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Computer Oral History Collection, 1969-1973, 1977

Interviewee: W. W. Woodbury
Interviewer: Robina Mapstone
Date: January 15, 1973
Repository: Archives Center, National Museum of American History

MAPSTONE:

The date is January 15, 1973. This is Bobbi Mapstone and I am interviewing Mr. Bill Woodbury in San Francisco. I'd like to start with the trip you and George Fenn took to New York in 1948 to convince Watson, or IBM to follow through on your machine hook-up. At this time, I believe, you had made the initial hook-up (601 and 405) at Northrop.

WOODBURY:

This is not unique, you know. The Watson Laboratory had pieces of machinery in the corner with lots of wires running between. I don't remember the combination, but they were mechanical and I think they used a relay multiplier of some kind, I'm not certain. The hook-up was a little faster than their production 601, which was all that was available, but it was of no particular consequence. The interesting point here was the existence of the electronic multiplier, which made it possible to produce a fresh product for every turn of the tabulator. The ratio of additions to products is such that there are many plus signs and only occasional multiplication signs. By using the tabulator and the multiplier, you could line up several counters and possibly make several additions during the time the machine performed one product. So this was an elegant device considering what was available.

It is quite a different story now, but at that time it was great having a multiplication every time you turned over this thing that did your adding for you. In effect, it made your multiplication times disappear. You always had enough adding to do, so you never had to calculate multiplication time; what you had to calculate was how many numbers you had to add up.

MAPSTONE:

In a way, though, you did something which was quite brave in the sense that IBM did not let people get their hands onto the innards of the machines, You probably broke the law there.

WOODBURY:

I don't know what the law of the matter is. I have the impression that IBM protected themselves from getting their machines torn apart or ruined by people that operated them by this type of prevention. I also had the impression, wherever I have been that they didn't quarrel very hard with anyone who could make use of the machine without damaging it, as it were. In other words, customer engineers would make accommodations with the people in the installation, and nobody welded up the covers to these machines. In fact, a good part of the machine service is done by the customer. Card jams, and difficulties that require taking off covers were always present, and still are. We still go into machines and take things out of them; that's how we keep busy and how we keep the installation going.

MAPSTONE:

Give it a kick and it will work.

WOODBURY:

Do you know the story of the kick on this machine (CPC)? The inter[?]ck on the multiplier section would occasionally stop when it was used on its own just to multiply extensions on the cards, and you couldn't let the cards out without taking the thing apart. One day Strauss Gibson was pushing buttons and trying to make it go. Finally Gregg Toben called over, "Kick it, Gib." So he did, and he kicked a cover right into a sixty ampere fuse block. It made such a hiss of sparks and racket, that Gibson never kicked another machine. (laughter)

MAPSTONE:

Whom did you see, and what was the reaction you got* when you went to New York?

WOODBURY:

I sketched out what could be done. There were, of course, people like Floyd Steele and so on who would have just loved to have done the combining of the machines, knowing that an electronic multiplier, the 603, was already in existence. Jack Northrop attempted to ca1l Thomas Watson, Senior, and didn't reach him, but the fact that Northrop was after Thomas Watson, Senior, to get something done stirred up the organization. They promptly flew George Fenn and me to New York. They sent me because I had sketched out what was to be done, and George Fenn, because he had the problem by which we could actually justify this solution, versus going to the ENIAC or to IBM's Selective Sequence Electronic Calculator (SSEC), a very large but slow machine. I don't remember what other possibilities were available at the time. Also, Fenn was more able to lean on people than I am, and, with my knowledge of the problem, he could make sure that he hadn't overlooked anything. The two of us went back there and carried on a dialogue.

MAPSTONE:

Do you remember with whom you carried on the dialogue?

WOODBURY:

I believe Al Kimball was in the offing, but the principal person was John McPherson. John was somebody I liked. When I left IBM I think he was very put out. He had strong feelings of corporate loyalty and so on. He impressed me as a super chauvinist. When I first went to IBM he assumed I was going to stay there and, of course, I did too. I did not assume that they would engage super bureaucrats like Manny Piore to be part of the company. I didn't reckon with how large a bureaucracy IBM itself was.

MAPSTONE:

Did you know Eckert?

WOODBURY:

Wally Eckert? Oh surely. He was the person that showed us the machine at the Watson Laboratory.

MAPSTONE:

That's right, because that was his baby.

WOODBURY:

He was interested in astronomical problems. He had a big machine down there which, I assume was almost laid out to do these astronomical problems. It didn't seem to me that they did anything very interesting with this machine, and certainly what I saw didn't make me feel it would be very easy to configure it to do a problem. There was no simple layout of selectors, accumulators, multiplier input and product return, which of course was what we got as soon as we had the 603 multiplier.

It had built-in column shift and rounding arrangement, plus eighty counters in the tabulator, and just lots and lots of relays that selected and cycled with the machine just the way you wanted them to.

MAPSTONE:

SWAC was not built at this time.

WOODBURY:

They were trying to make SWAC go when we arrived with the Wooden Wheel, and it was quite a revelation to them. One of their men came down and looked at the sequence from our cathode-ray tube. He was quite shook.

MAPSTONE:

I'll bet he was. They had a lot of problems with that machine, but it was a good machine when it ran, which is more than we can say of some machines.

WOODBURY:

Cathode ray storage is the place where I probably got to the top of the art. I don't think anybody ever achieved anything better than we did on the Wooden Wheel; in fact I don't think anybody came close to what we did.

MAPSTONE:

When we get to the Wooden Wheel, we'll go into that. To most people, the cathode ray represented really bad problems.

WOODBURY:

Ours was basically more reliable than the 604 circuitry that was supporting it. The repairs to the machine were down in the arithmetic circuit not up in the drivers and the amplifiers of the CRT.

MAPSTONE:

So McPherson went along with your plan?

WOODBURY:

Yes, they decided they would do it. I came back West and I gathered from some people that I knew at the time, 0. B. Schaeffer, Johnny Dager, and some others who did the actual work that Truman Wheelock was in on this. It's too bad he's gone. He was a salty individual and he should be telling the whole story

MAPSTONE:

What was Wheelock's contribution to the machine?

WOODBURY:

I'll put it in his words. Ha was an expert at separating bull-shit from buckwheat, [laughter] and he was able to recognize other people who could.

MAPSTONE:

Took one to know one, right? [laughter] Was he involved in the original CPC hookup?

WOODBURY:

I believe so, but this is behind the IBM *creen. I did not see this; I just heard this from him afterwards and I wasn't sure just what place he had in it. I just know he was in there doing his special work along with other people, for example, O. B. Schaffer.

MAPSTONE:

I believe you called the original hookup Betsy. Did IBM do the link-up according to your own design?

WOODBURY:

No, no, no. I spelled out what we required; IBM used their available relays and technology. I just didn't have any feel for how well they would execute it. I expected to do a great deal more plug wiring on that machine than I had to. Their cycling of it. Their addition of the second board and the way they organized it, was very well thought out. I gave a paper on the machine at Harvard and I was going to include a joke but I was a chicken. I would like to tell you this whole story, a true story.

Professor William Feller, who had been at the Institute for Numerical Analysis during one summer, came to see what we were doing. I got acquainted with him, and I believe he was instrumental in my obtaining the chance to go to Princeton and work for John von Neumann. He never let on that he was involved. He just said if I ever wanted any help to let him know. Then, when I wrote to John von Neumann, it all happened by itself. Anyway, I told him this joke and I said I was reluctant. I'd like to tell it. "IBM built this machine in four weeks, which didn't give them time to think, and it turned out to be a very good machine." Willie was a refugee from Budapest. He had come across Europe just ahead of the Nazis and on several occasions along the way barely missed being made into a lampshade. He said, "You should always tell a joke if it's a good joke because a censor won't understand it; and if it's a bad joke and he does get it, then you deserve what happens to you." (laughter) But I was chicken and I didn't te1l it, so I probably belong in a lampshade.

MAPSTONE:

It's a lovely story.

WOODBURY:

That's really true. Some people who knew what was wanted and had the New Product Development, and those people who must add on to a simple engineering notion the things they believe the customer should be spoon fed, didn't get their hands on it. Betsy was done by engineers who, I think, saw what I wanted to do, and proceeded to use the best things they had to do it with. I had no idea what those best things were. They did an elegant job, just an elegant job.

MAPSTONE:

How about with the CPC follow-up?

WOODBURY:

Product Planning got into the act, and they locked it all up. Instead of having the ability to break up these accumulators according to need--there were eighty of them; they came in twos, fours, sixes, and eights and we could combine them; sometimes we only went them to count to a hundred, other times we'd need ten places because you were going to throw away most of the right hand part of the integration--they [?]nked them up so it was fixed. It turned out that this machine with all its extra equipment and extra storage was not as effective as the original machine, even on simple examples. When we confronted IBM with this, they opened it up, which, of course, doubled their work. Instead of providing a simple, direct machine, there was this channel system built-in, and the over-the-board stuff that you could wire. Now you've got chaos.

That was very hard to work with and people did tend to take an instruction per card, and the instruction would be quite elementary. We had so much nuclear work going, and we had already gotten used to the idea of parallel accumulations to do our arithmetic. That it was not any great problem to use that part of the new machine. Many of our people did use it and wired it. But the more complicated machines that went to Douglas, and North American, and so on tended to be wired for fixed instruction sets with ten-digit multiplication, or eight-digit multiplication and division. That was the way it was operated. Then you punched cards for each instruction. We would never have gotten very many nuclear penetration studies if we had gone along with it that way. We wanted to introduce a new random number with every card in order to carry forward the bouncing of neutrons through shielding and so on. If we had needed a card per multiplication, by the time we calculated the results of a single random bounce, it would be many cards later. The essence of getting penetration studies this way is to get lots of particles.

MAPSTONE:

I believe at this point, you hooked up a cathode ray tube or scope to Betsy to trace the neutrons.

WOODBURY:

I don't recall this. I can remember using an oscilloscope to try and find out why Betsy wasn't operating just right. We used an oscilloscope to do some diagnostic work theme. It was interesting the sharpness of the impulse from the 6L6 or [?] 5 LCs, I've forgotten what the driver tubes were, that operated the magnets on the tabulator, were so sharp, it turned out that you would drip the latch in the mechanical counter, the point that received the signal going back, a little late because of the timing of the machine. That's not right, I know, but when we studied it our first thought was to delay it a little more an d then they worked okay, but than what I understood was that from these metals coming like this, these were going to fly down here and catch and carry. See there was a ratchet going here, each one of these representing 0, 1, 2, 3, 4. Well, it was bouncing from here onto the top of the next row, occasionally using a digit for in addition in coming back. This was just a fault of the machine that hadn't been run out. We made it a little later so it fell in farther so it couldn't bounce over this coming great well. Where it really wanted to land was on this upper ramp and slide down. So I just remember still deducing that in the night after we had made this adjustment that fixed it for the moment, and then realized what had been happening, how we had fixed by going later when what we really wanted to do was go a little earlier. So we changed the timing slightly. This had to do with a little less tolerance on the timing that you could turn on the pulses to drive the vacuum tubes to come back as opposed to what you would do if they were passing through relay points.

MAPSTONE:

What I was referring to is an oscilloscope device you had hooked up to follow the path of the neutrons. That doesn't ring a bell?

WOODBURY:

No, but a lot of this work went on after I went to Princeton. I remember something like this but it's not something that I was immediately involved in.

MAPSTONE:

The CPC--let's call it Betsy to keep it all clear; it's the first one that was hooked up for your project on special order--arrived in 1948, is that correct?

WOODBURY:

That would be my guess. I drove a taxi in the fall of 1947. It would have been spring, 1948 when Betsy arrived.

MAPSTONE:

It was after you came back from Northrop that you went to New York.

WOODBURY:

You understand I got my original education at Remington Rand during World War II. Then I came to Northrop and worked on this horrible aerodynamics problem. I wanted to do the work on the IBM machines, and that's how I got to play with them. That would have been in the fall of 1946.

MAPSTONE:

In February or March 1948 you went to New York and the machine came back some six weeks later.

WOODBURY:

Yes. I suppose there were a couple of weeks while IBM was deciding what to do. Then they flew our tabulator east and returned it to Northrop all put together. After that we spent the summer trouble-shooting to some extent, but getting our problem done at the same time. We worked the problem and also found examples of problems such as I described. We would occasionally lose one digit.

MAPSTONE:

After you got Betsy working, you went to Princeton, right?

WOODBURY:

Oh, yes, it was quite some time after Betsy was working. In fact, I think the CPCs were around by the time I went to Princeton in the fall of 1950.

MAPSTONE:

When you went to Princeton, were they in the process of developing the Princeton machine?

WOODBURY:

Yes. They were having quite a time and had lots of cathode ray tubes on it.

MAPSTONE:

Is that where you started to get your really familiarity with the cathode ray tube?

WOODBURY:

I didn't get admitted into the hardware fraternity in Princeton. Of course, it was where I wanted to be, and I was very frustrated because I was immediately put to work with Herman Goldstine. He is a mathematician and he was very glib on how arithmetic wants to powers of two and so on. While this was all as mechanical as teeth on gears, it wasn't the bailiwick I enjoyed. I enjoyed seeing how mechanism could be brought to bear to do these things. Whenever I could, I would puzzle out these circuits being worked out by Julian Bigelow to operate the machine.

