Interview with Jerry Mendelson
Conducted by Robina Mapstone
Sherman Oaks, California
September 6, 1972

RM: This is Bobbi Mapstone. The date is September 6th and I'm talking to Jerry Mendelson at his home in Sherman Oaks, California.

We've already discussed some of the questions. I was going to ask you if you knew where Greg Toben, Bill Woodbury or Lee Ohlinger could be found.

JM: I don't know where they are now, but I could do some work on it. Ross Miller or Fred Stevens at Northrop are probably the better sources for locating Lee Ohlinger. I haven't heard of Greg Toben for years. I saw Bill Woodbury at one of the conferences so I know he's still around.

RM: Another name you'd mentioned was John Postley.

JM: I can give you his address. He's a vice-president at Informatics.

RM: And Informatics is in Los Angeles.

JM: It's in Los Angeles and he lives just over the hill here.

RM: You said that he was a user of the CPC and a great source of information.

JM: Yes. I first met him at the National Bureau of Standards

where they had both card programmed calculating equipment and the SWAC computer. He came to work with me at Northrop and ran the first card programmed calculator center that the missile project had of its own.

The original Card Programmed Calculator was invented by Woodbury and Toben. It consisted of a 405 tabulator and a 603 electronic multiplier, IBM's first electronic multiplier. Woodbury and Toben conceived the idea of interconnecting the tabulator and the multiplier and sequencing it from control codes punched in the same cards that supplied data. They got IBM to build the machine over IBM's initial objections.

Subsequently, IBM built a commercial version. Northrop sent Woodbury and Toben to help with the design, and I described that in my previous tape.

RM: That was a pretty unusual situation, wasn't it?

JM: Very. The Snark missile project got the first CPC and we began supplying computing services to the rest of the company as and when time was available. By we, I should say Woodbury and Toben. In order to promote the idea of using computers, they went out and helped people organize their computing work for the original card programmed facility.

RM: In other words, Snark had first kick at the cat.

JM: That's right, originally. As time went on, the other departments of the company became more and more dependent on it and more and more demanding of its services, so it became one of these big, beautiful, political internecine warfares. That's when Ohlinger and Rice made their power move and grabbed all the computing facilities of the company, taking it away from the project that created it originally. That's why Ohlinger and Rice are no friends of mine.

RM: I understand.

JM: Okay. Now after that we had trouble getting time on our own machine, as it were. I ultimately acquired my own card programmed calculator facility and Postley came to work with me to run it. So that's when I met John. He reduced the calculations from something like 48 hours turn-around time, to some four hours on the same machine, from the times we were getting from our own people to the times that he produced.

RM: He was a great utilizer.

JM: Yes.

RM: I heard that somebody came up with a way of programming the plugboard to handle general purpose projects. Does that ring

any kind of a bell with you?

JM: Well, Postley was supremely good at that. There were 60 program steps originally and it ultimately expanded to 180. Every time you initiated the electronic program the machine went through all 180 steps. On a particular initiation, you could specify a control code which said whether a given step was or was not to be executed.

In other words, the board had — I don't remember how many-- ten or fifteen different control codes. You'd say, "Execute the board under control one." Then every step that was associated with control one would be activated on that sweep of the 180. So the problem was to come up with a very, very highly convoluted and interlaced series of steps, a sub-set which would perform a given function. You would do program one and maybe you'd get the sine by using steps one, seventeen, twenty-two, twenty-four and twenty-nine. Program two would give the cosine by using some steps that were in common with this sine and some that were not. So it was a question of how tightly integrated you could make the board so as to perform the maximum number of functions as sub-sets of that.

RM: Got it, yes.

JM: Okay.

RM: The name that I just was looking for was Jess Wright or White. Does that name mean anything to you?

JM: It doesn't ring a bell at all.

RM: Okay.

JM: Postley was very good at that.

RM: So he would be a good person to talk to.

JM: He would be very good.

RM: I take it that the Snark project was already underway before the CPC came along.

JM: Yes.

RM: What was available for doing these incredible equations?

JM: There was nothing. We had to use desk calculators. The CPC was originally developed to study the dynamics of the missile over the flight path when under control of the guidance system; how it would behave as the guidance system tried to hold it to a given path.

RM: Could we say then that the Snark project probably was one of the key ...

JM: It was the spawning ground for computers on the West Coast. It really was. I can trace most of the early computer

industry to the Snark project, in one way or another. I can trace you several family trees.

For example, the Scientific Data Systems history goes back to Packard Bell, and from there to Bendix, and from there to the spin-off of the Glen Hagen group which came out of MADDIDA. Bob Beck was one of the original designers of the MADDIDA 44, and Paleusky joined back when it was Bendix. Let's see who else there was. Stan Frankel at Cal Tech conceived the Librascope LGP-30. Frankel used to come over and talk with Steele and Eckdahl about the Boolean algebra logic techniques which were employed in the MADDIDA. MADDIDA was certainly one of the original key users, if not the only one, of Boolean algebra for the logic design. Frankel got many of his fundamental ideas and design concepts out of the Northrop group. He designed a machine at Cal Tech which they sold to Librascope in exchange for the first machine.

RM: I see. What did Librascope do? I'm not familiar with the company.

JM: Librascope was in the same kind of business as Northrop and Nortronics. They built a DDA for airborne and ground based military applications. They bought Frankel's design for the

LGP-30 and went into commercial work with it as well.

RM: When was this, do you know?

JM: Oh boy, early 1950's. Then another thing: you could trace what's now Calcomp out of that same organization.

RM: Out of Librascope?

JM: No, out of Northrop. I'm going back to Snark again. At the time Northrop was working on the Snark, North American was working on the missile called the Navaho. They were completely competitive missiles and guidance systems. The people on the Navaho project wanted to get computer know-how so they bought a MADDIDA 44 from Northrop and a CRC 105 or a predecessor machine to that. Anyway, they bought one of the very first CRC machines when the company was formed in 1950, primarily to get the technology and the logic design principles. They didn't buy the machines because they wanted to use them. They wanted the know-how that was behind them. One of the key guys in that operation was Les Kilpatrick who's President of Calcomp. So there have been spin-offs out of North American, out of the Autonetics division and so on, all of which trace back to getting the technology through the path from Northrop.

RM: And Snark.

JM: And Snark.

RM: Which brings to mind another thought, and that is, what were the patent problems of this period? It seems like there were none.

JM: I don't know. The only patents that seemed to do any good at that time were the original Eckert and Mauchly patents on the BINAC which transferred to Remington and Sperry Rand.

RM: Right. But as far as what was happening on the West Coast, the ideas seemed to stay with the people. As the people moved, they took their know-how with them, and then they built another computer and advanced on it. Were there no problems then in patenting?

JM: No, there was very little. I know at Northrop they didn't do any patenting until after the CRC people left. They didn't get to it as it were. As a matter of fact, the CRC group took the patent attorney from Northrop who was on the job with them. (Laughter.) He became the CRC patent attorney. (Laughter.) So then Northrop was out in left field with respect to patents. They prepared a patent paper describing the machine in great detail. The only claim they made at the time, as I recall, was on the scaling technique which I had developed. So they

were trying at least to get their foot in the door with something. I don't know whether that ever was prosecuted to completion or not. They might have patented under Hagen when they formed the 44. They might have done something there.

RM: But on the whole, the ideas just flowed very freely. If you wanted to buy a computer and pull it apart and figure out how it was put together, then that was up to you.

JM: Yes. That could still happen. Do you know the two page ad in the Wall Street Journal on IBM's announcement of virtual memory? Well, I have a patent on it that dates back to 1966. RCA had virtual memory in their machines and Burroughs has had virtual memory. I have a patent on the dynamic address translation which is the fundamental technique of the method. I don't know how everybody does it, but they seem to.

RM: There are ways around it.

JM: There's a way around everything.

RM: So this was not an issue in the early days.

JM: It didn't seem to be.

RM: Okay. Can we talk a little about DIDA? Was it called DIDA?

JM: It was DIDA.

RM: Was this first machine which was at Northrop actually built and used?

JM: Let's make sure what we're talking about; that's DIDA, which was before the MADDIDA.

RM: Yes.

