SMITHSONIAN INSTITUTION

INTERVIEWEE: Greg Toben, Jim Smith, Dave Montgomery and Roy Harper

INTERVIEWER: Bobbi Mapstone and H. Tropp

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HT: This is a discussion with Mr. Greg Toben, Mr. Jim Smith, Mr. Dave Montgomery and Mr. Roy Harper at ASD Lab, IBM in Los Gatos on October 9, 1972. Why don't we start with you, Mr. Toben. What events led to your joining Northrop and how you got into feeling a need for this computational device that later became the CPC.

GT: My background was in electrical engineering. I had turned to accounting and I was running IBM machines for a fairly large manufacturing company in Los Angeles, doing payroll for some twenty plants and many men. I was trying to keep from becoming obsolete, so I took a radio course. Radio wasn't called electronics in those days. The teacher was Bob Rawlins. It turned out the following year Bob Rawlins got the job of setting up the computational part of the guided missile program at Northrop. He ordered a large

computer--BINAC--and until this was to be delivered--it had been promised a year or two later--he contracted with IBM to get some computing done; preliminary computing, trial computing, feasibility computations. He remembered me from the radio class and he hired me to run these calculations in 1946. That's how I got into it.

HT: He was the assistant project manager on this project.

GT: Yes. Weaver was the project manager and I believe he was the assistant.

HT: Dick Baker, then, was primarily involved with BINAC.

GT: Dick Baker was sent by Rawlins to Eckert and Mauchly to learn the machine as it was being built, and to come back to Northrop with it. He was a technician at that time.

RM: Was the computing center set up primarily to respond to the Snark?

GT: The computer center came later. At first we were doing strictly missile guidance trajectories. It wasn't until we had that in hand and began to reach out and take in other computing, which was all over the place just crying to be done, that the center was formed. We did the wind tunnel data reduction just on the side; then we picked up stress analysis and we picked up weight control About that time the computing center to take care of Northrop itself, plus the navigation computing for the missile people, plus the nuclear group--we did a lot of work for them--became needed because groups that were around needed calculators. It was so much fun deciding which ones to do.

HT: The computing center, which is what Dick Sprague refers to in his article, "A Western View of Computer History," as the Data Processing Department. That's a later development.

GT: That's a year or so down the road.

HT: That's about when?

GT: 1950. It's represented by this block which has all these projects and people coming together. (See attached chart.) In 1947, 1948 and 1949, this was all strictly missiles. Along about 1949 we began to branch out here and pick up the general aircraft problems at Northrop plus the nuclear problems from NEPA. From then on it was a general purpose sort of thing.

HT: Do you want to go back and see what you can remember and recreate some of the early developments that led to the Card Programmed Calculator?

GT: The accounting machines which we tried to press into service to do the missile trajectories were not entirely satisfactory to do the calculations. In the first place the numbers were larger. The counters were continually handling nine digit numbers in eight digit machines and that wasn't too good. Secondly, there was no way to get an arc sine of 26.234. You had to go to a book, look up the table, write it down, key punch it, get it back, and it was awkward. There was no way to generate it from the series at that time. That was a pressure to get a better machine, one which didn't require you to walk back and forth between the machines with the card. You wanted

to go electronically around that loop.

One of the first things we did was to tie the machines together with cables--machines that weren't designed to be tied together with cables--to get the arc sine when we needed it.

HT: Which machines were you using at this point in time?

GT: 601 and 405. We did this, I guess, for a month and then we said, "Gee, wouldn't it be swell if IBM would build something like this for us." We got as high as the project management and asked, "When will BINAC be in?"

"Not for a long time."

"What else can we buy?"

"Nothing."

"Wouldn't it be nice if IBM would do it?"

"Yes, we'll see if we can get them to do it."

Jack Northrop, who was well known in the aircraft business at that time, called IBM and said, "Can we get this machine? If not, why not? We need it." He didn't get a hold of Watson, although he called him. Watson was protected. He was old and had lots of other things to do so he had a wall around him. I don't know how much is truth and how much is fiction, but it is reported that IBM was afraid to turn Northrop down because they thought he would get through anyway. They didn't think they ought to tell

Watson either, but they agreed to build the machine we wanted, exactly as we wanted, and they did this in six weeks, an amazing time. They picked our accounting machines off the field at Northrop, wheeled them out the door onto the air field, lifted them into the airplane and they went to Endicott. Six weeks later the airplane dropped down, the machines were wheeled out of the air-plane, wheeled into our department, and that was it. There was no long design time, no long customer product test time or anything. It went to work immediately and, with the help of Jimmy (Smith), worked almost uninter-rupted for two years. We worked around the clock, all kinds of problems. Those were great days.

HT: Conceptually, then, it was basically your tie-in with cables put into a format that you could use as a single machine.

GT: With a newer machine, a better machine. As I recall, there was a discussion about whether we should patent this thing. I remember Weaver saying, "Yes, but if we do, IBM will go underground and we will never be able to contact them again." (laughter)

RM: When the machine came back, did it have 603 hooked up to it?

GT: Yes. It had a cable about eight feet long--we used to call it the snake--which ran from the 603 and was plugged into the 405. When we would have trouble, we would isolate it so that we could prove that it wasn't in the 603, and

we would prove it wasn't here in the 405, and we'd burn the snake. (laughter)

JS: I think I have something to add here, which to me was extremely startling and I'll probably carry this one to my grave. I was working in a customer's office the day that Greg and his cohorts made this work, the first time that they hooked it together. I got a call to come because they had a real problem. In those days IBM service people had instructions that the customer was not to tamper with the machine. That was our job. I walked into Northrop, I walked into the room and it was solid mass of wires and relays all over the floor. I thought, "There goes my job." (laughter) We spent a real interesting two hours thereafter with Greg proving that he could start the 405 and make the 601 do things and transfer information back to the 405. It was fascinating.

HT: You did make some changes then in the basic IBM machines.

GT: Not changes, we just hooked onto them in places where they weren't normally hooked onto. We didn't replace any components or anything like that.

HT: You didn't do any major modification of the 601?

DM: How about the extra board and the two extra relay gates?

GT: IBM put those on.

JS: As far as these guys were concerned, they made no major changes. They added, because you had to have some way for the 405 to tell the 601 to go. It had to have that kind of capability.

HT: Looking back in time from your position at IBM, how did you view this new hook-up?

RH: It was one of the most interesting things that happened. We had been plugging along with machines that were handling payrolls and sales analysis and things like that. It was quite a new era, as far as we were concerned, to see the ability to use a machine like that to do some real engineering computation.

HT: What were some of the main problems that you had, Jim, keeping these machines going?

JS: Greg! (laughter) Once he solved one problem, once he knew how to handle one engineering type computation, he was after another one and generally the second one was worse than the first, just go on down the line. That first lash-up was a very attractive one, and as far as just every day, general trouble, it had the same amount of problems as the old accounting machines. Beyond that, the lash-up was pretty good. In those days we weren't worried too much about mean-time-between-failures. If it ran an hour, great; if it ran a day, great. Now Roy won't completely agree with me because he's pretty proud of some of the things that he did when he was in the field, but it wasn't the same emphasis in those days.

HT: When you first hooked this up, it was a special thing for Northrop with their unique problems and the ways they wanted to handle scientific computations. At what point did IBM decide that this was something that lots of people could use?

GT: About the time we asked them to build this particular one, which we sort of specified and they built in six weeks. They went back, sat down and said, "We can do even better than that. We will take our newest machine, which was the 604, and hook it up to something better than a 405; a 402. Our other customers, Los Alamos, China Lake and all those, are hollering for better computation too, so maybe we can plug this gap until the 701 comes along."

HT: Who were some of the IBM people involved in that particular development? Do you know their names?

RH: The 603 development, before it was hooked up to the CPC, was done in Bryce's department at 590 Madison Ave., N.Y. Some of the people were: Carl Bergfors, Byron Phelps, Arthur Dickinson, I was in that department too, Greg's brother, Bernie Toben, was there; I don't remember other names. The 603 was completely developed there and turned over to Endicott for production.

JS: The 603 was built during the 1943-48, something like that. It's another one of those interim machines. The 603 had a production line of some hundred units.

HT: I think it ended up with about two hundred.

DM: Yes, but it wasn't a big product. The 604 was really big and Wheelock, who was in on some later developments you have on your list, was product engineer on that machine at Endicott.

HT: He's no longer alive.

DM: He died several years ago.

RM: Wheelock is the third person involved in the name "Wooden Wheel."

DM: Yes.