It was watching this work going on, and the problems of making the storage hold still, that carried me into the thought about organizing pieces of a machine and bringing them together at the control panel. That already existed because the 604 did that in its way; but the 604 didn't have this general ability to address storage, and to do its selecting and computing of addresses as well to do its arithmetic. Of course, this has to be provided by either a succession of program steps or by suitable mechanisms. In either event, the suitable mechanism must exist, whether you use a single adder to take care of what address is next, or the product of two numbers. I was working with a 604. They had this 50 kilocycle pace, but I wasn't interested in trying to advance the electronic art--there were lots of people around to do that--all I wanted to do was to advance the organizational art.

MAPSTONE:

Was anybody really listening?

WOODBURY:

People were listening, but it didn't have much effect.

MAPSTONE:

Did you talk to Bigelow?

WOODBURY:

I talked to him a little but he was very difficult to talk to. I enjoyed Julian in his way, but I couldn't tell him anything about it. It wasn't till lot later that I realized there just wasn't as much known as I thought when I was entering into the field. I don't know how people got into electronics but I suspect from amateur radio. I have since concluded from what I've seen that amateur radio operators don't really know much about electronics. They know a great deal about what the chart says will make an oscillator and broadcast music, but they don't really have a deep understanding of these very subtle goings on.

MAPSTONE:

I think radar was one of the major contributors.

WOODBURY:

Did I discuss the ElectroData machine?

MAPSTONE:

No, it's one of the things we have to get back to.

WOODBURY:

Yes, because that was a very interesting thing to see the impact of radar on electronic computing.

MAPSTONE:

This is one of my questions to discuss later. You had your talk with Johnny von Neumann about your ideas, and he was very adamant about the route he was going, and you felt that your route had things to offer. Do you have any feeling about why he was so insistent about following his method?

WOODBURY:

He thought it through. Also he was a mathematician. After all, to a mathematician the notion that you can sit here and develop a list of things to be done and then key punch it, or whatever, and have the machine go through it is very nice. There's nothing wrong with it. I've no argument with that, except, to me, it was not using the machinery to its capacity.

MAPSTONE:

It's been said that he was not really interested in computing itself.

WOODBURY:

I think to some extent that is true. I understand that when he did use his machine, the problem of unraveling the bad programming that he managed to get, which we all do, usually fell to other people. So, even at the time, he was unaware of his own lapses in trying to program. This, of course, is the biggest fault with this process; it is the length of time it takes you to discover what you've done wrong. The longer the path from an action to discovering what's wrong, the harder it is to do something about it.

MAPSTONE:

This concept of bringing it all out to the console would give you the power or ability to put your finger on where the problem was.

WOODBURY:

When you plan a program on paper and you make a list of instructions to carry out, there is, shall we say, another layer between you and the actual mechanics of the problem. When you configure the machine to do a problem, you are bringing the mechanisms that you know in every day life, such as a ten-key adding machine or whatever, to bear directly on the multiplications, divisions and additions, which you also understand. When you are putting away, and digging out, and trying to keep track of a recursion in storage through the addresses and all, you put another little layer between you and your immediate sense of what is right and what is wrong. You make the tracking problem a little more difficult. Do you understand?

MAPSTONE:

Yes. You've got one more layer to . . .

WOODBURY:

Between the physical reality of the machine and the reality of the problem which you probably understand very well or you wouldn't be bringing it to the machine. In my dissertation I talked about simplicity of the tools. Any tool could be simple, the computer as well, in that if it was a simple tool, then the person understanding what he wished to accomplish through this tool would not require very much instruction for it. Nothing more than the labels on the buttons would be required, because, knowing what it was to do, and knowing what he had to do, it would be quite evident what to do with the device. A hammer and a screwdriver were obvious examples. But a ten-key adding machine is also an obvious example. A complicated tool. One that is no longer simple, is exhibited in these big computing machines. It is a very long trip from your clear understanding of the problem you are bringing to it, and making this beast do your bidding. There is nothing self-evident about the labels. There is nothing even self-evident in the instruction list about how to approach it. You've got something more; you've got to get your full program in there, and there was this self-evidence. I think Murray Lesser and the fellows that worked with the Wooden Wheel at Northrop found it true, that, since they knew what they wanted to accomplish with it, the nature of the program steps in the multiply and divide operations, working against storage in the accumulator, was just as evident as what to do with a ten-key adding machine. John McPherson made quite a comment on this thing.

I also observed that when a complicated tool gets put into a society where a simple tool would do as well, immediately you generate a group of people with a specialized knowledge of how to operate this machine, and therefore a conservative force which will make it almost impossible to do anything about it once you have established it. The people form a union. Just like the hood carriers. You see, they've got a special place here and it's going to be very hard to do anything about it. Whereas if you keep it simple so anybody could walk up and operate it, you will no longer get this, shall we say, group of acolytes who have a vested interest in the secret know-how.

To simplify something is a process that's done in the head with careful thought about how to do it. This could be done first, although, characteristically, when you start on a problem, you wheel up the machinery that seems necessary. Then, in the process of doing it, you come to an understanding. If you have been putting together physical hardware you will have a great deal left over that you don't particularly need. But in the process of doing this you will start finding shortcuts and figuring it out.

The attempts at Bell Laboratories, back when I was trying to calculate designs for relay networks and so on, all fell short of what a man who was willing to give some thought to it could do. They were never any better than how well, shall we say; the man wrote the program was able to analyze what he did when he made a relay network. If he was quite astute he would get fairly good programming design for relay networks, but he would never be as astute about program design in relay networks as he would be about the network itself.

MAPSTONE:

You mentioned your dissertation. What was it on?

WOODBURY:

It was supposed to be for mathematics. Now what happened here is, I went to IBM to build this machine with the idea that they would provide the dissertation. I described the 795, Wooden Wheel machine in forthright terms, and it's included in Patent #2,923,469, February 2, 1960, Electronic Calculator. The description of how you happened to build a machine, the way you did it, and the straightforward rationale, is not a mathematical dissertation. I suppose Princeton would have managed to make it a dissertation if the machine had gone to them. However, I was convinced that it was very important to do all this work on missiles and neutrons, so I made the machine go to Northrop. That probably was a tactical mistake because I would have my Ph.D. now and probably be unemployed somewhere down in Santa Clara County.

MAPSTONE:

You didn't get your Ph.D.?

WOODBURY:

No, I didn't get the Ph.D. I've done all the work but the dissertation, and I don't think I'll ever do the dissertation. I've pretty much decided I don't really want it. I got my Bachelor of Science degree from Cal Tech. If I pay Princeton the money, they will send me back a master's degree. I've never paid them the money.

MAPSTONE:

But you've written your thesis?

WOODBURY:

I've done all the work for the master's. I guess they didn't require a thesis. When you pass your orals for your Ph.D., you are then entitled to your master's.

MAPSTONE:

If you had written a dissertation you would have had it, your Ph.D., but you didn't.

WOODBURY:

I did all the work for a Ph.D., according to them. I still look back on my passing their mathematics orals as being a case of, "Take it easy on him, after all, he's doing something in the computer world, and that's good."

MAPSTONE:

Did you have any contact with Adele Goldstine as well as Herman?

WOODBURY:

I didn't. I must have met her, but I don't recall her especially. Herman I liked fine, except he was really a very pure mathematician and my whole attitude toward mathematics is the same attitude that Ogden and Richards had for language in their book The Meaning of Meaning. I don't think there's any mathematics that doesn't have some substantial means of establishing sense. To me, we learn the English language by experience. I can talk to you about an ash tray or a telephone and we have no problem of communication, but I only have to move into ecumenical prose and we suddenly discover we are unable to pin down what anything means and we start talking about the spirit. I can understand my enthusiasm to do something is the spirit, that's something we can recognize, but when we start talking about the spirit on another level most people just talk on and on.

The same thing applies to mathematics. If I want to describe how to build something, a bridge or an automobile, for example, I must do it precisely. Therefore, I am immediately involved in mathematics. If you want to talk about the physical world with any precision, mathematics has got to be there. But mathematics in a vacuum is just the same as literature in a vacuum. Stuck. At one point in some of these questions of continuum hypothesis, and points, and so on, I said, "This is ridiculous. You can't exhibit any of these things, and if you can't exhibit them, you are talking nonsense. Why don't you leave it to the Theosophists or somebody to write about it, but keep it out of mathematics. You've got to be able to nail it down." Later on I read The Meaning of Meaning, by Ogden and Richards and they tried to pin down this whole idea that you cannot establish experience reference by talk. There is no guarantee that two people are talking about the some thing at all, and there's no way to know it. It may be fun to string together words for music--I write them down as notes. In a sense, this is where I was in the oral test. I couldn't give the religious answers that were wanted to the mathematical questions, and I f felt they were admitting me to the" club when I was an iconoclast.

MAPSTONE:

You left Princeton to go to IBM?

WOODBURY:

Yes.

MAPSTONE:

You were hired by McPherson?

WOODBURY:

Yes. Though he sent me to Wally McDowell. While I was at Princeton I was on GI bill, so there were definite restrictions on how much money I could earn and it was a little complicated. During my first year, John von Neumann paid me the small difference out of the Institute funds. I wasn't any use from a programming standpoint, and felt as if I stood around and fiddled along. However, in actuality, I probably looked strong from the outside and I don't think Bigelow wanted me in there pushing on his circuits. He might have found me more of a contender than I thought I was. I just wanted to know what was going on; but as soon as I found out, I wanted to know why they were doing it this way and not another way. Pretty soon I'd say, "Look, you're stupid. Do it this way."

MAPSTONE:

You can lose brownie points that way.

WOODBURY:

Yes, true, I lose Brownie points. That was the difficulty at the Institute, and possibly the reason I didn't gat a second year. It didn't seem reasonable to me at the time. When I came back the next year, there was this opening to consult for the Forrestal Project, and this is where I got more intimately experienced with atomic energy. I had, of course, seen the nuclear penetration studios we did at Northrop, and that was fascinating to me because this was Fairchild investigating the possibility of nuclear energy propelled aircraft. I was kind of horror struck by the whole idea of having reactors flying around in the air, but I was interested in a result there which most anybody should know. If you put a U-shield around the pilot so he can only see forward, and you put a U-shield around the reactor so that it can only throw neutrons out backward, there would be enough bounces to try a pilot in no time at all. That's how hot this stuff is.

MAPSTONE:

It's frightening stuff isn't it?

WOODBURY:

Yes. You are dealing with this little sun that isn't sending nice quiet sunlight, but all this stuff that fries you.

MAPSTONE:

What exactly did you do as a consultant on this project?

WOODBURY:

They had a CPC which they wanted operated to work on this problem of the explosion wave of the hydrogen bomb. I suppose this is all out of security, so I won't worry about that. Except insofar as there is a lot of unintelligent bungling here. In all the time I've been inside tight security, I've never seen anything that seemed to me to be a genuine secret. I have seen a lot of things that were covered up because they might not look nice in public, but I've seen very little that was a genuine secret. There was a case in New York when the Rosenberg's went to Sing Sing for disclosing things to the Russians. It was an early case.

When they first popped the bomb, I wrote a letter to a friend who was on the inside and I said "Well, it worked," and I described what they did. He wrote back saying, "I can't tell you you're right." I thought the only secret was whether it would work after all they went through. Would it work? And that we demonstrated to the world. Yes, it works. So all these United States physicists who had been reading the journal up to 1939 or 1940 and who all disappeared had to be doing something, or they wouldn't have disappeared. Suddenly, the bomb worked. And they did, promptly. What's ridiculous is these people arguing as if spies were involved. My goodness, we had this cathode ray tube thing, which was much more subtle than the atomic energy thing in, in terms of precise workmanship. You can take a tiny microscopic experiment with atoms and you can blow it up to great production factories and you don't miss. That means that the relationship between the original experiments and what you are doing with these fellows is so obvious and direct that it's not really very subtle. It's not a subtle as the internal combustion engine which took a hell of a lot of anguish to get it to go nicely. They didn't go through any anguish like that with their reactors. They put one together and it worked, right there in Chicago. Whee. If the computations were exact it will work fine. Take the cathode ray tube, for instance, we had patents in front of us and we sweated those things trying to make them go, and, man, we did all the work ourselves.

The only thing we really wanted to know was whether you could do it. All the other information furnished us was incomplete or lacked the little details of touch that you needed to have in front of you to know just how to make it go. In a sense. laboring over trying to understand why we didn't quite work the way he said it would work, cost us more than if he hadn't told us. I thought whatever they carted off to the Russians probably set them back a few weeks, while they tried to study what was coming in from the outside instead of just going about their own business.

The whole thing was ridiculous. It was terrible to have somebody's life shed for his psychopathic traits that led him to do this. He was a psychopath. Who would bother with that, which knows anything about what's going on.

MAPSTONE:

Did you take tubes that were commercially built and work with them?

WOODBURY:

Yes. This is an interesting incident in the cathode ray tube business. We settled down one night and we worked up a specification for how we wanted the tube built. IBM had a tube laboratory and they were building a very sensitive tube with a long beam for the benefit of the 701 and 702. The next day we were set to meet with Dr. Samuels, head of the research part of the IBM laboratory. We had written down our study of the specification, and in the RCA book of cathode ray tubes, there was a 2BPl tube that was precisely what we wanted for $7.50 a tube. I've forgotten whether it was $180 or $160 for the special tube that IBM made then for the 701, 702, which was not very satisfactory. And it was not very satisfactory for the reasons which we put in our specification which led us to take the 2BPl.

MAPSTONE:

For seven dollars and fifty cents.

WOODBURY:

Yes. During the time we were building this variant, cathode ray tube storage, Ralph Palmer, Director of the laboratory, who had put his approval on the IBM cathode ray tube units for the 701 and 702 came around. "Why don't you use those I've got already built!" "They don't work." He was very unhappy, but eventually he had to get those tubes off the 701 and 702 and put something on them that would work. I was talking to Ralph one time and X apologized to him for Truman. I said, "Ralph, you've got to understand Truman likes to quench people's fire with buckets of gasoline."