JM: There were some prototypes of the integrators built. I remember seeing two. You can generate a sine or cosine function with two integrators, and you can generate an exponential function with one. So they built two of the DIDA integrators and ran them. I saw them running interconnected to form sine and cosine generation and I saw one running to form exponentials. When they proved they worked, they gave Hewlett-Packard a contract to built a number of the units. I don't remember the number, but there were probably 15 or 20 or them. Sarkissian may be able to tell you that. Sprague certainly could.

During the time that construction was going on, Steele conceived the idea of putting the entire integrator structure on a magnetic drum, and using a single arithmetic unit to process all the integrators serially. They set to work on that with real vengence; it just opened up a completely new world for them because they could build 20, 30, 40 integrators in a very small

arithmetic unit. Because of the way the DIDA worked, it was no faster, even though it had all this hardware.

RM: Yes. And it was parallel, too.

JM: It was parallel hardware. It wasn't any faster than the serial DDA because of the way the algorithm of the hardware was organized. They abandoned the DIDA concept because it was clear that they had a far superior technology and approach in the MADDIDA. Hewlett-Packard delivered the DIDA units, but I don't think anybody did anything with them except shove them over in the corner. So someplace in this world there might be DIDA integrator.

They were very big. Each integrator was probably three inches thick by three feet wide by a foot or a foot and a half deep. So they would have stacked 20 of them up in a great big cabinet and then plugged them together. You set up the problem by plugging between integrators. It was just a digital analog of an analog computer. I'm sure I could reconstruct the algorithm for you if you wanted to, but I'd have to do the work to do it.

RM: Well, that's a thought for some other time, but certainly not now when you are about to go traveling.

Okay, so the DIDA was dropped and MADDIDA started. Where

did Steele get the idea of the magnetic drum? What had gone before in this technology?

JM: I don't know. Sarkissian can tell you, because he actually did the drum. I went to Northrop in February 1948 and the events of the DIDA and the MADDIDA took place prior to that time. It must have been in the middle of 1947 or something. When I arrived, the MADDIDA was in development and the DIDA prototypes were running in the laboratory. I arrived at that transition period. The idea for the MADDIDA had to have come in the middle of 1947, I would guess.

RM: I can't think of any drum machines that were available.

JM: No, they weren't, but I think the idea of using a magnetic drum as a memory device was in the literature in the scientific field at that time.

RM: It was just a matter of adapting it.

JM: That's right. It was used as a delay line and not as a memory in the way we might think of drums. That is, information was recorded on the drum at a record head, and it was carried on the surface of the drum for a portion of the drum revolution to a reading station where it was read off. Then, if it was to be remembered, it was re-recorded at the record head. In between

was an erase head that erased the information. So if you visualize what's happening, the information's really changing its position on the drum all the time because it's being written at one point, rotating 180 degrees, say, and then coming off the drum and back and recording 180 degrees from where it was last time. Actually we used 300 degrees of the drum's surface.

RM: So actually the information was in the read head itself.

JM: Oh, no, it was recorded on the surface, held there for a portion of the drum revolution, like 300 out of the 360 degrees, and then read off and erased. But at the time it was read off it was in a flip-flop. From the flip-flop it was fed back into the record circuits and put on a new location on the drum, so it was always processing around the drum. It was a very dynamic process. The drum was used as a delay line which was one of the early storage techniques.

Now the BINAC, for example, used a mercury tank delay line. You put acoustic pulses in, delayed them for the transit time of the sound pulse down the mercury, transduced it into an electrical signal and brought it and re-recorded it. We just used the drum circuits the same way.

RM: That really explains the drum very well.

JM: Steele got the idea of putting all the integrators in sequence on the drum. Actually an integrator consisted of two tracks; there was an R and a Y section of the integrator. The Y held the integrator and the R held the partial sum to date, and the information was transmitted between the integrators in the form of pulses, overflows out of the integrator. That's in that paper that you just got a copy of.* They were stored in a third very short delay line called the Z line, which stored the outputs of all the integrators. As the integrator passed through the machine, there was a code section which said which outputs of the Z line were to collect for its integrand inputs, and which single output on the Z line, output from another integrator that is, it was to collect and use as its independent variables. After the code section pass, then the operand section came; the sum of the integrand inputs formed by that time were added to the integrand and written on the drum. The integrand was added to the R register and stored on the drum, and the output was taken back into the Z line for use by the integrators. Twenty-two integrators flowed by and we processed each circulation time which was somewhat less than the drum time because we didn't

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* "Scaling Techniques for MADDIDA"

use the whole drum surface for that.

RM: So it really speeded the whole situation up considerably.

JM: It didn't speed it up because the total processing time for the twenty-two integrators was almost exactly the same as the processing time on a single integrator in the DIDA technique, all of which was going on in parallel. So everything came out roughly at the same speed but with much less circuitry. There were less than a thousand diodes and something like 52 tubes in the MADDIDA.

RM: That's bringing it down almost to a compact computer.

JM: It really was. The whole thing was smaller than this coffee table.

RM: That was incredibly compact when you think of what was happening with the digital machines such as the ENIAC and the EDVAC.

JM: That's right. The differential analyzer is an exceedingly simple machine concept. However, it was very messy to work problems on because it had some peculiarities and idiosyncrasies which you had to learn to live with. But it was a very, very clever idea. That was Steele's concept. He was an absolute genius with ideas, some of which turned out to be sensational and some of which turned out to be wild, and you just had to have somebody who could

select the good ones from the bad ones. That somebody was a guy named Don Eckdahl. He's Vice-President of Manufacturing at NCR in Dayton.

RM: Right. What about Steele? Do you know where he might be?

JM: Steele always was around La Jolla, California. I can give you the name of a guy who probably still is in contact with Steele. His name is Aaron Whitehorn. Aaron was with Steele subsequent to the CRC days. After Steele left the Los Angeles area, he was always around La Jolla and he's probably still around. I think he's fairly independently wealthy now. I think really he might have even been before.

RM: I would really like to get ahold of him. He seems like a very key person.

JM: He is. I think you'd probably get a much more objective and realistic picture of what was going on and what happened from Eckdahl, Sarkissian, and Sprague than you will from Steele. Steele was the wild-eyed dreamer and he didn't really see the world very objectively or rationally. He was not a guy who would ever finish anything. The MADDIDA is classic of Steele's. There was no point in building anything because he would obsolete

it before you finished.

They had exactly that trouble with him at CRC. It was an interesting story. At Northrop he just drove us crazy because we never could tie him down and get anything built, and whey they formed CRC he was just God to those guys. He could do no wrong. But they finally realized that their business would go down the drain unless they built something and sold it. Finally they had to essentially dethrone him. They actually had to force him out of the company so they could get to the completion of something.

RM: That's incredible. Another thought that occurred to me was why did the West Coast lean towards the analog devices rather than digital computers? I'm talking now about the large scale electronic computers like the ENIAC and the EDVAC.

JM: They really preceded the West Coast developments. I think you're thinking of why did the West Coast people do things like the DIDA while the East Coast people were doing BINAC. Well, Steele was an absolute fanatic on low component count. His dream was to build the one flip-flop computer.(Laughter.) He was almost paranoid about that. I recall vividly having an argument with him one time. When they built the first MADDIDA they had 52 tubes in it, and the way we read it was with an

oscilloscope. You looked at one of the read heads, it was generally the Y which contained the integrand, on the oscilloscope, and you synchronized at given point in time and you saw the output of the integrator as it changed. We were looking for and were trying to build some kind of a read-out device. Steele allocated five tubes. He said, "You can't use more than five tubes." They said, "Well how can you say that?" He said, "My God, we're doing the whole differential equation calculation, the memory and everything else with 52 tubes. It can't take more than ten percent of the machine for output."

I remember saying, "How do you know that you didn't do the easy part when you did the arithmetic and control, and that the output's really the difficult thing?" You know, that's what experience has finally shown us. You saw that Hewlett-Packard gadget. It's got five chips and the entire arithmetic control is on two of them. So that's the trivial part. The hard part is driving the displays, reading the keyboards and driving the magnetic tapes and discs. But Steele would just say, "nope, five tubes." They tried it with five and it was a dismal failure.

RM: So Steele went off on his own at some point, did he not?