HT: What are some of your views on the impact of this lash-up of the two IBM machines in terms of computation and subsequent developments?

JS: When the CPC went on the market, Dave and I probably hated it. There were only about three or four of us in the Los Angeles area trained to take care of those machines, and we had five of them running around the clock, plus the number that were installed at White Plains and we were supposedly on emergency call for those. So on first reaction, we hated the sight of the thing because of the around-the-clock type of service. However, you couldn't help but sense the enthusiasm in the various organizations that had the equipment because they were getting work done, and quite a bit of work done, and it's fun to work in that sort of an environment in spite of the problems.

RM: Did IBM have this sort of excited reactions?

JS: From where I stood, and this is me personally, they were more involved with the 701 than they were with the CPC. We felt like second cousins in many cases. We knew we had something, we knew that we were getting one heck of a lot of work done on the West Coast.

HT: That's right, because the CPC got a lot done before other machines were available. Really, the CPC was the work horse until the 704 came along.

RM: How did the CPC get pushed into production?

DM: It pretty much was customer demand.

GT: Yes, it was customer demand. We had lots of visitors.

JS: They had a constant stream of visitors through the department to see that thing.

DM: Greg talked about IBM coming up with a better CPC. We called the first one (the 603-405 hook-up) Betsy. IBM then came up with the 604-402/3, known as Model I. It lasted a very short life. Customer demand forced them into Model II almost immediately. The main differences in these two models was that the customer could personalize Model II. Model I was virtually a card programmed machine, whereas in Model II internal parts were brought out to the plugboard, personalizing the machine, and then you could program it. That became a maintenance headache also, because every engineer using the machine had his own personalized machine plus his own program. We had to know each job.

HT: As I look at the computational environment in that period, everybody had new kinds of problems that they suddenly realized they could run. Were you running into difficulties with people who wanted to personalize the early machines to do their particular kinds of problems? You mentioned the new demands that you were getting from within Northrop.

GT: We welcomed them. It wasn't a problem, it was a lovely time. People would have new and more interesting problems

every day, and you felt like you had done something when you solved them and sent them away happy.

A point was just brought up, and it shows on this chart. (Enclosed) There were two CPCs, three really. The first one, Betsy, was built so quickly that it was probably the best of the three as far as the operators were concerned. It was built for people who were computing directly to their specifications. IBM kind of loused up the second one because they were sure the people wouldn't want to do all these little grubby things about computing, they would want them done automatically for themselves. Therefore it would be all right to channelize a good bit of the routine, and just put your data in this one place and it would go a little slower but it would get done.

The customers who had used our machine and had ordered one on the basis of what we had done, found that it was not nearly as useful as ours. We got the first one, and when the other customers found theirs couldn't duplicate it, they screamed to high heaven. The Model II was essentially an improved version. It had no drawbacks that I remember except the usual high-usage breakdowns.

HT: It's really the third version then of the CPC.

JS: Maybe we shouldn't even mention the second one because it didn't last long.

GT: It was very short-lived, six months or so. They maybe made a dozen or so.

HT: When we talk about 200, we're really talking about what you would call the third one.

GT: Yes, the Model II.

HT: I guess another question, because I've talked to Cuthbert Hurd about the 701 period, is what impact, again from your viewpoint, this had in changing IBM's thinking about its own role in an industry that didn't really exist yet?

DM: They were going to build 19 701s and rent them for about $15-18,000.00 or something like. In those days 200 was quite a wide market, it put them into general computing versus the very specialized.

GT: I don't think they were turned on by the CPC particularly. They didn't realize till much later that it was a useful machine.

RH: Well in those days they had pigeon holed calculating as belonging to a function and accounting belonged to accounting machine and they were a crucial mix. I know I worked for many years trying to mix those two and had a tough time getting it done.

HT: I just wondered if the initial resistance to your requests to lash these two machines together, was that it just doesn't fit into the general attitude as you mentioned of the period.

GT: They just didn't understand. Neither did von Neumann for that matter, as you will be able to tell when you talk to Woodbury. Bill Woodbury has always said that von Neumann

was not interested in computing. He was interested only in the idea that you could do everything--this is not a good explanation. He wasn't interested in the through-put, the turn-around time, the efficiency, he was interested only in the general class of machine that could do every-thing, and this would be a stored program machine. That it took so much longer and was so much slower could concern him not at all. In those days when they had so much to do and so few people to do it, it was of great concern to us.

HT: Did you have much contact with von Neumann and the group at Eckert and Mauchly or Princeton during that period?

GT: I personally did not. Woodbury was there. My first con-tact with Eckert and Mauchly was when they came to give a course at Northrop on how to run, particularly, the BINAC, but also stored program machines in general. As far as we were concerned, they didn't have anything to offer that was useful. You know, "What do we do with this stuff? Let's go back to work."

HT: In the pictures that you showed us, you pointed out on the floor where the marks are from the BINAC installation. Do you want to make any comments at this point about the period when BINAC was at Northrop?

GT: Baker worked very hard trying to get it to work and I don't know that there were any fundamental reasons why it wouldn't work; it was an awful job for one man. I do know that in general, and I'm not speaking disrespectfully of the people

who worked on this, it only did one problem in its whole life from the time it arrived until it was carted away, and Jerry Mendelson worked that one through by sheer genius; nobody else. It was not a useful problem, as I remember, it was more like a let's-try-this-and-see-if-we-can-do-it type problem.

M: I think it was a problem that related to the feasibility of his QUAC machine.

GT: At any rate, it wasn't bread and butter type of computing. We were as surprised as anybody. The BINAC had a typewriter for an output machine, and compared to the 405 the typewriter just isn't an output. It wore itself completely to pieces before it got to where it could even do a problem. The heat generated by hundreds of 35-L6s was terrific. Plug-in contacts, stresses in the cards--that whole plug-in card thing was new--they hadn't developed double-sided boards, they hadn't developed the solder, they hadn't learned about cracks in printed circuit writing, and a lot of the wiring wasn't printed circuit. The mercury delay lines were new. It was large, very formidable, and engineering-wise it was ahead of its time. It just didn't have enough years behind it to be a useful machine.

GT: The total input-output was this typewriter, so there wasn't much production even if the computer could run.

GT: Three hundred full lines a minute from the printer on 2 or 3 shifts was a lot of output, and there were no pages of

titles. It was solid, honest-to-goodness, real-world numbers. They were points on a trajectory, they were the inverse of large matrixes, they involved good hard stuff.

HT: The CPC was really the first working machine for that period that could handle large matrix inversion problems.

GT: Yes.

HT: Up until then people could only talk about the hope that they would do them and these other machines might eventually. The CPC was the only one that I know of that could do them day in and day out.

GT: For a couple of years there it was the one. There were some ILLIACS and the BINAC.

: Not until a little bit later.

HT: Now in this 1948, 1949, 1950 era, to do a 28 by 28 inversion was still a new thing on that order of magnitude.

RH: We were doing twelve by twelve in St. Louis at McDonnell on a standard 601. That was the biggest we had had. I'm out of place here with Western history; I was with the Midwestern section.

HT: That's another geographical area that hasn't come up much in their early period. Mc Donnell was doing work in the late forties, in the latter half of the forties.

RH: It was set up by Dr. Bubb and Dr. Arthur Compton and I was operating as IBM's representative on that, setting up that installation.

HT: What kind of equipment did they use?

RH: When I left, which was early in 1946, they used a standard 601 payroll device and a 405.

HT: This was when they were working on the BANSHEE and the Voodoo.

RH: Yes. We were not allowed to touch or make any alterations in the machine to help the customer.

HT: Do you remember some of the maintenance problems, Jim, that you were having with the early CPCs?

JS: Regarding Betsy? Yes. The actual circuit load within the equipment itself was considerably higher than what you would find when the equipment was normally used for payroll and things like that; CB problems--circuit breaker problems. The 604, which is the basic multiplier, if you will, of the system, was going through its phase two problems. IBM was going through their own development process of picking the better tubes for the job. And then just being available because most of these machines were on at least a 20-hour a day schedule.

HT: It may not seem like a relevant question in today's climate, but you started off with one machine for Northrop and suddenly, within a short period of time, you had 200 of these going around the clock, which meant that somehow you had to train a whole bunch of people to service these things. Now did that cause ripples in the heirarchy of IBM?

: Well again I don't know.

GT: It was gray hairs for Jimmy though. (Laughter)

JS: We never felt that it caused that much of a ripple. We

just had to take care of it, didn't we Dave.

DM: Most of the CPCs, twenty-five percent of them, were in Santa Monica.

RM: Was the West Coast the largest customer for the CPCs?

