with some difficulty, by going back and looking at the Bureau publications that came out. Reports were issued usually, and various people gave lectures, but just off hand, I'm very poor at sorting out what happened which year. I was only there the three years.

The machine began working a little bit better, but for my

purposes, I never computed anything useful on the SWAC for the three years that I was there. It was always being modified, and always had a rather small memory. They later added a drum, but no drum was working before I left.

We did have, however, a thing called a card program calculator--CPC. We had Model I, Number I. It had been really invented by a man at Northrop Aircraft, over IBM's dead body, sort of. He had to go to New York and get policy changed, allowing screwdrivers to be taken to their machine. Then so many people around L.A. began liking what he had done, they began pressuring IBM to make them, which they reluctantly did. We had that first one.

RM: Did the Northrop man have any involvement with that?

FA: No. IBM simply took his idea and unimproved it.

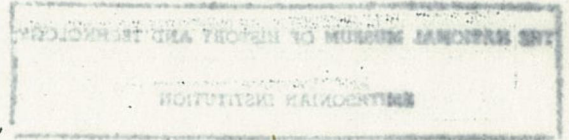
RM: I'm not sure I know to whom you are alluding.

FA: I'm not either. I would recognize his name if I heard it, but I have forgotten it.*

RM: I have heard it. So you got the machine at the Institute roughly in 1951?

FA: That was 1950, just about the same time that I was there,

* Everett Yowell, then of INA-NBS, would know all of this CPC history. -FA



or shortly after I arrived. Marc Kac and Wolfgang Wasow had cooked up a probabilistic way of solving ordinary differential equations, boundary value problems, to which they or somebody attached the name of Monte Carlo. This was sort of the birth of the Monte Carlo methods. I think I solved the first problem by Monte Carlo with the CPC. It worked but it was horribly inefficient, and I promptly lost my interest in Monte Carlo methods and I've never managed to recover it. There was a huge publicity afterwards. It was a catchy name and there were Monte Carlos in all sorts of things, most of which I felt were leading people down the garden path. Unless there was a natural probabilistic process at the heart of the problem, it seemed to be the wrong way to solve it. If there of course was a probabilistic process, then Monte Carlo was simply a way of modeling that process. That's natural and straightforward, and I'm all for that.

As I say, I'm interested in things that work. I'm an engineer who has been trained in some phases of mathematics, and I want to use these things. So other developments were coming along.

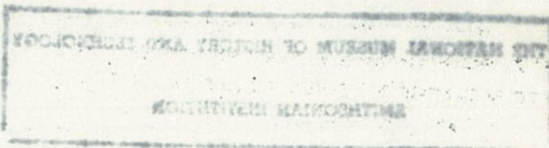
RM: Do you recall the specific problem to which you applied Monte Carlo?

FA: It was a second-order ordinary differential equation, which we constructed just for the purpose, because we could solve it

analytically. It had a probabilistic interpretation which is a random walk. I did it, and I don't think we even bothered to write it out. It wasn't all that interesting; it was obvious. I remember Wolfgang Wasow was the one that showed me the thing and so I went and tried it on the machine.

Magnus Hestenes was there in our group full time. I say full time, but that isn't right because he was at UCLA too. He got interested in finding eigenvalues of symmetric matrices, and various contests were run by Rosser. Rosser was very good at making up games with matrices.

Cornelius Lanczos was also on the staff, and he was the fellow who, more than anyone else for me, epitomized the type of thing that I like to do. He never proved a theorem in his life, as near as I can tell. He just said to give him a problem, and he'd think about it and say, "Now it looks to me as if the trouble is over here on this side of the graph, and we have a sort of singularity there, and we need a transformation to get rid of it." A few more waves of the hand, and you had a method outlined. You went and tried the method, and lo and behold, it worked. Nobody ever bothered to prove that it worked. In fact, Wasow told me a couple of years ago that it's almost axiomatic by now, that if you can prove that a method converges, it's so inefficient that you can't afford to use it. All the efficient



techniques diverge for so many cases, that you can't get any good proof that they should work. That's what it amounts to.

For reasons like this, I never was particularly interested in proving theorems. I was much more interested in trying to figure out whether something could be used or not, and Lanczos was interested in that too. He had worked for Boeing Aircraft and for Douglas Aircraft, and now he was at the Institute. Some years later, he went over to the Institute for Advanced Studies in Dublin, where he became Professor of Applied Mathematics, and provided he's still alive*, he's in that position. He had a severe heart attack about a year ago, and I never heard whether he's still alive or not.

RM: Did he know Synge?

FA: Oh, yes. Incidentally, there are only three professors at the Dublin Institute. The Professor of Physics was Schroedinger. It's a good little group, but of course Schroedinger is dead. Synge is the only Irishman of the three.

Let's see, what else was happening. Of course, McCarthy was coming to power-- Joe McCarthy that is. The California Loyalty Oaths were around and life in California was beginning to get distinctly unpleasant. Also,

* He is retired. His 80th birthday will be celebrated in 1973. - FA

just aesthetically, I had never really cared for Los Angeles. I'm an East Coaster from New Jersey. About that time, I got an offer to come back to Princeton to head up a weapons systems analysis group, at the Forrestal part of the Princeton campus, and I did so.