JM: After he left CRC he developed a very, very small, almost

desk-size DDA at La Jolla, which Litton picked up. It became the Litton-20, and I think that's where Steele made a lot of money. Steele got Litton stock and Litton made that machine commercially, with only a very modest success if any. Rudy Rutishauser can tell you about that, and so can Aaron Whitehorn.

RM: Where are these people?

JM: Rudy is a stockbroker in Los Angeles, I think, and I can give you the address of Aaron Whitehorn.

RM: Okay. Somewhere I saw a machine called DICO connected with Steele. Could this be the same one?

JM: I don't know. He had another idea called DYFUNCTIONS which I never understood. Steele had a great habit of creating things and giving them names and I think he recreated the DDA and gave it a different name. But that may be hearsay.

RM: In your previous tape you referred to some dialogue or lectures given by Steele and attended by Eldred Nelson. You said this may be where Nelson got his ideas or main principles. Nelson was with Hughes, right?

JM: Yes.

RM: Did Hughes contribute to the pioneering of the computer field?

JM: Yes. Postley can tell you about that, and a guy named Bob Hayes who is a Ph.D. and a professor of Library Sciences at UCLA, and some of the people who are now at Airneutronics. Is Harry Larson a name you have?

RM: It vaguely rings a bell, but I'm not sure.

JM: Harry Larson's an old timer in the business. He and the others can tell you about the activities at Hughes at the time. Harold Luxenberg is another Ph.D. I don't know where Harold is now, but probably around the Los Angeles area.

RM: Do you know what his Ph.D. is on?

JM: Luxenberg's is in mathematics.

Hughes was building airborne computers and I know they used much the same kind of logic approach as was used at Northrop. I don't know where I heard this, I just remember the guys commenting that Nelson got an awful lot. That's hearsay evidence, again.

RM: All right. While we're on the subject of the Boolean algebra, Steele seemed to be the man who broke through on the idea.

JM: He was certainly independent and unique in this area in that regard. I'm almost positive that that was his own single idea and that nobody else contributed. Now, I think it happened elsewhere at roughly the same time.

RM: What about Shannon's work, for instance? Was it known out here? It was published, but had it reached the West Coast yet?

JM: It must have. What was the date on Shannon's work?

RM: That's a good question. I think it was in the late thirties or early forties.*

JM: It was on switching theory. Maybe when I think it was Steele's idea, it was Steele's idea in that he recognized the applicability to computer design.

RM: Well certainly the West Coast was ahead of the East Coast in utilizing the Boolean principle for a total machine; is that correct?

JM: Yes, that's my opinion. That's my understanding too. It was very, very clear in the BINAC, for example. At one time I had a block diagram of the BINAC but it got lost in moving around. It was very clear in that because it actually showed data paths with gate structures and control signals permitting signals to pass through the gates. You could actually see the layers of machine design. For example, there was an adder. You could see where they stuck a complementing device in front

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* "A Symbolic Analysis of Relay and Switching Circuits". Master's Thesis, MIT, 1937. Published in Transactions of the AIEE, 57: 713-723, December 1938.-- RM

of it inhibiting if you were adding and permitting if you were subtracting, so the number that came through got complemented before going through the adder.

Then for multiplication you had repeated additions. You could see layers of control on top of control in the gate structures all laid out geometrically. You could see the machine like the layers of an onion. (Laughter.) You could see how the designer went, "Now I know how to add, I can lay in subtraction and superimpose on this control to do multiplication and division." A layer down here would have a permit and then there would be a gate that would inhibit that under certain conditions, and then the next layer would outlayer the gate that would inhibit the gate that was doing the inhibit. So you could just see the layers of the onion very clearly. You could see how the circuits got locked into single functions, because it just wasn't geometrically possible to take a particular flip-flop or logic function that existed in one place and think of it dynamically as being in another place some of the time. That's what the algebraic approach did. In the 102A, some flip-flops would be used for reading magnetic tapes at one time, be used as a carry flip-flop in an adder at another time, and be used to hold one of the

bits going out of the typewriter at another time. It just isn't possible in the geometric approach to represent that, so it didn't really get thought of.

RM: When did the Eastern computers start to utilize Boolean algebra?

JM: I don't know that they have. (Laughter.) You know, there is a drawback to it and in today's technology it is not really utilized. The drawback is that machine designs got very highly convoluted because one particular element was involved in many activities, such as printing, reading tape, acting in a multiplication operation and reading a hole in a card. So the logic got very entangled. If anything went wrong with that element the machine would sort of go berserk. That was during the days when each logic element, each major element like a flip-flop, was a very, very priceless device. Today they put thousands of them on a single chip and it's better to spread them out and keep them independent. But Steele, who was rabid on getting reliability in a machine when we had unreliable components, was always trying to drive the component count down. So he was willing to pay the price of these very convoluted designs.

RM: He didn't have to worry about keeping the machine running.

I'd like to backtrack a little bit. You'd mentioned earlier seeing one of the notebooks from the MADDIDA. Where is this?

JM: This is with Dr. Teague at the L.A. County Museum. He's in the building that's one the northwest corner of the complex.

RM: Right. As far as you know, this is the only notebook that's around today.

JM: Yes, he contacted Ross Miller asking him for assistance on the MADDIDA, and that's the only thing Miller could dig up. It wasn't a total notebook, it was only the operating log from a given point in time. It was after the machine came under my responsibility. The notebook that would be priceless if we could ever find it, would be the one that Don Eckdahl kept. Because that's a notebook where page after page would be written: "The design has the following problem." The problem would be described, followed by a description of Steele's approach and then the actual solution which we worked out. So you'd have a problem and Steele's brilliant perception of how to attack it, then Eckdahl's brilliant execution implementation of Steele's sort of general direction. That notebook would really be priceless.

They're blue, patent-type notebooks, hard bound, and the

pages are numbered. They are about legal size notebooks. If you get to see Teague, he's got it down there. If you could ever get Eckdahl's, it would be a dream.

I can remember Sprague not really believing that Boolean algebra would ever work. He wrote some Boolean equations and then went down to the laboratory and wired together the logic elements to implement them. When it actually did what the logic said, he was exuberant.

I can remember the excitement of putting the logic into the MADDIDA. The MADDIDA logic was implemented with diodes. The original diode board was a three-eighths inch sheet of black phenolic in which they drilled holes and press fitted into the holes model airplane engine spark plug clips. Then the diodes were clipped or just pressed into these two vertically standing diode clips, and held in place like a spark plug at both ends. The other side of the pin stuck through the board and that's where the wiring was. So the whole machine was wired with none of the electronics in place, and then the wires went off to the tubes which were wired in place. So now to put the machine together to get it working, you plugged in diodes for the preliminary functions, such as getting some counters going. So

everything was fired up and they plug in enough diodes to make the bit counter work. A look on the scope and, by God, the bit counter was counting. That was just the most exciting moment; people were cheering the first time the machine added one and one and got two. (Laughter.) People were sitting there saying, "Now when we press this it should pick up that dy input and it should add that to that and this thing should count in that integrator." And when it did it, you know, it was really exciting.

RM: I'll bet. Did you all go out and get drunk that night?

JM: I didn't design that; I was only an observer and I don't drink anyway. But I'll bet you they did. They just had to.

RM: Those were pretty exciting days, weren't they?

Can we talk a little about Glen Hagen and his contributions?

JM: He's going to sue me for libel.

RM: We'll give him a chance to protect himself. (Laughter.) After Sarkissian, etc., broke off into CRC, Hagen was left at Northrop.

JM: Yes, there was a computer group in the Snark program and most of the key people went with Steele, Eckdahl, Sprague and Sarkissian to CRC. There were a few that were left behind. Either they didn't want to go or they weren't particularly

wanted. Hagen was one that was working on some odd ball things of his own and wasn't, in my opinion, particularly wanted. I don't think he could have gotten a job at CRC. So there were just the remnants of this computer group. But Hagen is a super salesman-- a promoter, entrepreneur type.

RM: He was able to succeed where the others could not.

JM: That's right. He talked Northrop into the validity of a commercial computer development.

RM: Which was the MADDIDA 44.

JM: Right.

RM: And was this machine built and sold?