DM: If you include the White Sands area, yes. That took a big chunk of it. They had 7 of them, I think, and they were running around the clock.

GT: China Lake took a bunch.

HT: It's hard to really go back and see the impact of the CPC in terms of later developments.

DM: We were so isolated from the 701, for instance, we hardly knew what it could do, what was being proposed, and what its specs might be. All we could see was what was in front of it.

HT: One thing is clear, though, the CPC did teach a lot of people how to use the machines for computation. Almost everybody that I've talked to who grew up in that era cut their computational teeth or maintenance teeth, if you will, on the CPC.

JS: One thing that's kind of interesting about that era and it's so different in today's world. In those days the programmers were either engineers or physicists or mathematicians, and not only did they program but they also, in many cases, ran their own jobs. That was the beginning of computation as I see it. That had a direct effect on people like Dave and me.

HT: When a mathematician at Northrop had a problem, did he come to your data processing division and say, "Here's my problem. Can you do it for me?" Or did he come to you and say, "I've got a problem," and then you would show him what he had to learn so he could run his own problem.

GT: That's the way it worked. We ran an open shop at Northrop. That is, anybody could bring in a problem and be assured of help to solve it. But no one could drop a problem in a slot in the door and come back and find somebody else had solved it. There was a good reason for that. As you say, many engineers were self-trained to use computers in this way, but it was deemed too dangerous to let anyone who was not intimately connected with a problem to run it clear down to the answer stage and then hand out the answers. We discouraged floating decimal for the same reason. If he couldn't scale a problem, he probably didn't know enough about it to safeguard it against inadvertently dividing by zero, or truncation, or round off, or loss of significant digits. If you had a program worked out, and there were no programs like that in those days, where they had been proved against all of these things, fine, but since you didn't, you didn't dare let somebody get in there and run a set of answers and get down and build something from them when it might not be right. It was very easy to divide by zero some place when you subtracted two numbers that were very close together. If you didn't see those, if you didn't follow them down the page, if you

didn't have a feeling for them, you could make some pretty bad blunders.

: I'm wondering whether they didn't have some of the same type of factions that I saw at Mc Donnell where there were a very strong group of traditional boys, the sandbags-on-the-wings-till-they-broke sort of people, and they are the ones who wanted to do the same things by computers. They fought bitterly over which was the best way to go.

: Didn't Northrop have an example of taking off the F89? The computer people were predicting whether or not it take off?

GT: I don't recall that particular one. The history I got was they emptied out the parking lot that was at the end of the runway and that still wasn't long enough for the computer.

HT: You've raised an interesting area in terms of computation. Numerical analysis concepts were there, but really the heavy work had not been done in order to do certain types of error analysis, truncation analysis, round-off errors, this sort of thing, and as a result people had to have a real feel for what they were doing and the numbers they were dealing with, to know when suddenly their data was going totally off the scale and they were getting worthless numbers.

GT: We were afraid of this anyway, and we thought it would be advisable to let the engineer run his own problem.

: Northrop had a very good second shift that was run mostly

by engineers. After it was debugged we would sometimes run jobs for them, but generally the guy who was looking at the paper coming out knew what was going on.

DM: You were asking about maintenance a while back. After Jimmy went on to other areas, I took over maintenance at Northrop. I never had to go in on the second shift. The operator would call me at home and give me the indications and I had a set of drawings at home, and he would pull out his replacement tubes. They had very good service.

HT: Tube standards were something that nobody knew very much about in those days and various projects set up different test procedures for deciding how long the tube had to run before they would let it go out of the machine, and then later how to find out which tubes were failing. Did you have any standard procedures that you thought were innovative in that?

JS: Two coat pockets full of tubes. (Laughter)

DM: There was also a CE memo sent out by Wheelock, much to everybody's horror, on how to weed out tubes that might be failing. One of the consistent problems was heaters that were either in danger of shorting or burning out. He recommended that they pick up the 603 about six inches and then drop it and replace all the tubes that didn't work. Everybody was horrified. (Laughter)

HT: Did you ever try that?

DM: No.

RM: I would think the first problem would be to pick up the 603. (Laughter)

DM: It was fairly light, very small. Sitting on a table.

HT: What were some of the more interesting problems that you remember at Northrop that you were able to do once you had the CPC?

GT: The missile trajectories. The numbers would then later go on the tapes to modify the signals they got from the Loran system. This was probably the highlight, it was a live sort of thing, and point by point you knew where you were going and the fact that you got there exactly with the world's most precision navigation was pretty good. Wind tunnel data reduction used to thrill me. They would put a model in there and take the various balance reading. Now these were not terribly complicated calculations, but they were tedious. There were drag coefficients, lift coefficients, a bunch for each line, and the fact that the CPC would do those, bang, bang, bang, I could see those curves jsut growing right in front of me. That was nice.

JS: How about your three-gyro platform problems?

GT: That was interesting, but I hadn't even thought about that. We picked up Woodbury a little after we began to pick up these outside problems we picked up Woodbury with one of them   . I should say he came in with one of the problems. They had a problem in supersonic drag. They wanted to make the wing that would go through the supersonic barrier without taking itself apart. Woodbury was one of

the few who could even understand the basis of the problem. He worked very hard, very long, and there's a lot of stories and legends that went on at that time. At any rate he solved it successfully and the plot showed how the wing could be made and where it could be used and so forth. That was a great sense of satisfaction. There was a three-gyroscope problem. It was thought at one time that the stable platform could be installed in the plane from which it could navigate, but Woodbury, who is always right, walked around the thing and poked it and said, "No, it won't work." Many months of calculations later, (Laughter) many people had to eat crow on that one.

JS: The other one I recall, Greg, is the shielding problem you were doing for the Atomic Energy Commission.

GT: Yes, I had more to do with that than probably anybody else. I hope it's not still classified; I'm sure it isn't. Nuclear energy for propulsion of aircraft (NEPA). The problem was how much shielding do you need to keep the personnel intact, and how long does it take to get so many energetic particles burned up, and so on. We constructed a simulated pile. Rand Corporation's random number deck was just coming into use. We got one of those, which was a great stack of cards with random numbers from one end of the 80-column card to the other. We would change our wires and take one strip of random numbers, turn the wires around and take another strip, or we would take one on each end. We would get our random numbers that way.

These machines would chug along at 150-cards per minute, and each card would introduce a random particle going in a random direction, hitting another particle to do one of six things. As this developed, we could trace the history of the particles until finally it either went outside the pile, hit a rod or fissioned. You could stand there at the machine and watch that, and get a good deal of the feel for the problem.

HT: You were actually simulating?

GT: We were actually simulating the use of the pile and we put a scope on top to visualize the output.

DM: I was going to say you are being modest. You really did something great there.

JS: He added digital-analog converters and control decks and wide access to the scope. It had an area on there when the particles would get out in the danger area.

GT: I didn't add anything to the problem.

HT: A visual Brownian movement.

GT: Because they were not particularly critical, those would run in the evening. We would turn them over at five o'clock and go home and they would run all night with the second shift operator. Later, when it was plotted, it showed statistically what had to be done and what couldn't be done.

HT: You have a tendency, I think, to minimize that particular development. I wish you would go into more detail on how you decided to do it and what you ended up doing, because that really is very exciting.

GT: Well, again, somebody walked in with a problem. We said, "Fine, let's do it." The engineers helped. Some fellows from Oak Ridge came out. Dr. Marcus, I believe was one name.

HT: You talk about a visual scope. Would you describe this in a little more detail?

GT: It was a cathode ray oscilloscope with a digital to analog conversion on it, so that each time a particle came in, it had to go from here, the direction the cosine indicated, say take off at forty-five degrees and go two centimeters. The relays would click, the voltages would appear and the line would zip from here to here (along persistent screens), and you could see it. Another card would pass, bang, bang, bang, bang, bang, bang, bang, bang, and it would get real bright and disappear and then start again from scratch, somewhere.

HT: You had the deck of cards then that gave you a kind of random number generating function.

GT: Yes, and a little program to do the various things you wanted it to do.

JS: That was one of Rand's famous random number decks.

GT: Yes, the Rand number deck was very much in at that time.

JS: I don't know whether you know, but they had quite an elaborate system for creating that random number deck.

HT: Who was involved in that?

GT: I don't think I really know.

HT: Willis Ware might know.

JS: Getting back to the impact of the CPC on industry, I believe

that the CPC was Rand's only machine in the early days.

HT: Before they got JOHNNIAC, that's right. That was the only equipment they had. They had a number of CPCs though, didn't they?