MAPSTONE:

Did you get your expertise on cathode ray tubes while you were working at the Institute?

WOODBURY:

No, I got that on the test assembly at IBM. I saw what they were doing with the tubes and I understood the mechanism before I went there. There were people talking about cathode ray tubes; there were lectures on the fact that you had secondary emission, I think they called it, from this green tube. A fellow named Nate Edwards described this as "calculus on a flat rock effect." If you make a straight hit on a dish in the rock, it will be empty when it gets down, [laughter] whereas if you hit the neighborhood they will pile up. That's how you deposit electrons on the screen. You get a pile of electrons out there and they will pile up around where you make your direct hit. My real feel for the problem didn't develop 'till I was working with the test assembly. The three of us, [Woodbury, Toben and Wheelock] worked very hard on the Wooden Wheel. At the time, I misunderstood the direction in which patents were going. In fact, I didn't think much was patentable in the whole thing. An intelligent person looking at the structure of a 604, the available stuff, knowing what's wanted for general purpose calculators, and having inspiration, would have put it together the way we did.

However, a great patent was written on this and it bumped into the ENIAC. How the ENIAC was patented when IBM tabulators had been in existence for thirty years, I don't know. All these mechanisms were in essence in their mechanical counterparts in IBM machines that had been plug wired for a generation.

At any rate the patents were written. As soon as it became evident how you could make gear teeth with electrons, suddenly all the old computer mechanisms emerged in electronic patents. Since this was an obvious extension, I could never understand how a patent could be granted. To anybody knowing about the electronic art and what's wanted out of great teeth, it's automatic. Roy Harper was just loaded with this kind of thing. On the other hand the person who gets a transistor going in the first place, has really done something. That was a real contribution. It's like discovering gear teeth, or the original flip flop circuits, or understanding the use of vacuum tubes to achieve these kind of things. Once you've achieved all this stuff, the rest is just an obvious extension.

MAPSTONE:

But somehow one has been able to patent such extensions.

WOODBURY:

The reality is that's the way it is. The patent was written, but it bumped into the ENIAC patent which was Remington-Rand territory. The ENIAC patent was still in the Patent Office, and every time a new claim was submitted, it was determined that ENIAC could do it. Rather than have Remington-Rand continue to copy claims from our machine, IBM didn't pursue it further. The main things they kept in it had to do with the scanning off of information from the cathode ray tube to go to printer or punches, and the reverse direction from the card feeder. The elegance of the organization of the machine got no coverage at all. That patent [#2,923,469] should have been Truman Wheelock's instead of mine. But I didn't understand that. I had brought a machine conception to IBM which Truman had nothing to do with. I looked at the test assembly, and I said, "Okay, here is the hardware from which we can work; let’s go to work and build it."

Meanwhile, in the test assembly, the questions of how to scan to and from cards, and so on, were pretty evident, and already being worked out. Finally, they were written into the patent, and it probably should say Truman Wheelock, just because of the claims, not the description of the machine. It's kind of a double thing. The machine described here is

William Woodbury's. All the claims were things which were germane to the machine but not the machine, and that was Truman Wheelock's work and that's what was patentable. Otherwise you have to figure out how to discriminate between this elegant, single, small plug board mechanism, and this great mass of stuff that surrounds the room which every detail has to be wired--how do I describe this? You don't wire one wire digit by digit, you just want to say go from A to B. That's one wire instead of fourteen wires, as it were, and that simplifies it. How does one discriminate that in terms of a patent? Also theme was the question of what IBM was using that belonged to Rem Rands, and eventually there was a settlement of seven million dollars, or something like that paid to Rem Rand. It was all part of the package. This is only hearsay to me, and indirect.

A second patent came out of this work with cathode ray tubes. I had always liked symmetry. I had tried to build a push-pull FM receiver when I was at Princeton just because I liked symmetry. It occurred to me that this cathode ray tube work was an ideal piece. My idea was: don't write on one tube and try and find what's on it; write on two tubes. Write a zero on one tube at the same time as you write a one on the other. Then, when you try to read them; compare them. Don't attempt to evaluate the signal coming off the tube. I suggested this at length over manhattans with Greg Toben and Bernia Toben, and immediately we started to do it.

When that patent [#2,884,619, April 28, 1958, Information Storage System. The use of Williams' Tube electrostatic storage mechanism in "push-pull" or symmetrical circuitry.] came up, I insisted that all of us be in on it. They kept digging, did I really do it myself? Well, I suggested it. I loved symmetry and I did suggest it, but I felt that everything I had rested on our working together in a group. So, I insisted their names go on it. It would have been better to have all the names on the original Wooden Wheel patent and just my name on this one for cathode ray tube. On the other hand, the second patent is not of great interest, except that at the high point of cathode ray art it made the difference.

We made two or three test arrangements that told us a lot about the alignment of the cathode ray tubes, so we could set them up and get them so they would fold their information over the span of the weight of the' machine. We had this symmetry that allowed us to build a very inexpensive, elegant amplifier to get our signals back. And to drive them by having alternate turn-on/ turn-off so that you could get one line in the top row. The other line in the bottom row. and read them both at once. So you get a symmetrical and an opposite signal out of the amplifier at the class A level. Do you know what I mean by that?

MAPSTONE:

I think so, but please enlarge on it.

WOODBURY: If I just magnify something, you will see it on a larger scale, but if I magnify a wave, for example, so that its extent is several times the height of this room, and I am limited to what the room can show, all you will see in the room is a line at the top and a little line down below. I have now clipped that in to a pulse. Class A means you can still see the actual shape of the signal coming through. When I drive a class C, all of a sudden if it goes down it doesn't come through at all. If it goes up and it goes past my room boundary and then later it comes back down, I've got the square pulse out of what was originally a surge of an electron charged change on the surface of the tube.

All your gear keys are class C. When you are drawing a picture with a pencil that's class A; but when you have definite things, either one gear tooth has passed or a tooth hasn't passed, that's called class C. You switch a light on or off; it's never half way between. It may be, because of the nature of momentum and inertia, but it's not useful in-between. Obviously, in your tape system, it is the in-between that counts. That's the difference in questions of ham radio radar and the rest of its and this electronic computer system. People working with computers at Ampex, for example, are recording bits; they are always thinking in terms of signal to noise ration. Well if signal to noise ratio is a question, you're not really in the computer business yet. You must have achieved a relationship between signals and noise so it's not your concern. Instead, your concern is that occasionally there will be holes in the tape and you will lose a whole bit which might be the millions place on somebody's check. That's vitally different from thinking about a random loss of bits because there is the signal to noise ration, and just at random, once in awhile, one of them disappears. You don't tolerate that, and as I see IBM machines developing, I realize that this message got through. I used to talk about this at IBM too. When you are working with a computer, if something gets lost once in a while, you have to find out how come and fix it. You don't have some argument that there's a certain probability that a bit is going to get lost in this computer. You don't work that way. If it gets lost, something real happened to it.

MAPSTONE:

You can't drop a stitch in one place and than pick it up somewhere else, because it's a totally irrelevant stitch.

WOODBURY:

This has been a hard thing to get across to people who work with audio things, where they believe this audio signal has to be so much above the noise because that is what the ear tolerates. The real [?] is that gear teeth clatter a little bit, but you don't let them clatter, so once in awhile they skip. You don't even design gears on that basis; you design gears that mesh, and [?] fact that there's a little backlash in there is room for the oil.

MAPSTONE:

What I would like to do now is backtrack to when you went to IBM. Toben came to IBM. Was this because you had now started work on the Wooden Wheel computer?

WOODBURY:

No. The order of events is as follows: I also got a two-day-a-month commission from IBM to consult. I believe the intent of this was to get me acquainted with IBM. I got a look at the 650. It didn't seem to me to be what I conceived in the argument with John von Neumann; the machine I conceived which led me to argue with John von Neumann to see if some point had eluded me. What I wanted out of talking with John was: had I forgotten something? Is there something here that leaves this machine not general purpose? I wanted to determine whether this concept I had--for the Wooden Wheel--was truly general purpose.

MAPSTONE:

Was it a completely universal?

WOODBURY:

He said, "Certainly, but I can't see why you would like to build it that way." That's when I said, "Well, I like Beethoven and you like Bach." And he said, "I don't like music." It made me feel very sad, because what had been a pleasure and a joy to me in the world had passed him by.

MAPSTONE:

That's quite an astonishing thing, because there seems to be a correlation between mathematics and music.

WOODBURY:

Many people like him are very intelligent about music. Anyway, he acknowledged that it was general purpose, and then I take it he had acknowledged something more from the standpoint of numerical computation and extension of the slide rule, that maybe this was something that was needed, rather than the monsters and the big masses of programmers that were trying to do this work now.

MAPSTONE:

However, he wasn't ready to go that route.

WOODBURY:

Not then, but I assume when he wanted to talk to me later, that he was looking around for some way to get out from under this burgeoning thing that had come into being. In effect, I had predicted it when I said, "This is not a simple machine, and it's going to generate a great mass of people who will form a union and be as resistant to any change that imperils their job as a bunch of steel workers, or what have you." That's the way it turned out. John McPherson said, "That's an astute observation." He agreed. Nevertheless, you realize IBM's role and their corporate future rested on this type of multiplication, and they had no real interest. you see, in doing something along the lines of my concept.

MAPSTONE:

Why? Because it would have been such a shattering reversal?

WOODBURY:

In the market places it might have been, but to have built a technical calculating machine of this sort to move into, at the least, colleges and universities, plus such places that had this kind of work to do, and all the people that took CPCs for example, were the natural spot for it, Instead of moving up in to the rental levels for the 701. We need to discuss this at length, with rental and cost and prices and so on because this is quite germane; and the radar attitude, that's also part of the package.

MAPSTONE:

Let's go into that right now.

WOODBURY:

Okay. We brought the Wooden Wheel out here and we showed it to quite a few people, among them, ElectroData. I was impressed enough with ElectroData to buy a little stock. The principal observation I made was: they were using circuits operating at 50 or 100 kilocycles, the same speeds were using in our machine but instead of just cabling up with cheap plug units, tubes and so on. Here was all this elegant workmanship as though they were dealing with thirty kilomegacycles. It was as thought there were going to be osmosi* between wires at this frequency. I was still all impressed with the difficulties in noise and all the rest ,f the things in radar, and so on, where they were trying to pick up faint class A signals, and not sending simple turn-offs and turn-on's which you can do down a copper wire, damn fast, it turns out. I don't know how fast, but a tenth of a microsecond with a forty volt swing was no great problem on our cathode ray tubes. On transistors, of course, the swings are very small and even faster things are happening. Anyway, I discovered that we could operate this stuff up to ten megacycles with the wires in cable; just cabled up like a phone cable. We were using little square plug-ins built by Erie Radio for about 87cents. When Ralph Palmer, at IBM learned that it was costing them $6 or $7 to build one plug-in unit, he said, "You're crazy," and he found Erie Radio who built them cheaply. With all the resistors and everything in place and ready to put into the tube. The 604 was manufactured for a cost of five or six dollars per plug unit divided over the whole machine, but it was per envelope in the 604 [?]. They put that out and rented it for $750-$800 per month.

Then, all the other people started arriving with about the same number of envelopes in their magnetic drum calculators with a little extra expense for the magnetic drum, and discussing $3,500-$4,000 a month rentals. You can imagine IBM's enthusiasm when they discovered the kind of price they could be getting. So they built the 650s and they built lots of them. It was costing ElectroData $20.00 to plug in a tube; it was costing IBM $6.00. No wonder ElectroData had to find capital somewhere. Even at the $3,500-$4,000 price, their cost was so high that they couldn't expand out of their own funds; and IBM was making money so fast they didn't know what to do with it all. Of course, IBM's got a great sales organization and lots of fur-lined enterprises to take care of that. On the other hand, as far as the machine itself was concerned, it was a tremendous ratio of cost to production.

What burned me was that these ElectroData engineers wouldn't even go down the street to see how people were building that machine. I would talk to Jim Bradburn, and he would say, "IBM's got those sewed up, we can't take them." I didn't say it to him because I was with IBM, but I thought to myself, all he's got to do is snarl "Justice Department" at them, and those things will come to him maybe a little cheaper than IBM gets them.

It's all because these fellows came in from Navy radar in World War II, and felt this stuff had to be built sanitary as though they were dealing with whispers. Instead they were sending signals out on the lines that were like the blows of [?] guns. [laughter] If you are in doubt, look at the 604 and its reliability. Imagine the mass with which you drive something. For example, if you have a heavy hammer driving in small nails they will fold up. If you get the right-sized nail and the right-sized hammer, the nail drives in pretty easily. That's the best example I can think of driving peaks. Suppose I want to send a shocker down that line; I want a heavy hammer. In those days, the heavy hammer in electronics was the cathode follower, and the measure of this heaviness was the smallness of the impedance, or the resistance to being moved, or to moving, of the thing doing the driving. We had 6 ohm impedance driving our lines to these cathode ray tubes by using cathode followers. I believe there were four in parallel, so I guess one would be 24 ohms impedance If I use this tube which has a cathode and a plate, the cathode, by being on the edge of conducting grid current, will have this fantastically low driving impedance or grid power to drive a line. However, there was some question about whether someone else had a patent on this circuit, and IBM didn't want by any chance to find themselves paying for this privilege. So the 604 was built using the plate where the impedances were up in the hundreds or thousands, or ohms, I don't remember the numbers any more. This meant that the 604 was somewhat sensitive.