JM: I don't know how many they sold, maybe 20 or 30 somewhere in there. I don't remember the price at all.

RM: Was it exactly the same as the MADDIDA?

JM: It was the same theory, but it was a different implementation.

RM: Can you enlarge on that at all?

JM: Yes. I described earlier on this tape how the code section of an integrator preceded the arithmetic processing of it. First you picked up the inputs to it and then you processed them arithmetically. The code section and the operand section were in the same two tracks. So the 22 integrators really consisted

of 44 pieces. There was first a code section for integrator one and then an operand section for integrator one, then a code section for integrator two and an operand section for integrator two. What they did on the 44 was to take the code sections out into separate tracks and offset them from the corresponding arithmetic section. So they were doing the code section of the next integrator while they were processing the current one. They rewrote the logic completely in order to double the effective number of integrators at the price of a couple of tracks on the drum and a little bit more parallelism in the logic.

RM: Did you design this?

JM: I did not, at all. That machine was designed by a gentleman by the name of Eric Weiss, who has since died, and Bob Beck. They did the complete logic on it. I lost a buck to Eric Weiss when I bet him that there would be at least one logical design error and the machine was turned on and ran.

RM: Oh, that's fantastic. And you lost a buck. Well, it was probably worth it.

JM: Yes. I bet him that he couldn't possibly not make a logic design error, but they never had to change a wire. They might have had to change some eventually when they changed the

function, but it did the function it was designed to do in the first place.

When we were talking earlier, you said that a year and a half after the MADDIDA was operating, there had been a logical error. Maybe you could just relate that story. It's rather interesting.

JM: Yes, I was involved in that. We were running a problem and it was one of the few problems where we actually filled up the machine using up all 22 integrators. It was a big problem. There was a critical zero crossing that I wanted to observe. The way you used the machine was to run it continuously. You programmed integrator one to put out an output pulse at the end of any desired fineness of time, and when that occurred the machine would halt. Then you could read on the oscilloscope the values of the integrands that you had to scan out, by rotating this selector switch to select which integrator you wanted to look at. So you sat there and tabulated with pencil the values of the operand every second or five seconds or whatever time interval you were interested in.

There was also a mode in the machine enabling it to run single cycle, an integration step at a time, which was typically

a thousandth of a second or ten thousandths of a second at a time. It was very fine grain. If you read every step, you could see step by step what was happening every ten thousandths of a second, or hundredth of a second, or whatever the finest grain of time division that you were running the problem at.

At this zero crossing point I stopped the machine and went into single cycle, and was single cycle stepping the thing through and the problem blew up. It would run continuously and go through zero, but when you single stepped it, it exploded, it just went marching straight up as fast as it would go. We just were beside ourselves because we couldn't study the problem carefully. Finally, after we spent days on it, we discovered the problem.

Do you remember, I told you there was this Z line on which the outputs of the integrators were stored? The integrator was 48 bits long and the Z line was 47 bits long with a couple of bits off of the drum in the flip-flops. When you stopped the machine, you re-circulated out of the 48th bit so that it stayed in sinc with the integrators and the 49th bit was thrown away. So when you started it up again, the bit which had been in that 49th flip-flop was missing. It was just a duplicate of the 48th bit but it was that logic that was missing. That bit happened to be the output

of the integrator which was feeding integrator 21. It was the first time we had ever used that particular combination of integrators so it was the only time that that bit was ever used in that way in the calculation.

RM: Up to that time, you had not been utilizing the machine fully.

JM: That's right. We had never hit that. Or if we did lose a bit, we'd never really noticed it since we were stopping the machine so infrequently, once in every thousand or five thousand or ten thousand iterations. But I was stopping it every time and losing it every time. It took only two diodes to fix it. It just took a couple of diodes to say, "Hey, when the machine stops, hold onto that bit and when you start up again, put it back into the line." We fixed it in no time at all once we knew about it. That notebook I mentioned describes that thing.

RM: This is the notebook that Teague has?

JM: Right. There's also a hilarious anecdote about that time. Hagen was just getting ready to complete his MADDIDA 44 and was out selling to customers like mad. His pitch would stress the simplicity of the machine, the fact that it never made a mistake because it was so simple, and when it did,(contradiction in terms)

it was so easy to find because it was always catastrophic because every integrator passed through all of the same logic. So if it failed in one integrator, it failed in everything and the thing came to a screeching halt, making it transparently clear that that something was wrong. However, since it was a single logic organization, it was easy to find and isolate the error and repair it.

He had a customer in and was going over the machine at the time the event I just described took place. A very dear friend of mine who is now back in Los Angeles, by the name of Bob Douthitt, a very enthusiastic guy, was helping me run down the problem. Hagen came in with this customer and Hagen had never seen Douthitt before because he arrived after Hagen had gone over to the other operation. Bob grabbed him and said, "This is a very interesting problem. We have been having it for about two weeks. It happens in only one integrator and not in the other and we can't figure out what the hell's going on." (Laughter.) Hagen just about went through the ceiling, you know.

The next thing was that he came over and he sidled up to me and he said, "Who is this guy?" I said, "His name is Bob Douthitt." Hagen sidled out of the room while Bob was still cornering this guy, and the next thing we heard over the loud speaker system was:

"Bob Douthitt, telephone please." (Laughter.)

RM: Did he stay with the company?

JM: Well, he was working with me, not Hagen. Incidentally, he's the guy who can tell you all about the UNIVAC Larc program.

RM: Oh, what's his name again?

JM: Robert Douthitt. He's now at Computer Machinery Corporation and he lives in Santa Monica. He just joined that company about two months ago.

RM: He's a neighbor of mine, that's good.

You talked on your tape about a two week computer course that you gave at Northrop. I thought maybe you could just talk a little about it and describe what a computer course was like in those days.

JM: Well, I can remember what I did. At that time we were looking for people to design what was going to be the MADDIDA 44, and I taught them the structure of a general purpose computer using the BINAC as a model. We had the BINAC.

RM: But it wasn't working.

JM: Right. But if you reeall on the other tape, they sent seven people out to study the BINAC logic and programming. They sent seven others to study the maintenance. At the time it looked

like it was going to work and there was a great hue and cry around the country to take it away from Norhtrop. They said, "You're putting a Stradivarius in the hands of a fiddle player." So when they sent the people out to learn the machine, they took the very senior guys in the project and me. I was probably the only one that was really going to ever use the machine.

RM: The other guys were just there for prestigious purposes?

JM: That's right. They sent Fred Stevens who became the Vice-President of the Nortronics Division. They sent George Fenn, Irv Wieselman, Irving Reed. How many did I name?

RM: Four, at last count. I think you named them in your tape.

JM: Yes. Well, anyway, I knew that I was going to be the only one who was likely to be in close association with the machine, so I really learned it. When we came back, I knew that machine inside out. In fact, when they brought BINAC back, I taught Rutishauser and Sprong who were the engineers and who were going to work on the machine. Dick Baker, the guy who lived with BINAC for a year and a half, asked me to teach them.

So at any rate, I knew that machine inside and out. I presented the structure and organization of the von Neumann type

machine, using the BINAC as illustrative material. I presented ideas on binary arithmetic, binary representation of information, Boolean algebra and its use in implementing these kind of structures.

I also went into the DDA, presenting the idea of the differential analyzer and how it was organized and implemented with these techniques. I went into ideas of memory, including things like the Williams tube memory for static storage, the acoustic delay line mercury tank, and the magnetic drum as a delay line device. In other words, I gave them fundamentals of the kind of building blocks that had been used up to that time and that they would be using in the kinds of machines they were going to work on. Much of that evolved into the course I taught at UCLA. I can show you the final exam from that course to give you an idea of the kind of coverage.

RM: Yes, that might be interesting. When was the two week computer course?

JM: Well, it had to be in 1950 because the guys left to form Computer Research Corporation in 1950.

RM: That's right, it was the same time. Now, that's not the course where 70 people attended.

JM: Oh, no, no.

RM: That's another one?

JM: That's another one. That was after I joined CRC. That was a company course. I taught a course at UCLA in 1956 or thereabouts.

RM: Yes, right. Did you also get into programming principles as well?

JM: Yes. I started out as a programmer.

RM: For which machine did you write your first program?