RM: Six or seven.

GT: He used to come over to our place and . . .

HT: Who? I'm sorry.

GT: Hahn. Didn't he write a book something about . . .

HT: Linear algebra, wasn't it?

GT: No. The last war, the big war or something. Not on computers. I remember we used to always argue because at that time he was postulating the probability of success and failure in war. We used to always try to get him to try his formula on past wars and figure out who had won. (Laughter)

: He wouldn't do it though, he said it was for future wars. The name Herman Kahn seems right.

HT: K-A-H-N--that rings a bell. I was thinking of Franz Holm the mathematician.

GT: Woodbury will remember it.

JS: The CPC at North American was used for predicting horse races. (Laughter)

GT: That doesn't go down to posterity.

HT: Yes, that ought to, because the horse race prediction problem is much more difficult than almost any kind of statistical attempt to analyze data and position.

JS: I don't know how much of this is fiction, but a man got canned for doing it. The company came to find out that their useful production went so low that they actually hired him back.

HT: You mean he was moonlighting on the horse race prediction problems. Dick Spague talked about something, I don't know whether it was at Northrop or maybe later with the CRC, when they got involved with the roulette wheels at Las Vegas. That must have been during the CRC series. When you are talking about predictions and probablistic aspects, the whole origins are in gambling or various aspects of gambling.

DM: I think a discussion on some of the additions they put on the CPC may be worthwhile for the flavor of the customers problems. We talked about the CRT tubes that Greg added for output display. There was a real-time plotter added to one of the CPCs at Northrop. It was much like the original CPC in that Northrop engineers specified it and somebody else built it.

RM: Where did the ice box come in?

DM: The ice box was this little mechanical storage and they just called it ice box because it was a storage unit.

JS: Actually it was two complete storage units from the 602.

DM: It looked something like an ice box, a storage box.

DM: Old Betsy was instrumented and tied directly to the wind tunnel over telephone lines, so they had the telecommunication and real live instrumentation at Northrop. Almost

every one of the CPCs had a ten-level keyboard on it so that the engineer could set constants or data directly into the CPC.

JS: The 604 was equipped with a large bank of relays to allow you to make three program selections under punched card control, so you could have three different programs.

: Model II incorporated that later on in all of them.

DM: We tied in an old 604 and had dual processors on one of the CPCs at Northrop.

HT: Had there never been anything else happen in the computational field, the CPCs were constantly being added to and developmental changes were made to get more and more computational ability.

JS: They were reaching their limit in a hurry, that was the whole point.

DM: Production went up something like ten times by doubling the storage on this one CPC, but to tie two of those together was a real clobbered up installation.

HT: I think another point that tends to be ignored is the fact that once you have computational equipment like this available, it tends to generate problems.

GT: Oh, yes. You recognize that you can solve more problems.

HT: Because people have said, "We need a machine, we need it to do this particular problem." Suddenly, the fact that you can solve that particular problem, tells other people that there are other things they can do. This may have been some of the motivation for changes and additions that

were made, because suddenly the original machine was no longer capable of the problems that were now generated.

: I've got a good example of that one. The missile part of Northrop started computing where the stars were going to be, the stars you could navigate by, over several years. They estimated that it would take something like 200 years to complete that problem on the CPC, but they got started knowing that bigger computers would come.

JS: Greg has underplayed the role to a certain degree he had in making that department successful. I can remember very distinctly when we first started out with Betsy. He was almost begging engineering for work. When I left it was just exactly the opposite. He probably kept the door locked so they wouldn't come in. Dave will have to follow on because he followed me at Northrop. Greg did a lot of selling.

HT: What was the atmosphere like at Northrop for the kinds of things that you felt ought to be happening in the computational area?

GT: It was a good atmosphere. There were sharp people and sometimes you only had to start your pitch and they would grab it and run. Wonderful atmosphere.

HT: You talked about Eckert and Mauchly coming out to give the course on the concepts of stored program machines, and what they saw as their applications in the future. I guess Grace Hopper was along at that particular time. In talking

to people about John Mauchly in particular, I get the feeling of a man of some great vision. What do you feel were some of the ideas and catalytic effects that his group had on the environment on the West Coast, and its subsequent development?

GT: I think Woodbury would be a better one to answer that question, so be sure and ask him the same thing. Personally I didn't get much from him. I just don't know how to relate him to the things that happened. Von Neumann's machines, and that type of machine, seemed to be destined to be accepted everywhere in spite of anything that happened. It seems as though it wasn't visionary as much as it was good fortune, serendipity or whatever. Nothing could have stopped the adoption of von Neumann's machine.

I can remember how it used to bug me when they first started computing the election results on UNIVAC I. It missed every time 180 degrees, but it didn't affect anybody in the least. In no way did it affect them.

HT: Except for one. The Eisenhower prediction was accurate, at least the printout that I saw was. That was the 1952 election.

GT: It didn't do much when Truman was running, but it didn't seem to hurt him in the least. This didn't seem right to me, and it was the same in the previous years too. They could do no wrong no matter what happened..

HT: Do you feel this was primarily attributable to the tremendous reputation that von Neumann had; it's the impact

of that personality and reputation.

GT: Perhaps. It was like sweeping back the tide to make any deviation from that type of machine, no matter how inefficient or how slow or how costly it was.

HT: Philosophically, the group that you were associated with was really in another world from the group at that time centered around the Moore School and then at Princeton.

GT: Yes. Our philosophy was different.

HT: How about contacts with Howard Aiken and his group at Harvard?

GT: No contact at all. He had somehow or other fallen into disfavor a year or two before we got going, so there was no need. Also his machines were quite a bit slower than the CPC.

HT: I know there was a period when he just absolutely refused to recognize the coming electronics, although when he did, he did go into it head first, but that was still in a period when . . .

GT: That was before our time, maybe a year or so.

HT: How about any impact from the work of Stibitz and his group at Bell Labs?

GT: I don't recall any impact. We had many visitors and I may be forgetting some of them, but I don't remember any from Bell Labs.

HT: Who do you remember as some of the visitors that made the strongest impression on you during that period?

GT: Fermi, and a fellow whose name I don't recall in China Lake; Hubbard of IBM, a good man at the time; some people from Oak Ridge.

HT: Were Austin Householder and Chuan Chu among people who came?

GT: Yes. They had an appreciation for what was being done and suggestions on how to do it better. They were in computers.

HT: They were involved in some pretty heavy problems at that point.

GT: That's right, and there was a world of difference.

HT: I know the group at Oak Ridge and Argonne and Los Alamos were concerned with reactor design and that involved some very, very difficult computations.

GT: They built special machines of their own for doing special problems and they had the know how.

HT: Before we break for lunch, Jim or Dave, are there any comments in areas that Bobbi or I haven't thought of to raise that you might feel would shed some light on this area?

JS: I don't really think so. Your research into the various names of people that contributed and were instrumental in getting this going would more appropriately come from Greg and/or Woodbury or someone like that. I was a party to and observed a lot of things, but all the visitors came through Greg's shop and I didn't see a lot of them. I had a big territory to take care of.

HT: There's the danger, of course, of minimizing the problem that you were directly concerned with, and that was keeping these things going.

JS: I didn't really say that to minimize it. It was a new way of life for Dave and me, because IBM had been keyed in the past to an eight-hour day and even the users were keyed to an eight-hour day. Suddenly, we had this twenty-four hour day dumped on our shoulders. Personally that was a big reaction.

HT: In all of the early projects, the people involved just assumed the twenty-four hour day, seven-day week, and it was just part of the way of life.

DM: My overtime the first year exceeded my straight time.

HT: That's a very telling comment.

DM: I think the longest up-time we had on any one of them was 13 days continuous without a technical failure. That's because they never turned it off in 13 days. Any time you turned one of them off, you had a more than 50 percent probability of them not working when you turned them back on. The 701s were much worse than that.

JS: We always had a concern about continuous operation. Having lived through that eight-hour a day situation, we weren't really very sure as service representatives what additional problems we were going to run into operating these things around the clock. For instance, with the mechanical parts, we didn't know when the wear was going to show. We didn't even really understand tubes that well yet,

or what kind of problems we were going to have with them. I wasn't kidding when I said I carried two pockets full of tubes. I'm sure Dave did too.

One of the big things that we were faced with in the beginning, was we didn't have time to do an awful lot of sophisticated trouble shooting. These people were trying to get as much computing done as possible, so we learned the technique of narrowing the problem to a given area and replacing all the tubes and going. Believe it or not they are doing the same thing today. (Laughter) Get it down to three and fix it and get out. We didn't have the time to set up a scope and do a real formal analysis of the problem. As Roy said a while ago, that kicking routine was great, we used it a lot.