Now ElectroData had most of the problems in this. IBM was real concerned about this patent of ElectroData* and they negotiated, or maybe even tried to take a more realistic view that; here was a hundred-to-one chance that this patent couldn't be made to stick, which it couldn't; it was a simple use of the vacuum tube and the way it was used from the time it was invented. So that fell by the wayside.

Meanwhile, IBM had brought out this great big 604 without the ability to drive their lines with this sledge hammer power drive. Instead, they were using the plates of the tubes which were not as powerful.

MAPSTONE:

The 650 has been called the machine that killed everything as far as any competition is concerned. Was it primarily this price factor?

WOODBURY:

I think so. It wasn't as good a machine as the ElectroData machine. Let's not use price factor; let's remember that IBM has a sales department.

MAPSTONE:

Do you recall which ElectroData machine we are talking about?

WOODBURY:

It was the first of the ElectroData machines, that's all I know.

MAPSTONE:

It would have been after CEC became ElectroData because there was a merger.

WOODBURY:

Yes. [?] with Bradburn, Robinson and some others. I knew Robinson later. I worked for him at Friden. Later on at Friden, Robinson stated that there was no problem with anything IBM had a patent on; that was just as good as being public domain. Evidently, they understood later, but at the time they were unwilling. Of course, that's the difference between supplying these little square plug units at a low price and having patent material, but I am sure the same anti-trust considerations would hold. I'm sure when IBM goes to a sub-vendor and gets a good price on something like that, and then somebody says to antitrust, "look, IBM is using this in-the-back-pocket agreement with these suppliers and it is a way to drive us all out of business. We need these things, too, or we can't compete with them, and we want to be able to buy them." I'm sure that there would have been a lot of letters written and that IBM wouldn't have objected. After all, IBM's control of the market depended on sales; they never depended on engineering.

It's interesting to note that, in general, the things that are available to IBM to market are far in advance of what they have marketed, and the principal here is not to market something until someone has done something better in the market place Printers were a particular case in point. They were building this very clumsy and very expensive wire printer while Analex had their drum printer going along at a faster pace and quite reliably.

I went out and I did a report on this. I thought the Analex printer had as many parts for a whole page width as the IBM wire printer had to print one character in one position. It turned out that my immediate superior, Ray Johnson, was the…and the supporter of the wire printer, and Ralph Palmer for whom the report was made, practically stood on his head in mirth when he got this report. Later IBM got out of that were printer business because it was too clumsy. It turned out it was going to cost practically more than they had in revenue just to keep the thing going.

Now they have shown up now with a wire printer on a console that writes along fairly fast on the computers. One of the more ridiculous conjunctions in the computer business is this typewriter pecking away to get messages out to tell you what to do next. My principal work in programming, by the way, has been to get rid of the surplus in all this stuff, and to get messages printed on a listing printer and only go to a typewriter for things that have some special reason to stand out. We keep the typewriter out of the action as much as we can, and just use the listing printer for all this garbage that comes back from the computer to tell what's going on. The difference in economy in the use of computers at Singer and with the IBM computer is it's quite large. Our computer is working most all the time because it's not hanging there waiting until it can get this queue of messages out.

MAPSTONE:

We've sort of gotten off the subject a little bit. We're talking about the 650 and competition and the fact that ElectroData had a pretty good machine at that time. Were any other machines also in competition¨?

WOODBURY:

Yes. I believe that Computer Research Corporation's National Cash Computer must have been a fair enough machine. I do not know how reliable these machines were, and I don't know what the service problems were. Was this machine really as available as the 650, or was the simple fact that they hadn't really gone in for education and all the other expenses IBM had. Yet they had to cover them. This, you see, accounts for what price differential will do. IBM had some money left for educating customer engineers, and for seeing that there was plenty of service on the spot. Now if it costs you twenty dollars a tube to plug in this machine, you don't have money for all these other things you must cover.

MAPSTONE:

We started to talk about the chronology of when you joined IBM and when you started working on the Wooden Wheel.

WOODBURY:

At this point Greg Toben was hired by IBM. His brother, Bernie, was in charge of the development of the original tape drives. He was a real gung ho IBMer, too. In those days I used to suggest, here we go aside again, that IBM really ought to be willing to sell their equipment. However, I have since decided that the rental business has such a stabilizing effect on the economy, that all these things, and automobiles too, should only be available on a rental basis through the manufacturer. Then there would be none of this nonsense about sudden lurches in employment and people's payroll, because once the market is satisfied, then you have this continuity over a long period of time that allows the economy to come to rest. This happened in the big depression when the automobile market got saturated.

Going back to this chronology now: Greg went to work at IBM, and I was at Princeton. I was consultant up to the end of my term which must have been mid January. I took my orals and I was finished but I was still living at Princeton in the housing project there, because our baby was going to be delivered in Princeton Hospital. I was working at Poughkeepsie and Greg had already located Truman Wheelock in the test assembly. What we were trying to do was use an oscillator to make the cathode ray beam make a circle, or turn off the oscillator and have it hit the center. You splash electrons into the center by facing the circle, and empty the center by hitting it straight on; or you do the reverse, you hit the center to fill the circle and then you read the circle by running your trace over it. If you hit the place you last hit, you get one response, and if the place you hit was not the place you last hit, you got the other response.

All these things seemed to be about equivalent, and we decided after we worked with this awhile, and we did get it going, that the biggest problem that Truman hadn't mastered and Greg, as radio man, dreamt about, was how to get a good enough driver for this thirty megacycle circle drive and to turn it on and off. We both worked on that; I was the one who realized that this was a phase splitting job, and I calculated the phase splitter for thirty megacycles and put in the network. Greg knew how to make an oscillator that had plenty of soup to provide the power to do the oscillating, and we made the circles show on all the tubes where before the lines were driven so weakly that the top of the assembly would have perfect circles down in the end of the diagonals [?] with the other shift in phase as you went from the very top to the bottom.

MAPSTONE:

This was a very good marriage, you and Toben.

WOODBURY: Oh, yes.

MAPSTONE:

One thing I just need to get clear in my mind is when you two or three came together, were you there to build a machine?

WOODBURY:

I intended to and I gave Ralph Palmer a schedule of what I expected to accomplish over six months. When I gave it to him, he said, "I had no idea you had it thought out this well." I knew when I gave him the six month schedule that we weren't going to make it, but it was a target indicating the sort of times it took, and it wasn't too far from true. The machine was pretty well together about six months from the time we actually started. However, before we started and after I had submitted this schedule, we sweated out the cathode ray storage. The first thing that was obvious was that the single end cathode ray storage was not reliable. It wasn't going to work. When these long beam tubes, which were the ones we were working with, first came off the test assembly, Truman told Ralph that they were not going to work. It was true. But Ralph had to do something because they were committed to the 701 and the 702 already. He had to build them.

MAPSTONE:

They were already committed to the 701 and 702?

WOODBURY:

Yes, this was one of the things that generated the heat. I was at Princeton that spring when they announced the 701 and 702. As I said in my paper, it was outsize for any actual work to be done at Northrop. Feller said North American gets a 701 and Douglas gets a 701, how can poor little Northrop get along without one? You can't just have a computer for your needs; you've got to have a computer that produces the right gimmick, or whatever it is, in the Washington office when they give you the contracts. The fact that Douglas calculates vibration of their airplane wings on their computer by an algorithm that is not quite correct, because it's all hidden in the program, and that we are doing our calculations on a machine that's much more open. so we realize when we are making a bad calculation, is beside the point. A big machine with a program that doesn't tell what you really calculated is going to win. However, that didn't keep me from feeling that the painting had to be painted.

MAPSTONE:

Were you hired by IBM because of your know-how and because of your philosophy for another type of machine?

WOODBURY:

I had the feeling I was hired as a way of repaying me for the CPC. Look, the CPC is an interesting thing--and there's a little bitterness here. I went to IBM in good faith and turned over this other stuff. I said, "Whatever I've done on this thing, I'm coming in here, I expect I'll work for the rest of my life, I'm not going to worry about a patent pay-off, or getting lawyers, or fighting over the contract with whatever I'm bringing in." But the CPC sat at Northrop and we thought the only way to get IBM to move was to tell them that we had no vested interest in this idea, and to go ahead and take it. All the time I'm around IBM, everybody was a little bit mealy-mouthed about how many they were making; maybe there were seventy-five or one hundred. Then, one day, I'm looking at a U. S. census and learn they had made over 700. I realized that they have marketed a hundred million dollars of a product of which I was about fifty percent. Fifty percent of a five percent patent royalty is quite a little money. That would keep me skating and enjoying myself for a long time. I never even had a sniff of anything like that.

At any rate, I would argue with Jim Birkenstock, who would say, "We're not using anything in your patent. We're not using anything of yours." Well, that wasn't true. All you have to do is look at the innards of system 360 to know that its center is the concept that I had painted in the Wooden Wheel. I gave that to them, so I'm not going to argue with that. "What about that CPC, Jim. We just wanted one out there with no patent liabilities. Then we turn around and discover that you are going to make a big market out of it, and who is going to get paid?"

MAPSTONE:

Did IBM put out patents on the CPC?

WOODBURY:

I don't know what kind of patent material there was on it. All I know was they had an idea that made them millions of dollars. I have no idea whether they patented it; it's not germane. The main point is I kicked them in the shins to build it, and then I stood down there at Northrop and explained it and sold it for them. They brought many, many people through to see the original Betsy. I really wasn't so outraged when I thought I was getting an honest figure on what they were producing, but when I realized that they had been making seven or eight hundred and had been telling me how few of them were built and how little money was in it, then I was kind of outraged.

MAPSTONE:

You didn't 1earn about this until you were with the Corporation?

WOODBURY:

I was in the company, and much later from a government report.

MAPSTONE:

You went to IBM and started working on the Wooden Wheel. Somewhere along the line you put it all together enough to write it down and say, "This is the machine I would like to build?" Who bought that process?

WOODBURY:

Ralph Palmer, I suppose. Palmer was involved in 701 and 702 because of Cuthbert Hurd and John von Neumann.

MAPSTONE:

There was money in IBM at the time, and the philosophical phase; if you have an idea, we'll try this machine and see if it works?

WOODBURY:

This thing was bootlegged in IBM, in a sense. There was never any real cult for it and they had a great deal of trouble finding out how we spend $100,000, although that's about what it cost to build this model. That's not anything at all in this megabuck business. There were three or four of us and we worked with enthusiasm into the evenings.

Do you know the story of the club where you were entitled to ask a question, and if nobody knew the answer but you, you were entitled to a drink on the house, but if you didn't know the answer you had to buy the house drinks? This man propounded the question, "How come when a prairie dog digs his hole, there's no dirt around the top?" Everybody gave up and he was entitled to his drink since he knew the answer: "Because he starts from the bottom." Somebody asked, "How did he get to the bottom?" "That's your question." [laughter] When we'd find a pulse on the scope coming out of the machine and somebody would say, "Where are we getting this pulse from?" The answer was, "Do you want to buy?" That would be the end of Friday night and we would head for the bar and drink. Greg just drank seven-up since ha was a diabetic. That was the tenor of it. In general I worked a couple of weeks, sometimes Saturdays and Sundays, and then I would go out and spend three or four days on Cape Cod where we had gotten acquainted with some people. We did this right into winter until the machine was finished.

MAPSTONE:

Were you still on a two-day consultant basis?

WOODBURY:

No, this two-day consultant basis ended as soon as I got done at Princeton. Then I became an employee and got together with Greg and Truman at the test assembly and we worked on the cathode ray tube problem for several months. We had a Board of Directors meeting which would establish a date in spring 1952. The salty old directors sitting down on one of the work benches and talking to us. We had had this thing running without a mistake for some hours, at which point it made its first mistake. Now this was the old style and it wasn't working under the kind of conditions that the 701 demanded, nor was it working under the conditions that we wanted on our machine. We did get them lined up on a circle dot basis. It was when we made them operate for thirty-six hours without failure that we said, "We know enough about this." We took off to build our small plug units for the 795 (Wooden Wheel) using the 2BPl tube and so on. That was the point at which we wrote the specification for the tube and then discovered there was one in the RCA book that fitted our needs.

[End Tape 1, Side 1]

[Start Tape 1, Side 2]

WOODBURY:

RDA was making ugly noises about perhaps changing the specification in phosphorous in some way in which the tube wouldn't be satisfactory for computer service, and they thought maybe we ought to have a specification for the tube and we couldn't think of anything except the specifications that were written for the tube in the first place, so that was a stand off.

MAPSTONE:

How about telling me a little bit about the Wooden Wheel itself. Some of the parts that made up the machine. What were some of the ideas you had for the machine, conceptually and philosophically?

WOODBURY:

First, the 604 had a fixed sequence of program steps which was a suppression hub for each step, and by delivering the suppression hub that step would be eliminated and that the signal to the suppression hub could be made the subject of a result in the machine; suppress on plus if plus suppress on minus. This introduced negative logic into the process of organizing these steps, because you have a long sequence out of which some of it could be removed, and it kind of turns your mind upside down to think about what you want to do.

The first thing I wanted to do was to control the succession by simply connecting the steps I wanted connected. You could then build an operation involving several steps, tie them together to happen one after another, and then choose this particular sequence as a result of the selection before, to follow an initial sequence that might come. The first thing the machine had was a set of step drivers. These consisted of an IN that started the step, and an OUT that received the signal when the operation being controlled by that step was complete. A step could be an ADD, or a SUBTRACT, or a movement of information, or it could be a MULTIPLY or DIVIDE.