RM: Was this in 1951?

FA: This was 1952, August, 1952. I did that for four years. I naturally had some interest in numerical methods, and since there wasn't anybody teaching a numerical methods course around here, I soon found myself doing that.

RM: In the Math Department?

FA: Yes, the weapons systems group was in the Math Department. I came back in the Math Department at Princeton.

RM: Are purists allowed to the citadel?

FA: Al Tucker was Chairman at that time, and my group was out at Forrestal and provided about the only way that a mathematician or anybody else could learn weapons systems analysis, namely by getting in there and doing it. It was an instruction type operation. We were trying to create people who could do this kind of work and we had contracts with Bell Labs under the Defense Department, which allowed us to choose the job that we would take on. This made us quite attractive to lots of people in various places, and we would get all sorts

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of requests asking if our group could come and look at their particular troubles. About a third of these were good for instructional purposes, meaning I felt that we could actually accomplish something in three or four months, that type of time scale.

RM: How about WSEG and the Pentagon itself? I believe it was a group set up by Norris, prior to this, perhaps in 1949.

FA: We had a couple of contacts with them, but more often our contracts were right on the experimental level, the people who were actually conducting experiments on things.

RM: Then you were, in a sense, something of a competitor with groups like Johns Hopkins.

FA: Yes, very much so, except that our function was first educational. We were trying to produce weapons analysts or systems analysts really. We hoped we could make a contribution in a short length of time.

RM: In connection with that, wasn't it a bit unusual that a department that was as unapplied in its fundamental orientation and a department which would attract more or less graduate students interested in abstract students, how did you get people...

FA: How did it happen? Graduate students were no problem. There were a good number of graduate students and we were a source

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of support. Besides that, it was a chance for them to explore the world of applied mathematics without making a permanent commitment. It was a short term job, and they would join us for a year or two, on a part-time basis sometimes, but usually full-time.

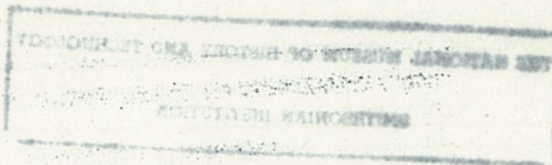
RM: Did they get any dissertation or graduate credit for this?

FA: No. This was usually just after they had gotten their doctorate. They had to be at least within one year of the doctorate, and more often than not they were one year past it. They were a very smart group because the Department had and still does have very good graduate students.

But the project came about because John Tukey and Sam Wilks were in statistics, and statistics was still in math at that time, you see. That's where the motivation came from, and Tukey of course, through his connections at Bell Labs, so there is an obvious connection there. That's how it all came about. It was something Tukey set up in the first place, and then wanted somebody to run because it was set up and he was off on something else. So that's how I got it.

RM: You had known him?

FA: Yes, I had worked with him briefly in this little similar project just before I got drafted. I worked with Merrill Flood, Al Tucker, Tukey, Charlie Winsor, now deceased, Marvin Thrall, George Brown,

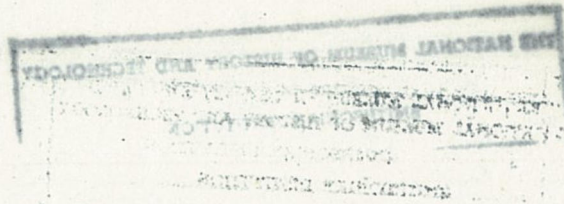


and some others. There were a number of people(8). The core in that group was inherently statistical, and later in my group out at Forrestal, there was a strong statistical component, but it was by no means the only measure. Bob Aumann was in it, and he has no statistics whatsoever. He's Chairman of the Math Department at Hebrew University in Jerusalem now. He was a very smart boy. We got into a problem that involved some information theory, and more or less as a side issue, he proved a small theorem. It was a problem that came to us from the Bell Labs group and so we went up there every once in a while. Every two or three weeks we'd go up and talk with them. I indicated that Bob had something he might like to talk about in information theory. So we went up there, and there were about fifteen people in a seminar room and Bob gave a nice little talk and there were a few questions. On the way back in the car, Bob said, "Who was that fellow in the back who kept asking most of the questions, the fellow with the long hair?" "That was Claude Shannon." "My God", he said, "why didn't you tell me?" I said, "If I had told you, you wouldn't have been able to talk."

RM: This was in 1953.

FA: Coming back to Princeton at this period, in conjunction with this group out at Forrestal, we had a Card Programmed Calculator.

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RM: Not Model I, Number I.

FA: No, that wasn't even the one out on the West Coast by that time. We got Model II, Number II out there by that time, which IBM had gotten back closer to the original design, and removed most of the so-called improvements that they had made. We did have one, but I was not instrumental in getting it. John Wheeler of the Physics Department was, but I took it over and ran it. It was the first computer center at the University. We ran it on a no-profit, no-loss basis, and the operation was definitely unstable. We tried every method of charging people that the comptroller, Ray Woodrow, and other people could devise, and none of them worked.

