INTERVIEWEE: William F. Gunning

INTERVIEWERS: Robina Mapstone, H. Tropp

DATE: October 9, 1972

RM: The date is October 9 and we're talking to Bill Gunning at Palo Alto Research Center of Xerox. Maybe we could start off by you telling us a little bit about yourself and how you got into...

BG: How I got into it all?

RM: Yes.

BG: Well, I went to work at Douglas Aircraft in 1941, I think it was, and was working in the research laboratories there. The RAND Corporation evolved out of Douglas Aircraft. It was originally called Project RAND, as a part of Douglas Aircraft. It was while it was still Project RAND that we started making some computer like things, the first of which were some special analog computers. I remember we made an analog computer to solve for the temperature distribution in heating an airplane windshield. It was a real problem in keeping these things--windshields--so that they would melt the ice and still not get so soft that a bird would come through. This was

the kind of a problem that you couldn't solve except by making an electrical ...

HT: Dynamic heat transfer.

BG: Yes, it was a dynamic heat transfer problem that had to take care of the impact of flying into icing conditions and not get so hot when you flew out of the icing conditions to melt the plastic in the middle.

HT: Of course, at that time you weren't concerned with rapid acceleration and high altitude.

BG: No.

HT: That change of temperature problem.

BG: Yes. This was about the time pressurized planes were just coming in.

Well, anyway, there was that analog device and then Project RAND came along and they started looking at some things that were more digital. We made another special purpose machine in the late forties, that was essentially a statistical experimental machine, and it used a little gadget that would take a bunch of ball bearings and move them over a target. It was

John Williams' bombing investigation problem that we were trying to solve. The only way to do it was through many replications of an experiment which was done by having a hopper come over and fill this little carrier with ball bearings, and then you'd move it around to the selected XY position and push your foot to open the trap door. The ball bearings would fall through the trap door, where the trap door constituted the target. So this was really a digital computer if you want to think of it that way. We just counted the ball bearings each time they fell down.

HT: John Williams was the mathematician?

BG: Yes. He was the guy who was in charge of the Mathematics Department at RAND that spawned all of this.

HT: Just a very trivial question: Was RAND an acronym for something?

BG: Research and development.

HT: That's all.

BG: RAND later had to have access to all of the aerospace industries to do its job for the Air Force, and so it had to get out of Douglas and be a separate non-profit corporation. I don't know if that's relevant, but that's the history.

RM: No, I didn't know that. I knew about Project RAND, but I couldn't figure out when RAND and Project RAND came together.

BG: Yes. It had absolutely nothing to do with Remington Rand.

RM: Which is what people usually think.

BG: A lot of people make that confusion. Another thing we did for John Williams was to make a machine to generate a million random digits. I don't know if you've ever seen that.

HT: We were going to ask the question about that random number deck that RAND produced. Do you know what I'm talking about?

BG: Sure I do. A fellow by the name of Walter Franz and I did that machine at Douglas.

HT: Is that ANZ?

BG: Yes. He went to Boeing in the fifties.

HT: That's where I heard the name, the mathematical group there.

BG: Yes. Hell of a great guy. Tremendous. He also did the theoretical analysis of that windshield problem at Douglas before that.

The number generator used a random noise source. Just the noise in a resistor. It then counted the number of pulses for a finite amount of time, I think it was for the order of a second. It counted about a hundred thousand pulses and then saved only the last decimal digit. The counter would count modulo ten around, around, around, and wherever the last digit was it would punch that into an IBM card. It just would run and run and run. It was a lot harder to make random numbers than we thought.

HT: I was going to say, we're not even sure those are truly random.

BG: They weren't, it turned out. We found the reason. And this is still a problem in digital computer design today. If you get a series of pulses--after all noise is a Gaussian amplitude thing--you've got a threshold for triggering. Sometimes a pulse would just barely come up over and come back down. The ones that would just come up over and go back down again would cause the first flip-flop count. It had a preference for ones over zeros because it would flip one way more easily than it would flip the other way.

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That's why numbers had a bias of odds versus evens. But then they were able to massage the numbers and that all...

HT: It's almost impossible to get a true random number.

BG: That's right.

HT: Nice pseudo random generating function.

BG: Yes.

HT: Nice ones.

BG: Yes. But this one was one based on the so-called cosmic ray approach to random numbers, as opposed to the pseudo random, you know, squaring numbers and taking the middle or whatever.

HT: But lots of people thought they had ways of generating random numbers and they turned out not to be random. (laughter) Anyway, this random number deck of yours turned out to be a very usable device. In our discussion this morning with Greg Tobin, he described how they used it to get a kind of Brownian motion effect on a problem having to do with the shielding for an atomic pile. They then developed an oscilloscope essentially for watching these particles move around and it was your random number deck that they used to...

BG: That they used to drive it? Yes. Well, now, George Brown read a paper, I remember, on what they did to sanitize those numbers after we found this odd even

bias.

HT: That's a beautiful word. (laughter)

BG: Well, maybe unsanitary is better.

HT: That's great. (laughter)

BG: Yes, you should talk to George, by all means, because he was right in the thick of all of this. It was George Brown and John Williams and I that made the trip back East.

HT: Is John Williams still alive?

BG: No, he died quite some time ago. Oh, five or six years ago, I think.

RM: I think one of the funniest things is that memo he wrote in here![1] It's just beautiful.

BG: Yes, it really is.

HT: There is a marvelous book that he wrote. I'm trying to think of the title.

BG: "The Compleat Strategyst."[2]

HT: "The Compleat Strategyst," yes. That's a classic.

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1 The History of Johnniac, F. J. Gruenberger. prepared for the U.S. Air Force Project RAND by the RAND Corp, October 1968.

2 The Compleat Strategyst, J. D. Williams, a RAND Corporation Research Study.

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BG: Yes, that's right. He was a great guy.

HT: That's where I first knew of him, through that book. Beautifully written.

BG: Yes, it was really wonderful.

To go on then, after the random number generator the next thing was to put in an analog computer. RAND bought a REAC in the late forties, 1948 or something like that. We decided to completely rebuild it. Electronic analog computers, differential equation solvers, were designed for control theory servo-system real-time problems by control theory guys. The RAND guys were trying to do real (more generalized) mathematics on them. You know, open loop problems, not closed loop servo problems or guidance problems or that sort of thing. So we were trying to make a machine that you could more easily program. For instance, all of the other machines were set up without a removable patch board and you would stand there like a telephone operator with a giant console and patch this thing up taking three or four days. Once it was patched, you didn't dare touch it. It couldn't be used on second shift for another problem or anything.

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We put a removable patch board on it and changed it so we could set up the coefficients automatically and do other things like that. It would give digital outputs, so it was really a type of hybrid machine.

HT: People are talking about hybrid machines again.

BG: Yes.

HT: They are back in vogue.

BG: Well, yes. The company I was at before and coming to Xerox made some big hybrid machines. Probably it is still the way to do a very large differential equation problem even today. If what you've got are hundreds of thousands of replications and if it's a non linear problem with variable coefficients, a hybrid machine really does pay. But they're God awful expensive and it was a business that you couldn't make money in.

HT: What kinds of problems were you solving on the Reeves Machine?

BG: Well, there's a paper that goes back to that. I found some stuff, you know. Everything didn't disappear like I thought. You can have any of this stuff that would be useful. But there's a paper that is on the REAC.

HT: The machine was built by Reeves.

BG: Reeves Electronic Analog Computer, I think it was. They got completely passed over and surpassed by Electronic Associates who made high quality machines afterwards.

RM: What happened to Reeves? Did they just fade into the blue?

BG: No, I think they're still around.

HT: I saw their machine someplace.

RM: Really?

BG: Which?

HT: The REAC. I saw one in some lab. It was still usable. That kind of a machine doesn't become obsolete. It still can solve certain kinds of problems.

BG: Here is a RAND report.[3] Let's see, what was the date on this thing; it's probably in the fifties. I can establish a date on it. It goes through all of the junk that was done to make that machine different from the original Reeves Machine. There was a lot of patent bickering about it too. After Electronic

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3 Summary of REAC Experience, W. F. Gunning, A. S. Mengel.

Associates passed Reeves up, Reeves got very interested in the early history of this and the patent situation in an attempt to make some money off their patent position which was pretty good.

HT: This particular machine goes back to about 1946, then.

BG: Yes.

RM: Were there any problems with you making changes to the machine? They didn't object to that?

BG: We bought it.

RM: It was yours to do what you wanted with.

BG: Yes. Since it wouldn't do the things that the mathematicians wanted done, we just went in and changed it to attempt to make it do so.

RM: This was just a one-time project.

BG: Oh, yes. And so was the JOHNNIAC. It was delightful from the point of view of those of us who had the opportunity to work on it, you know, because we didn't have to worry about documentation or any of the things that are objectionable usually to engineers. (laughter) And we didn't worry about patents either because, again, it was for the use of one installation in a non-profit company.

HT: Here you talk about the plug board and using the ones from the IBM 405 tab machine.

BG: That's right.

HT: Was that the one you ended up using?

BG: Yes. Well, we bought four or five plug boards but the material that was in it--yes, it's a crummy picture, but that's what they looked like. This was the 405. It had only three panels. I think we made a four paneled patch board, but drilled it out of polystyrene because this material for the 405 tabulator machine leaks too much, electrically. You could probably get a copy of that report from RAND if any of that is of interest.

HT: Well, this is of interest, yes.

RM: Could we make Xerox copies of this one?

BG: Sure. The last part here has to do with applications.

HT: Very good.

BG: Arnold Mengel wrote that part. I'm a hardware type.

HT: Wes Mehlan. I've seen Wes and we haven't had a chance to talk about this.

BG: Well, and here's Dr. George Brown again. We all worked for George who worked for John Williams. George knew numerical analysis, I guess that is what you'd call it. It was solving problems that you couldn't solve any other way.