JM: The first one was written for the BINAC and the next was for SWAC and then the CRC 102A. The first machine had 512 words with two instructions per word. The second machine had 256 words and it was a four address machine with one instruction per word. Then finally we were in heaven, because we had 1024 words in the 102A.

RM: And you were writing in basic machine language.

JM: Yes.

RM: So actually nothing really started to happen in programming until the 701, is that correct?

JM: Yes, until machines got big enough memories to be able to support some of this software. You know it really started with Grace Hopper on UNIVAC. Grace Hopper really started pioneering the software development in the interpretive mode on

the UNIVAC. I think she's probably the pioneer of automatic programming.

RM: Yes, I think so.

Were you involved with SHARE?

JM: No. I was a consultant to IBM on the 701. It was called the Defense Calculator.

RM: You were a consultant in what capacity?

JM: Well, Don Pendry, who is now at Xerox in the East, was the IBM representative at Northrop. He brought Ted Codd to see us to talk about what we would want in the instruction set of the machine that was ultimately the Defense Calculator. Codd and/or others visited a number of people around the country. So when I say I was a consultant, it was in that sense.

Ida Rhodes was another one. Apparently several of us told them that if they didn't have a logical product in the machine, they were out of their minds. The original machine only had an instruction called store address which permitted the jamming of an address into an instruction word so it was a logical product of a fixed nature. We told them that at least they must have a variable logical product. They shoehorned that instruction in.

The machine was a half-word and full-word machine. You

could do either half-word or full-word arithmetic or operations. The sign bit on the instruction word said whether the instruction was to apply to a half-word or full-word. They did away with the half-word multiply and the signed multiply became the logical product. So it was clear that we finally got to them.

RM: They listened.

JM: They listened finally but they had already made up their minds that they didn't need it. They had painted themselves into a corner and they had to make this weird leap out of the corner by doing away with one of the functions of the ability of the machine and substituting this weird negative multiply which became the logical product in a very non-parallel fashion.

RM: They also had to bring out a new machine pretty quickly after that -- the 704.

JM: Well, yes, but I think they learned a lot. The 701 was good for its time. The 704 was just what they learned out of building the 701.

RM: Yes. Did you ever have anything to do with it afterwards?

JM: No. In that respect, I think the British were much more advanced than the Americans in the way they approached the machine design in the early days. I was looking for a machine

at the time but there was nothing available. One of the machines that I looked at was the original Ferranti Manchester Mark I, and that machine was very much better thought out and considered from the programming viewpoint. You know, they invented index registers.

RM: Did they invent index registers?

JM: Oh, yes. You'll find index registers referred to in the early literature as the B tube.*

RM: Is that a B tube?

JM: A B tube is an index register because the Ferranti machine was a cathode-ray tube machine and the registers were in tubes just as the memory was. The A tube was the arithmetic accumulator and they had eight index registers which were in the B tube, and they were called B tubes or B boxes. They invented index registers before they built their first machine because they examined the programming problem that came up in using machines.

RM: I hadn't realized that. I'm glad to hear my countrymen were ahead.

JM: They're brilliant, but you see, they just never get

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* B box is the common designation, I think -- HST

around to doing it. That's the story of the British life. Well, you know, they invented the jet engine but they never exploited it.

RM: Well, they got their EDSAC out just before EDVAC.

JM: Yes.

RM: That was a little bit of feat going on there.

JM: The British have done brilliant things in the computer world, but they have never exploited it the way the Americans have. They've done it in many technological areas.

RM: I think that's a comment on the whole way of life.

JM: It certainly is. You know, at SDS we had a British and a French affiliate. The French were building 40 million a year and the British were still waiting around to be introduced. They finally managed to get their license away from them because they never used it.

RM: How about just talking for a while on QUAC?

JM: All right. Let me give you the motivation or background for QUAC. This was classified 30 years ago or 24 years ago (laughter) but I don't think it is now. I'll make it as unclassified as I can. The Snark missile required an onboard guidance tape which required that certain mathematical functions be carried onboard the missile. The approach was to carry the data in incremental form,

or step form, so essentially the data is reduced to pulse rates. For example, a function which was increasing but increasing at a slower and slower rate and tapering off would be a high pulse rate at the bottom, and then as you approach the turnover point, it would slow down and slow down and finally go to zero. You just accumulate the steps onboard with stepping devices.

So in the early days we built linear pulse rate generators. We would generate a constant pulse rate for a given period of time representing the slope of the function we were trying to drive along, and then another constant pulse rate at a slightly different slope. So we would make this arc that I described as a series of straight lines.

To do that for the number of channels on which we had to carry information was going to be enormous. At that time we had already the MADDIDA experience, and I think it was Don Walter who suggested that we could build a MADDIDA-like implementation of this linear interpolator which had been a tube machine.* That was the earliest machine that Northrop built.

It was clear immediately that we could do it. We designed

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* Incremental Slopes Computer. — RM

it in a matter of a couple of weeks. It took almost nothing to lay it out. At that time I worked for George Fenn and Irving Wieselman, and I believe it was Wieselman who suggested that we make it a second order interpolation. This meant that we had to calculate the points between which we did the linear interpolation; in other words we would calculate one point and then the next point and then we would know what the straight line between them would be. These are the calculations that Postley did on the Card Programmed Calculator prior to QUAC.

Q Well, if you can generate a high order interpolation, a quadratic interpolation, then these points can be further apart because the parabolas between will fit the curves better than straight lines. This would decrease the amount of pre-calculation prior to QUAC that would have to be done on the CPC or whatever computer we evolved to.

So we set aside the linear interpolator design and set out to make a quadratic or second order interpolating device. Although it seems silly now to look back on it, we didn't recognize that we were really re-inventing the MADDIDA, because we really recreated independently what amounted to two integrators hooked together in series to generate a quadratic function. We didn't recognize

it at the time that we did it. So having the wheel in front of us, we re-invented it. (Laughter.)

At any rate, Don Walters and I laid out a machine. It had 28 words in it, 28 function generating boxes, if you will, sequentially stored on a drum and processed sequentially just as in the MADDIDA. The words one, sixteen, seventeen and thirty-two were used for control purposes. The drum was split into two halves logically. Words one and sixteen controlled the first half and seventeen and thirty-two controlled the second half, and the other 28 words were function generators. We built a machine that could simultaneously do second order interpolation on 28 functions, 14 of which could be at one interpolation interval and 14 at the second interpolation interval. So we could have a set of fast interpolations and a set of slow ones.

I don't think we ever used the feature, but we had the capability. (Laughter.) It used a punched card reader as input, and it read in the data for the next interpolation interval while it was carrying out the operation on a current one.

The output was directly connected to a magnetic tape recorder which had 24 channels on it. Actually, there were 12 physical channels and there was a frequency recording scheme to

duplex each channel. Essentially, we would calculate a primary trajectory from launch to target, and calculate control information assuming a perfect missile flight between launch and target. That was done with a Card Programmed Calculator-- four points, roughly ten minutes in time apart. Then the QUAC did all the fine grain interpolation of all the intermediate data points and put the recorded variable pulse rates onto the magnetic tape.

The tape was then carried onboard the missile and played back to provide the primary trajectory information. The airborne calculations in the missile were very simple perturbation equations around this idealized trajectory, which were done in an analog computer. By pre-calculating the bulk of the computation and doing only a minor perturbation equation calculation around the pre-calculated data as the flight progressed, we were able to get essentially digital accuracy onboard a missile in the days when it was impossible to fly a digital computer.

RM: That was a pretty good way of doing it, wasn't it?

JM: It was a very ingenius scheme. I think that was George Fenn and Irving Wieselman's scheme, although I'm not sure.

RM: But you actually did the implementation.

JM: I was responsible for implementing it, and that's where

I got into the computer business. I came into the project to do the trajectory equations and then they had to be put onboard in some way. I sat in the middle of all of the computer developments of which there were three going on-- the CPC, the BINAC and the MADDIDA-- trying to figure out how to use some combination of these technologies. I shifted from the mathematics to the applications to the programming and got more interested in what made them tick than in how to use them, and then shifted into architecture.

RM: Quadratic Arc Computer; is that correct?