If it was MULTIPLY or DIVIDE, there were a limited number of the steps--these steps, by the way, were units, and each one involved some vacuum tubes and connections to a control band. A MULTIPLY/DIVIDE step, in particular, had a common hub which was wired to instruct the machine to perform either MULTIPLY or DIVIDE.

It had a hub which you could wire to designate the location of the multiplier, and another hub which you used to designate the location of the multiplicand. For division the dividend used the same hub as the multiplier because that's only on for one cycle after a series of reduction or add cycles b. So there were only these hubs on those steps.

Since there was internal switching on these particular steps, depending upon which kind of cycle was taking place at the moment and this involved several add cycles--there was about the same number of tubes involved but only three hubs available. Four hubs were available for controlling addition and subtraction, and the extra hub could be used for amending an address, for example. This provided the succession; the control of succession was by switches of selectors.

A selector could be set by a signal from the machine, or reset by a signal from the machine, or reset as a part of a block. I believe they could be reset all at once in blocks of ten, but I don't recall exactly how this was. Signals available from the machine to set selectors were accumulator positive, accumulator negative. Nothing transmitted over channels. In other words, an ADD or SUBTRACT or STORE was made and there were actually no numbers transmitted. There was a division overflow, and an accumulator overflow plus or minus. All the odd things that might happen in the machine were signals that appeared at the control panel, and could be wired to set a selector or reset selector a so as to change the sequence from one step to another.

In addition to that for selection, there were two distributors. These were just existing mechanisms with control panel connections. In the process of developing a quotient, you count up the quotient digit, as you subtract the divisor. The counter, which was a single decimal digit device that counted from zero to nine, also had a common and ten hubs on the plug board. Into the common you could put a signal and it would be delivered at one of those ten hubs according to the value resident in that particular counter. This provided translation immediately from a decimal digit to + selection on the control panel of the program step, and was provided specifically with the idea that if you wished to use instructions in the machine, and obviously they were going to be represented by decimal digits just as they are represented by binary digits in binary machines, this was the place that you decoded an instruction into a succession of program steps.

MAPSTONE:

It was a decimal machine.

WOODBURY:

Yes, it was a decimal machine. In the process of multiplying and dividing there was a column shift position to be registered. Again, this was controlled by a counter which stepped. Accordingly, there was a second 0-7 ring or ring or distributor in which there was a common hub to deliver a pulse which would be returned from one of the eight outputs, and the shift position of the machine would be indicated in this except, of course, when you were automatically shifting and you weren't delivering pulses through here. You could read numbers to it and out of it, and you could do it in two days: you could read into the shift counter or you could read from the shift counter, whatever was in it. It was a set single digit from 0-8, because it was a seven-digit precision machine; and I'd like to get into the logic of that later. Nine digits was ideal, but seven was adopted because, in all the physical problems that I know of, more than seven digits is just surplus. Except in a few rare cases, there are few instances where we can go to double or multiple precision. We just don't measure accurately enough to justify having more digits than that apply in computations. In any nominal physical problems when you hit seven digits, you hit the limit of your ability to discover anything about the physical situation you are working with.

MAPSTONE:

Does that still hold true?

WOODBURY:

As far as I can tell. Some things have been improved a little; perhaps the purity of some of these ultra pure metals that are used in transistors get a little better than that. It is hard to know on the scale of an inch anything much more precise than a millionth of an inch, and working in this neighborhood. You really can't tell much better than ten-millionths of an inch. Which means ten to the minus fifth (10^{-5}) so you have two more digits there. This doesn't mean there aren't cases where you need more precision, it just means that in the basic physical world for which you are doing computations, this will do it. Nine digits would be nicer with a decimal system because you have an accumulator and you carry a nine digit by nine digit multiplication, and you get an eighteen digit product. There are now ten shift positions to get back that product according to which portion you want. There is a zero position which gets you the lower most digits; there's the nine position which gets you the high nine digits. So that round* up. Your shift counter now becomes a 0-9 counter just like your quotient counter. They are the same.

The other way of entering this shift counter was by the precision of the number passing through the machine on a given cycle. You could ask for the precision of a number, and I have forgotten whether it was a hub you get of just automatically whenever you transact a--No, it couldn't have been automatic because then it would have destroyed the content when you didn't want it destroyed. You could ask to read into the shift counter at the same time you were reading a number out of the accumulator or into the accumulator through storage.

I would have to look back at the actual data on this to be precisely sure what the hub connection was. This meant that if you had a five-digit precision number, the shift counter would be set to five just like that; if you had your full seven digit precision, it would show seven, and if nothing showed it would be set at zero. This allowed you, with one look at a number, to discover how to standardize it; that is how to get seven significant digits out of some product in the accumulator whose high order position you didn't know for sure. In other words, first you would read it out against the shift counter and get the high order position, then read out per shift counter and get seven digits. then you would store the shift counter itself and you have the exponent. This was the basis for floating decimal arithmetic or arithmetic using a number and an exponent. The shift counter would have the exponent in it for the number after one reading. And using the exponent you'd shift out seven digit precision, or if you would prefer, six digit precision, and then store the shift counter along with the number so that you would have the number and the shift value in sequel in the storage register. The shift counter, of course, was part of MULTIPLY and DIVIDE facility, because you counted up the shift counter in order to multiply, and counted down the shift counter to divide.

Soon after we built it and people began to work with it, a group that worked with Cuthbert Hurd, all of whom I liked fine, although I never could understand why they wanted to work for Cuthbert (forgive me Cuthbert)--pointed out that the machine would multiply quite a bit faster if you programmed it on the plug board rather than using the built-in MULTIPLY and DIVIDE. After all, you could discover whether the number was two, five, ten, and you could add in one in the higher place and subtract once to get it multiplied by nine, so with two cycles you could multiply by nine, instead of nine cycles. It was also pointed out that it would extract square root directly, that you could wire square root on the plug board, and it would do it in about the same time that it performed division. The diagram of this particular operation, calculating square roots, is in the patent and is one of the things that looks just like a flow chart for a model 30 instruction decoding operation. It had different objectives, but this process of microprogramming the square root in the machine had more sophisticated arithmetic structures than any machine that had yet been built that I know of. The reason we unfolded our arithmetic completely was because we thought that was the basis on which we would do the rest. Why get ourselves worried about the NOTS and ANDS in a machine that can take care of the things that you really think with. I don't do my logic thinking about things in terms of ANDs and NOTs and ORs', I do it in much larger chunks. So we tried to provide the basis for that.

MAPSTONE:

You are not a Boolean algebra type?

WOODBURY:

No. And I've never seen anything very interesting come of it.

MAPSTONE:

I believe the real value of the Boolean approach was to reduce the components in the machine.

WOODBURY:

This is what I was commenting on about the relay net-works. That was the Boolean approach. Bell Labs worked a long time on this, but they were never able to find any Boolean approach that was as good as a man sitting down and thinking hard about it.

MAPSTONE:

How about the Northrop and CRC machines?

WOODBURY:

I looked over the shoulder of Eric Weiss--he has since died--who was one of their real logicians, and I saw the overlapping of the work he was doing there, but I saw him recording what he had thought out using the Boolean notation; I didn't see him doing any thinking with the Boolean notation. I felt that was kind of a put on. The people thought quite carefully and brought it together and then it became apparent. After all, we have these two basic controls for multiply and divide in a parallel decimal machine, and we made them become our instruction decoding mechanisms as well as our shift control, number standardizing, floating point arithmetic mechanisms. These are all the same thing; the recognition of the digit as such, the use of it as a number in exponent arithmetic, and to multiply and divide. It's all the same game, and the same mechanisms do all that work for you. When you multiply a few components with a parallel machine you have tremendous gains. First off, if something fails, you can troubleshoot it by simply shifting between two parallel channels and then see if the trouble follows with what you switch. It's very helpful to have more than one thing doing the same thing.

The big serial machine, the 702, that was being built at that time, was one of Truman Wheelock's ways of getting at Ralph Palmer--quenching his fire with gasoline. He would say, "So Jerrier Haddad came in with this little bit of stuff which is the whole arithmetic unit for the 702. By the time they had built the circuitry to route information to and from this unit, they had filled three 604 gates, which was considerably more than it would have taken to use the proper parallel arithmetic." In other words, the simple process of dispatching to this simple serial device was much greater than the problem of building mechanisms to take care of some of the various facilities that were needed in conjunction with the arithmetic.

I mean, how do you feel about laying something out on a piece of paper versus trying to schedule it out in time. It's much easier to lay it out side by side on paper. It is all the way down. It is a lot of work to get these things scheduled out in time. Actually, you can spread time the other way and say, "Okay, we'll go down this way,," but as soon as you do that you must have the tick that ticks one thing after another in and out in time. When you get done, it's awfully hard to argue you've saved very much.

In design it is easy, because you can say this is all I need. But when you get down to building it, you find out what the overlaps and problems and unexpected peculiarities are. For example, the problem of the fugitive one in keeping track of whether you have a zero or a minus; you know all nines in a counter. That type of thing comes up all the time in arithmetic, and you always have to build special little circuits so you don't get negative zeros coming out in the printer, or when you get done with the division that you're not left with a one in your remainder that shouldn't be there. All these problems take extra circuits, and whether you have gone serial or parallel, or whether you have talked about overlapping mechanisms or not, you wind up with special mechanisms to do these oddball things that have to be done. If you have simplified too much in your elementary circuitry, you find that there are bigger chores than they were when you spread out the machine. The fugitive one is no problem at all in the parallel accumulator; it is a dirty, little, nasty thing to hang on to in a serial accumulator. You have to come around and catch hold of things at the beginning again to return it, or you have to work in tens complement so you have a kind of an arbitrary complementing process, or you have to build something to flip in a one arbitrarily on the first trip which is always a little extra mechanism. It's never there when you're telling someone what a simple machine you are working on.

MAPSTONE:

What were some of the other gadgetries and gadgetries about your machine? Are there any more that we should talk about?

WOODBURY:

There were 100 addressable storage locations put in it to have addressable storage to simplify the needs for control. That seemed like enough for where we were at the time. We were not trying to build a ten thousand dollar a month machine; we were trying to build something equivalent to about two 604s, and we were kind of out-raged when the company wanted to charge about $7,000.00 when the 604 went for $750.00. We used about $1,500.000 worth of stuff. and when they finally rented it for $2,500.00. we thought they were making too much money. At that point, I was not aware of lesser machines hitting the market for $3,500-$4,000. Cuthbert Hurd made quite an analysis of what the machine would do, and his analysis was reflected by the rental price. In other words, ha wanted the equivalent of two 650s, but as somebody said, "It was hard to find a problem where it wouldn't even beat a 701."

The addressing mechanism was a whole separate arithmetic mechanism which only added and subtracted. It had four two-digit accumulators--I made it two digits because I only had addresses 00 to 99 to address--and an emitter, so that I could arbitrarily put a one or a twelve or a seven or a seventeen into the channel, and I could tell one of these four accumulators to add, or subtract, or to clear. I could also tell one of them to read out, and another one to read in, or to add, so that I could transfer from one to the other In other words, I could do all kinds of adding arithmetic modulo one hundred in these four registers.

These registers were called A and A prime, B and B prime, because on the control panel was a signal giving every cycle indicating the state of A versus A prime. That is A is less, A is equal to, or A is greater than A prime, and the same for B and B prime. Therefore limit addresses could be established for register A and register B by setting than A prime and B prime. Then the path of the program could be signaled for change at the time A became equal to A prime or greater or performing addition every fourteen microseconds. A characteristic of the Wooden Wheel was the overlap of address arithmetic and actual arithmetic. You addressed storage after you performed the read out of the accumulator to go to storage. The readout actually went to the storage register, then you addressed from storage register to storage, if you were reading out. The readout went from storage to the storage register during time before you rolled, and then during the number rolling times is when the arithmetic was performed on the address registers. You could always have an address register that had addressed what you wanted to go into a computation, also changed and ready to address where you wanted it to go into a left [?] computation. This would always be overlapped. Because of this overlap, the machine was very fast on problems requiring a lot of index arithmetic.

A good example was the one of averaging the Laplace equations in space. Our machine was about as fast as a 701 because it didn't lose any time; it merely performed the six additions and the division, that's the productive arithmetic involved. And did nothing else. The 701 performed six additions, where each addition required considerable computation to find out where to get the number to add, the division was fixed against this particular place, and the calculations of where to put the results also entered into it. There was considerable computation each time around, and at that time the estimate was that machines like the 701 would have about ten percent productive add cycles. In other words, ninety percent of the time it was churning things that could be handled concurrently as part of the background, if the machine were organized in a little more sophisticated way. Well, as far as I know, few machines have been organized that way even now, but work has gone on where access to storage and so on is overlapped with computation so as to make this up in other ways.

MAPSTONE:

This was part of your plan and you've documented it in your patent.

WOODBURY:

Oh, yes. That's been documented in effect in the patent. You know when I wrote down what I was going to write, it was only a-page-and-a-half of type. It indicated what we were going to do this month and what we are going to do next, and it implied each of these mechanisms. Mostly, I drew these things on my dining room table at Princeton before I actually moved to IBM; the program step panels. the accumulator controls, all these things with one exception: the multiply-divide. That was one of these cases where you would say Boolean algebra was helpful to collapse everything back; but I didn't use any Boolean algebra. I simply correlated the operation of multiplication and division. Multiply involves add in and shift until you decrement the counter and you get a zero signal; divide mean subtract from the same place I was adding from before, and shift same control until the accumulator gets a negative signal. ... So I had the same control, only I could choose one negative signal versus the other negative signal, as it were, to stop the process and make the shift happen. Another thing is that multiplication went from right to left and division from left to right, so we had an inversion of the shift counter. We had to have that anyway, because it turns out that when you are finding the shift position of a number, you want to look at it one way, and then when you want to shift a number out according to its shift position, you want to look at it another way. So that cross over was necessary.