RM: Was this the first time that the community, general and academic, etc. had had one?

FA: Yes. Of course, there was the von Neumann machine, but it was being used by a very small in-group. Officially anybody was welcome; however, if you went out there and asked to use it they would give you the manual of the absolute instructions and say, "Here's how you use it." You would ask, "How do I compute a cosine of an angle?" And they'd say, "Oh, you write yourself a routine and use the series." You inquired, "Well, hasn't somebody already done it?" Well, they supposed there were some around. If you poked

about, you found there were five or six of them by this time. Of course they were all written in absolute machine language and you had already written some of your stuff in that same part of the memory. After all, there are only a thousand words available. These things were just unstable. They didn't have any standard input/output routines even that a customer could use. As a result, you had to be pretty dedicated and go there and sort of live in order to use their machine.

RM: Of course they did run some problems, and they were running some for the services on contract but I don't know how much.

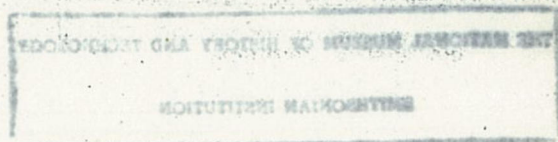
FA: It was all programmed right there to the best of my knowledge. There was another group, a meteorological group-- Jules Charney and his boys.

RM: There was no formal program or set of routines to which one could go, as far as I have been able to find.

FA: There was nothing that was useful to the outsider. This problem was beginning to bother some of the people at the von Neumann center. If I remember rightly, Irv Rabinowitz was there at that time. He lives in Princeton, incidentally. He's in computer sciences at Rutgers now. He was in India with Harry Huskey and me. In fact, I've got a note on the blackboard to call him about something when he gets home tonight.

So there were several people around there who were beginning to

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worry about what we now call assembly language or symbolic assemblers, which were not really symbolic, just relocatable. Numeric code but relocatable, and with a sub-routine structure so that you could put a 'bunch of these things together and I guess you'd call it a loader. It's just a loader. It picks up these sub-routines and stashes them away in memory, suitably relocating them so that they work in the places it actually puts them. But they weren't symbolic; they were all numeric for the very simple reason that they didn't have any symbolic input/output on the machine. They had base 16 numbers, just 6 letters and 10 digits, and that was it.

END OF SIDE I

FA: We were speaking of the Institute for Advanced Study, and as I say, it was an in-group with a few people beginning to worry about what people nowadays would call rudimentary software in the form of assembly programs. I think you'd actually call it a relocating loader. A couple of people had actually written them, but I never used any.

RM: Who did you deal with when you were out there? Was it Goldstine?

FA: I knew Goldstine, I knew Charney, Bigelow,... I knew most of the people out there, it wasn't that big a group. I didn't go there that much, and von Neumann himself was not around very much. I never knew him. That isn't entirely true. Do you know John Tukey?

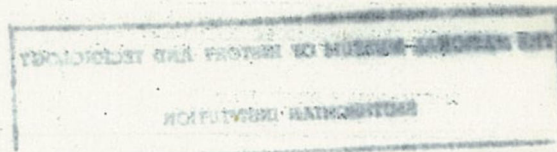
RM: Not personally, no.

FA: Is he a guy you're going to interview some time?

RM: Yes.

FA: I think it would be fun. John is a very, very rapid thinker; he's fast. He also fortunately has infinite patience, and so the proper technique for anybody with him, especially students, is to say, "Now wait a minute, John, I don't understand that. Go over it again slower, please." You can keep doing that and it doesn't bother him.

Well one day, I was with him and he had to go see von Neumann



and so I was tagging along and I sat quietly in the corner of the office for about three-quarters of an hour while Tukey sat there, his back to the wall, sweating and trying to keep up with von Neumann. It was great fun. I didn't understand what they were talking about, but that didn't make any difference.

RM: I don't think he ran any problems, did he?

FA: I don't think so. He of course had been instrumental and central in the development of the computer, because basically, he wanted to tackle higher turbulent motion problems, which he felt that a hundred years of analytic approaches had failed. He wanted to conduct a lot of numerical experiments in the hope that a pattern would emerge which would guide people into the fruitful analytical lines that ought to be followed. It's significant that we are just now getting computers that can carry out the numerical operations that von Neumann hoped he could do with the early generation of computers.

RM: It's been my impression, please correct me if this is erroneous, that the Institute for Advanced Study, at Princeton, was not only a rather unusual place to do something like the electronic computer project, but it wasn't necessarily one of the most hospitable climates in which to build such a machine. On the other hand, I should have thought that your group, by the early fifties, 1953 perhaps,

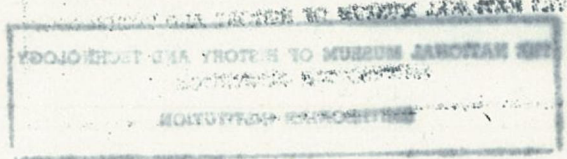


there would have been at the University, an increasingly aware group insofar as computer-related problems are concerned. Which group might have offered support to the Institute group?