JM: Yes. That was really a spoof; it was intended as a spoof on all the idiotic names that were going around -- MANIAC, ILLIAC, etc.

RM: Well, it's always made me laugh when I read QUAC.

JM: It was intentional. We looked for an acronym that would give it some silly simple name. At one time it was called a Digital Integrator and Parabolic Air Resolver which came out DIAPER, with the slogan: "Do you dirty work in a DIAPER." (Laughter.) I don't remember who drew up the name, Quadratic Arc Computer.

RM: So this was the device that was actually utilized on

the Snark.

JM: Yes. Ultimately, after I left, transistors came and it was possible to fly something. A fellow by the name of Joe Stalder, who is now at Magnavox Research Labs out here someplace, designed an airborne transistorized version and did away with the magnetic tape. Essentially, instead of punched card input it had paper tape input. It carried a small paper tape reader with the primary data points between which the interpolation was done, and the output of this went directly to the devices being controlled instead of going to a magnetic tape.

RM: So the bulk of the work was still done before, but now was carried out onboard with paper tape.

JM: Not the bulk of the work. Just the Card Programmed Calculator work was done before and that was upgraded to a more modern computer than the Card Programmed Calculator. Joe could probably tell you, but it took not more than a few minutes at the most of pre-calculation to produce the paper tape. Then that could be loaded onboard and the missile flown.

Now the QUAC operated and recorded the tape at eight times the playback speed. So it was going at eight times real time. If it were an eight hour flight, it would take an hour

to record the tape which was played back in eight hours. So we just sped up time in the recorder in order to cut down the pre-computation. Now all of that hour would be eliminated. It was four hours in the CPC and then an hour of recording for an eight hour flight. I don't know what they got the time down to, but probably just a very few minutes. You could launch the missile and the calculations that took that hour would go on in real time as the flight progressed. So it was the next evolutionary step.

RM: Was QUAC ever produced in quantity?

JM: I don't think so, but I don't know since I had left the project. They certainly had some operational Snark squadrons and they must have done something to provide that computation ability. I know it didn't evolve any further than the QUAC approach, so they must have had some limited production on whatever the final computer was.

Incidentally, at that time, although we didn't pursue the idea, we proposed using the QUAC approach to generate machine tool control information. It was a natural for contouring and it was exactly that kind of technique or some related technique that was used in the early machine control systems.

RM: Did they go anywhere using QUAC or something?

JM: We just said, "Hey, we could do this." We didn't seriously propose it. The company was very single-minded in this devotion of getting the Snark done. I think perhaps the best thing that happened to Autonetics was losing the Snark with the Navaho, because they had all this technology for which they then had to find diversified use. Northrop stayed on that one track and never expanded their horizon very well. So things like this that might have evolved never got anywhere.

RM: Was the atmosphere that you were working in such that you were able to follow new ideas without really staying onto the specific contracts?

JM: No, not at all. Everything was geared to the goals of that missile project. Everything we did was strictly along that line. We did not pursue other avenues. The company really wasn't tuned to it. That's how the CRC people left. They said, "Hey, we've now created a viable product that can be built at a reasonable cost and has significant commercial possibilities." The company said, "No, we won't do it." So they left.

RM: That was really the great split that started everything.

JM: That's right. When they left, I stayed behind. It was after they left that we got the idea for QUAC. The machine

that was used before was called the Incremental Slopes Computer.

RM: Tell me about it.

JM: That's even more ancient history and Will Dobbins can also tell you about that. The first linear interpolator that preceded the QUAC that I described earlier was a simple machine. Had you heard of that machine before?

RM: Yes.

JM: Okay. It consisted of three counters. One counter counted time. It counted the interval between slope changes. We counted 51,200 pulses per second or 512 times a hundred, so that time intervals were typically something like 81.92 seconds, which is 2^{13} over a hundred. That was the linear interpolation interval. So we had a counter that would count 51,200 pulses per second until it came to a count which occurred at the end of 81.92 seconds. I have to do some mental mathematics. At that point in time it put out a pulse which indicated the slope was to change from the current one to the new one. The slope itself was generated as a pulse stream, constant pulse rate, out of the second counter.

Remember, I said there were three counters. Well, there were really two different pulse streams being generated. There

were two linear interpolations going on concurrently. So let's just stick to the time counter and one of these other two. The way that counter generated an output pulse was to count from a pre-set value until it filled. When it overflowed it put out a pulse, and that overflow was returned as a pulse to re-set the counter to the initial state. So if you wanted to count at a pulse rate of a hundred counts per second, you would set into the counter the complement of the number of counts it took to get a hundredth of a second. Say the input rate was a hundred pulses per second, just to make the calculation easy, and the counter had 20 stages. You'd put in $2^{20} - 100$, feed in a hundred pulses per second and out would come a pulse that would come through a couple switches, and re-set the count to $2^{20} - 100$. And it would keep repeating this cycle.

Well, there were two sets of switches. There was the set that was active at this current time through which the re-set pulses came in to re-set the counter, and a second bank which was being set up with the proper re-set setting for the next slope. When the time counter reached its limit, it put out a pulse which triggered a flip-flop which channeled the output pulse from the first counter around to the second bank of re-set

switches. So now you're re-setting for the second slope, and while it was re-setting for the second slope, you'd re-set manually the toggle switches to change the rate which it would next switch to when the time counter put out another pulse. There was a third counter with its bank of switches and you had two sets of linear interpolations going on at the same time. We sat there with IBM tab sheets of the switch settings to be made and we had 81.92 seconds to get the switches thrown and checked and be sure they were right.

Incidentally, the patent application on that machine had cited against it a control mechanism for a zipper making machine, which had a similar scheme to control the amount of zipper length and the amount of blank webbing between zipper lengths. That machine would count until it got so many inches of zipper and then it would shift. Then the count would come out and the pulse would come out and then shift over to another counter which counted the time to get the space between zippers and so on. I don't know whether that patent was ever granted.

RM: That's incredible.

JM: I was asked to review the citations back against it.

RM: How was the Incremental Slopes Computer used?

JM: The same way the QUAC was used. It's output was recorded on a two channel magnetic tape. This was to be used strictly for the test flight program. We managed to get all of the control functions we needed to put the trajectory information onboard the missile on two channels for the simple tests we did.

RM: But it did seem apparent that something a little more sophisticated was needed.

JM: That's right. We had to get many more channels for the actual thing. This one had 20 bit counters, there were 20 flip-flops per counter, there were two 20 flip-flop counters and there was the time control counter as well. It was clear that if we were going to get 24 channels we were going to have flip-flops coming out of our ears. You know, we actually planned a very large machine. In fact, the machine I described was the first experimental one and it was replaced by a larger one that had more tracks whose switches were set by reading IBM cards. So there was a four channel card control unit device built.

RM: You mean the cards actually controlled the switches?

JM: The switches were replaced by relays and the cards were read in through the switches. When the time counter went off, the bank of relays that had been set previously controlled the paths for the re-set pulse. The card reader was started, and the

bank that had just been in use was re-set from the card reader so that the next slope would go through them. It was clear that if we were going to have to build a 24 channel unit with all these thousands of relays and all the storage it would take, that we'd go out of our minds trying to maintain it.

It was at that time that it was suggested that we had to do something. That was when Don Walters said, "Why don't we build a drum machine?" So when I arrived at Northrop, they were using the hand-set two channel Incremental Slopes Computer. That got upgraded to a four channel unit driven by punched cards, and then we replaced that with the QUAC.

I did the programming; the mathematics first and then the CPC program. I shouldn't say programming because that isn't exactly right. I did the mathematics of laying out the CPC procedures. Toben translated them into programs on the CPC that generated the tab sheets that we used to hand set the switches originally, and that generated the punched cards used to drive the punched card-driven later version. Okay, that's the evolution.

Now Will Dobbins and Harold Sarkissian built the Incremental Slopes Computer, I think. I know Dobbins did. That's a favorite Sarkissian story. He arrived on the project and he asked Dobbins

what they were doing. Dobbins handed him one sheet of paper with a heel print on it (laughter) and a bunch of scribbles, and that was Dobbins'es whole design work.

RM: We seem to have covered practically everything that I had made notes on. Can you think of anything that maybe you'd like to add at this point— any other anecdotes that have come to mind?