There was a place to key in ten numbers. Rather than using typewriter input, we had a switch panel with ten decks of seven digit numbers with signs. If you were doing calculations where you wanted to twist the numbers and explore, this was the place to work from. There was no console for the machine in today's sense; there was no typewriter. There was a little set of lights that showed the accumulator, and the address registers, and the program steps it was on, and the various states of positive, negative, overflow position and so on. There was a button that would make it go a single step at a time; there was a button that would make it go a single flip flop cycle at a time as it went around its loop of twenty-four steps per add cycle; there was a button that let it go from program stop point to program stop point. You could wire the board with a step that would tell it to go to a stop point. Then you would hit the button and it would go back a step and stop, and you could see how things were doing through these short loops.

Then there was the panel on the side of a card reader. We had two card readers; you could put the parameters in one card reader and cards for processing in the other, or vice versa; and there was a punch. The number standing in the second card reader, or in the keyboard, could be read repeatedly. We had a special card reader for when cards were standing still, so they could be read any number of times, and a regular card reader for getting information in and out, which you asked to read a new card. That was represented in storage by a space sufficient to hold the image of that card twice. When you came to the program step in the machine at which you needed a new card, your input storage would say release the card reader. This allowed the card reader to read the next card in and further, if the card after the one you had last been reading had already been read in, it would simply switch storage position.

There were two storages for cards; one is for all the reading, the other is loading. When a card is released, that tells the machine you are through with the current card and that it can start loading the next one. It also says, you want to read this card, but you don't know if it's there or not. Now you come along later, and require the next card. At this point the computer would stop, unless the card had been completely read in, because the next step is the one that's going to start reading into the computer. Meanwhile, while you are waiting for this to read in, it goes ahead and it starts loading the next one.

MAPSTONE:

Sort of a buffer.

WOODBURY:

Yes, it is a two place buffer. ... That was complete. We studied the idea of using more buffers but this turned out to be the best method. With one location loading while you're working with the other, you can do computation as fast as you can read the cards. Whereas, if you don't have a buffer, then the machine can only go for however long it can go between cards.

It's the same thing for printout, but there was no buffer necessary for the keys or for the other card reader because that was a stationary arrangement. It stayed in place for as many readings as you wanted and then you could ask for a new card when you were ready. You could release when you start moving the new card into place, not when you said you required the machine to stop.

These card images were divided into ten sections, seven digits and sign, and you had ten addresses on a card. Ten addresses to print, ten addresses on the switchboard, and ten addresses on the other card. You could use the unit position of the address registers to address card fields or print fields. It was purely a numeric machine, and we had no intention of providing anybody with any alphabet or literature; we were just building a slide rule represented in the heart of an arithmetic machine.

MAPSTONE:

Did it stay that way?

WOODBURY:

The machine didn't evolve in any direct way, you understand. The 797 was a one-thousand word storage machine and there were four of them. They originally went to Northrop, and then later they were distributed. UCLA got one, Stanford had one that they did a lot of work on, Ann Arbor has one, and, I believe, one got broken up.

MAPSTONE:

The 797 was the production model line of the 795.

WOODBURY:

That's right. Here was a nine digit machine. Remember I said nine digits rounds out the shift system. That was a nice machine. Stopping at a thousand words of storage was the result of available core technology, and the amount of money they wanted to spend. Now you could probably go to a million words instead of a thousand, that's of no matter except that the address registers get bigger. I think the 797 had three address registers, A, B and C, instead of just A and B, they were three place instead of two place registers. The Wooden Wheel had 100 program steps, and I think there were 240 on the bigger machines. This extension work was done by people from IBM when I was out here worrying about the RAMAC.

MAPSTONE:

The 795 was the only one you built?

WOODBURY:

Just the one; just an experimental model that we built in the laboratory and went out to the field to operate.

MAPSTONE:

It went to Northrop?

WOODBURY:

Yes. We mentioned the SWAC, and that had the cathode ray store on it. Dick Baker, I think it was Baker, came to see our machine and at the time he was working on SWAC. I believe he worked on SWAC after he left Northrop. He was looking at the oscilloscope signal from one of our cathode ray sections, and he said, "Let's see some of your other spots." He thought he was looking at merely one spot because the trace was so clean. That was something that nobody else had seen on cathode ray storage ever, where an entire scan tube made a trace on the oscilloscope which looked like a single place on the tube. The other thing we did that told us a great deal about it was to make it so that we wrote alternately one and then zeros on the tube. We called this the chaos switch and we used it for testing, because while it's doing this we must have clean separation of the signal both ways. This is what we used for the alignment so any stable situation sequence from the tube looked as pure as the clean pulses down in the registers of the machine, because we aligned them under this critical condition.

We did not have a read around problem. This is one of those celebrated problems, we knew about it and we designed around it deliberately. Access to the cathode ray tube involved two of the twenty-four cycle points in the full cycle, and it was done either before or after accumulator operation, and the rest of the time regeneration is taking place. That meant, even if we accessed the same place over and over, that the whole tube got regenerated between cycles. It guaranteed we would never have this read around problem, which upset them on the 701.

In investigating this alternate method of read write zeros, read write ones, read write zeros, read write ones, we saw so much difficulty with writing a spot cleanly on a single try, that we concluded a great deal of the read around problem came from the tube's inability to change information which it had been designed to retain. They imputed their inability to change a spot, which they had built into a tube, to spill from read around. Do you follow how that could be? You look back at the spot and it's not what you expect it to be. I wrote a zero there just now, but the one before it was so wall built in it didn't rewrite. We put this switch on to make it write alternately so that we could see that we got a clean separation of signals even when we were hanging back and forth as fast as we could. That did a lot. That was the first thing. And that, by the way, was our key to the thirty-six hours of single ended of operation and we got the circle spot. I think this chaos switch was my suggestion; I don't remember, but nobody was individually patenting any of these things. I recall suggesting that the worst possible case is when you change from one spot to the other, back and forth, so we started lining up a little bit of circuitry to force this condition on the machine. I remember using the Greek lettering of chaos for the label on the switch. You would turn on the chaos switch and start lining up the focus and so on, until you have got the best possible signal. By the time we had the two-ended system out at Northrop it was by far the cleanest part of the machine.

When we first got out there, the machine was fried one time because the thermostats didn't work. It needed quite a repair job after it got too hot, but it operated for a long time after that.

MAPSTONE:

How did that occur?

WOODBURY:

I wasn't there. I just heard about it. I heard it got too hot, the blowers went off, and nobody noticed. When it began to smell, then they turned it off.

MAPSTONE:

By this time a lot of damage had been done.

WOODBURY:

Yes, and there were no thermostats in it to stop it.

MAPSTONE:

Let's talk about some comparisons. Was the 650 already on the market when you started on your 795?

WOODBURY:

No. The 650 and the 795 were put into a mock competition to go to market.

MAPSTONE:

How was this done?

WOODBURY:

All of a sudden the company decided that they should enter a computer in the field, I suppose because of the prices they were beginning to see and the cost of building the 650.

The question was whether to enter my machine or the 650. I said, "If you want the most machine, it will have to be this one." Later on, one of the fellows, Don Payole or somebody like that was over in the factory. A kid, I believe it was Ernie Hughes, said, "Well, have you found a machine that you could do faster than the 795 yet?" I don't think the 650 ever got within a ratio of about four to one. I think they took about four times as long, but that wasn't the point. I told you before what Cuthbert Hurd had said about it. I was already aware of this point so I couldn't really enter into the thick of it, and I think this disappointed Greg and Truman. I couldn't really tell them that up top there was a very definite knowledge that this machine couldn't go to market, although they could use it to generate a lot of heat over the 650 group; such as holding this competition. They wanted to get those 650 people, who had spent a couple million dollars building the machine, off their asses, and get the machine out in the market place and start getting some of the money that was obviously out there. Ralph Palmer said, with his commitment to 701 and 702, that it would take him thirty-six months. The 650 people said, "if we move heaven and earth we'll have this thing out in ten months." It took them quite a bit longer than that, but they undertook to do it and they were hobbled by patent rules. The company said that Remington Rand had the patent on the short delay lines, where you read off the drum and write back immediately ahead of the read head, so that you have quick access to a short track on the drum, and it will cost too much money to license.

So they went to the high-speed drum. They did a lot of things like that and they got their machine out, they got their money in, and they did exactly what was wanted. Some time later I heard Felix Wesley, who was a sidekick to Red LaMotte, long time vice president in Washington, say that he made that decision. I told him that he didn't do anything of the kind. The company couldn't market the 795 no matter what, and I don't care who told somebody that Endicott would get it out in quick time, that's the only way it could be. He said no more.

MAPSTONE:

Did it bother you knowing that this machine was not going to be marketable?

WOODBURY:

Not especially.

MAPSTONE:

Because you wanted to pursue it anyway.

WOODBURY:

I would like to have pursued some more complicated ideas in this direction. I would like to have made a machine like this available, shall we say, as toys, for interested people to play with. I felt, and still do, that the field of technology is one of the art forms and this insistence of multiplying for economic reasons and to take up surplus population is a ruinous thing to do.

MAPSTONE:

Your ideal would have been to have had this machine available to people who could have used it, say, in a university.

WOODBURY:

You see, there is an economy where IBM can play that game, but according to them, they can't play it. IBM's got to multiply the uses of people to expand itself, and it's doing just fine. No objection. I am certainly no one to guess how you can organize an economy so that people are taken up and busy, and so that this whole mass of people outside of the economy get brought back into it, and so that instead of keeping people busy by destroying all the orange groves and apricot orchards, we keep our population centers concentrated and our countryside readily available.

MAPSTONE:

The 650 became the commercial machine. How many 797s were made?

WOODBURY:

Just the four on special order.

MAPSTONE:

One that went to Northrop.

WOODBURY:

There were four at Northrop.

MAPSTONE:

Oh, four went to Northrop?

WOODBURY:

I don't know for sure. By this time I was at San Jose, and this was going on outside my purview. I know some went to Edwards Air Force Base but I'm not sure whether they were originally sent to Northrop, or what the deal was there.

MAPSTONE:

The 795 was your prototype and that went to Northrop.

WOODBURY:

Right.

MAPSTONE:

Did you go with it?

WOODBURY:

I did. I was there from August to December.

MAPSTONE:

Training people?

WOODBURY:

Training people along with Truman and Gregg, living on the beach at Manhattan Beach, and generally living in the lap of luxury. I spent the afternoons standing on my swim fins in the surf of Redondo Cove.

MAPSTONE:

When did you teach people how to work the machine?

WOODBURY:

In the morning. When the weather wasn't too good. We did a great big Stokes equation on the thing and learned a great deal about what was wrong with it.

Sometime in here there was another interesting piece of dialogue which was another contact with John von Neumann. There was a meeting held in Poughkeepsie with Cuthbert Hurd, John von Neumann, Jerry Haddad--Haddad is a vice president of IBM now, but he was the 701 msn--and several others in this group. John said to me, "Bill, there are lots of problems which just have to have more storage." "That's true, John, but you have to examine them quite carefully to be sure that storage is really the limitation." He said, "Well, take the weather for example." I said, "John, I'm sure that this machine's memory does not limit it in connection with the weather problem; it's the multiplication rate. I believe we can punch all the results out and re-circulate them on cards faster than this machine can make the computations."

This silenced John for about a minute while he made some calculations in his head. I had already seen the weather problem while I was at Princeton. He said, "You're right. It's limited by the computation speed." At that point, Cuthbert replied, "Oh, but you wouldn't want to do the weather problem on this machine, would you?" He was outraged that this little toy would do weather problems. John said, "I think it would be kind of interesting." Murray Lesser was at Northrop when they tried to bring the weather problem out and fool with it a little bit. He said, "It's just too big to put in any machine." Even with the card flow outside the machine, reorganizing the data through the machine was tremendous, and it was too awkward. The reading and punching was faster than the computation, but it still was too clumsy.

MAPSTONE:

That's an ever-going problem, though, isn't it?

WOODBURY:

Yes, but I think the real problem with the weather is getting sufficient observational data.

MAPSTONE:

While you were working on the 795 machine, the 701 was well underway. Did the work that you did on the 795 and 797 get implemented into 701 in any way?

WOODBURY:

No, it was too late to go that direction.

MAPSTONE:

How about the later machines; 702 and 704?

WOODBURY:

That I couldn't say. However, having worked on the model 30/360, and having had reason to want to make some changes on how it controlled the typewriter console; I got into its microprogramming. I was struck by the elegance of the substitution of punched cards for control panel wires. We had thrashed around the idea of replacing this control panel with a punched card, but I don't recall whether that discussion germinated elsewhere. It seems as if it were the kind of thing that would occur to almost anybody trying to replace a control panel. I was struck by that and also, that for our purposes, we felt this microprogramming should be readily accessible to the user because there was so much more to be gotten from the machine if you had some limited ability problem. Remember this may be transient circumstances.