FA: Well, out at the Institute, I can only guess at what the internal politics were, but I think there has been enough written in the public sector. I think it's well known that von Neumann got his project through, accepted by the Institute in the first place, only by getting himself an offer of a full professorship at the University of Chicago with permission to build it there. He sort of laid that on the table and said, "I'm going to Chicago unless you let me build the computer here," and that's how he got it.

But the Institute remained hostile, or at least disinterested and I think that lack of interest was massively reproduced among the pure mathematicians on the Princeton campus.

It was also quite clear from the description that I've already given, that the machine was not very useful to a wide group and I suspect a willingness to assume that it never would be or couldn't be for one reason or another, by people who had never really looked into the matter. In a way they were right. The machine was a first machine, and was not designed to be maintained. The only people that could maintain it were the people who had designed it and they were



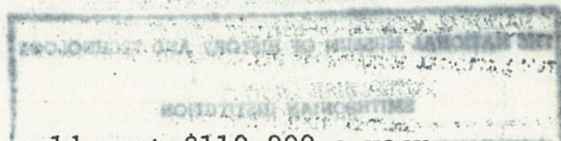
very high priced people. So it was a white elephant. It served its purpose by proving that you could build a computer and by sparking a lot of pseudo copies which in most cases were better engineered, and starting off the whole business of electronic computing at a fairly sophisticated and high speed level.

As you know, we're only getting small computers basically in the last five years. They started big and they got bigger, but the gap between the von Neumann machine and the desk calculator is only now being built.

RM: The von Neumann machine is a big ...

FA: It was big by anybody's standards in those days, though. After all, it filled a room this size and when you added the air conditioning equipment, it filled two rooms this size. It didn't work a lot of the time.

You raised the question about was the University sort of a hospitable place to take over when von Neumann was not an active force at the Institute any longer. The answer is the University did somewhat reluctantly accept title to the machine. Oppenheimer gave it to me. This is about the time that Goldstine went to IBM and he told Smythe, the guy really responsible, Harry Smythe, that the University could run it for \$20,000 a year. I told Harry that



he was being sold a bill of goods. It would cost \$110,000 a year. Smythe was too kind to say, "you're young and inexperienced, I don't believe you," but that was clearly his feeling about the matter. He accepted the machine and budgeted \$30,000 for it just in case Goldstine's \$20,000 had been a little too conservative. Instead of my \$110,000, it cost \$120,000 that year.

RM: This was in...

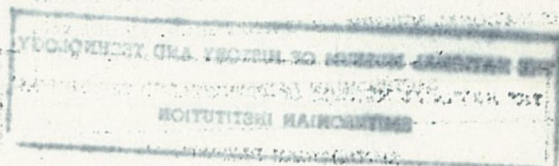
FA: Oh, golly, when was this? This is about 1954, around your cutoff date, or it might have been 1953. I had my CPC out at Forrestal and I was running my first class on that. Then I was beginning to beg and borrow, it might have been 1954, I guess, and steal computer time for my class where I could find it. RCA got an IBM-650 and I got my class in RCA Labs one day a week, one semester.

RM: Was Sarnoff...?

FA: Yes. For free. They were very nice considering all the security restrictions, etc. It was really quite nice of them.

RM: The 650 was a serviceable machine.

FA: Yes, it was good. Then Electronics Associates had a lab, they are closing it down now, out here on US 1 a little way. They had an Electrodatta-105, which was the nicest machine of that category that I ever encountered and I tried for a long time to get one at the



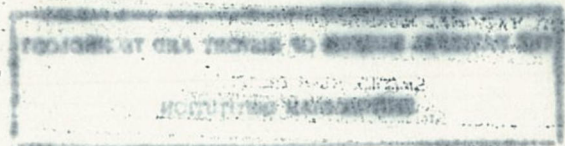
University. But the Burroughs Company, who had taken it over by that time, was somewhat unenlightened about university finances, and didn't see their way to making a reduced rate for us, so we never did get one, unfortunately.

But again, Burroughs Electrodata did help us to the point that they didn't charge Electronics Associates for the time we used on it. Electronics Associates let us in at night up there. Finally there was a guy named Hans Maehly. Do you know him? He's dead now, unfortunately.

RM: He was one of the authors of the final report of the IAS.

FA: Yes. He was here and I met him fairly early on, and he became convinced that something had to be done about the von Neumann machine. Entirely on his own and with no encouragement from anybody else that I could see, he wrote an interpretive routine which gave the von Neumann machine a very neat serviceable language that could be used by people without having to know the machine language at all. It was only for baby problems, because naturally it used up about half the memory with the service routines. Nonetheless it had input, it had output, and it had a beautifully logical structure that you could remember, because you didn't have to look up things all the time. I used that for two semesters with my class.