HT: Of course, the Institute for Numerical Analysis was either non-existent at that time or just coming into existence.

BG: Yes.

HT: In the late forties.

BG: UCLA?

HT: Yes.

BG: It was about 1948 or 1949, somewhere in there.

HT: Right 1948, I think it was. This is very interesting. I won't read it now.

BG: All right. There is another paper on REAC.

HT: This is by Wes Mehlan.[4]

BG: Yes. He was President of SDC.

HT: Right.

BG: You know, SDC grew out of RAND. Systems Development Corporation was initially a project inside of RAND and then it split off.

HT: He does say you added a fourth panel.

BG: Oh, okay.

HT: "Wondered why we needed to add a fourth panel to the 405 plug board, why we needed this number of contacts when the patch panel included scarcely one-half."

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4 Modifications of the RAND REAC, Wesley Mehlan, Feb. 26, 1951.

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So then he goes on to discuss why. (laughter) Your memory is very good.

BG: Yes, I remember that. (laughter) It was our job to drill that stuff. We had to drill it under water. Otherwise it would melt. Polystyrene has a low melting point, you know, and if you tried to drill it it would gum up and stick to the drill.

HT: So by doing it under water it was self cooling.

BG: Yes.

HT: Of course, at this time RAND is also beginning to take on its own unique complexion with the spin off and with the ability to do your own kinds of problems. Essentially, RAND Corporation as we know it today is beginning to evolve as a separate identity, rather than just an extension of what it was at Douglas.

BG: Oh, yes. It was completely transformed. Basically to allow the people in RAND to have free access to proprietary information inside of other aerospace companies, so that they could more easily project what things the Air Force could reasonably expect to have in the five to ten year future and longer. When it was part of Douglas you couldn't do that. You couldn't expect Boeing to tell a bunch of

Douglas cats what their plans were.

RM: And you were under contract to the Air Force?

BG: Well, RAND was a non-profit, I think California corporation, of which the principal supporter was the Air Force. But I think that even in the beginning, as far as their charter was concerned, they were not specifically restricted to Air Force things.

I've got some stuff on RAND that you could also get from them. A History of RAND. There was an article published in Fortune Magazine, 1951.

HT: Make a Xerox copy of one and send it to me. (laughter) I'm learning, I forget where I am. Xerox has become part of our language.

BG: Yes, it has, and that's what they worry about. You know, it's like aspirin. Now aspirin is written with a small "A".

HT: There are so many other examples, you know. You can split the hair only so fine.

RM: All computers are IBM machines to half the world.

BG: Sure.

RM: And so it goes and you can't beat it anymore.

BG: No, but the lawyers want us to try.

RM: All right. While I'm on your premises I'll do my best.

BG: Good.

HT: The Reeves machine then I would gather stayed around for quite a long time.

BG: Oh, yes. It wound up in the Air Force Academy just south of Denver, Colorado.

HT: That's what I thought. It's still there, I think.

BG: It could be. I think it's still there.

RM: Operating, or as a museum piece?

HT: No, operating.

BG: I think it probably was.

HT: It still is operating.

BG: The guy who did that was later at Boeing, Bill Quirk.

HT: Q-U-I-R-...

BG: ...-K. When in the Air Force, he was a scrounger extraordinary and found the REAC being declared surplus or something. He picked it up, put it together and got some students on it and kept the thing going.

HT: I think it's still running there.

BG: Could be.

HT: I think that's where I saw it last March.

BG: Yes. Well, those two projects overlapped. That is, the finishing up of the REAC and the initiation of

the JOHNNIAC.

RM: Was there any specific project in-house that made RAND feel they needed a large scale computing device, or just a lot of things going on?

BG: It was lots of things going on.

This is a picture of the output of the machine. There's the patch board. Now all of this stuff was built, this whole console thing, was built by the RAND people. Here are a set of patch boards. Okay, four panels of polystyrene. You can see through the patch boards.

HT: You just slid them out and popped them in.

BG: Yes. The way you put arbitrary functions in is that there was a wire cemented on the paper. The independent variable was the rotation of the drum and the wire was the arbitrary function, f(x). The drum was rotated by a voltage that came out of the machine producing X, and the voltage that was picked off by this wire on a potentiometer slide wire along the top then gave you Y or F(x) and it was fed back into the machine.

Originally these were all manually tracked, following a curve on the paper.

HT: With wire.

BG: We put the wire on it. This handle was the thing that you would sit and turn. Originally, when we got the machine, somebody would have to crank this thing back and forth whenever you ran a problem.

HT: You'd need a movie camera to watchyour movements. (laughter)

BG: Yes. Well, people would get tired, you know, and then everytime you would run the problem you'd get a little different Y function. (laughter)

HT: I would like to copy those. If you would like them back, we'll return them.

BG: Yes, I kind of would. It's nostalgia.

HT: Before we leave I'll let you write a small identification on the back of each and then we'll return them.

BG: Okay. I'll just stamp them with a rubber stamp.

HT: Well, I meant, identify what they are.

BG: Oh, what they are, okay.

RM: We'll give you that responsibility.

BG: Okay.

HT: We need to be sure that when people ask us for photographs of machines, we know what they are and what they depict.

BG: Okay. That's a photograph of the JOHNNIAC computer. Those things down at the bottom are all transformers for the tube heaters. These are the individual chassis. This is patterned after the Princeton machine. And this is probably about middle 1952.

RM: This is pretty early. You hadn't started changing technology yet.

BG: Well, the technology was the same in the sense that it was vacuum tubes and the same circuits, we always stayed with that. What we changed was the actual dimensions of the chassis and the number of chassis. We made provisions for about twice as many tubes sockets as the machine at Princeton because it's always the case when you build something for the first time that you haven't got enough room. You know, "If we only had three more tube sockets, we could blah, blah, blah." It was also made so that you could get one of these tubes out. We deviated from Princeton in that we used plug and socket connections between the chassis so that you could...

HT: So you made some kind of module for each chassis.

BG: yes.

HT: Some kind of modular construction. That was a fairly new idea.

BG: (laughter) Well, no. Really...

HT: RAYDAC had it.

BG: Yes, indeed. They had very good modules. In fact, that was a very well engineered machine from the point of view of the modularity. The SEAC was very modular.

HT: Was SEAC modular.

BG: Yes.

HT: I didn't realize that. Because you think of modularity as a post 1955 generally accepted concept.

BG: That was really a much more venturesome machine, I think. The SEAC was really quite a sport, you know. It took off in a different direction from everybody else. The basic idea of the SEAC was the predecessor of the 701, the circuitry of the so-called Haven's Delay Circuit was 701. But I don't know really on a legal basis whether it was a predecessor, but it was put into hardware before anything else.

HT: Getting back. While we're talking about the JOHNNIAC, you were faced with computational problems.

BG: Yes, yes.

HT: Now, what are some of the various directions you thought about going?

BG: Well, RAND had the largest IBM installation for scientific computing anywhere in the world and it was running four shifts, you know, weekends, nights, around the clock with card programmed calculators and 604s.

HT: You had about half a dozen of those.

BG: Yes. That's right. And the up-time was not all that you'd like. There was an enormous number of vacuum tubes in the 604 and they (RAND) saw that they were not going to get the work load done. It had grown enormously, and if you just projected another year, you couldn't get enough watts in the building to run that many machines. So they decided that we'd better do something about this. That was the trip that John Williams and George Brown and I took when we looked around at several machines.

HT: Okay, I'm really interested in that trip. Bobbi had mentioned that you talked to her about it. First, did you write a report at the conclusion of that trip?

BG: Yes, but I can't find it. It must be in the RAND files somewhere.

HT: Can you give me an approximate date on the trip?

BG: Yes, it could not be later than--no, now wait a minute.

RM: It's probably documented.

BG: It probably is in the "History of JOHNNIAC."

HT: But is the trip report in there?

RM: No.

BG: But the date ought to be in there. I can't remember if it was 1951 or...

RM: 1950.

BG: It was late 1950?

RM: "Williams, Brown, Bill Gunning set out to tour..." Looks like it was in 1950. It says, "In 1949-50, RAND rented from IBM and operated a pair of CPC's and some 604's. In 1950 the need for more computing power was felt in the issue of larger and faster equipment. Should RAND attempt to build a machine for its needs, or to buy and if so, what?"

HT: I guess one of the reasons I'm interested in your trip is that essentially your trip report or what you saw is the state of the art at that moment.

BG: Yes.

HT: I've seen the state of the art a year or two earlier, three or four years earlier and a couple of years later. I'm really fascinated at what the state of the art was in terms of the machines, of building, planning, working and ideas that you saw on that trip.

BG: Well, there must be a trip report in the RAND files someplace.

HT: Well, without that trip report, what does your memory tell you?

BG: Well, let's see, we visited the BINAC as I recall, either on that trip or I'd already been to see it.

HT: Which was at Northrop at that time?

BG: No, at Spring Garden Street in Philadelphia. Eckert and Mauchly.

HT: Philadelphia.

BG: And let's see, we saw UNIVAC, we saw EDVAC.

HT: None of these were running when you saw them?

BG: No. That's right. They were all in quote, "debugging". You know, 99 percent finished, as these machines were for a long time. There were the famous things called the von Neumann constant.

HT: Right.

BG: So most of these machines were in that state. The Aiken machines were running. We saw Whirlwind.

HT: Whirlwind 1 was going then.

BG: I don't think it was.

HT: It was pretty close to operating.

BG: Yes, that's right.

HT: Closer than any of the others.

BG: That's correct.

HT: And the Harvard Mark II was going.

BG: Yes.

HT: As well as the Mark I.

BG: Yes.

HT: The Bell Machine, did you see the Bell Computer?

BG: Yes, the Stibitz machine?

HT: The Model 6 machines were going.