JM: I should think this will give you plenty to work on.

RM: Oh, it certainly will.

Let me just bring up one thing because I'd like you to put this on tape. When we talked earlier, you were telling the story about how you had discussed the fact that you were about to revolutionize an industry.

JM: It was really Steele's point and he was very persuasive about it. He had me convinced and he had all the other people convinced. He said, "We are going to unleash a technological revolution on this country through these computers that will dwarf the industrial revolution." He made this point to us and he made it publically and we carried the word to our friends in various contexts. People would tell us we were just crazy, that we were just mad scientists and we should just go back to the laboratory and forget lunacy like that, and that the businessmen weren't going to get involved

with anything as hair raising or wild as this. You know, it made us very irritated because we really believed it.

The latter part of the anecdote was a number of years later. I really got very angry when I read a magazine article, Fortune or Newsweek or one of those, that said that the early designers, in their wildest imaginations, didn't have any idea about the effect their work was going to have on our society. It was bad enough when they said: "Go back to your laboratory, you're crazy." But it was even worse when in effect they said: "You never said it." So first they said, "you're crazy", and they they said, "you never said it."

RM: Well, of course, I have a feeling that that article, or articles, was all based very much on the Eastern philosophy which I think was : "We're not going to need very many of these machines to fulfill our needs."

JM: Well, the story goes, and I don't know whether it's apocraphal or not, but I'm sure you must have picked this up, that IBM was literally forced, kicking and screaming, to build the Defense Calculator. They didn't want to and it was the government who twisted their arm. They said, "Fifteen of these machines will satisfy the computing needs of the country forever." The

story goes that they built sixteen* because that was a nice binary number but they didn't know what they were going to do with the sixteenth one. I don't know if that's true or not.

RM: It's pretty close to true, I think. I believe they actually built nineteen but I don't believe they realized it was going to go as well as it did.

JM: Apparently T.J. Watson, Jr. certainly pushed it as soon as he got control.

RM: T.J. Junior was for electronics and T.J. Senior just did not feel that the electronic computer was a necessity. He thought that his large-scale machine, the SSEC, the electronic multiplier and that kind of machine was going to take care of all of their needs, and so there was a very real battle between father and son.

JM: I don't know if you know the insides of NCR. You know that NCR bought Computer Research Corporation in early 1954 or late 1953. If you examine it, you'll find that NCR has electronic circuit patents and computing going back into the 1930's. Really, they had very elaborate computing development activity in electronics and never exploited it.

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* Nineteen machines were built.— RM

RM: They just stayed with their regular machines.

JM: They just kept it in the research laboratory stage and never did anything with it.

RM: So they didn't actually branch out until they bought CRC.

JM: That's right.

RM: Which then became their computing division.

JM: When they bought CRC, (this is telling tales out of school and probably they'll get angry at it) they actually had a project called NEAM, National Electronic Accounting Machine, in development in Dayton and it was going to be essentially a paper tape analog of an IBM tab system. It had paper tape input, magnetic tapes for intermediate storage, and they were going to have a sorter and a collator and do everything on paper tape with a magnetic tape intermediate storage. It was incredible because at that time at CRC we were recording 134 bits to the inch, and they were recording 16 bits to the inch in magnetic recording, and that's how they were going to build an electronic accounting machine.

We had a big internal internecine warfare immediately. They asked us what we thought, so we told them. We were the upstarts from the West. That's about as traumatic an experience as you can

get. Taking the young West Coast electronic computer company at the birth of an industry and trying to merge it, or amalgamate it in some rational way with an old line East Coast electro-mechanical monster company.

RM: Tell me a little bit about that merger.

JM: (Laughter.) Sarkissian could do a much better job. I really wasn't in the management echelon. I was strictly technical and I have always been strictly technical. I had a title in XDS, but it was, "Okay, you've worked here long enough and you've done your work, so we'll make you a vice-president, but don't think you're really a vice-president." It was that sort of thing. (Laughter.) I really wasn't privy to it.

You know in those days when NCR would build a new cash register, they would build 20 or 30. They would develop it, work all the bugs out in the laboratory, put them out to test at application sites that were representative of the uses the machine would have, run them for a couple of years, bring them back to the laboratory, and rebuild them. Finally, seven or eight years after they first started, they introduced them to the market. Now here's a computer industry where as soon as you get a paper design done, and you're pretty sure you can build it, you announce it. Well, bring those

two concepts together and they just couldn't conceive of it.

As a matter of fact they blew their position completely, and this is what drove most of us out of NCR at the time we left. We designed the NCR 304. I was responsible for the architecture of that machine along with Al Sharon and Mark Shiowitz. Mark, a very dear friend of mine, did the transistor development. This was in 1954. We completed the design before the end of 1954 and we would have had a machine on the market in early 1956. NCR paniced when they thought of the West Coast upstarts building a machine, so they gave a production contract to GE in Phoenix to build the first 304. They helped put GE in in the computer business. GE had the ERMA contract and that was all. NCR just handed them the transistor technology and the entire 304 logic and concept designs; that cost over two years delay. They didn't deliver a 304 until 1958, whereas we would have delivered a transistor machine in early 1956. They delivered a transistor machine finally, but it was obsolete almost by the time it came out.

RM: Instead of really being ahead of the market.

JM: That's right. It was a machine that would have been in advance if it had come out when it should have, and was really obsolescent when it did come out.

RM: It's a funny thing about this East-West battle. It really is a battle. I mean, it's been constant. There's been 3,000 miles and an awful lot of enmity between the two coasts. However, have there been any strings of communication?

JM: Not in the areas where I was working. I suspect you'll probably find that there was communication at the more academic line of things. RAND, for example, built the JOHNNIAC out here. I'm sure there were paths that way, but not between the pure West Coast industries like CRC, the Autonetics developments, the Librascope developments, what started out to be Consolidated Electro Dynamics which produced the Datatron, and later became Burroughs.

RM: Was Datatron a West Coast development?

JM: That was a West Coast operation. Datatron in Pasadena. They came out of Consolidated Electro Dynamics and then they became Electrodata. Now, that is not derived out of the Northrop Snark project. That was really totally independent of Steele.

RM: That's another story.

JM: I don't know it, but I believe it came out of Berkeley and Cal Tech. There was a Scandanavian * at Cal who was really the

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* Ernst Selmer -- RM

father of the Datatron.

RM: What did you just say was the company?

JM: It was originally Consolidated Electro Dynamics. Then it split off and became Electrodata, and that was bought by Burroughs. Of course, Burroughs had an Eastern development in Philadelphia and the relation between Electrodata and Philadelphia, I would guess, is probably similar to the UNIVAC-Blue Bell versus ERA. You know, the group in Minneapolis/St. Paul was originally Engineering Research Associates.

RM: Right.

JM: Those two groups are at each other. I don't know what happened between Burroughs and the East, but I suspect there is a "weans" and "theyans" relationship there. (Laughter.)

RM: Where is Burroughs West?

JM: In Pasadena.

RM: Do the names Walker and Pierce mean anything?

JM: Yes, if you go back to the BINAC days. Although I've always alluded to Steele as being the head of the computing operation, when I arrived the actual head was a gentleman by the name of Eric Ackerlind, I think it was Dr. Ackerlind. Steele and all these guys I've talked about reported to him but there were more people to the

group than that. Walker and Pierce were in that group as mathematicians. When the BINAC arrived, Wälker and Pierce were assigned to be programmers. I was only in contact with them on one program which was an abomination— a total disaster.

RM: Your program or their program?

JM: Their program. We gave them the equations that we had been running on the Card Programmed Calculator, equations of the trajectory dynamics of the missile and told them to put them on the BINAC. What we got back was coding sheets in octal code without a notation of any kind whatsoever. There were no remarks and no analysis.

RM: There was no documentation?

JM: No documentation whatsoever. There was just the final, what would be a binary listing, if you will.

RM: That's what everybody likes to read before they go to bed at night!

JM: Yes. I tracked through it and discovered that the integration algorithm that they used was invalid and we just threw the whole thing away. We covered the code. Well, we sent it back to them and commented. I tracked through the commenting code. Their method was not correct and that's why I said it was a disaster. I don't know of any other contributions that they made.