RM: This was in 1954 or 1955?



FA: This must have been later. Let's see. Ned Irons was in that class; he would have been a sophomore, I guess. He was class of 1958, so that would have made it 1956.

RM: Was this a formally...

FA: The machine was under our control.

RM: Turned over to the University?

FA: The machine was University property at that time.

RM: Even though it was in the EPC Building?

FA: There was no question of moving it.

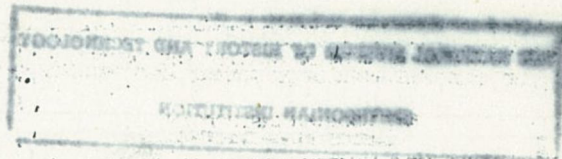
RM: That pertained to the Institute, one of the divisions.

FA: The building was the Institute's, the machine was made over and sold to the University for \$1. That was just a little legal matter, you see.

RM: Were there any on-going projects, like meterological work, using the machine at this time?

FA: No, meterology had disappeared about that time when Charney had an appointment at MIT. He took Norm with him, if I remember right. I don't remember Norm's last name* but he was a blond meterologist. Anyhow the meterology project folded when Charney left.

* Norman Phillips was the name -FA



RM: That was the bread and butter for the machine. Didn't ballistics tables problems...?

FA: I don't know where the support for the machine actually came from once the University acquired it. Thank goodness I wasn't saddled with that problem. Hans Maehly came with the machine and was added to the Math Department, although I don't think Fine Hall ever saw him. But that's all right.

RM: He was not academically figured.

FA: No, nothing like that. Nor was I at that time. Hans did a couple of pieces of research in numerical methods sorts of things, rather nice, before he departed for Syracuse, if I remember rightly. Then he went from there to Argonne Labs and he'd been at Argonne about two weeks when he died.

RM: Which housed one of the successive machines ...

FA: Right. Bill Miller was running the Argonne operation at that time. Bill later went to SLACK at Stanford and then became Vice-President in charge of Research and Development at Stanford. He was down in Chile with me ten days ago, and while he was there, got a cable making him Provost of Stanford, so he's moving right along.

RM: To get back to computers, who administered it when the Institute, in the form of its director, made the gift of this white

FA: We made Hans Meahly the guy that actually ran it.

RM: He ran it, administered it, etc., but this through the University.

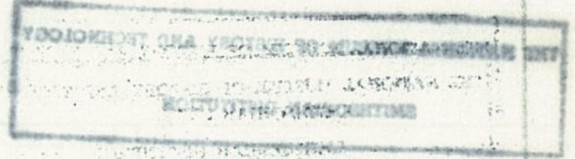
RM: He ran it, administered it, etc., but this through the
University.

FA: All he was doing was a housekeeping operation. I mean we

RM: There was no charge for the building, I take it, since it

FA: No, it was the Institute's. I have no idea how ... The detail

RM: Commuter service. What I had in mind here was your prediction



of the Math Department budget?

FA: I don't honestly know (but it seems very unlikely). Smythe had to fund it. He was the guy that made the decision to take it and he found money somewhere. It was also clear that the machine was not very serviceable and that it was a white elephant, because it was preventing us from getting any government money to acquire a good machine. They could say we had one.

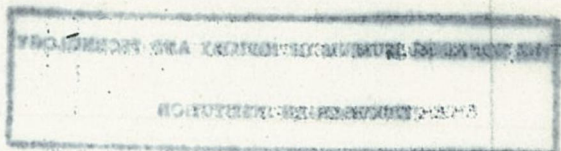
RM: Was any effort made to try to get customers to use this machine?

FA: No. The University has been very pure on selling services, probably the purist university I've ever encountered that way. They are very careful about tax structure.

RM: I meant in connection with such things as system evaluation, or related topics.

FA: The machine wasn't big enough, you see. We had a drum on it by that time but even so, the only language that was any good was this Maehly interpretive language and that gave you storage for about fifty numbers. What could you do? I mean, you could compute quite rapidly with it and it was great for student exercises and indeed, my students did quite well and they learned a lot. But no, it was not a practical machine. We ultimately let it lapse. It had periodic breakdowns.

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zero correlation between people with contract money and people with interesting ideas for computing. The people I wanted to get on didn't ever have any money. They were students or graduate students, and I was convinced that we could not run a computer center on the basis of charging for it. By this time I'd pretty well convinced the University administration, because I had gone along with all the suggestions they had made and we found out by experiment that they didn't work.

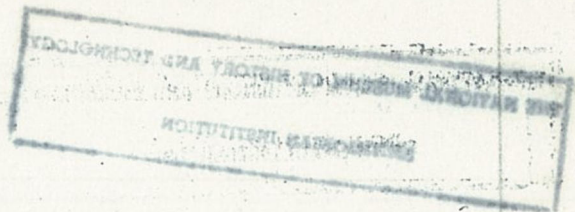
The typical difficulty is, of course, that you can't make a profit. Therefore you've got to spread the actual costs among the users, and that has to be done over a time period, like maybe a month. Well if you have a lot of users in a month, the cost to each of them is low. So the more you get into the month, the more who try to get in, and if it's quite clear there are not going to be any in a month, then they all stay away. It's just a totally unstable situation.