One time, during the time I was working with Greg, we found a cathode peeling problem. Do you remember that one Greg? We thought we did. We'll never know for sure. The emission material on the cathodes of the tubes appeared to have peeled and burned out to a point where we would short out a tube. We went through all kinds of these.

HT: Did IBM generate any research on tube development?

: They had a fair amount of research going on in Poughkeepsie at the time.

DM: But remember, their research was aimed directly at general tubes in all of them.

HT: They weren't concerned with the specific problem of building the CPC?

DM: About 1953 they came up with a wholesale replacement of all the tubes on the 604s and CPCs. They did come out with a much better tube. The basic tube in the 604 was a front-end tube of a TV set, a regular commercial tube.

JS: We made a lot of friends that way; we always had tubes available. (Laughter)

HT: As long as your pockets could hold them.

(Machine turned off for lunch)

HT: (Session after lunch.)

We have identified the photographs. This tube that you are showing us that we were just talking about, you identified as the 2BP1. What do these symbols stand for to separate it from the other tubes that you thought this might be?

GT: Two inch is the first one; B is the series or more particularly the phosphor.

JS: No. P1 is the phosphor.

DM: P1 is the standard green phospher, P4 is the black and white you see on television now.

GT: B is the series.

HT: This tube then was made by RCA? And you had a comment about it.

GT: A face plate was evaporated on the front of it. This tube doesn't seem to have it so it's not one of the originals.

HT: You had a comment about Greg's contribution.

RH: What Greg proposed, and what we did on the Wooden Wheel was to use two tubes for each area of the storage, write ones on one tube, zeros on the other, and vice versa we used a differential amplifier to pick up those tubes, which tended to cancel out the noises which are caused by deflection, and it also tended to give optimum results in one tube and the worst condition on the other tube where you were repeatedly reading in areas around a particular spot and read that spot, they would tend to be filled by secondary emission from the tube and that well filling one tube would be the least filled in another tube. So you tend to average out and get good signals.

HT: It's the redundancy of self-correction.

RH: Not exactly.

RH: Noise cancellation.

HT: Let's go back then to this period and start with this document that Mr. Toben showed us, "The Differential Analyzer of the University of California," as the beginning input.

GT: That was the beginning input of the first CPCs. Before lunch we talked about those CPCs, and we've got up to the place now where we left to go with IBM and Sprague and company left to go with CRC. It was a splitting up; the job was done and it was a time of realigning, getting on with the next generation. For us this was the beginning of the 795. That's what we are talking about with the cathode ray tube. The early CPCs had electronic calculators but

not cathode ray tube storage.

HT: Then you wanted to continue our discussion from where we left off earlier and talk about what people know of as the Wooden Wheel.

GT: I believe that's the next thing sequentially.

HT: The 795.

GT: Bill Woodbury met von Neumann at a lecture at UCLA and apparently they hit it off very well at the start. Von Neumann invited Woodbury to come back to Princeton and do the work for his Doctor's degree. At that time he was at Cal. Tech. and didn't have his master's. Woodbury left and went to Princeton and did the work for his master's degree. He was aware of the work on the JOHNNIAC, Johnny's computers and held philosophical discussions on them, but he was never able to convince Johnny that a better machine could be made. Johnny didn't want to go that far into the basic concept and that was it. He had other things to do.

At Poughkeepsie we joined forces with Wheelock, Amadeo, and Harper and developed a calculator using cathode ray tubes for storage of data and plugboards for the control program. Storage was very expensive then, more so than it is now, so the use of expensive storage to store instructions for housekeeping was unthinkable. It was much a faster machine and easier to use than the CPC, probably an order of magnitude faster and easier to use. It was completed in about a year. We spent a few months becoming familiar with their

technology and about a year to build the machine, including many hours of overtime in the snowy, cold Northeast.

RM: Did you realize you were going to get sent to Poughkeepsie when you joined IBM?

GT: Yes.

RH: Just didn't know what Poughkeepsie was like.

GT: Part of the agreement was that when we finished, we could come back to San Jose where they were opening a new lab, and they honored this promise very completely.

RH: Prior to his coming to IBM, we were building what we called a TPM, Tape Processing Machine. Wheelock's part in that was to build the storage for a test assembly. By that time we had succeeded in making the Williams storage device which was sensitive enough so that in the old Kenyon estate, where we were building it and where the bug screens in the windows were made of wires, every time a bug flew in the window it would erase our memory. (Laughter) We changed all that and particularly with Greg's contribution of the push-pull operation of the tubes. We got to the point where we could turn off the machine, walk out for lunch, come back two hours later, turn it on, and it would remember every one of those words of storage that were written on that face of the tube. That was phenomenal in those days for electrostatic storage.

RM: This was the IBM state-of-the-art before they got into 795.

DM: Yes.

GT: No. The 701 wasn't doing quite as well.

RH: The bug screen was more our state-of-the-art.

GT: We scrounged a lot of the parts, some of them were built, and the Wooden Wheel was delivered to Northrop and put into service with a minimum of trauma.

HT: At that time Northrop had the CPCs. They also had a 701.

GT: They had one on order. There were no 701s yet.

RH: No 701s had been delivered.

HT: They hadn't been delivered at that point.

RM: Did this machine precede the 701?

GT: Yes.

RH: They got to the field before the 701.

RM: The 701 would have made it obsolete?

GT: It depends on who you talk to.

DM: It depends on your viewpoint.

RH: We thought we could produce it at one-tenth the cost and about the same speed.

HT: Well the reliability up-time, I am sure, was considerably more than the 701.

DM: Yes, a lot.

RH: They didn't pick up the differential tube storage that I was talking about; they picked up a lot of things from the circuits out of the 795s but not that.

HT: Eventually they (Northrop) got a 701.

DM: No, they got a 704 or 709. They never had a 701, I don't think.

HT: So at that point, all Northrop had were the five CPCs and one 795.

DM: They had four more on order.

HT: How many 795s were eventually built?

DM: Just the one. The rest were 797s.

RH: It went to a 797 which had core storage instead of cathode ray storage. I think there were eight or nine of those.

HT: Was that the first IBM machine with core storage?

GT: Yes.

HT: That would have preceded the 704.

GT: Yes, it did.

RM: When was this work completed?

GT: I think it was delivered in 1953, it may have been 1954.

HT: It did have core storage, though.

GT: Yes. 2000 words.

HT: What was the storage capability of the cathode ray tube machine.

GT: 150 words of storage.

RH: 150 7-digit words. Woodbury maintained that 7 digits was sufficient precision for anyone. He said that if Fort Knox for example, gives the amount of gold to fourteen places, as it did in those days, concentrating all that gold off center on the earth would so displace the center of gravity they couldn't measure it better than 7 places. (Laughter)

HT: For trajectory problems, you probably felt the need for more accuracy than 7 digits.

RH: Subsequent presidents have overcome that difficulty of concentration.

HT: No longer a problem.

RH: Some users of course programmed in double precision on both CPCs and on the 795.

HT: How did people come to attach the name "Wooden Wheel?"

DM: Woodbury, Toben and Wheelock. I don't think you guys instigated it but I think it came from outside.

GT: I think I came up with the name. I remember some confusion from outside wondering whether the machine really did have wooden wheels in it?

HT: It had a wooden wheel on the outside anyway.

GT: Yes.

RM: I wondered about that, too.

HT: Are there any of these in existence? Or have they all been cannibalized?

GT: As far as I know, there are none at all.

DM: This one was supposed to go to the Smithsonian Institution, but some place along the line it got delivered to the wrong place and got chopped up.

RH: There was another one that was supposed to go to some bureau investigation marine life in New York. It arrived in Poughkeepsie on the same day that an ultra-secret cryptography machine from Washington was coming in. They mistook the 797 machine for the Washington one and took the sledge hammer to it before they found out about it, which made the Bureau very unhappy.

GT: I believe that Cal. Tech. may have had one.

DM: They had this one. I flew down and maintained it at Cal. Tech.

GT: The one in the picture? (See Wooden Wheel file.)

RH: Stanford had one of them for a while.

DM: Are you sure? This one was scheduled to go to Stanford and they turned it down and then Cal. Tech. picked it up.

GT: I saw one up at Stanford.

: Probably one of the newer ones.

GT: Were there plans to make this a large production model before the 704 came along, or was the 704 far enough along?

GT: No, only by customer demand.