RM: So as far as you know, would there be much reason to contact them?

JM: Not in my opinion, at any rate. But I know Walker is still around and acting as a consultant, I believe.

RM: Do you know his first name?

JM: His first name is Claude. I don't know if Ackerlind is around. If he is, he'd be very old now because he was fairly old then.

RM: And then the other person whose name you mentioned is Dick Baker. Do you know where he might be?

JM: I probably have a number in my phone book. He was at one time in the Palo Alto area at Stanford Research Institute. Later he was a consultant on his own in the Bay area.

RM: Okay, and would he be a good person to talk to on BINAC?

JM: He would be very good to talk to about what happened at Eckert and Mauchly on the BINAC development.

RM: Then he came westward?

JM: He was sent by Northrop to Philadelphia to be the resident engineer during the BINAC development, and be their representative on site. When the BINAC was shipped he came back with it, and it was his job to install it and get it running. He hired two engineers--

Jim Strong and Rudy Rutishauser. Do you have those names?

RM: You've given them to me, yes.

JM: I taught them the BINAC. If you ever come across that block diagram I described to you, I want a copy.

RM: Okay. I hope I do come across it.

JM: The guy who taught us the BINAC back East developed that diagram. Oh, God, I could have told you his name and now I can't.

RM: It will come.

JM: Yes. What else do you have?

RM: We have a reference to RECOMP. What is it or who built it?

JM: RECOMP is the commercial computer developed by Autonetics. Autonetics is the North American contingent. They're the people that started out by buying the two DDA's, one from Northrop and one from CRC. The people at Calcomp could probably steer you on that one.

RM: Okay.

JM: Kilpatrick.

RM: We've talked about him, haven't we?

JM: Yes. When Autonetics lost the Navaho contract to the Snark, they had all this technology and had developed several things for inertial guidance systems. They developed a computer group and built

a small drum machine which was called the RECOMP.

RM: Okay, that's good. That's one more offshoot. It's incredible how this thing fans out, isn't it?

JM: Yes.

RM: The other question I have is a really general question and I think we've talked about it somewhat, but let me ask it anyway. What do you consider to be the great ideas and concepts that came from the West Coast pioneering era?

JM: Primarily the Boolean approach. I don't know of anything else that's fundamental.

RM: That's a pretty good one in itself, isn't it?

JM: I think of memories but I don't know that that's a West Coast phenomenon. Machine organization. As I told you, the original CRC design technique was what would be called in Wilkes terminology or in general terminology, word time microprogramming, and maybe I should describe that. When the CRC people shifted from DDAs to general purpose computers, they devised the idea of microprogrammed machine. I'm sure this was under Steele, although I shouldn't say I'm sure. This is speculative. You'd have to get Sarkissian or somebody like that to confirm it.

The CRC 102A and all the CRC machines were organized so that

there was a program counter inside at the electronics level. That's an unfortunate choice of words because that's not the program counter that has been associated with the program, but rather associated with the electronic program of the machine. The state of the program counter determined the subset of the logic that was active for a given word time. So it was a word time microstep. During that word time, the fact that the counter read 005 in octal would cause a certain set of logic gates to be active and the logic associated with those gates would be effective during that period of time. At the end of that word time the counter either counted, in this case from five to six, or it stuck at five to repeat that set of logic. Inside there might be a little counter that would say how many times it had been repeated, or it skipped like a program jump to another count state as a function of the events that occurred during that word time. Whatever new count it took on then, activated a set of logic which was effective for the next word time.

For example, in the 102A machine there was a print instruction. You specified the address at which to start printing, whether you would print with or without addresses, that is,print the address or the data or just the data containing the address; whether to print the contents of the location as six alphabetic characters,

nine bcd characters or 12 octal characters. You could print out the whole memory with one instruction. This electronic routine and the program counter would just circulate in that routine until you finished the entire print. You could print one word or you could print the entire memory.

In the 102D, I added in the decimal mode the ability to break the decimal word into subsegments, to zero suppress in each segment, to insert a decimal point any place in each segment, and to tab between segments. So you could do all this with one instruction.

A suggestion from Bob Douthitt and a design that I worked out was a sorting instruction which sorted a complete drum track. It was an electronic subroutine which essentially broke the track into eight eight-word groups, or sixteen four-word groups, or 32 two-word groups, or 64 one-word groups. You could specify any subset of any word within the group, any set of bits, every other bit if you wanted, as the key word and it would go around and pick up the first key word, rank that key word on the next drum turn with every other key word on the track, and when it got back home to the item that you were sorting, write a new address in the second track. If you were sorting 64 one-word items, it would take 65 drum turns and it would completely write a new address track according to the

rates of the keys of the items. You could do that on two different address tracks and then you could refer to the items on either of these tracks by their new address.

There was another command called Merge, which would allow you to merge these two tracks, picking up one according to its addresses and the other according to its addresses and merging them in by rank back onto the physically constant address drum. Those were all very complex electronic subroutines.

To sort, you'd name where the key word was and how long the item was and it would go. You also named which bits of the key word were the sort control bits and it would take off in an electronic subroutine. Well, the machines were all designed that way and I gave you a flow chart of the 303 which was done in about 1953, but it was a successor to the others.

RM: The 102A.

JM: The 102A. When I got to CRC, I said, "This is great, you really ought to publish it." Their attitude was: "No, this is a secret of the universe and we're not going to tell anybody." In my opinion, they preceded Wilkes by a number of years. However, they never got any credit for it because they didn't publish.

RM: There was a real thing about not publishing in this

whole development. Why do you think that was?

JM: Because that was your advantage over your competition. Your particular technology, your particular way of doing things was the thing that made you in your mind at least better than anybody else. Eckdahl and Sprague and that crew were convinced that word time microprogramming was a major advance and to publish would be to just hand your competitors, whoever they might be, the knowledge that you had generated.

RM: But actually it was a major advance that was never fully utilized by them.

JM: That's right. It was to the extent that we had the resource to utilize it in terms of technology at that time, and we said if ever we could get a fast memory in which we could store these microprograms instead of writing them-- they were physically in diode networks so they were wired in the machine-- we could make them variable, we could design machines that we could dynamically change, we could make field adjustments to machines and we could do all kinds of things. But of course, we had no such memory.

In fact, Steele called it the "Tabula Rasa", which represents the blank tablet, the unimprinted mind on which you could imprint any kind of knowledge that you wanted. He talked about how we

could organize a machine which is very similar to the kinds of machines that IBM is building today with the floppy disk that permits them to set up the microprogram. Now we have the technology, so they have been exploited. But all I'm pointing out is that Steele and Eckdahl, as far back as 1950, really had these fundamental ideas. We kicked them around in the early fifties but they never published or documented anything because they were afraid they would be giving their advantage away.

RM: It's possible it could have worked the other way, isn't it, by publishing?

JM: Well, Wilkes got a good deal of fame. When did he publish? He published in 1953.* I've always admired the man for what the Jews call "Chutspah". He gave his famous paper on microprogramming, entitled "The Best Way to Design a Computer" at the dedication ceremony of the Ferranti Mark I Manchester machine which was a machine that did not use this technique.

RM: That is "Chutspah", isn't it. (Laughter.)

You know, it's possible that if they would have published, the companies who had the research and development would have gotten

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* The first paper was in 1951.— RM

on the bandwagon a few years sooner.

JM: It's possible that they might have been more successful too. There could have been recognition that here was a group that had really done things. On the other hand, that may be just a hopeful scientist talking and it's the realities of how many machines you sell and to whom that counts, and not how many scientific minds you convince of your brilliance.

We got on this from West Coast contributions. Well, the DDA was a unique West Coast contribution. That never really was a huge commercial success, but the DDA has been used quite successfully. For example, it was used in inertial navigation systems, which is what the Snark really was all about in the early days. I'd say almost any of the digital inertial navigation systems have a DDA buried in them someplace. I shouldn't say any, buy many. I know all of those at Litton, for example, had DDA's in them.

RM: So it really was an on-going device.

JM: It was an on-going thing but it never stood on its own feet. It was a peculiar machine. It had some peculiarities all its own that made it very tricky to use.

RM: Well, maybe on our next tape we can get into some of that.

END OF TAPE