BG: Yes. We saw a lot of stuff at Poughkeepsie. We were shown a lot of proprietary stuff. It may be that trip report for some reason is still classified as proprietary, but I should think that it could be declassified by now. IBM was very open in showing us several different approaches that they were taking to electronic computation. One was the 604 with a drum. Sort of the basis of what became the 650. I can't remember what

the other things were, but we spent at least a day there. But it all looked like--well, the technology in the 604 plus drum--the 604 technology with which we were all too familiar, and there were no plans to do anything different.

RM: They were not yet talking 701.

BG: Oh no. Not at all.

HT: The SEAC was being built then or maybe it was built.

BG: I don't remember what state the SEAC was in. It was going because I'd been working on the SWAC memory. In fact, I've got a photograph of the memory of that machine which I'd like to show you.

RM: Oh, good. IBM was still in their rinky tinky phase.

BG: They had definitely not decided on the 701 at that time. As a matter of fact, Cuthbert Hurd was the guy who shepherded us around Poughkeepsie, and he was a very sad man at the end of that day because we told him that we thought, "Look, you guys are just not on the right track." They were not really interested in the stored program idea. Well, there had not been a company commitment made. You know about the famous dinner later that Watson had and gave out all the watches. Well, George Brown was a recipient of one of those watches.

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RM: Oh, was he?

BG: Yes, indeed.

RM: Because Keith Uncapher said that he's always wanted to really get to the bottom of the story of whether your trip, not just your trip, but RAND's need for a computer, had stimulated the 701 decision in any way.

BG: Well, we thought so. When we finally decided that we were going to build a machine, we were IBM's biggest customer in scientific computing. And that's something that IBM always pays very close attention to.

RM: That's right.

BG: Properly so.

RM: Yes. It must have been a very sad day.

BG: Well, it was for Cuthbert. He was a very sad man standing on the train station. We were off to Chicago to see the machine at Argonne Labs which was another of the Princeton family.

HT: Chuan Chu.

BG: Yes, that's right.

HT: He was building two machines simultaneously.

BG: That's right.

HT: One for them and one for Oak Ridge.

BG: Yes.

RM: So you had a lot of semi-built machines to juggle around with?

BG: Yes.

RM: How did you come through to your final decision?

BG: Well, I just glanced through that Gruenberger paper again, and it's right. My recollection is that the thing that we liked best about the Julian Bigelow-and-his-disciples approach was their emphasis on reliability. That is, they were attempting to make a machine that was much more conservatively engineered than anybody else's.

George Brown had been associated with Burks, von Neumann and Goldstine and liked the idea of the parallel machine. The EDVAC was a serial machine. The SEAC was a serial machine, Whirlwind was a parallel machine. So the parallelism from an organizational point of view was attractive, but mostly we were attracted by the idea that their machine and their approach to machine design seemed to have the highest probability of getting some continuity of service.

HT: Of course, Whirlwind ended up with pretty much the same level.

BG: Yes, that's right. Whirlwind also, but Whirlwind was just more than we could comtemplate duplicating.

HT: That was a really innovative machine in many, many ways.

BG: Oh, yes.

HT: That was ...

BG: Well, you walked around inside of it.

RM: But you needed the university to build it.

BG: And a lot more money than RAND wanted to spend.

HT: In terms of your visits, was there any thought about visiting England where there were a number of operating machines?

BG: I don't know of any thought.

HT: For example, at that particular time, I think it's 1951 or so, the University of Toronto bought the Ferranti machine, which was probably the first time a machine came on the market commercially. It was sort of a fluke. It was due to the change in government in England that the machine became available.

BG: Those machines were drum machines, weren't they?

HT: Yes. Wilkes was working with mercury delay--EDSAC.

BG: Yes. Again, a serial machine. The Princeton machines were different. The 701 really followed that in the

sense of using parallelism and Williams' memory, which was what most of the Princeton machines used.

HT: Of course, Williams was in England and I've forgotten the name of the machine that he was associated with.

BG: I don't remember either. Williams came over.

RM: The Manchester Machine.

HT: Yes, I remember that.

BG: Yes. Williams came over and was a consultant to Huskey on the SWAC electrostatic memory.

HT: Yes, Huskey had been in England just prior to that.

BG: Yes, that's right.

RM: SWAC had already started, was underway?

BG: Yes. I spent about half time on SWAC for maybe six months before that trip.

RM: Oh, before the trip.

BG: Right. I was assigned to the memory on SWAC and worked with those guys to get the electrostatic Williams tube memory out. So I also had a pretty good idea of how flimsy the tubes were as a technology.

HT: So you already had experience in building a machine.

BG: Yes.

HT: You say you considered going to England.

BG: No, I don't think we did. In retrospect I would guess that it was because those machines were primarily serial or drum machines. In those days, it was the high speed capability of the IAS machines that was attractive. The Whirlwind was the fastest machine around but it was exploring technology that we didn't think we could duplicate.

HT: The cost was just going out of sight.

BG: Sure, sure. It was a valuable research project.

HT: So once you decided to build your own machine, you essentially decided to model it after the von Neumann concept that Bigelow was constructing at Princeton.

BG: Yes.

HT: Then conceptually that was your model, but you did make changes?

BG: Yes, yes. We made them timorously, you know, because Julian was really a very superior designer, and we thought a long time before we made changes. Julian didn't want to have any plugs in his machines because he thought plugs and sockets were unreliable. But we spent a lot of money for the plugs and sockets we used. They were all gold plated. It was with great

misgivings that we made changes from his design. But it was at that level of detail. We didn't change the basic architecture. We used the same kind of analog adder circuitry--all of those machines initially were analog. There were four different voltage levels for the four conditions of adding two binary numbers. The digit detector circuit selected which state it was by an analog comparator. The vacuum tube circuits had 50 volt steps. We went along with that basic concept maybe making a few changes in resistor values. We put in more fuses and we put in more care in the way you would regulate the voltage to the heaters of the tubes and a lot of stuff like that.

HT: I think if you take a look at the whole class of machines that are best named von Neumann derivatives, and there are a large number of them, each one took on little individual characteristics...

BG: That's right.

HT: ...design modifications or sometimes polishing.

BG: Yes, right.

RM: But you actually didn't push the technology.

BG: I don't think so. We decided not to use crystal diodes, as they were called in those days, semi-conductor diodes. Whereas the SEAC machine took the bold stroke of saying they're good. And they made a machine with 19,000 diodes in it. Unheard of at the time, you know. And they made it work. That was a really tremendous contribution.

HT: Of course, the big thing that you did that was different was using the selectron. That's an engineering problem all by itself. It's a different problem than the problem of the Williams tube.

BG: Yes, that's right.

HT: You might want to talk about the solution. (laughter) What was the solution of that problem?

BG: Well, Keith Upcapher did a lot of that, he sent me some photographs. Have you got the stuff on the selectron memory?

RM: No.

BG: Oh, you haven't?

RM: No.

BG: Well anyway, let me start back. It was a real struggle with RCA, who were just beginning to invent in the field of core memory.

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Jan Rajchman was the inventor of the selectron but he just couldn't see it anymore, he didn't want to bother with it. The part of RCA that manufactured the selectron tubes just didn't want to talk to us. It was a tremendous hassle to get them to build tubes for us. We finally did because they were still including them in the Bizmac Program--they were going to be the memory for the Bizmac (an RCA computer for commercial applications). But they just couldn't make them clean enough to make them work. A little bit of lint floating around would short out one of the eyelets.

There are several papers on the selectron. The essence of it is that there are 256 eyelets that are about an eighth-of-an-inch in outside diameter with a tiny hole in the middle. The eyelets store the binary information. These eyelets have to be electrically isolated so they can have two different voltage states with respect to the cathode. They will be driven to a high voltage state or a low voltage state depending on the way you pulse some of the electrodes. If a little piece of lint trickles down to one of these things it will short out one of the eyelets and now you've got a bum tube.

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But, it turns out, and we learned this from RCA, there's a fix for this. You take the tube and a Tesla coil, 10,000 volts or something, you hook it to any wire that goes inside of the tube and let it spark around in there and it will vaporize the lint. So we would take a tube that had developed a short in the machine, put it on one of these Tesla coils and spark it. It would clean up and then we'd put it back in the machine and use it some more.

Now, I've got a picture here that shows this gadget that we used to burn in the tubes. How about that. (laughter) See these things would all sit alongside the machine. Well, here are some pictures of the chassis and the inside of the frame.

HT: Oh, wow. You can see it sticking out at you.

BG: Yes. That's the back side.

These were all interconnections. We changed the design of these wires and again made them plug and socket. See these are all pins that will plug into female connectors. Same way across this way. And the

selectron memory when it went in sat on top of the machine. Here they all are sitting up. These little frames around them were to cause the air that flowed through the machine to be constrained to go past the tubes. The air had to go through the holes in this plate right along the surface.

HT: You'd cool the surface of the tube.

BG: Yes. We wanted the air to go by that way.

Here's Tom Ellis and Keith Uncapher. (laughter) Something like 3,000 wires came out of the machine to go over to the console.

HT: That's great. How about the cost of the tubes. Did that bother you?

BG: It sure did. Sure did.

HT: What did a single tube run at that time?

BG: Oh, you know, I don't really remember, but I think there was something in the John Williams memo about that.

RM: It was several hundred dollars.

BG: Yes, it was like a couple of hundred dollars, and we were concerned about how long they would live. Nobody knew, but we were able to extend the life like a cat. We got maybe nine lives out of it with this Tesla coil business. When we had the selectron memory in that machine, it had the longest mean time

between failure of any electronic machine. Because all of the rest of them were dependent upon the Williams tube and that was a very flaky technology, as you know. It was something that wasn't ever intended to work.

HT: But the engineering of the Williams tube in the machine was much easier than what you had to contend with on a selectron. That's why I asked you the question...

BG: Oh, I see.