RM: Feast or famine.

FA: Yes. It's just completely unstable, and blows up on you. There are all sorts of other problems but that will serve. So the net result was that when the University finally did get a commercial computer, they were committed to a policy of putting it on overhead and running it like the library, and just letting people use it.

RM: This was the 650?

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FA: This was the 650 and this was our big machine until ten years ago.

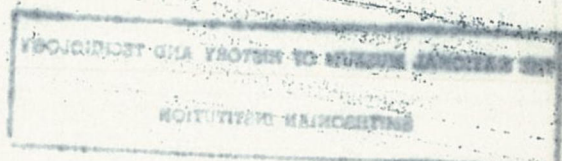
RM: When that was introduced, did you find yourself as sort of Mr. Computer for the University? Once the financial restrictions in terms of cost was removed, did you find that you were spread around or was there in effect an evangelical effort on your part to illustrate to some of the departments which might not otherwise see applications?

FA: We didn't do any massive missionary work. I taught students of course...

RM: Were they from all kinds of fields?

FA: Mostly engineers. It was a course in the Math Department but it was a numerical analysis course. Then by that time I was teaching an engineering analysis course and I worked that into the lab too, so I was teaching two courses that had this lab work. That was about all I can remember that were actually on it as a matter of university duty.

The other users were people who wandered in, found out about it and the machine was kept moderately busy. We did not have an operator, but we did have a girl there in the daytime. She was an operator, yes, but her real purpose was to schedule the machine. You signed up for the machine and you came in and you had been checked out previously



by her and you were licensed to turn it on and turn it off.

RM: How about programming, debugging and ...?

FA: We did almost all our work in the Bell interpretive system, which is a three address numeric system that was pretty good. The IBM language on the 650 was abominable. You've probably heard other people on the subject. In fact, I think it is safe to say that in any field that IBM has made a machine, somebody else has made a better one. IBM has the trouble that they design the machines by committee. They are not the only company that does it, but it is not the way to produce a work of art. The 650 is not a work of art.

RM: Did this apply equally well to the language of the machine?

FA: The language of the machine was just unusable by all but the most dedicated persons. Even their sort of symbolic assembler called SOAP was not often used at Princeton.

Now I had another ax to grind on the 650 because the IBM people who were thinking about designing it had come out to the West Coast while I was still out there with Huskey and Company and had asked: "If you were building a computer, what characteristics should it have?" This was before the SWAC was even working, or not working very well. We had been doing some thinking about it (we and large lots of people), and we said: "Well, we really don't know very much about it. After

all, our machine isn't working and we only know one thing, and that is that the memory should be no smaller than 4,000 words. We're really sure of that." So of course you know what they did. The 650 was designed with 1,000 words and as an optional extra, you could get 2,000. I think they built one 1,000 word machine and never sold that. They sold quite a few 2,000 word machines and just before it died, they came out with an optional 4,000 word machine. But I always held that against the IBM Company. They of course made a number of classic errors with that machine, like estimating that they could sell 20 of them but they would be real risky and build 50. I think they sold 2,000 of them. All of that is part of the old business of people estimating things by linear trends when in fact they are exponentials.

Well all of this is later material. I guess...

RM: One question about the composition. When was the first formal course given, involving the electronic computer?

FA: You mean at the equipment level of our computers, etc.? I honestly don't know, because I've never been connected with it. There were courses on the logic at UCLA given by Harry Huskey and his group.

RM: I'm referring to here at Princeton by you.

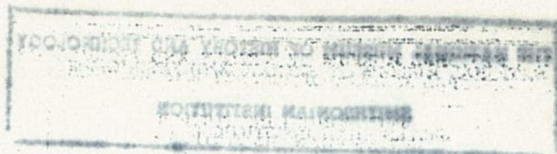
FA: Well, the first formal course that I gave... Well, let me see

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if perchance I can find out anything by looking at a very old notebook here in which I had grades. This is Engineering 414 in the spring of 1956. This was an engineering analysis course, but I don't think that used the computer. I certainly had been teaching a course before that, in numerical methods. I honestly don't know whether it was 1953 or 1954.

Of course I taught one at UCLA in the spring of 1950, but that's a long time ago. The Institute for Numerical Analysis, in its short life out there on the West Coast, was a very interesting place because everybody and his brother went through for varying reasons. All European mathematicians coming to the U.S. wanted an excuse to go to the West Coast and their excuse was usually to see our operation. Being a bachelor, it frequently fell to my lot to take them to dinner and to show them around. I met all sorts of people from everywhere.

I remember one Saturday afternoon when I had gone back up. It was summertime and I was in an old dirty Tee shirt and pair of cotton pants with one knee ripped out. It was air conditioned up in the office but I was about the only person around. It was about 3:00 in the afternoon and I got a phone call from Dean Boelter of the Engineering School. "Oh, Acton," he said, "I'm glad you're there. Sir Charles Darwin and a friend are on the way up to look at your machine and I said you'd probably be able to show them around."