Perhaps genuine problems at the limit of the machine's ability don't exist in the contemporary state of machine development. I don't know. I don't know what people are running into on some of these big problems; not in terms of how big a problem they manage to generate, but how real the big problems are. For instance, the slide rule was quite adequate for the hydrogen bomb problem. There was no point in using a computing machine for the work we did at Princeton on that, yet we had people from Los Alamos flying all over the country using every machine in sight to do that same example and get results that were in agreement with the slide rule estimate. Since this happened they presumed they must be right. We were wrong by a factor of 100. I suspect that today's giant problems are that kind of thing, so there are questions in my mind whether this ingenuity of being able to configure the machine in the hands of the customer is of the value that it was at that time. Taking the weather problem, the fact that you could configure a machine that used half a millisecond add cycles, meant that you could fully utilize the mechanism to suit the project and, therefore, get maximum utilization. If we had gone to electronics and had gotten our multiplication down to a few microseconds and additions down to a half microsecond, which would have been possible with the cathode ray screens--by comparing the signals in two tubes, instead of just accepting the signal that was in a single tube, we had shown that the screens were ten times as fast as the 701 units...was in connection with the value of the machine for large problems.

The weather problem is still too big, and while they will experiment with it and I suppose learn things, there is not enough observation, there's not even enough knowledge of just exactly what we need to know to calculate the weather problem. There's nothing straightforward about that. The interesting thing is: when is a tornado going to strike? For goodness sake, that's a detail that's so fine compared to the grid they've got to work that problem on. When you start looking around and see where the reports are of what's going on in the northern hemisphere and over the continents, you find these stations are scattered here and there in cities. They are not on any regular array to allow any regular approach to the computation. You'd like a nice rectangular grid, or at least a well organized grid and you would like stations at every one of them, and you would like a report every half hour or so. Now you begin to confront a mass of data which no machine would touch. So I haven't found the weather problem of real deep interest. I think there are some simple things to be worked out with a computer.

MAPSTONE:

Do you feel that the work done by you and Wheelock and Toben in any way influenced some of the major computer developments?

WOODBURY:

The computing parts of IBM's System 360 are built on it.

MAPSTONE:

Were you familiar with Wilkes' work at this time?

WOODBURY:

I ran on to Wilkes somewhat after that, and I am trying to remember what I ran onto there.

MAPSTONE:

His first paper was in 1951.

WOODBURY:

I wasn't familiar with it at the time. I was there at the Institute arguing with Johnny about how to build this machine. If I ran into his 1951 paper, it would have probably been two or three years later. I recall work relative to it.

MAPSTONE:

You mean relative to his paper?

WOODBURY:

I recall something that Wilkes had that was in the same genre with configuring the machine by microprogramming. However, I believe he didn't allow that anybody would want to re-configure his machine on the spot. This was how he was going to make the machine go. But I can't say for sure. This is dim memory. I wasn't very interested except to observe, like everything else, that when he gets confronted with a problem he finds the same solution. If Leibniz and Newton hadn't discovered calculus, somebody else would have.

As I have said, I never felt that IBM was especially obliged to me for this work, except it would have been nice to have a little credit for sowing some seed around IBM of the idea of configuring the machine by a program as well as operating by a set of instructions. It was the CPC that interested me. Even though we came from Northrop with that idea, we were pretty innocent of the implications of what we were doing. And IBM had, and still has, considerable obligations. None that I am going to enforce; but I'm not above giving the needle to the people in IBM that could do some thing. At least they don't deny it anymore.

MAPSTONE:

Many interviewees indicated that the CPC was a crucial point in computing, and that there really hasn't been another point quite as dynamic, because it made people aware of the fact that a machine could be used to handle computations of the kind that had not even really been conceived of.

WOODBURY:

You could get a continuing computation going on; the recursion could go on indefinitely.

MAPSTONE:

The automatic process of computation. With this, scientists, mathematicians and business people, although they came later, were able to see the new ways to approach problems that up to this time had been grinding them down for one reason or another.

WOODBURY:

That's a nice thought. I'm glad I'm great

MAPSTONE:

I thought you would like to hear it.

WOODBURY:

This and thirty cents will give you a ride on any subway in New York!

MAPSTONE:

That's right.

WOODBURY:

That's quoting Truman Wheelock. He used to say, "That and fifteen cents, Bill, will get you a ride on any subway in the city of New York." I miss Truman. I was sorry that he didn't want to stay out here with us and even sorrier when he got impressed into doing the 7030 storage. That was a giant machine and involved him in a large bureaucratic engineering organization which was quite unsuitable to him. I ascribe his early demise to a quite unsuitable job for the man.

MAPSTONE:

Give me a brief biography of the man. Since he's deceased it's rather difficult to get it from him.

WOODBURY:

His parents were in the care business somewhere out of Chicago, and he knew lots about the business of making a cafe work. It must have been late depression when he got in as a customer engineer for IBM, and he fixed machines on the outside for a long time. He knew Roy Harper from there. They arrived at the laboratory in Endicott, originally, but they were both at Poughkeepsie when I first knew them. He was married to a girl named Arlene. I can remember joking about their courting days in Chicago and being parked out in the giggle weeds. There are roads out in the prairies where there was lots of high grass around to give you a little privacy in the car; they called the grass giggle weed. He was very outspoken. I characterized him quite early with: he liked to quench people's fires with buckets of gasoline.

When we needed the card reader for our machine, he set about Gavin Collin, who was in Production Control in Endicott. We went over to see him and to find out what to do about the paper work, and how to get the machine. Gavin proceeded to export one to the back of the car, which we took away with us. Truman thought a great deal of Gavin for that and said that he had known him as that kind of a person anyway. But that would have been separating bullshit from buckwheat. He took care of getting card feed to us and later we had several different people out of the accounting department around trying to discover how to account for this transaction.

We also had a man who was quite intent on putting accounting labels on everything. We had gotten a hoist. The power supplies for the machine were heavy and we wanted to lift them on and off. We were working with the problems of building these power supplies, especially the 1500 volt supply for the accelerating voltage of the cathode ray tubes, and we wanted to be sure that they were not accessible, even accidentally, while people were working with the machines. So we got an overhead hoist to handle them. I don't remember if it was on a track or not, but I remember that we got it up there and mounted before the property accounting man could get around with his tag. There it was up on the ceiling and, of course, we had removed the ladder. We did our best to get him to sit in the bosun's chair and hoist him up to put on these tag". But he knew what we planned, and he was quite right. We were going to leave him up there for a while. Finally he got a helper and a ladder, and he climbed the ladder to do it. We were the boys in the back room of the Poughkeepsie Laboratory.

There was a wiring shop run by Don R. Casey, with about a dozen girls wiring the panels for plugging into the 701s. When we needed a unit for our machine, we slipped it through on the 701, 702 account, which was why they had some trouble with accounting. They got it all together and accounted for eventually, but the process of building the 795 was almost entirely this kind of sub-rosa operation with Ralph Palmer's blessing.

Toward the end of the process the model shop ran out of work. The machine had already been named, The Wooden Wheel because of the Woodbury, Toben, Wheelock combination. Sometimes it was called the Wooden Wheeled Harp, because of Roy Harper. Our consultant on electronic detail getting into the act. Which is what he did when we had an electronic detail. For example, we found out we needed one more plug unit in a space that was all used up. He could usually devise some kind of a plug unit that would do the job in one unit, when we thought we needed two. He was an electronic circuitry man. He laid awake nights and thought about it.

During the period that the machinists and the machine tools were idle, each one was given a spoke to make. I don't know whether the shop boss turned out the hub and the rim or not. At any rate we had this beautiful, satin chrome model of a wagon wheel which was the emblem on the machine. Wally McDowell saw it when he was getting ready to send the machine out. He said, "We normally frown on that sort of thing" but since it doesn't say anything, I'll let it go." Subsequently, after the machine went back for patent work, Truman Wheelock had it, but I don't know where it is now.

MAPSTONE:

I wondered what happened to it. It's possible that his wife may know.

WOODBURY:

She died first. They are both gone. Other vignettes on Truman. Something was kind of wrong all the way there. His wife dying ahead like that, and him getting arthritis and dying. Greg ascribed it to all the nervous energy that went into quenching people's fires with gasoline, as it were. I don't think the big product engineering project did him any good at all. That's when it began to develop, when he could no longer be a salty known rebel against IBM, still harbored by the company. I have an idea that the death of Mr. Watson, Sr., was the beginning of the true IBM bureaucracy. It was weakened and it was already becoming somewhat of a bureaucracy. I work for Singer now and that bureaucratic surf riding is wonderful to behold compared with anything I ever saw at IBM. Surf riding, I think, is a very good example for what it is to live with a bureaucracy. You're surf riding all the time.

MAPSTONE:

Was Wheelock electronically trained?

WOODBURY:

He may have been an amateur radio operator. I know he went to college and graduated, but I'm not sure if he had an electrical degree. You can check all that.

MAPSTONE:

Did he work with you on the 795?

WOODBURY:

Yes, and he built the 797. He looked after that. I think he still was thriving when he was doing that, because I visited him at a place in Pennsylvania where they built them, and he was still in good spirits. As far as I know, the arthritis and the problems hadn't yet started to occur.

MAPSTONE:

Was he an older man?

WOODBURY:

Not much. He couldn't have been. He went to work for IBM along about the time I was getting out of college too.

MAPSTONE:

What did he do after the 797?

WOODBURY:

He went to the Stretch project and he said that he wouldn't touch it with a ten foot pole. So they had a great big drunken orgy of a party and gave him an eleven foot pole. I think the whole laboratory attended that party. What was missing was a place to continue the kind of thing that went on at the test assembly, or the kind of thing we did with the wheel. I got my enthusiasm dampened for doing that kind of thing out here in San Jose. I kept bumping into political mechanisms; ones I don't fully understand. When I see things about it, I'm even quoting with a feeling, "Is that really what I heard?" At one time I wanted to build a machine and I said that I'd call back to Wally McDowell. I think it was Ray Johnson who said, "If you do that some people are going to get hurt." I wasn't anxious to stir anything up. I mean, if they can't bootleg a machine out here, they can't bootleg a machine.

MAPSTONE:

Are you talking now about a new idea?

WOODBURY:

I'm talking about work on the RAMAC at San Jose. I didn't have any squawk. After all, I got the CPC together to do the job, and then, looking at the CPC, I built an electronic equivalent embodying what came out of the 604, using existing electronics to accomplish a complete machine in the von Neumann sense. In fact, much more than a complete machine; a machine with many facilities to make things that were normally difficult to plan out very easy, because you just went straight ahead. I want to augment this counter by one. So you wire ahead. Every time I go past one gets ticked into this address so I march down my addresses. I want an end address, so I set it up and away I go. These things were much more direct than writing them on paper and introducing them to a machine and getting all your assembly addresses and everything straightened out through the computer.

MAPSTONE:

This was the second machine. You were talking about RAMAC.

WOODBURY:

The first thing germane to RAMAC was a meeting at the Union Pacific Averell Harriman's estate. We took over the place for three or four days. They put me in a group on small machines and I argued repeatedly that computing for business purposes involved very little arithmetic on masses of data. I said that the problem is like a grocery store, where you tick off the inventory of cans one by one and just subtract them from the value of those you have in the store, then when the supply is small it's time to order. Inventory control kind of embodies everything that happens in a necessary sense in a business establishment. Fun and games with sales trends and so on, can be computed, but you don't really know what you're talking about when you get done. Who is to say what the roll of the dice is going to be. Business computing needs not very much arithmetic, and lots and lots of storage. Lou Stevens was around on that. He was one of the people who was out here at San Jose, and he had worked on the 701. Ray Johnson wasn't back here. San Jose started the process of trying to build very large storage, and that led, of course, to the disk storage. Then came the question of how to organize a machine around this storage. The principal thing that dominates the situation is: on what are you going to print your information? I was not interested in a big project like the 701; I was interested in doing something small enough so three or four of us could do the job. I went back East and proposed that we use a printer that was already extant in IBM, but not in production; Bud Beaty's line printing machine. It would print fifty characters a second across the line, feed card serially, and it had an arithmetic arrangement with a little drum on it so you could do arithmetic with about ten accumulators using the drum. It was a very simple machine. It was scheduled to rent for about $275. It turned out that it would outperform the 407 on printing and calculating new work, but on straight listing it wasn't as fast. That looked to me like the printing arm on the instruct line on the disc file laying on its side, and I thought it would make a nice little package about the size of a small desk.

I kind of enjoyed working that up, but with no feeling of urgency about it like I had on the 795. The 795 had to be made and exhibited quickly so people could see it, and also I knew where I wanted to go. There were a lot of CPCs out, and I wanted something nicer than a CPC, especially for people who would have to jump from the CPC with relatively no programmers to the 701 which exemplified what I saw in the BINAC. I think I wrote about my feelings of the high school geometry teachers programming that instead. I believe that's a significant factor in the corruption of mathematical education in the country. These people have all been drained off into programming instead of sitting still and teaching the young.

I didn't have any feeling of urgency here, in fact, knowing IBM, I felt that a big machine was what they wanted to market. I didn't see any reason why there shouldn't be a small one in the patent morgue to exhibit what could be done in this direction without a great deal of expense, and what might show in the market place just possibly if somebody wakes up to the fact that it costs three times as much to do a job in a machine that's twice as big as it would in a machine of a smaller size. I have never been able to get along with this equation of how much it costs for multiplication. It doesn't seem to be the real cost of the machine. The cost of the machine is getting all that system programmed in, getting all those people to look after it and all the rest of it, and when you double your machine, you quadruple all that. At least that's my top off the head guess. That's what happens with the larger machines. Friden would be a good example.

Going back to this disc business. We went back to New York. John Hanstra suggested using card-o-type discs; Lou Stevens, who was a big time boy from Texas, wanted to use the 407 for the printer, and I suggested this intermediate printer, When I got back there I saw they were lined up in that order, and I also saw that they wanted to build the machine I suggested. They wanted that printer put into production, so they would have it as a possible production printer that was faster than that typewriter and slower than the line printers. They hadn't yet come around to doing anything about the very high speed printers.