HT: ... about the design and in terms of...

BG: Well, yes, but it was digital, most of it. We again designed the circuits that drove that memory using the basic design philosophies that Julian laid down; if you're going to cut a vacuum tube off, you must design the circuit so that it will drive to twice the normal cut off voltage of the tube, and things like that.

HT: It was a large safety factor.

BG: Yes, right. And everything was direct coupled. Whereas ordinarily you would think of a capacitor and a DC restoring circuit, we'd string a long set of resistors to go from plus 300 volts down to minus

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200 volts, or whatever was necessary to run that memory. When the guys at RCA saw it they asked, "Why are you doing it that way? Why don't you do it the way we're used to doing it in radar and television." Again, that DC coupling was a derivative of the Bigelow school.

HT: So this was a true von Neumann machine.

BG: Yes, it really was. I guess the only one in that sense, if you want think of it that way.

HT: You know, the only one that used the original memory. (laughter)

BG: That's right.

HT: Do you have any comments on von Neumann? You talked about Bigelow.

BG: Well, most of our contact was with Bigelow because we were down at the nuts and bolts level. Von Neumann, I remember, introduced the idea of magnetic core storage in a lecture at RAND. He said, "Look, this is the greatest. Why don't you look into it." We went back to Lincoln Labs and did. He was the kind of a guy that could do so many things and still be such a fine human being that it was discouraging. (laughter) You just couldn't point at anything.

HT: Inadequate is the word. (laughter) For the rest of us.

BG: And without condescension. You know, he could talk to engineering slobs like us who were concerned about these things and then he could turn around the next minute and be talking hydrodynamics to somebody; just really a tremendous person.

HT: Who were some of the other key people that impressed you in that era?

BG: Well, Eckert of Eckert and Mauchly. I thought his approach to engineering was also very conservative.

HT: It's interesting, you know, your commentary on him and on Bigelow from that point of view. In a recent conversation I had with Maurice Wilkes on how he was able to get EDSAC going so soon, the key was conservative engineering.

BG: That's right.

HT: That's what did it. He just decided that he would sacrifice certain things and would be very, very conservative because he wanted to get it going.

BG: Yes. That really was the point. Now, they had not done that on the BINAC. The BINAC was really pushing technology beyond the capability of the time. That's

why it never worked really effectively. I think it worked enough to meet the acceptance test and get it shipped out to California, but...

HT: But it never worked.

BG: It was never possible to get any really useful work out of it. The UNIVAC used a lot of the same things that they learned in the BINAC, backed off by a factor of four.

HT: So UNIVAC was effective and useful.

BG: Yes, right. Let's see, who were some of the other guys--well, certainly the guys at Whirlwind.

HT: Oh, yes. That's an impressive project.

BG: And then I spent a lot of time with the guys in Lincoln Labs during the core memory period which came later. There were big contributors there.

HT: Who were some of the names?

BG: Dudley Buck.

HT: B-U-C-K?

BG: Yes. He was an extremely inventive guy and came up with that original cryogenic logic element and oh, Bill Papian. Let's see now, who were the guys that really were...

HT: Norm Taylor, I think, was one.

BG: Yes. Well, the guy who's running DEC, Ken Olson, came out of Lincoln Labs.

HT: I haven't run into him, but everybody that I've met who was connected with Whirlwind was very impressive.

BG: Yes.

HT: So, RAND decided to build the JOHNNIAC and your direction was clear. We have described some of that. What are some of the problems you ran into in the building of JOHNNIAC and getting a realizable, workable machine?

BG: We almost built a boat in the basement. (laughter)

But let's see, what were some of the other things I can remember?

Since we had all the history of the basic circuit design, that went fine. But I remember one of the other deviations that we made from the Princeton machine was to bring out to the console the ability to sense the state of any of the flip-flops, of which there were several hundred. When the machine would hang up, you were able to look around (at the console) and to help find where the trouble was.

Those thousands of wires that you saw Keith working on were all those test points, in effect, being brought out to the console to light the lights.

They were isolated from the actual circuits through 30,000 ohm resistors. I remember the first time that we ran the machine in a problem solving mode computing prime numbers, which is what everybody started out with. It would compute prime numbers up to about, I don't know, a few hundred, and then it would hang up. It would hang up in the same place. Very baffling. It wasn't the program and it finally turned out to be the "30,000 ohm resistor effect." This was because a certain combination of states in the machine would produce capacitance coupling to this bundle of wires, which when coupled back through the 30,000 ohm resistors would prevent the flip-flops from toggling. (laughter)

HT: Built in your own override.

BG: That's right. It's the kind of thing that still plagues computer manufacturers today. The machine will run an acceptance test, but it won't solve problems. A memory system will run all the memory diagnostics, but when you put random numbers in it, you know, real problems, you'll find some states that fail because of the coupling. And, in today's machine, because of the arrangement of the etch on the

circuit boards or the back plane wiring, one circuit will couple into another and cause the thing to have a glitch at a certain combination of signals.

So we solved that by taking the 30,000 ohm resistors out and putting neon bulbs in to couple all those cables and pulse them externally when we wanted to see what the state was. That was really a show stopper for quite a while, too. Took us a long time to figure that out. That was one of the most serious problems that we had to overcome, that and the lint in the selectron tubes.

HT: Well, that sounds like a marvelous solution.

BG: Yes, right. We didn't think of that one.

HT: Who did you learn that from?

BG: We learned that from the manufacturer. From RCA, because that's the way they would get tubes into a state that they could ship. And I think we learned it by visiting the factory and seeing them do this and saying, "What's that?" (laughter) But I don't think they would have told us otherwise.

RM: Did building, what did you call it, JOHNNIAC Junior...

BG: Yes.

RM: ... Was that a great help to you?

BG: Yes, indeed.

RM: Getting it produced quicker.

BG: Well, it was again sort of a confidence builder. We were able to test all the circuits that were fairly expensive to build in that reduced sized machine. I guess we must have found some things wrong in there and some things to change, but I don't remember what they might have been.

RM: But that was your proving ground?

BG: Yes, right.

RM: And it was literally a scale model.

BG: It was just one fourth of the machine. A ten bit machine.

RM: If somebody had wanted a ten bit machine, they could have operated on it?

BG: It did, right. It ran little iterative loops and we were absolutely elated when it would run for a day without anything happening. It was built just to test out the selectron memory circuits and of course all the rest of the registers. It was a ten bit slice and we were able to take the stuff out of it and put in the main machine, or keep them as spares. I don't remember which.

RM: So it didn't stay around?

BG: No, it was cannibalized as far as I can remember. We learned a lot of other things about vacuum tubes. There's the cathode, which is a sleeve, and then there's the electric heater which goes inside. We learned a lot of things about how to test for leakage between the heater and the cathode which was a perennial problem, especially when you've got 5,000 vacuum tubes which is about what that thing had.

HT: I was going to say, you also had the problem of developing your own maintenance.

BG: Yes.

HT: Because you hadn't bought a machine from somebody else who was to take care of it.

BG: Oh, yes, that's right. Well, that was what all those wires were for; we were attempting to build in maintenance aids, the thing that IBM has since made a real science. At that time they were not very conscious of maintenance; the 604 was a God awful thing to maintain.

HT: That's why I was so impressed with the early Bell machine.

BG: Yes, yes, with error detection.

HT: You could spot an error instantly.

BG: You bet. Well, they knew more about that. Those guys really think in terms of reliable systems that will run unattended. They knew what they were doing. You know, the computer guys had never heard of that.

HT: Well, even the ones who thought about it, though, realized that the size and cost was going to be prohibitive for what they were going to get out of it, at least in the early days.

BG: Well, now, let me step back. The RAYDAC was a very ... remember?

HT: Okay, RAYDAC is always the exception.

BG: Yes. It had an awful lot of error detection in it. And so did UNIVAC. Remember UNIVAC had two parallel arithmetic units that would do computations and compare. And they carried along error detection. UNIVAC did a very good job, too.

I guess the reason we were scared off from UNIVAC was that we happened to come in at the time when they were debugging the machine. There were two or three occasions there when through some fault or another they blew out all the diodes in the machine. It was awful.

HT: Of course, BINAC had two parallel banks, too.

BG: Yes, it was two different machines, if I remember.

HT: This was the way of checking. It was the only way, you know.

BG: Yes. Well, that's what the telephone company does today. The ESS systems are duplicate computers. Electronic switching systems hook up telephones when you dial a number. That's under computer control and has been for five years at least or more than that. There are two independent, completely duplicated machines, including memory and everything. One keeps track of what the other is doing and if it fails or if an error is detected the hot standby will take over.

HT: Okay, when you got JOHNNIAC running, you didn't just get rid of all of your other computers.

BG: No, the REAC stayed in.

HT: The REAC stayed. How about the CPC's and what other IBM or other equipment did you have at that point in time?

BG: Well, let's see. The CPC's probably stayed around until the 701 came in.

HT: You got one of the early 701's.

BG: Yes. I don't know the serial number, but it was probably one of the first few. That is, five. If you discount the one that went into headquarters.

HT: At what point did you put core onto JOHNNIAC?

BG: I think the date is actually in the eulogy, but it was around 1954 or something.

HT: 1954. In terms of problem solving ability, which was the big reason for the machine, what were some of the things which began to happen then in random results?

BG: You know, I just don't know that. Paul Armer would be the best man and he's in this area, too by golly. Okay. Don Madden is in this area, too.

RM: Yes.

BG: Don and Paul and George Brown are the guys that could tell you that kind of thing.

HT: When did MortBernstein join RAND?

BG: Well, I think it was later, I don't remember. But he was there when I was.

HT: He was there when the 701 was there, I think.

BG: Yes.

HT: And I'm not sure how much earlier.