RH: We were in favor of it, of course, but we were biased, and so was Northrop. When IBM said they had no plans for it, and said they couldn't deliver because it was a one-time deal, Northrop offered to go out and sell 200 of them if they would deliver some more.

DM: I think one of the interesting facets is where the follow-on machines were built.

RH: Archbald, Pennsylvania.

RM: Which machines are these?

: 797s.

RH: It's the end of the line, down among the slag heaps.

HT: How did that happen?

RH: I think our group was a thorn in the side of the groups that were delivering the 700 systems.

RM: It seems like there really should have been a demand, and I presume there was, for this type of machine that was somewhere between the CPC and the 701.

HT: The 704 really, once it had core storage on it.

RH: Most of the customers were waiting for the 701s and were putting all their effort into programming.

GT: You couldn't stop that trend of programming.

HT: I look at the size of this, though, and realize that the low cost and low space requirement, its capability, would have a natural kind of market for people who couldn't afford and didn't need the large machines at that point in time.

GT: I would have thought so too.

RH: One of the complaints you heard from people once they got into stored programs, was that, suddenly, pushing wires into a board hurt their fingers.

RM: Would another thought have been just the fact that when IBM finally did make the choice to swing the other way, they became totally one-track minded and almost turned away from the solid work-horse machine.

DM: Our customers tend to be that way as much as ourselves. For example, the same thing happened in the electrostatic storage. We thought we had a very good, and I am sure we did, and reliable electrostatic storage unit in the 795. SWAC, at UCLA, was built with electrostatic storage and they could never make it work. That gave it a bad name throughout the industry and we couldn't propose any more electrostatic storage.

HT: That's right. I think that comment was made in terms of the 704. It was originally planned with electrostatic storage, but it was soon realized that the customers wouldn't accept this and so they had to delay it until

they could put core storage on it because everybody insisted on it. Of course, when core storage finally became workable, it went from the most unreliable part to the most reliable part of the machine and nobody worried about it anymore.

RH: A lot of the work on our 797 core memory was initiated by Eric Bloch, who did a very fine job.

HT: It does strike me that there was an incredible market at that time for machines like this, of institutions which couldn't have afforded and weren't for a long time able to afford large machines.

RH: Well at that particular time, IBM thought there were only about nineteen customers for scientific computing.

JS: The control panel did have some advantages, some pluses, when you get over the sore finger. For example, the average person could wire that board faster than a program could be written for a stored program machine, and you could certainly load that equipment faster, so there were some pluses in its favor. It's just that it's time had passed. One of the key things about a stored program computer is that it's much easier to maintain than a personalized computer. You can write routines that check the stored program machine whereas the plugboard machines are unique with just about every user.

HT: If this machine had come along 4 or 5 years earlier with electrostatic storage, there probably would have been great demand for it, I think.

: Yes, even with electrostatic storage.

GT: With the end of the 797, things swung over to the stored program and they've been there ever since.

RH: But the biggest swinger against the 797 was the fact that there were no tapes to monitor the program. There were circuits for putting tapes on it, but none were put on.

HT: But you could have?

RH: We could have. We planned on it in the design and that portion which is necessary in the machine was already in it.

DM: One of these machines at Northrop had a thousand-line-a-minute Analex printer, which was put on by Northrop personnel.

HT: One of the questions that hasn't come up but that's been implied in this whole discussion, is this working relationship between Northrop and IBM in terms of these individual, if you will, personalized machine developments related to Northrop's own special problems. Do you have any commentary on the development of the relationship between Northrop's needs, IBM's ability to make machines, equipment that it had, and the kind of relationship that evolved between these two organizations?

GT: I don't think it was anything unusual. I was a tab operator as they called them in those days and I had several years of experience with IBM machines in payroll and accounting even though I was an EE. In the payroll accounting type of work, IBM always took care of the customer, no matter what. The Bureau of Power and Light needed something with three

forms going over the printer, IBM made something with three forms. If some other customer needed a wider, taller, narrower machine, IBM made it for them. It wasn't unusual for a customer to ask IBM to make something special, and it wasn't unusual for IBM to take care of them. It was a matter of course. It wasn't any new relationship.

HT: But you're describing a 6-year, 7-year continuing interrelationship with IBM, its engineering staff, its research people and production ability.

GT: We probably carried it to a greater extent than most other customers, but the basic idea was that IBM took care of its customers. I'm sure that's why Northrop didn't really worry when BINAC didn't work; they knew somehow or other IBM would back them up.

RH: Another customer with whom IBM had good relations was Monsanto.

RH: After I had left they ordered the first 701 or 702 and were always eager to try out new things. When IBM announced a new machine, they would call me up on the phone, I was their service man at that time, they would call me up on the phone and ask me if I knew anything about that machine. I would tell them no, I haven't got any material on it yet. Well let's order one and find out what it is. (Laughter)

HT: What kind of computational problems were they involved with that were of a similar scientific nature or related scientific nature?

RH: I didn't get into that like I did with Northrop. It was more chemistry.

HT: How about Mc Donnell in terms of your experience? You talked about some of this earlier.

RH: The fellow in charge of the was Henry Kryzer. He was stone deaf. Back in those days it was no handicap around some of those machines. (Laughter) He was a very capable man early. As I said, the Computational Laboratory was first set up by Dr. Bubb, Columbia University in Missouri and Dr. Arthur Compton. Bubb was sort of a frustrated analog computer man. He had developed the optimization of oil well drilling and sold to one of the companies one of the oil well companies for five thousand dollars and was highly disturbed to find out that they turned around and were leasing it to other companies for a $100,000 or $50,000 at a time. (Laughter) He had more to do with that area. I think you should speak to Henry Kryzer if you really want to find out their history. McDonnell was relatively unknown, and weren't able to get the cooperation that Northrop got on special machines.

HT: Could I back up a little bit on the Wooden Wheel and speak to the point of the individual roles that you played and Woodbury played and Wheelock played in the development of the 795 and later the 797?

GT: Yes, Woodbury was the key man. It was his design. I did the cathode ray storage part, the power supply and the auxiliary feed. Wheelock did the electronics for the whole machine, the logic and trouble shooting. Harper designed the circuits; Woodbury did the systems design;

Amadeo, our technician, scrounged parts in a wonderful way; and that was about it.

DM: After it arrived at Northrop, I did the punch and the printer electronics.

GT: Dave Montgomery serviced it and kept it running for 2 years.

HT: Is that about how long it stayed operational at Northrop?

GT: Maybe 3 years.

DM: It was operational 2½ years at Northrop and then it went to Cal. Tech. and was their main computer after that. I don't know how long it stayed at Cal. Tech. I made trips down there for 6-9 months.

HT: Who were the people at Cal Tech that were most closely connected? The only name we have is Professor Frankel.

RM: He's no longer at Cal Tech.

DM: I don't know the man's name, but there was one man working on his master's who was doing most of the maintenance on it.

HT: Who were some of the people who were using it?

DM: Generally the trips down there were when they couldn't fix it. I would leave here about 5:00 in the morning and I would be back by 9:00 at night. I didn't spend any time with the people.

GT: I don't remember the name of the fellow that was, I believe, first head of their computer center there. He's at UCLA now I think and he had some connection with UCLA's cathode ray tube machine, but I can't think of his name now.

HT: What are some of the other West Coast installations that

you remember as being involved in this early period? Take Hughes for example. Do you remember any early work at all?

GT: China Lake was the second one that I recall. Of course Lockheed and Douglas were very close after us. They didn't do any developing of their own, but they picked up a CPC right away. China Lake Naval Ordnance Station was even earlier than that.

: Wasn't North American doing some work in development?

GT: I don't remember any. They were really very slow to pick up new stuff actually.

HT: How about the university installations in the West. I'm thinking primarily of UCLA, Stanford and Berkeley.

GT: No. Except for the Differential Analyzer.

: That's not true, UCLA did put in this cathode ray tube machine and had a computing center.

GT: I don't recall them doing any work at that time. UCLA had an ILLIAC about 1954 after somebody else had worn it out and given it to them.

HT: Berkeley did build their own machines, the CALDIC. That was the same Paul Morton that is in this list on the differential analyzer installation.

: Who was doing the MANIAC?

HT: MANIAC was Nick Metropolis and a group at Los Alamos.

GT: That's right. At the same time that we were working on the 795. I think we might have beat them by 6 months or something like that.

HT: Chuan Chu's group was working at Argonne on a pair of machines; one for them and one for Oak Ridge.

GT: Yes, that figures.

HT: He would have come out of that same environment at Princeton that Mr. Woodbury was in. It would have been about the same time.