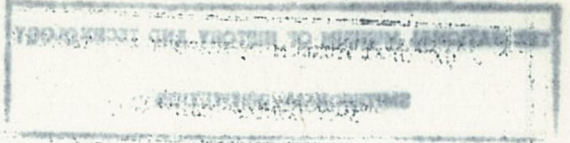


Sir Charles Darwin, in addition to being the grandson, was also the recently retired head of the National Physical Laboratory at Tedington, you know, the Bureau of Standards of England. Sir Charles and his friend showed up on schedule and I showed them around. Then it turned out that both John Curtiss and Barkley Rosser were there, if I remember rightly, so I passed him off to them, but just before that he inquired about getting back to Pasadena by bus. They had come over by bus. I assured them it's one thing to come over at 10:30 in the morning to Westwood by bus from Pasadena, but another thing to get back at 5:30 in the afternoon, even on a Saturday, and I'd be glad to drive them over. They really didn't want to bother me and all that.

So, while they were talking to my bosses, I went back to my room and got changed into something respectable and got back there just as they issued forth, and Sir Charles' eye lit upon me, and said: "Ah, good, then you will have dinner with us." I later saw him at Cambridge.

The only other thing was that on the way out, one of the electronics technicians from the Huskey machine was heard to remark: "Gee, he doesn't look a bit like an ape."

It was an interesting place. There were a lot of little things going on most of the time, and it was a pretty happy group. It's too



bad that the Eisenhower economies which nearly wrecked the Bureau of Standards at that time, as you may remember... The small battery additive flack that Allen Astin walked into. (My name being somewhat similar to his, I used to get his mail every once in a while. Astin and I got to know each other quite well before I left the Bureau.)

RM: To go to another phase in your own career, how did the transition from the Forrestal campus math department project, how was this affected...

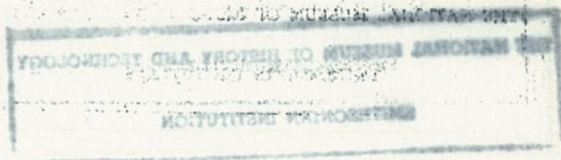
FA: You mean how did I get here?

RM: You then arrived on the scene here.

FA: Of course, I was a Princeton product of the engineering school, and my senior thesis was on conduction, convection and heat release in catalytic converters. I had as co-authors two assistant professors at the time, named Walter Johnson and Dick Wilhelm. By this time Walter Johnson was Chairman of Electrical Engineering and Dick Wilhelm was Chairman of Chemical Engineering. The former Chairman of Chemical Engineering, Elgin, was now Dean of Engineering. So obviously I knew people here. I pick very good co-authors, without any foresight whatsoever as a matter of fact.

I had worked into the business of not only running the group at Forrestal, but teaching courses on campus in the Math Department -- one

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in the Math Department and one in Electrical Engineering.

RM: These were primarily courses which attracted engineering students.

FA: Yes. The Math Department and the Engineering School got together and decided that since most of the customers were engineers, why not find Acton a place to sit in the Engineering School. Somebody averaged chemical engineering and mathematics and came up with electrical, so here I am in the Electrical Department. The answer, of course, is that Electrical Engineering used more math than any of the others, and my job was to be an internal applied mathematician consultant to the Engineering School essentially. The Electrical Department was hospitable enough to give me a home which I still occupy. I enjoyed it very much.

I sort of stepped out of the line of running computers once they became big business. When you've got a budget of one and a half million a year, that's not fun anymore unless you enjoy running things with budgets of one and a half million a year, which I don't. It's personnel problems, not math problems anymore.

I am still in the numerical methods game. I published a book last spring, which you may or may not have seen. I have to get a plug in here. Have you looked carefully at the cover?

So this is still my field of interest, and I suspect it will

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continue to be.

RM: Would you care to just briefly enumerate some of the people you have met in the course of the work you described? Certainly some of your colleagues at the Institute of Numerical Analysis, in the field of, say, weapons systems evaluation. Bill Morse was a product of Princeton.

FA: I think he was in the Princeton Physics Department at one time, but I encountered him only a couple of times. He tried to interest me in coming to MIT in the numerical methods game, at one point. I think Don Ling at Bell Labs is the name that really comes to mind. He is also known as Mr. NIKE in some circles, and he had a very definite interest in weapons systems analysis. I guess he's also still in that business and still at Bell Labs for all I know. I have been out of the game now since 1956. There were not many people around in it.

Rosser had been in it since the World War II period and I encountered him even at that time. He was down at the rocket testing grounds near Hagerstown, Maryland, and worked down there. This is artillery type rockets.