BG: Somewhat earlier. Cliff Shaw, of course. Cliff was one of the guys that influenced the machine as much

as anybody. He and George Brown and Don Madden, were the guys I remember most when we were trying to decide exactly what the form of the divide algorithm would be. You know, what are you going to do with the remainder, and how would it work so that you could do double precision. Exact little curlicues of what you would now call an algorithm. We'd never heard of the word then, I don't think. [Laughter] I'd never heard of it.

HT: ... found in the old literature, not in the new literature.

BG: We certainly didn't call them algorithms.

HT: Right.

BG: But I just don't know that part. I'm a hardware slinger.

HT: Well, where did you, in terms of your own engineering direction, where did you go from JOHNNIAC?

BG: I went to International Telemeter.

RM: Yes. There's a story.

BG: Yes, right. Willis and I wrote these horrible specifications for people to bid on; IBM, Remington Rand, Bendix and Telemeter, I think, were the bidders. I ended up with Telemeter having to try to meet those specifications. The specifications were so restrictive that we'd get back maybe ten or twenty pages of exceptions from somebody like UNIVAC who had their own set of design rules and didn't like the ones which told them what to do. For instance, if you've got to use a vacuum tube it's got to be driven to twice cut off, and if you're going to put a resistor in it, it has to be derated two to one, and all resistors have to be pulled and twisted before you can put them in the circuit, and stuff like that. Willis Ware, who was with Bigelow and then joined us at RAND, was really primarily responsible for documenting all of that. A copy of those specifications, I think, would be a thing for your archives, because it probably is one of the most complete documentations of that philosophy.

So then I ended up at Telemeter, which is where George Brown was. I was assigned when I got there to Louis Ridenour.

HT: Telemeter by then had got the group of people from RCA?

BG: Yes.

HT: They had several of the people who had worked on core.

BG: That's right.

RM: Telemeter was the Paramount Company.

BG: That's right.

RM: Isn't there some tie-in here with McCarthyism?

BG: Yes, that's right. I had to leave RAND because of being a victim of the McCarthy era. I lost my clearance, and George Brown offered me a job the next day. As a matter of fact, I was at RAND for a year after my clearance had been suspended, but I still hadn't had a hearing. Well, I'd had one or two. I had three hearings all told. I went through a long period there; as a matter of fact I didn't get my clearance re-established until 12 years later or something like that. It was a long period and it cost a lot of anguish and a lot of money, but it finally turned out that...

HT: There are still cases in the court for that period.

BG: Yes, right.

HT: They still haven't been settled. The group actions. Some of the individual ones have but many of the group action cases are still in the courts.

BG: I would never have been able to do it either emotionally or financially except for the assistance I got from the company that I was at before I came here. It was a real struggle.

RM: To reacquire your security?

BG: Yes. Just to get that back.

RM: Even after this many years you still had to fight.

BG: Oh, yes. It was a real fight.

HT: There was a group action in New York of the teachers who were fired in the public schools that is still going on.

BG: Yes, Well, there were postmen, there were thousands of people. You know, a postman. Now why should you worry about his security clearance? My wife was a psychologist working for the VA in a TB hospital and her loyalty was questioned.

RM: Oh, the loyalty oath?

BG: It was the loyalty oath. Her right to work for the government was denied. It was really a rough period. It still grabs my stomach.

HT: Well, as I say, there are still people who have not recovered from it in any manner.

BG: Well, it was Ridenour, Brown, and Gilbert King, all Ph.D.s.

HT: Had Ridenour just come from Illinois, or had he been someplace in between?

BG: I think he came directly from Illinois. I don't remember where Gil King came from, but George Brown came from RAND. They were the ones that did the original paper on the photostore[5] and eventually Gil King did the 1360 trillion bit memory that is over at Livermore. He did the predecessor at Telemeter. The reason I mentioned this is that they put that concept out; using optics and very large memories. It was originally intended as a lexicon in an automatic language translation machine. People didn't know how hard that was. (laughter) So they built a successful machine and sold it to Rome Air Force.

Ridenour was really interested in computing, and Paramount pictures was really interested in pay television. They had this name Telemeter coined for metered television, and they didn't know that

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5 Photographic Techniques for Information Storage, George Brown, Gilbert King, Louis Ridenour. Proc. IRE, 41, #10, 1953.

telemetry meant something else to the scientific world. (laughter) ... So Louis joined them on the basis that by doing pay television there would really be computers involved. His main interest was in the computing part. He brought Raymond Stewart-Williams and Milt Rosenberg from RCA, both of whom were working for Jan Rajchman in core memory, and they started the core part which later became Telemeter Magnetics. Milt, you know, is a vice-president of Electronic Memories.

HT: And this is why RAND selected them then?

BG: Well, no, they really did have the best proposal. They had the most responsive proposal. I think it was a fair competition. From our point of view they were, we believed, the best. The bid was let before I left RAND. I didn't know that I would be going there. They responded to the specifications, and they came up with what we thought was the best way of doing it. I think that it turned out to be the best way of designing the memory, and although they had a lot of trouble making it work because it was really pushing the state of the art, I think it was the right decision.

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They did a hell of a good job. I don't know how many of them they sold; five or six or something. But in the process, they learned an enormous amount about solving problems that other people hadn't faced yet--for example, the transmission line problem of driving a long line through thousands of cores. That became part of the whole core memory art and still is today.

Stewart-Williams and Milton Rosenberg were really very, very superior engineers.

HT: Where are they at the moment?

BG: Rosenberg is Technical Director/Vice-president of Electronics Memories which has probably got some other name now. I've got a picture of him, and the JOHNNIAC. Trude Taylor is the president. Here it is. This is a picture of the original memory that they delivered on the top of the JOHNNIAC. This is what replaced it whenever this thing was put out. Of course, now we've put it in much less space than that with a semi-conductor memory. That's Milt. This is the space where all the selectrons were and we just lifted that off. I guess by the time we put the selectrons on--when we

were doing the detail circuit design of the selectron memory--we were pretty well convinced that we were going to go over to core memory.

You know, why should UNIVAC bother with making a special like this? That was another reason why Telemeter got it.

HT: But you think Electronic Memories[6] has changed its name?

BG: Yes, they have a new name now.

HT: How about their address in Hawthorne. Is it still the same?

BG: Probably.

HT: Because we could locate it through that.

BG: Yes.

HT: This would be an interesting document for making Xerox copies of. (laughter) I almost goofed, didn't I?

RM: Yes, did it right.

BG: Have you seen this thing which Fred Gruenberger put together?[7]

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6 Electronic Memories and Magnetic Corp., 10960 Wilshire Blvd., L.A., California.

7 The History of JOHNNIAC.

HT: He was the reason of course that the ILLIAC was built.

BG: Yes, I'm sure that's right.

HT: The biographical work that I've seen on him is just very, very impressive.

BG: Yes. He had tremendous ability, but he didn't believe it. Now I'm being a lay analyst. (laughter)

HT: I'm trying to think of the name of the gentleman at Argonne who had similar difficulties also.

BG: Moll Flanders?

HT: Moll Flanders, right. He too was much beloved by all who were associated with him and also came to a tragic end.

BG: Yes, now Louis was really a delight, you know, he liked to live first cabin and it was really wonderful working there, it was just fine. I spent most of my time, however, not in the magnetics group. I worked on the pay television part.

HT: Let me back off to your six months on SWAC.

BG: Okay. Let me see...I'll show you a picture of the SWAC memory.

RM: I talked to Harry Larson the other day.

BG: Yes, okay.

RM: He had a great time recalling those days.

BG: That's right. Harry and I and another guy by the name of Ben Ambrosio.

HT: Zephyr? (laughter)

BG: That was what they called it for awhile.

RM: Oh, SWAC was called Zephyr?

HT: What was the project name?

BG: Most of the machines were Winds, if you remember: Whirlwind, Cyclone, and what was the Raytheon machine at Mugu?

HT: Hurricane.

BG: Hurricane.

HT: Right.

BG: I don't know why that happened. Now this is the slave tube so you can see what was going on, and the actual tube that's doing the storing is in here; it has the same deflection voltage. It was encaged because it was dangerous. The screen at the end of the thing was around minus a thousand volts. This was a picture in focus of what the surface looked like, and here are all the dots and these would be the ones, a dot-dash scheme. We borrowed the idea of working on the diagonal. Originally people just deflected them sideways. This was defocused to try to make it look like a sign. (laughter)

HT: Yes, I have a copy.

BG: All right. I have that in the files, too. That refers to an awful lot of documentation that backed it up.

RM: Yes, it does. It's a good document.

HT: It lists a lot of people.

BG: I think either this one or something mentions George getting the watch from Watson. I remember that.

HT: Yes, I have a copy of that.

BG: Okay, good.

HT: That's great. He really writes very well.

BG: Yes, he does. Where were we?

HT: Telemeter, and I was going to ask you about Louis Ridenour.

BG: Okay.

HT: He's a very impressive fellow.

BG: He certainly is. You know the 28-volume Ridenour Series[8], of course, which was the Bible. He wasn't von Neumann, but he had a lot of the attributes in terms of being a very interesting social human being, as well as being extremely bright.

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8 Massachusetts Institute of Technology. Radiation Laboratory Series. [Louis N. Ridenour, editor-in chief. 28 vols. New York and London: McGraw-Hill. ed.]

HT: Although nobody has a picture of it, Leon Harmon who was a technician on the Princeton machine tells me that one of the first times they tried to do something organized on this tube it was a scatalogical picture. (laughter)

BG: I remember getting a very bad shock. I touched my nose to the end of that tube one time and I just about got knocked off the stool.

HT: Went right through you.

BG: Yes. Harry and Ben Ambrosio and I were the memory team on that machine. and I learned a lot about that kind of stuff.

HT: You mentioned some of the things that bothered you about the Williams tube, but I think it would help if you went into some detail.