I would like to back up to the 1947 period and your experience and the input from the differential analyzer at the University of California. You talked about this when we first walked in as being the thing that turned on so many of you. Could you just recount the experience at UCLA.

GT: We had been calculating trajectories for maybe a year and we were very interested in all kinds of computers. We wanted to hear about anything that had to do with computing, and if it could be seen we went to see it. This thing was just being installed. UCLA didn't have very many buildings like they have today; it was a rather sparsely settled campus at Westwood. We went over en masse. Everybody just picked up and went over to look at it as soon as it was available. I think several other Northrop groups also went over. For some weeks after we would get together and say, "Gee, why couldn't we do this with our machine? That's a good idea." I thought that's what started DIDA.

HT: The original DIDA, the digital differential analyzer.

GT: I don't recall hearing about it before the UCLA experience

and I do remember seeing that idea pushed three or four or five places right afterwards. I know it influenced our thinking. We were doing strictly formula type calculations, A + B + C, and we hadn't attacked differential equations. Immediately when we came back from UCLA, we immediately pushed this garden variety stuff on the second shift and went after those differential equations. Things really hopped after that. Difference equations weren't as well known as they are now, but Woodbury, again the key man, applied them practically instantly. Less than a week after we got back from UCLA we were solving some pretty fancy differential equations.

HT: So really the impetus for MADDIDA goes back to that period, which would push it back to Vanneuar Bush's very, very early developments as being the prime influence.

GT: It seemed to occur to everybody at once. It just blossomed, in my recollection, anyway.

RM: According to the chart, Mauchly came to Northrop quite early, in the late forties?

GT: Yes. 1947.

RM: Did his lectures have a lot of impetus on where you went from there with Northrop?

GT: I don't know. It didn't have any affect on me. I couldn't find anything useful in his lectures. But I think the other groups did; it fit more along their line of thought. For card walloping, day-to-day computing, he didn't have much stock.

HT: How about your contacts with Pres Eckert in terms of the engineering aspects of the electronic machines?

GT: Woodbury had all those. I didn't have any and they didn't come to Northrop to talk about the technical part of the machine. Baker went there and all the conversations were between Baker and Eckert. I don't think Eckert realized IBM existed at that time, or if he did, maybe it was a source where you get the cards punched or something.

HT: It's interesting that you had so many different channels going at Northrop almost simultaneously. You had the channel you were involved in with the CPC, you had the evolution of DIDA into MADDIDA, you had the BINAC on its way and eventually delivered. Jerry Mendelson jumped off into the Quadratic Arc Computer, and earlier you had devices for taking readings on fixed stars and computing trajectories. I'm really impressed at the number of channels at that time that were going, all because of computational needs.

DM: There's another one you didn't mention. We were trying to do some automatic numeric control, or automatic control, on manufacturing.

GT: A contour follower or something. We were going to carve a wing out of solid metal, mill it out. They did.

: It was Remington Rand's equipment. computers.

GT: The flying wing, a revolutionary concept, came out of Northrop.

HT: What kinds of computational problems did you get involved

with relative to the design and development of the flying wing?

GT: There were so many that I can't summarize them except to say that stress analysis was probably half of them. Windshield design--there are certain temperature strains in glass when you take off quickly and fly to several thousand feet. It's phenomenal. How many heating elements do you put in and how far apart.

HT: How did you get involved in the nuclear design problems?

GT: They had a contract for a study, but no work was done until the computers came along.

HT: This was with the Atomic Energy Commission itself?

GT: Yes, at Oak Ridge--a reactor design. Shielding design.

I have often wondered why so many good people showed up at one place at the same time. I've figured out that maybe it wasn't that they were all good people, but that they interacted and stimulated each other. That is a more likely explanation.

HT: I think an important aspect of any development, particularly when you're breaking new ground, is to have an environment where people can interact and can try out ideas. I have the feeling on many of these problems that you really didn't know what you were going to do until you started doing it. If you had to sell a contract on the basis of where you thought you were going to go and nobody would have listened.

GT: Right.

DM: On the Wooden Wheel, the computation that impressed me the most were the calculations on how to strap a man into a rocket sled, and how to decelerate that rocket sled by calculated Gs to a man and still survive. Colonel Stapp was the man they put on the rocket sled, they got him up to speed and started scooping the water and his eyes came out on his cheeks and things like which are history now. The thing that really impresses me is the mathematician that was running that job was not really mechanically inclined. Most of his jobs were the only ones that wouldn't run on that machine, so I generally assumed that it was just the man. After the test was over with, because by this time I found out what he had been doing, I wasn't sure whether I'd maintained that machine right for him or not. He survived.

HT: On that picture that you were just pointing to of the Wooden Wheel with Murray Lesser in it, there's that Wooden Wheel symbol on the machine. Where did that come from?

GT: Somebody in Poughkeepsie gave it to us as a gift, as a good will gesture.

HT: Was this on all of the machines or just that one?

GT: Just one.

HT: Just on the first one, the 795? Just that one?

GT: Yes. It was too maverick a machine to have the IBM logo on it. (Laughter)

DM: I don't believe IBM was on the machine any place.

HT: They did give it a number.

RM: There is documentation, isn't there?

: Probably not.

DM: I probably have the only documentation on that machine. Because they clamped down on security and we can't throw anything away right now, I just happened to still have it.

RM: What is it?

DM: It's the documents I used to maintain it with. It's a book about this thick.

HT: Is there any chance of getting a copy of that?

: Yes.

GT: I have our own write-ups on the basic machine.

HT: I would like that very much. We'll be happy to return it.

RM: All of this will be returned to you.

HT: A document entitled "Electronic Calculators," that's 797 though, the follow-up version.

RH: Do you have that report by Woodbury which starts out "The 795 is a collection of parts awaiting an assembler," or something like that?

GT: No, I don't.

RM: Do you?

RH: No, Woodbury might.

HT: If you run across any of these early documents, we would be most interested in them.

RM: Unfortunately Woodbury doesn't have any.

DM: That's really a surprise.

HT: Here is the block diagram, too. It is a much smaller

machine than I had pictured, not having seen it before.

DM: I think if you look at that block diagram you will see that it's quite competitive with today's thinking.

GT: It had the first address registers ever.

RH: It had high-low-equal compare on them.

: Do they say if this is bigger than that?

: They had something else I recall.

: A significant figure counter, a significant digit counter.

RH: There was some other technique used in this I was trying to remember. Do you remember a discussion during the time they were building the 704 and Woodbury's contribution to this was brought in. One of the engineers said, "I wouldn't touch it with a ten foot pole," and he was told, "You can't, you're standing too close to it." (Laughter)

HT: That's another area that we haven't even touched on. In terms of the design of the 704, what relationship is there between the 797 and the eventual 704?

GT: I guess Harper has a better feel on that than I do.

RH: I would say that physically there was no correspondence between the groups; different groups designed each of the machines. As you know in the case of 797, it was even developed in a different state. Actually there was a good deal of peering over the shoulders, and quite a few of the ideas of these machines ran into the other with sometimes grudging acknowledgements.

GT: I recall in the 705, at least, that they had begun to pick up these ideas of control by registers and shift control.

RH: The interrupt routines for servicing I/O.

GT: The I/O, yes. Those were first worked out in the 797.

DM: Fully buffered input-output registers in it? It was easy to do floating point if you wanted to, because all the data that was transferred kept track of the significant figures in it. It was set up to make decisions on it.

GT: They were good floating point machines.

HT: We were talking about your power design for this. What kind of power were you faced with in a machine of this size?

GT: Two 604 power supplies approximately. We did build with the materials at hand, and it turned out that that was the easiest to get. We got special transformers and special parts from Power Equipment Company in Galion, Ohio, but it was essentially 2-604 supplies. We had more gates, more tubes and therefore needed the same type in voltages.

HT: Who were your contacts at this point in time with the people at MIT working on Whirlwind and core memory?

GT: No contact at all.

HT: How about at an earlier period? We mentioned some of the other Eastern developments and we neglected MIT. Did you have much contact with Jay Forrester?

GT: I believe we had some MIT visitors at one time, but we never went up there. We were working at least fourteen hours a day every day.

HT: So you really didn't have time to go out and visit the others?

GT: Right. We knew what we were going to do and we didn't have any unsolved problems that we needed to scratch our heads and go seek help on. It was straightforward, let's get it done and go back to California.

RH: The weather helped.

GT: The weather did help. (Laughter)

HT: Were there any particular engineering problems that you had to overcome in terms of the cathode ray storage?

GT: It worked pretty much the way we expected it to and we had no hitches, glitches, morning sickness or anything. Harper was chained to the machine at that time.