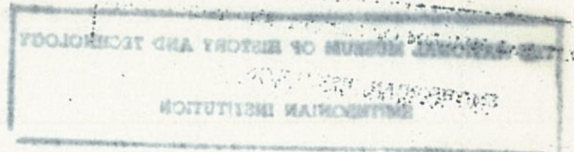
No names come to mind. John Tukey could fill you in much better than I can, so I suggested Tukey call you. You really ought to talk to him. He lives in Princeton.

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RM: Do you recall any who were students during this period?

FA: Into this work? I think it was pretty temporary at that time. I don't know whether Kurtz did some more or not. That isn't entirely true. A number of the people went on and did this kind of work in the military when they finally got around to their military service. They would get placed at places like Aberdeen Proving Ground and would get into work of this sort. Another chap was in the Navy. I can see him perfectly well and he's now teaching at Boston University, but his name escapes me. He worked on this sort of thing in the Navy.

There were students that went on and did this kind of thing. I think the most useful thing that comes out of it though, was that it introduced a lot of people to the fact that mathematics and mathematical type thinking especially, was useful in considering fairly complicated systems. There wasn't any specific discipline you could point to and say was necessary. What you needed was simply clear thought and experimentation. I think the notions that experimentation is an important part of it was sort of new to the pure mathematicians. You could formulate models of your system and they invariably had thirty-seven parameters and you then had to find out which three of these were crucial. The others, it usually turned out, didn't matter very much. When you're trying to optimize something or other, you get something

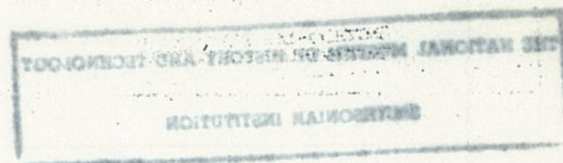


fairly nasty, like shooting somebody down effectively. What you did was, you worked out lots of cases for lots of different numerical values, and pretty soon you found out that most of these parameters didn't make much difference.

Right around this time, I got involved with a medical doctor named Seed. That phone call a few moments ago was from him. I'm still involved with him, and if you look over there in the corner, you'll see a small medical department there. This is the dispensary of the Electrical Engineering Department. John is down here one day a week and takes care of the faculty, as well as working on medical systems, and is giving a course in it this semester for the first time. He and I worked on a mathematical model for glucose metabolism in the human and it turned out to be quite successful. It successfully explained experiments by seventeen different experimenters who had thirteen different theories to explain their experiments, and they were lucky if they got within twenty percent of their data, and we came in within about three percent of their data all the way through. We sort of figure we knew what was going on and they didn't. I'm exaggerating a little bit, but that's the sort of thing that happens once in a while, and when it happens, you're very happy.

RM: Is he on the faculty here? Electrical Engineering?

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FA: He's a visiting research associate. We don't pay him but we give him computer time. We may be paying him for the course, I don't know. That's an administrative matter I don't have to worry about.

John and I work on various problems, medical type problems that come up where a computer is useful. His interests have broadened now and he wants to use the computer in office work-up in the poverty program in the Bronx, if he can, for medical records keeping.

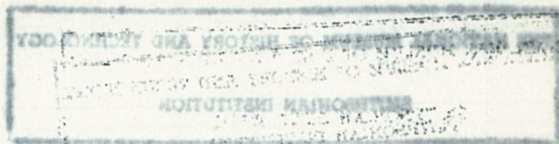
RM: Where does he commute from?

FA: He lives in Scarsdale, works in the Bronx, and comes down here once or twice a week. He's with one of these OEO programs, taking care of people at 172nd and 3rd Avenue. Everybody within five blocks, if they will leave their guns at the door, may come in and get free treatment.

RM: Have you been involved in any of the mathematical biophysical problems?

FA: Not personally, no. I guess Theo Pavlides in our Department here is actively working in that field right now, but he's out of town today. I'm pretty much a classical, how-do-you-solve-differential-equations-and-integrate-things. I work pretty much with continuous problems, continuous variables.

Ken Steiglitz upstairs has since gotten interested in discrete



problems, integer problems, network problems, traveling salesmen problems, the shortest distance through a network. Between the two of us, we sort of divide that kingdom in a friendly way.

RM: One final question. Do you happen to have a list of your publications?

FA: Sure enough. What do we have. I guess the book is the principal thing right now. The article with John Seed, and I have something marked biographical here, which I can give you.

END OF INTERVIEW

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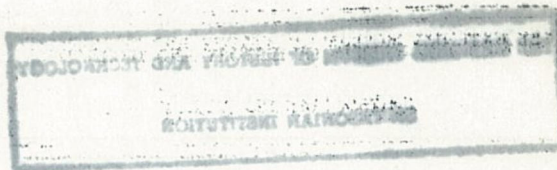
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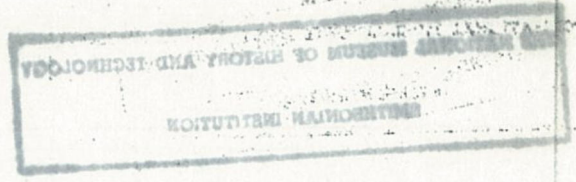
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