BG: Well, the amount of energy associated with getting a one or a zero signal out of the face of the tube was extremely low. You had a very large metal target pasted on the outside of the cathode ray tube, and all the business was going on inside. The electron beam would explore a given area to find out whether a dot or a dash had been written the last time. If a dash had been written, the beam would, in effect, dig a hole; splash some electrons out, okay?

HT: Greg Toben was talking about it.

BG: It's like poking a hose down at something that would almost freeze. If you moved the hose over one way then some electrons would splash back in the hole that the hose would dig originally. However, if you just left it without deflecting it and then turned the beam off, the hole would still be there the next time you came back. So the difference was the amount of electrons that would flow out of that area when the beam was first turned on. The area of the spot is a measure of the amount of capacitance of the capacitor that has the charge in it that you're trying to measure. Okay? The signal pickup screen had to cover the whole surface and it had a large area so the ratio of those capacitors was very large and very unfavorable. That meant that the signal that you got on this electrode, (just a piece of wire screen that's pasted on the outside of the tube) was very low. And therefore, the susceptibility to noise was very high. That's why we had that extra screen over the face of it, because if you left it out in open it became an antenna. The pass band went right through the broadcast band and you could pick up all kinds of noise from anything.

HT: Anytime a fly hit the screen. (laughter)

BG: That's right. IBM did a hell of a good job of engineering on the 701, but still you could go over and flip the light switch and probably produce an error on the machine. So it was just that delicate a thing.

HT: What were some of the things on the SWAC, though, that impressed you?

BG: Well, I was not terribly, favorably impressed with the circuit design of the SWAC. It took after the Whirlwind, but it did not have the money or the fundamental understanding of good circuit design that the Whirlwind guys had. I really didn't think the SWAC was going to work very well. You can see how wrong I was.

HT: Yes, it ran a long time.

BG: It ran longer than any machine, probably. I thought that the SWAC was probably one of the more flakey, unreliable machines that were going to be turned on.

HT: Go back into the realm of conjecture for just a minute. When you were making your trip around the country and looking at machines and wanted to build your own machine, you had unique problems at RAND.

BG: Yes.

HT: It was a one of a kind at that time. Where did you see the future of computing and computing needs at that point?

BG: I didn't see beyond the end of my nose. As far as I was concerned, it was just an enormous opportunity to have a lot of fun and build something that was challenging. I didn't see the future of computing, really. I was right down there figuring out what kind of a socket to use and whether to use this tube or that tube and I didn't see any of that, I'm sure.

HT: How about conversations around you? RAND was the kind of a place where there was a lot of intellectual interchange.

BG: Yes. Okay, now, I'll bet you that there are good memos in the files at RAND. They were one of the first "future" companies trying to really use the scientific method to predict what technology would be in the future, in the ten or twenty-year future. I know there were guys there that were talking about what computers might get to and I'm sure they missed it by a factor of a thousand. (laughter)

HT: Well, some of those are mentioned in Fred Gruenberger's history.

BG: Yes, 17-701's will saturate the world. I remember that.

HT: Fred quoted somebody as saying that in ten years there would be 2,000 programmers. That was a 1950 quotation.

BG: Well, I probably thought that was ridiculous. Where would you ever need 2,000 programmers. (laughter) No, I absolutely had no insight into what was going to happen.

HT: Well, how about the inter-relationship between work at RAND and general development computational ability on the West Coast? How do you see the inter-relationship between what was going on at Northrop, what you were doing, what was going on at UCLA, at Berkeley and many other installations on the West Coast?

BG: Another machine we visited, as a matter of fact, or had contact with, was the one at Berkeley which was called the CALDIC.

HT: CALDIC. The one that Paul Morton built.

BG: That's right. To come back to your question, what was the effect of RAND, the JOHNNIAC didn't have any

engineering effect on anybody. The real effect of RAND, I believe, was in some of the ideas that Cliff Shaw and Al Newell and those guys started evolving, that were based on the availability of the JOHNNIAC, and later on the 701 and 704 and so on. They started working on problem solving techniques and the JOSS program. JOSS, I think, had a big influence on the guys at Berkeley. As a matter of fact, the nucleus of that group, by the way, is here now, in the Palo Alto Research Labs. Butler Lampson and Peter Deutch and some of those guys who did the original time sharing development atBerkeley on what was an SDS computer are now part of this research center. Very creative guys. Extremely strong guys.

HT: Of course, that set of specifications you wrote for core memory was a landmark of that time.

BG: Well, yes.

HT: Because it really hadn't been done before.

BG: Yes, but I wouldn't put that up--for instance, I the think the specifications that the SEAC guys had for their engineering designs were equally rigorous respectable.

HT: Well, I guess I'm thinking in a more general category.

BG: I'm not trying to be modest, I'm just ...

HT: Oh, no, but this is an important piece. You think of standards in contemporary terms, you think of machines passing tests of some sort. We're talking about an era when there weren't standards, weren't tests. You know, somebody says, "Well, here's the machine." How do you see if it's acceptable? You go back to what you told us to do and if we do what you told us to do the machine is yours.

BG: Yes.

HT: But what do you do on it to see if the machine really works? In setting standards for the core memory, you were in a sense beginning this concept of establishing some levels of standards. I don't know if that was a first time attempt, an only time attempt, but it was one of a number of attempts --

BG: That's right. But there were other standards.

HT: that people are still fighting about.

BG: Sure, that's true. There may have been other things, that is we had built into the machine hardware to allow a

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memory diagnostic to run, which was not commonplace then and is today, I think. Also, we built into the machine the idea of the channel, which was not in IBM machines until the 360 series. Data could go between a secondary memory drum or disk memory directly to high speed memory, which was core, without the conscious part of the machine being involved. That is, it could make block transfers. Now, I'm not certain, but that idea was probably borrowed from somebody else. But it was implemented there.

IBM, through their association with Lincoln Labs, as a deriv tive of the Whirlwind project, were also very concerned with proper conservative engineering design. I think that's where it got into IBM. I think that's really the way IBM's good design was initiated. Because there was not good design on the 604.

HT: I guess it's just a very difficult question to try to repaint today what the computational environment was like on the West Coast and how the various groups affected each other through social contacts, intellectual contacts, etc.

BG: Yes. Madden, Brown, and Armer are the guys that were really key in that.

HT: Because suddenly the ability to do computations generated a lot of stuff.

BG: That's right.

HT: And that's the key thing.

BG: That's right. The availability of the tools stimulated all sorts of innovations.

HT: It's how these stimuli operated on each other that's fascinating, and the flow of ideas between people as they move around. I mean, I mentioned your move into Telemeter and people coming from other directions.

BG: Well, that's right. Northrop was an enormous nucleating factor. The guys that came out of Northrop and then formed Computer Research, that later became National Cash, spawned a lot of things.

HT: During the JOHNNIAC period, they were building the CRC machines.

BG: That's correct.

HT: How did you evaluate that series of machines?

BG: Well, we thought that we would buy a CRC machine. I've forgotten what model number it was but it was one of the drum machines.

HT: 102?

BG: Something like that.

RM: It was a general purpose machine?

BG: Yes, it was. Yes, other than the DDAs.

HT: That was the 101.

RM: That's right.

HT: They had a number of series.

BG: Have you talked to those guys?

HT: Yes.

RM: Yes.

BG: Jerry Mend son?

RM: Jerry, yes.

HT: Sprague.

BG: Yes.

RM: Sarkissian.

BG: Yes, right. Where is he these days?

RM: He's got a company in Costa Mesa called Major Data.

BG: It's still there?

RM: As a matter of fact, Speer's there.

BG: Oh, you mean Bill Speer?

RM: And there's someone else.

BG: Speer, Sarkissian and somebody else by the name of Saylor. Bill Saylor

HT: S, S, plus "S".

BG: Right.

RM: I don't know where Saylor is.

BG: He is still with them.

HT: That was thr original SSS Company.

BG: Right.

HT: Somehow I had the misconception that Floyd Steele was one of that group.

RM: Yes, so did I.

HT: That's somebody we've lost track of totally.

BG: Oh, that's a shame.

HT: Do you have any idea where he would be?

RM: Well, we've got a clue, but it's not very good. And that is Palo Alto.

BG: He was associated with Litton the last I remember.

HT: That's much too long ago, though.

BG: Jack Connelly, who came to RAND for awhile and then left RAND to go to Litton was in charge of the building of the Floyd Steele differential analyzer. Floyd Steele was in Laguna or someplace, you know, he didn't want to come to the big city. He was responsible for the logic design. He was a great one for minimization.. In those days, you know, if you take a diode out, boy, it didn't matter how complicated it got, even if nobody could understand it. But he was obsessed with the idea of logic minimization.

HT: If you look at the MADDIDA, everything had multiple purposes.

BG: Sure.

RM: His concept was the one flip-flop computer.

BG: That's right, yes.

HT: Another man that we can't seem to make contact with is Glen Hagen. We can't seem to locate where he is. Do you remember him?

BG: Sure, I remember him. I don't know where he is. That was ALWAC, wasn't it?

RM: Yes.

BG: Hagen was an entrepreneur, he wasn't primarily a technical guy. But he was the one that got Wennergren or whoever it was.

HT: Wennergren was the Swedish millionnaire.

RM: Of course, I think Hagen had Williams and Williams was, I believe, the engineering type of the two.

HT: Who were some of the other people we were trying to locate? It's a large crew. There was a large number of people involved. Some of them gave contributions.

BG: Have you got a copy of this meeting? Have you seen this thing?

HT: 1953? No, I haven't.

BG: There were a lot of people that were there. This was a meeting at UCLA and it reviewed the status of things as of that date.

RM: I haven't seen that one.

HT: No, I haven't.

BG: Well, we can make you a copy of that. (laughter)

HT: Here are some of the names I've been looking for in terms of other places. Eldred Nelson was at Hughes earlier?