HT: Before you decided to go the route of the cathode ray tube storage, had you looked at some of the other potential storage devices and if so why did you reject them?

GT: I don't remember.

: I don't believe there were any others of interest.

HT: In the early period, the Princeton people were trying to design the storage with the Selectron tube that was being developed at RCA.

GT: The price for that was enough to discourage you.

HT: So you didn't even bother to look at it?

RH: They were working on it in the lab. I'm sure we looked, but special tubes kill a small machine.

DM: This rented for $2,500 a month.

HT: The 797?

DM: The whole business, punches, cardreaders.

RH: The 795 development was so cheap and developing the 701

was so espensive, that they took a sizeable chunk of expenses from the 701 and transferred it to the 795. They said, "You swiped your parts from us, we're going to charge you." That's not really true.

HT: The time period was relatively short, partly I gather due to the weather.

: Greg left in late 1952?

GT: Early 1952.

: He was back at Northrop in 1953 installing the computer, so it was a little over a year.

HT: One question that all three of you ought to respond to is: who are some of the individuals you think we ought to be sure and talk to, in this period.

GT: Murray Lesser is the first one I would hope you would try to see. Woodbury of course you are going to see. I wish I could remember the name of the fellow at China Lake. I haven't had contact with him since I left Northrop and his name doesn't come to my memory. If you get down there, nose around and find out who did the work on the early CPCs, because he was good.

: He was a maintenance man?

: No, he was an engineer.

: I remember the first CPC we got, they really didn't give us a complete CPC. They made one and then they split it up. They gave the 604 to China Lake and they gave us the printer.

: That was the first Model II.

GT: Yes. Joney Dayger was in charge of the printers. I guess he was in charge of building the CPC too.

GT: I think he was in charge of Betsy. I thought Wooding was the engineer on the CPC.

: Rae Wooding was one of the engineers that put that original one together. He's retired.

: He was one of the engineers on the Model I CPC.

RM: Joney Dayger has also retired?

: Yes.

: Jack Trout would probably know the name of an awful lot of people.

: Jack Trout was kind of the central maintenance in Endicott, center of competence or something like that. He would know some of the maintenance people or some of the other problem areas.

RM: Where is Trout, do you know?

GT: Endicott.

RM: And Dunwell?

RH: Dunwell is in Endicott as far as I know.

RM: What was Dunwell's contribution?

: He was project engineer, I believe, on the CPC.

RH: At least maybe in planning. He was in the Future Demands Department.

GT: Oh, that's one I haven't heard for years. Perhaps he was.

RH: He was doing a good deal more than most of the Future Demands people. He was actively getting in to designs of machines.

GT: You had some comments on the 701 that you made before we turned the tape on.

RH: There was quite a bit of competitiveness between the 701 and this machine at the time. They were trying to get their Williams tube storage running, and we had officially been assigned the job of building the storage that they were supposed to pick-up. They chose to go their own path and a year after we had ours running solidly they were still going down that path. Finally, some of the ideas crept back in, but not all of them.

GT: Your other comment is the one where you indicated that the original impetus for the 701 was Prudential.

RH: I think it was Prudential who had come to us before they had just heard of the possibility of electronic computers. They came and asked if we could produce a machine for them for actuary tables and so on within a given length of time. That was originally given to the engineers under George Daly in Endicott, and shortly after, in the summer of 1948, was transferred to Ralph Palmer in Poughkeepsie. Nat Rochester, Byron Phelps and a few other people were part of that from the beginning. The instigation was the Prudential Insurance Co.

GT: You also mentioned that Palmer was a key person to talk to about this.

RH: Yes, he is definitely a key person in IBM so far as electronic computers are concerned. He started working on electronics, triggers and things like that, back in 1939.

He did quite a bit of tree shaking to get some of these electronic apples to fall during that period. It wasn't until 1948 that he got his chance.

HT: Where is he now?

RH: He's retired and I believe is living in Boca Raton. He set up a laboratory which was excellent for the type of development that was necessary for IBM at that time. For example, Endicott was quite severe and there were "No Smoking" signs all around. McDowell called him up and said, "You'll have to put up a "No Smoking" sign in your laboratory too." Which he did; behind the bulletin board on the wall. He was a great man to have on your side in that early development. He prepared a culture that really caused those seeds to germinate.

HT: I'm impressed with this inner weaving and the kinds of intellectual discourse that went on. I look at the chart of Northrop and I see all these different divisions with different names attached to them, but then as you talk about the period, I find you talking about people as though they were all one large division. It seems to have been a fairly free-flowing kind of operation.

GT: Yes. Security was our worst enemy but even so we got through it.

HT: The SNARK project, at that time, was a classified project. This may have been one of the inhibiting factors in terms of information coming out about what you were doing. That's important to remember.

GT: It hurt us too, because we couldn't go into their building after a year or so. They had a whole building by themselves with a ring of guards.

HT: Did you have much connection with the bootleg activity of the neon tube developments that were going on?

GT: Other than to observe them and be interested in them, no. Hagen did the work. I believe I still have a box of those tubes.

HT: Theoretically it looked like it should have worked.

GT: It was neon with something else added, maybe.

HT: He and Charlie Williams apparently ran a bootleg operation for a while.

RH: We did some work with neon.

GT: Everybody did.

By using special tubes they were able to make them pretty small. They didn't have any readout like we have on LEDs now, but they did have a little geiger counter that would fit in your shirt pocket. You could read it out binarily. It glowed. The glow was strong enough you could read it.

HT: You got a binary readout and you converted it yourself.

: It wasn't hard because the two, four, eight, sixteen and thirty-two was right above the light so that you could get a second level count.

RH: We did some of that too and we ran into what had been known as the Macy effect. Did you ever hear of that?

HT: No.

: That's kind of interesting. Apparently they had some neon tubes as voltage regulators in a closed-in box installed in the fifth sub-basement of Macy's. The voltage wasn't regulating, so they took the cover off to find out what was wrong and it worked perfectly; put the cover back on and it failed. Apparently what had happened is that neon will not ignite or turn on at all if there isn't an instigation from either cosmic rays or light, either one of which will start it going. Normally you can put the cover on at ground level and there's enough cosmic rays to trigger it, but down on the fifth floor sub-basement there was nothing at all. The Macy Effect was this effect that neons would not be triggered at all, you couldn't make them operate as storage or anything else, if they are operated in a completely shielded environment. (Laughter)

HT: You probably reintroduced the phrase. There must be something analogous to it. Bobbi, can you think of any other areas that we have ignored or left out?

DM: I have something I want to say about the reliability of the tubes for storage design. While I was at Northrop, IBM was going through the throes of deciding what technology to go to next. They actually sent out a man by the name of Max Paley, and another time Dick Jones, to review the failures. There were some very high level meetings concerning staying with this technology instead of switching to cores. It took a rather traumatic meeting of several

people to decide to go to Cores. One of these times, when the waters off from Florida were SNARK invested, they had had some major computing activity on the Wooden Wheel, and they went 13 days straight around the clock with no adjustments or anything to the tubes. I think the 701s were probably getting daily maintenance in the storage area.

HT: Of course, I'm not sure that the 701 wasn't envisioned as sort of a stop-gap thing until something better could be developed. As you said, it was planned originally for very limited production, although I think if they had wanted to build them they could have sold a lot more.

DM: I think the plan of how many we can sell was typically misjudged. Marketing people almost always are low.

: They figured they could sell twenty-five at the beginning of that project.

HT: Back in 1940s, people never really saw the need for more than a small handful of computers to solve all the computational problems that we were going to have for a long, long time. There was still a carry-over of, How many of these big, high-speed computers is the world going to need? Nobody really knew.

GT: The word computer didn't mean the same then. It had an entirely different meaning. The machine was a calculator; a computer was a person. I can't quite find out when that shift occurred. The most authoritative source, unfortunately, goes back too early. The Oxford English Dictionary of 1933,

gives computer as only a person. Sometime in the forties it changes.

GT: Friden and the desk calculator began to change that.

RM: I've seen some turn-of-the century, handy-dandy devices, named Mr. So and So's ready computer.

HT: In some of the early acronyms, the capital C usually stood for calculator or calculations. That's a fairly recent thing within a little bit more than two decades.

RH: Early in IBM development the distinction was that a computer had a stored program and a calculator did not.

: The 701 was called the Defense Calculator.

HT: Do you know the origin of that title?

: I think it was the Korean War. They changed the name because they would be doing something to help the Korean War. That was about 1951.

E N D   O F   I N T E R V I E W

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