BG: Yes, right. Harry Larson worked for Eldred Nelson.

RM: Now, I've pretty much got that. Frankel worked with Nelson.

BG: In fact, it was Frankel and Nelson that did some of the really early papers for the ENIAC.

RM: Metropolis, Nelson, and Frankel did the ENIAC work. Frankel and Nelson had a consulting company in L.A.

BG: Yes, but weren't they using the ENIAC? I think that was the only machine around, as a matter of fact, when they did some of their earlier work together.

HT: Oh, yes, Metropolis, I think, ran one of the very finest problems on ENIAC.

BG: But it was Metropolis and Nelson or Metropolis and ...

RM: Frankel.

HT: No, no, it was Metropolis and somebody else, but it wasn't Frankel.

BG: Is Nick still in Chicago?

HT: No, Nick's at Los Alamos. Now, who were some of the other people that you ran into in that period in terms of West Coast development primarily that we might have overlooked?

BG: Yes. You're thinking of the --

HT: Of course, we know about Paul.

BG: Arsenault. Let's see, there was an effort going out at the Naval place.

HT: I just talked to Professor Lehmer a couple of weeks ago.

BG: Oh, yes. Wonderful guy. Norm Kreuder, he was the guy that later did the series of Burroughs machines.

HT: There's a new name for you.

RM: Oh, yes.

HT: K-r-e-u-d-e-r?

RM: Where is he now?

BG: He's at XDS, in El Segundo.

RM: In El Segundo?

BG: Yes, he just left Burroughs six months ago.

HT: I'll copy that name down. Mina Rees I just talked to. Rex Rice I'll be seeing tomorrow.

BG: Yes, right, he's just up the street, I think. And Jerry Mendelson.

HT: Warren Meisner, where is he?

BG: That's right. I think they were big on analog computing and hybrid computing over at NOTS Corona. That's where I was trying to think of.

HT: ... is at Berkeley now.

BG: Jack Nash, I think, is at Lockheed.

HT: Yes, I've talked to Jack Nash and he was just killed in a freak accident recently.

BG: Oh, no.

HT: Yes.

BG: Oh my. Automobile?

HT: No. It was after the Apollo 16 launch, the most recent launch. He was having dinner in a restaurant and something stuck in the windpipe, a real freaky thing. They treated it as a heart attack and when they found out what it was it was too late.

BG: I hadn't seen him for a long time.

HT: Well, he gave me a lot of information from the LLIAC period and some documents even for SILLIAC, which he had.

BG: SILLIAC. [Laughter]

HT: You know that was the semiconductor copy of ILLIAC.

BG: Oh, I see.

HT: It looks exactly like it. That stands for silicon, not silly. [Laughter]

BG: Bill Bell was an early contributor, and he's dead. I think he died of multiple sclerosis or something.

RM: Someone else I'm trying to find is Bob Beck.

HT: Ev Yowell I just talked to, also.

BG: Where's he?

HT: He's at Dayton. He's got his own consulting firm.

BG: I see.

RM: Do you recall Bob Beck?

BG: Oh, yes. Yes.

RM: He's someone I'm trying to locate but I could probably get him through XDS, I think.

BG: Well, he's been gone from XDS for a long time and he's got a ranch in Montana.

RM: Yes. He's a big shareholder of XDS. [Laughter] It might be possible. That's why I thought of getting him that way.

HT: In terms of RAND, one question because I've never been there; what's the best way to approach RAND in

order to see what in their archival material historical sites is available?

BG: I'd say through Willis Ware.

HT: Start with Willis Ware?

BG: Yes, I would because Willis was there through all of the important part, contributed to it, knows the people and is now associate director or something. He's high in the administration.

HT: Right.

BG: At last I heard.

HT: Along with some of the material you mentioned today, I would like to see some of the very early problems run on the different levels of computational equipment up through JOHNNIAC. To see what the problems were first on the REAC and then later on the CPC's and then on the JOHNNIAC. Even some of the early prime number problems.

BG: Well, we were trying to do problems on the REAC that today you would certainly solve digitally. We were trying to do non-linear problems and problems that involved inequalities, things like game problems. Now, that was big in those days.

HT: Is what you were getting into--what's the name of the man who works in linear analysis, the phrase has escaped me. I don't know what you call it now. (laughter) Linear systems.

BG: Oh, yes, I think I know what you mean. I can't say it either. (laughter)

HT: I know what it is.

BG: Linear programming.

HT: I'm trying to think of a method. Simplex method.

BG: Yes, that was there. I didn't understand that. Still don't.

HT: And the Monte Carlo techniques.

BG: Yes.

HT: That started right there.

BG: Sure. And that machine with all the ball bearings was essentially a Monte Carlo solution to a statistical problem.

RM: Right in the beginning you talked about the analog devices of McCann?

BG: Oh, yes. Gilbert D. McCann, Cal Tech, was in charge of computation there and was the chief exponent of the so-called passive analog computer or the network

computer. That is, the electronic analog computer used operational amplifiers to be the explicit analog of the operation of integration. To do partial differential equations was difficult. They were good for ordinary differential equations. But if you wanted to do partial differential equations, which were a major concern to the aircraft industry, particularly in the flutter problem you had to turn elsewhere. The most serious computational problem in aircraft in the 1940s was how to figure out aeroelastic instability. You know, where the load increases the wing bend and the load gets worse so the wing falls off.

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To be able to compute the stiffness as a function of position, they'd have a hundred girls, or often they'd actually be males, and they'd be pounding away on desk calculators trying to solve that problem. Gilbert McCann came along with this passive thing which would make a lumped electric circuit analog where inductance was mass and capacitance was spring and resistance was damping. They'd hook up these arrays of electrical components which would be the electrical analog equivalent.

HT: Then you could do the partials of them as a function of time?

BG: Well, no. Now this is a direct model. You'd put a voltage on this end of it like pulling down on the wing and you could get a distribution of deflections as a function of time. And so Gilbert was the guy who brought that art to the highest state of perfection.

HT: I didn't realize that you could even get a partial differential equation on an analog machine at that point in time.

BG: Oh, yes. You bet. It was very successful.

HT: I just hadn't run across any others.

BG: Well, those were taken over and built commercially by the William Miller Corporation in Pasadena and somebody at Burroughs was a part of that; a guy who's been around this business for a long time was at Burroughs and he was in charge of their research or machine development, and he worked on those machines.

HT: Is McCann still at Cal Tech?

BG: I think so. Yes. He's still going great and IBM installed a lot of machinery over there for him and they did a lot of development in interaction using displays. A lot of which was pushed in biomedical

engineering where they're taking insects and instrumenting the optic nerve and finding out a lot of things about brain research. Wooldridge went over there and wrote the book, "Machinery of the Brain", on a year sabbatical, well, sabbatical from industry. Excellent book. And a lot of that either led to or coupled in with McCann's work. So he's still right in there, you know. He made the transition.

HT: Was Richard Fein at Cal Tech involved at that point in time in any of these developments? His name has come up in just a couple of places but not prominently.

BG: No, I don't remember that name in that connection. I could probably find the names of some other guys who worked with McCann.

HT: McCann's a key name that we've been looking for at Cal Tech.

BG: There was another meeting that was referred to that took place in Pasadena in about 1947 or something like that. Let me see if I can find it.

HT: Oh, is that the one where, no that was later. I was thinking of a Pasadena meeting in the fifties. I'm sorry. I'm thinking of another group.

BG: Yes, now this was earlier and I found something about that somewhere here.

HT: This is one of the symposia that was held at Harvard under Aiken's aegis. [Referring to program announcement]

BG: I was over working with Huskey and several of us went to that.

HT: This was the 1949 one and I see a paper here by George Brown on the linear inequalities. "Monte Carlo method" Stanley Ulam at Los Alamos.

BG: Right.

HT: He's a good one to talk to.

BG: Oh, yes.

RM: Where is he?

HT: He's either at Cal Tech or UCLA or Berkeley, or Stanford. He's in this general university area. I'll check him out for you.

RM: Okay.

HT: Emanuel Vallarta was the gentleman that Norbert Wiener was with in cybernetics.

BG: Oh, yes?

HT: Derek Lehmer's mathematical methods. Oh, you have an historical talk by Louis Couffignual and a futures talk by Louis Ridenour. [Laughter]

BG: Well, damn. That was an interesting meeting in which Gilbert McCann among other people talked. Maybe it's in this stack. Let me see what's in here.

RM: That's some stuff -- let me see.

BG: That's what you came in with, I think.

HT: I'm not sure I have this program. The one I have is a 1947. I think I would like a copy of this just to play safe.

BG: I don't know what happened to that but I'll find it somewhere or another. There was this meeting in Pasadena at which Gilbert McCann talked. In those days he was putting down digital computers a lot. After all, these things are just something that will help you count on your fingers. The real way to solve partial differential equations, and he was right then, was to do it with this kind of an analog computer. Douglas Aircraft bought one, for example, and several other people did.

HT: Right in the sense that at that time these could be done if there were special purpose machines.

BG: No, they weren't really special purpose except in the sense that they were for partial differential equations. But you could simulate any kind of a problem by making

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an electric analog. That's in the same sense that a hybrid computer is still a more economical way if you've got an enormous number of ordinary differential equation runs to make that are non-linear and perturbed by noise, say, in a missle problem, which is where the hybrid analog digital computer comes in. The digital computer to keep track of things and do some of the very critical computations. But the analog computer to do all of the integrations fast, because you could run maybe a hundred times real time. It was a more economical way up to at least three years ago. It may not be true now because the cost per multiplication is going down. But there were a different kind of stability criteria that comes in when you try to do differential equations on a digital computer. You know, there's the discrete nature of simulating integration discretely, by multiplications and adds, that will introduce instabilities into a problem that are not really there, depending on the kind of integration algorithm you use. So the analog computer, although it has its own stability pecularities, at least did not have those. That's for ordinary differential equations. To make them solve a partial differential equation was very much more difficult.

HT: Well, in the early days of analog computers, the problems that people were dealing with didn't involve partials. They weren't worried about them.

BG: Well, the RAND people were solving partial differential equations.

HT: But, in the 1930s and 1940s the problems that were around were ordinary differential equations, primarily.

BG: No, sir. Not the flutter problem. The aeroelastic problem is where this acre of Marchant and Friden calculators and people were being used.

HT: Did that come in that soon?

BG: Right. The airplanes were breaking up in the sky you know, and those guys had to try to predict.

HT: In the pre-1940 period.

BG: Pre-1940. Now, I'll have to take that back. This was in the 1940s.

HT: Yes, yes.

BG: Okay.

HT: But in the pre 1940 period that really wasn't bothering anybody.

BG: I think that's right. They did it by the seat of their pants.

HT: Right.

BG: They'd send a test pilot up and fly it, and maybe go ten miles an hour faster, and I think perhaps the wings would come off.

HT: The movies did a good job of telling us how courageous the test pilot was. (laughter) How much faith he had in the seats of the pants of the designers.

BG: Yes, they bid on those jobs, you know. They still did when I was at Douglas.

HT: I think it was pretty much near the beginning of World War II.

BG: Yes.

HT: At least I hadn't run into anything prior to that in looking through the literature and aeronautical engineering and structural drawings.

BG: Right. There was a fairly fundamental thinker along those lines at Douglas. Paul Somebody was his name, that really introduced the mathematical method of solving that problem.

HT: The wind tunnels in the 1930s were pretty crude.

BG: Sure. It was hard to make a model.

HT: Hard to get good data out of the wind tunnel in that period.

BG: Well, but you wouldn't get aeroelastic data because it was too hard to make a model that had the same kind of stiffness and mass distribution as an actual full-sized wing. So that was the real problem. Furthermore, the problem was "What stiffness and mass distribution should we make in order to have it be stable?" So they were going through all these different nodes. I'm sure they didn't use as many as a hundred. It would take them weeks just to compute one cycle. The best thing to do was just build one and then go out and shake it in the laboratory.

HT: Which is why a lot of prototypes were built of airplanes before they were put out in mass production. (laughter)

BG: That's right.

BG: The tremendous expense of doing that was really much of the stimulus for computing on the West Coast. It was that kind of problem that was terribly expensive and terribly important during the War. Because if you want to make fast airplanes, and there was no way of predicting what was going to work, no feasible way of getting the computation done, that's why they went in for all of these funny ways.

HT: You mentioned that you started with Douglas and Project RAND in essentially 1941. Where had you gotten your training before that?

BG: Well, I graduated from UCLA in 1941 and it took me six years to get through to get an AB.

HT: In electrical engineering?

BG: No, they didn't have an engineering school. It was in physics and I fixed radio sets. I had a forty-hour a week job fixing radio sets. I learned about electronics and reliability the hard way. (laughter)

HT: You know, that little hand book for radio electronics or what have you, the section on electronics in that period was still a good beginning course. The radio handbook...

BG: The "Radiotron Designers Handbook" or something like that?

HT: Something like that.

BG: Yes.

RM: Is that the one you recommend?

HT: The one I recommend to you. If you want a quick course in electronics...

RM: Quick, down and dirty.

BG: Well, it will be vacuum tubes instead of transistors which might be a little distracting.

HT: We're dealing with vacuum tubes period.

BG: Ah, so I see, okay.

RM: That's the thing here.

HT: This is the period that we're concerned with. Once the transistor comes in our interest fades out.

BG: Yes, I see.

HT: You know, we're not interested in Bobbi turning into an electronics engineer.

BG: Okay.

HT: In terms of the technology what we're dealing with is the first generation, zero minus one generation of the machines.

BG: Yes. Well, the Huskey handbook covers that.

HT: Right.

BG: I think there's a section on transistors in it, but it was not the major part of the book.

HT: That was about 1957?

BG: It was like two or three years in putting it together.

HT: That's very good. Huskey and a few other people, or Huskey and one other person.

BG: Huskey and Korn, was it?

HT: Korn, that's right. Huskey and Korn, K-O-R-N.

BG: Yes. Korn was a big man in analog computing and wrote a couple of books on the history of analog computing.

RM: Do you remember the title?

HT: I have it at home.

BG: Korn and Korn. You mean on analog computing?

RM: No.

HT: No, the Huskey one. He's a very interesting person.

BG: Oh, yes. But he wasn't primarily an engineer. He was a good mathematician. He did everything on the SWAC, including the welding the console together. He just did everything.

RM: He set it on fire, too.

BG: Yes, but he's a great person, you know.

HT: Well, his Ph.D. in mathematics is on a higher plane and with minimal surfaces.

BG: Sure, and he had the guts to go ahead and tackle all of that and he pulled it off. But there were a lot of misconceptions that mathematicians have about electronics.

HT: But the interesting thing is that it worked and it worked well and it worked for a long time. (laughter)

BG: Yes.

HT: There's another area and that is when you are building a machine from the standpoint of the engineer, you have certain goals, certain standards, certain things that you know have to be done. Then there's the mathematician, the physicist who says, "I want to be able to use this machine." Now there has to be communication between these groups. Maurice Wilkes talks about the kind of a tension that he felt existed and still exists between mathematicians and engineers.

BG: I can give you a very good current example.

HT: I was going to say would you comment on this?

BG: ILLIAC IV, which is right down here at Moffat Field. The most ambitious computer project ever, I guess, in terms of money. Designed by mathematicians to solve mathematical problems, partial differential equations, the weather problem, with completely inadequate attention paid to the design of the machine from the point of maintainability. Even error detection. Between the secondary memory, which is a large set of Burroughs disks, there is a one thousand bit bus, one thousand bits in parallel that go between the drum memory and the central part of the machine, without one bit of

redundancy for error detection. That's ridiculous. An engineer would never have done it that way, but it was designed by mathematicians; Slotnick, you know, is a numerical analyst.

HT: Well, could you comment on this kind of tension between the two groups in the RAND period?

BG: Oh, we led a sheltered existence, you know, we were fortunate enough to have very understanding mathematicians. I don't know why this was true, but it worked very well at RAND and we were able to communicate in such a way, I think, that allowed us to make a machine that satisfied the needs of the mathematicians and also allowed us to put all the junk in that we felt should go into the machine. That hasn't been the case always in projects.

HT: Well, when we say Wilkes generalized in the sense that he felt this was a continuing kind of thing ..

BG: Yes.

HT: ... and one that ..

BG: I think ILLIAC IV is a beautiful example.

HT: Contemporary example, and I guess if we went back to the 1945-1955 decade we would find many such examples and instances.

BG: Yes. I don't know what went wrong with EDVAC, for instance. Did it function?

HT: By I think about 1952. Even though conceptually it led to EDSAC which was working in 1949.

BG: Yes, right.

HT: I mean EDVAC was the instant result of ENIAC.

BG: Yes.

HT: And EDVAC was conceived before ENIAC was completed.

BG: Yes.

HT: As soon as they saw the jump from what ENIAC was to a general purpose stored program computer, this became EDVAC. But I don't think that it worked until about 1952. Maybe 1951.

BG: Well, it's also true that there was the software/hardware problem and that worked out well at RAND. I think, generally speaking, that it's certainly true that the hardware guys were the ones that built the machine and the software guys took what they got, pretty much. I've heard it stated, and I think this is true, that that's colored the whole development of software in that, perhaps by a natural selection process, the guys that went into and stayed in software were professionally inclined to solve difficult problems without altering their environment, the environment given them by the hardware engineers. Whereas the hardware engineers were the guys that were in there trying to change the environment in what every way

they could, you know, build a machine and make it work. I don't know where I heard that, but I think it's a very true observation. Increasingly now, and particularly since OS, is the recognition that software is where all the pain and strain is, so we'd sure as hell better do something to make life easier. Shift some of that responsibility back on those engineers. They understand it now. I don't know if that gets at what you were asking.

HT: I think you've responded very interestingly to an open ended question I dropped on you. Can you think of any other areas, Bobbi?

RM: No at the moment.

HT: Okay. Let me turn the machine off. [Recorder off] The Toronto business. [Laughter]

BG: The thing I remember most vividly was the enormous conflict on the theory of the Williams tube between a guy by the name of Katz.

HT: It was Katz at that time, it's Joe Kates now.

BG: Joe Kates, right, and a guy at NBS in Washington, D.C. who had a different explanation of how it worked and those guys were at it hammer and tongs. So I went

up to talk to Kates, or Katz. I believe he had an associate by the name of Rats.

HT: Rats, right, Katz and Rats. [Laughter] And Gottlib was essentially the head of that.

BG: Yes. So I went up there, again I can't remember if this was before the JOHNNIAC or not. It may have been, but we were trying to understand this beast called the Williams tube.

HT: Well, they were doing a parallel machine.

BG: Yes.

HT: And Katz had done a tube, I don't know if it was on it then, call the additron.

BG: Right. That was a special tube that would do binary addition problems. Seems to me it had deflection plates that would deflect the electron beam to different collectors.

HT: What was your impression of that prototype at that time?

BG: Hm.

HT: It's interesting because from the standpoint of theoretical physics, it still, I gather, is an interesting question whether it would have worked or not.

BG: They were more concerned with inventive components and other derivitives in this additron than the Williams tube itself. Of course, that came out as a commercial product.

HT: Somebody made a few but that's all.

BG: Is that all? Okay.

HT: One of the local companies did manufacture a few, but it didn't go anyplace.

BG: I really don't have a clear recollection of the machine because my primary purpose was the Williams controversy.

HT: Thanks. I just wanted to get that on tape because very few people know about that project.

BG: Well, I found that trip report of going to Toronto.

END OF INTERVIEW