I got back to San Jose and thought I was going to work on it, and discovered I wasn't. At some point, Lou Stevens said to Ray Johnson, "Why not give Bill a little appropriation? He doesn't want much; let him go ahead with it." Because of their experience at Poughkeepsie, they took for granted something that was quite untrue. Ray Johnson, the boss at the lab, told Lou that he would give me the money to do that and that we would have the thing going and put it into production while Lou was trying to get started up. After all it took a little less than a year to build the 795 computer, which was about the size of the 650, and that took about two or three years to bring about. This was magic. It wasn't any magic; it was just not biting off anything but the problem in mid… This other thing I had a lot of things on mind. I could have worked on it for many years. Even when Piori came in and put it in the morgue, I had a long way to go. But it never came about.

We did some nice things with the computer. There was a little punch that went along at about a hundred columns per second on the card, and I found myself in competition with IBM's mechanical mechanism genius, Larry Wilson. This was Norm Vogel's.

A puzzling thing happened to Norm Vogel: he put a machine together when we were going to demonstrate it to Tom Watson, Jr., and entourage, and we put it together missing one of the keys. When I turned it on without synchronism it sounded like a machine gun and the entourage left the rooms. Norm has gone up in the company to the head of Mohansic laboratory and I left the company. But I don't think there was anything deliberate there, it was an easy thing to do. It certainly was strategic; in fact I don't think he would have anticipated what would happen, unless he did it some time when I wasn't there to discover it! But it was an exciting moment or two. They came back later, after I had it put together.

MAPSTONE:

You didn't actually work on your printer when you came out to San Jose?

WOODBURY:

Not immediately; but I did some other things that were interesting. In the first place there was this big disc file with fifty discs in the stack and extra ones at the top and the bottom to protect air drafts. They had an air access mechanism that worked with a big motor at the bottom, and clutches to withdraw it, transport it, and place it. They also had a big gear at the bottom, another gear and a motor, and in order to get to this bottom gear box and motor, you had to take the whole thing apart. The shaft was about ten to eleven-and-a-half inches in diameter, and I looked at it and I said, “That's a place for an inside-out-motor." Norm Vogel and I were just getting together on our project, and the first thing we did was to get ourselves an inside-out motor and get this device going.

It was interesting. We took the directory and looked up fifty motor manufacturers. That's all there were that looked like they could possibly do this kind of thing. We sent out a very nice letter to each one, personally typed up, no multiple typing, nothing to give the least flavor of any thing but personal attention. We got back quite an interesting gamut of replies. We found a man in Massachusetts who was willing to make us two or three at about $170 apiece, but he didn't really want to any more than that. We found an outfit in Santa Ana that would be glad to undertake a research project and turn one up for $21,000. We found some people in between at all prices, some reasonable. The $170 was a perfectly good price. We found some that even had a motor practically like it being used for driving very large grinding tools for the people in Niagara Falls.

Finally Lewis Allis Company of Milwaukee said "we don't really want to build a motor to plug into your shaft, we would like to make you a shaft, motor and all, and we don't really want to charge you, we would just like to show you what we can do." We said, "we can’t do that, you will have to put a price on it, because we do not want to be obligated in any way to buy them. They made us one and we put it together. We had it horizontally under our machines and the word had gotten out to Lou Stevens, who was responsible for this big gear box design and so on, that we had a very nice quiet running machine. He came over and he said, "Gee, turn it on, fellows. I'd like to hear it." "Lou, it's going." He looked down and was quite startled. It took them a little while to get converted over, but that's what they used after that. Several million dollars worth of motors and saving several million dollars too. It cleaned that up.

At the same time we did something kind of interesting with our access mechanism, which now rotated instead of going in and out through a slide, and transported the same way. We took an hydraulic mechanism where we used--you see, you are generating precise distances to move here--a set of cylinders pushing hydraulic fluid, where each cylinder had a distance of push and a diameter so that the displacements were maybe one, two, four, eight, so that you would add one and two to get three, two and four to get six, four and one to get five and so on.

Which one you chose, was chosen with an eccentric. In other words, you have a shaft with a round cam on it that's eccentric, and the amount of eccentricity is the amount of throw you give the cylinder, and you have one on each side. You've got this axis, and it's got a piston in the middle, and fluid on each side. The fluid is going into the piston that's being relieved, at the same time it's being pushed in by the piston that's being pushed on the other side of the same eccentric. This was the way we selected our positions, and we had two of these. One would turn to restore to zero--that would bring the head rotationally out, if it's a full transport; the other one would turn moving the arm to the position of which disc we wanted. Then the first one would now turn to the new addresses set by letting go or not letting go the particular eccentrics for the displacement we wanted.

We drove that off the power of this whole mechanism; its momentum was being delivered to the shaft and we fixed the speed at, I think, a tenth of a second for this motion, two-tenths of a second up and down, and a tenth of a second in, so a long transport was four-tenths of a second and a short transport was just one tenth.

Eccentrics give quite a jerk. A jerk is what you feel if you just hold the brake steady on a car when you come to a stop. If you let off the brake as you come to the stop, you can come in sort of unwound and you can't tell just when you came to the stop. An eccentric is a strong jerk, which is hard on mechanical things. The head gets started too fast and slows down too suddenly and it sits there and shakes for awhile. You want to be able to move it in place and stop it just right. In order to accomplish that with eccentrics, we made it so the shaft turned at different speeds as it turned. We had one big cam out here that really generated the movement we wanted to make this fast, smooth action, and these little eccentrics that decided…would be chosen to get the movement with the art we wanted. That operated about at four-tenths of a second, and I believe the best the other device did was about eight or nine-tenths of a second; no, l.2 seconds, I think, was their average access time. And this system was also quiet and smooth and didn't shake the machine because of this effort that went into designing the cams.

Norm Vogel said afterward that was quite an introduction, this idea of calculating out just how the motion had to be and then making the cam accomplish that motion. He said he had never really known mechanical engineering where they tried to unravel these elastic problems before they actually did their cam cutting. He was later able to find some chapters in the backs of cam books about polydia= (?) cams and so on, where this actually was accounted for. It was new to him. Than we got another man who was quite interested in doing this kind of analysis work. A man I would like to see again. Since I left IBM I haven't heard anything of him and I can't remember his name. If I think of it I'll let you know. Anyway, he sat down and really made a fine analysis of this.

MAPSTONE:

This was one for the 350 RAMAC?

WOODBURY:

The 305. This was good for the 305. It was used on this model machine where the discs were set on their side under the printing arm, and the whole machine was supposed to be about so wide by so high by so deep. This was the complete RAMAC. It was called the mini RAMAC or something like that, I've forgotten. We had it quite well along, but we were experimenting with electronic technology, and Greg was looking at the possibility of using transistors and ferrite cores alternately to generate ring circuits and to do the arithmetic. We had that fairly well worked out, but with nothing executed to show, when the division between research and product development and so on took place. I wound up working for Piori and Dr. Tucker and they took this project and put it in the morgue. What he really did was to ask for a report supporting my work. I said to Ray Johnson, who told me to write it, "Ray, that's the end. All you need to do it get a nice written report like this out where the people who want to lower the boom can look at it, and then they can write their rebuttal and that's the end. I'm just asking for suicide." He said, "Write it." I said, "Do you understand that?" He said, "No, he thought this was an opportunity to make it go." I just wonder how stupid you can be. Maybe he believed that; maybe he knew exactly what he was doing. At any rate that's what happened. When you work these things out and present them as fait accompli, you've got something. On the other hand if sales originate something they want built because they can sell millions of them, then you go build it. You're an errand boy if you are an engineer who does that for a living. But if you are projecting something new, the less exposure you have the more likely you are to get away with it. Let's assume that you provide something in writing, you give somebody something to take their axe to. No matter what you write down, somebody can take their axe to it. It's very hard to write the kind of mathematical prose that can't be argued with.

MAPSTONE:

So what happened next?

WOODBURY:

I can't really recall what I did too much after that happened. I know that I did a lot of reading. I had a nice office and I had a painting by Bob Watson up on my wall. It was all very nice. I remember one day I was reading a book by Laswell on political science and (?) came in and looked over my shoulder and said, "Well, our very next machine is going to be a political machine, huh?" (laughter)

Some time after that, Gardner Tucker called me in and told me I should build a small computer. What I lacked was any contact with any feeling for what to build. I didn't know where to turn and I hadn't done anything at IBM now for a year or two. I felt like a pensioner. I really did want to be doing something, but I couldn't see any way in IBM to do something with the feeling that I would be able to continue and work something out like I had done on the original machine. I was too exposed now, and if I really tried to bite off something, pretty soon I would have problems. In other words, I should generate a two million dollar project. That's not the way. If there's going to be a two million dollar project, let's divide it up into ten $200,000 projects and see if we can't generate ten responsible people instead of one. I don't know whether this is possible, but I'm the idealist that hopes it can be done this way.

I have a feeling that when Mr. Watson, Sr., was running the company, that this is what he managed to do. That feeling is: sometimes people had a larger order and they needed some more money for larger, more complicated mechanical projects, and then very small things which one or two men had worked on. I had a feeling that Endicott was just a warren of operations of this kind, and I could cite a number of products that developed like that.

I didn't feel anything of that sort was going to exist under Piori. I had the feeling of the Navy research bureaucracy in operation, and I just wasn't certified in any way, and the way to get put to work was to get thrown out. I said, "I can't do it. I just don't have the know-how or whatever it takes to build such a machine." Tucker said. "Then you will have to go. We'll give you six months to find some other job." It turned out I was only entitled to four and a half months because I hadn't worked long enough to have six months paid time to find another job. He said, "Come back on February 12 and we will fire you." This was in December, 1959, along about Pearl Harbor day, as I recall.

MAPSTONE:

So you left?

WOODBURY:

Well, they fired me. They threw me out. I got six months pay, but I had to stay on for a month and a half so he could make good his commitment. I exercised my option to talk to Tom Watson, Jr.; which I did and enjoyed. Then I went down to Princeton and talked a little bit about the dissertation and then I wondered what to do and I came home. I would go to work at Princeton occasionally in the remaining month and a half, and then I went my way.

MAPSTONE:

Did you talk to Tom, Jr., about your conceptual ideas of computing machines?

WOODBURY:

What was there to say to him? I had the impression that in talking to him, I would be contradicting a lot of miscellaneous information that had been fed to him about the source of things in the company. I didn't especially want to fight for that. I wasn't one to shout at him, "I did this, what about it?" What's more, I might have had to convert fifty thousand bureaucrats if I did say so. I didn't know. I did talk to him about the relationship of IBM to the economy, and this relationship of the phone company to communication. In due course you are going to get the same treatment as the phone company; you will be told what you can charge and you're going to be run like that. He said, "Yeah, but the longer we can keep them off our backs, the better we'll do." I said, "There's really much more reason for IBM to be subject to regulation than the phone company, because IBM's built into the power system with much more degree.

MAPSTONE:

It's happening, too.

WOODBURY:

Oh, yes.

MAPSTONE:

When did you leave IBM?

WOODBURY:

Well, the official leaving was on Lincoln's birthday, 1960. It must have been the great emancipation. [laughter]

MAPSTONE:

What did you do after that?

WOODBURY:

I had quite a lot of IBM stock which was going up nicely and I kept careful count and I was spending money as fast as it was accumulating. So I went along for quite awhile. Then on leap year day my mother passed away, and I found that I had quite a little more stock. I kept on and I'd say, "I'll retire now and go back to work later." So I took up skiing, enjoying myself.

In the fall of 1962 I needed work. I made a list of my patents and what they were about and gave them to Walter Johnson of American Forest Products. I was interested in Walter Johnson, and I knew he was the man that financed Stanley Friden of Friden Calculator.

I met with him and then he called up Leland Robinson and asked if he knew me. Well, he sure did, and with that I got a six month, extended to eight months, consulting contract at Friden.

[End of Tape 1]

[Start Tape 2]

We picked this up where I was at Friden after being engaged by Robinson and Hair to consult. I think I already had discussed the Polish notation. (on the other tape)

MAPSTONE:

No.

WOODBURY:

That didn't get on the other tape? They talked about a Polish notation stack machine, and I was curious of its origin. They didn't know where they had gotten it from. They Just had the mechanism they wanted to implement. I went to UC library and started digging and finally came across papers by Z. Pavlyak of the Polish high school, or Warsaw high school in Warsaw, I've forgotten, but I could get out his letter and read these papers.

The machine was fine for a non-recursive computation, but any time a computation was recursive then you had difficulty. You had to go back to reform the stack, and there was nothing systematic about this from one recording to another, so that the system broke down for any technical computation with recursions. The biggest liability was that it required planning. When you go about a computation and you have a layout of what you want to do, you would like to go straight down it and have the computer at hand have the right buttons on it to let you do it the way, rather than having to revise it in order to conform to the vagaries of the machine.

This began to give me ideas about how to organize a computer so that it went straightforward about most any computation, and without multiplying the keys so that you were faced with an inordinate number of keys. The idea is to be simple, so that with an understanding of what you want to accomplish, and the labels on the keys, with only a little bit of experimenting you can find out how to do what you want. That was the beginning of writing a disclosure, which I subsequently submitted to IBM as something I hoped to do. They turned it down. It was also dealing with the number system.

The numbers or magnitudes sometimes have a certain precision, and much programming in machines is done to take care of numbers on a floating point basis. At the same time. numbers are also dealt with as integers or counts and have a specific value, and sometimes the quotient doesn't matter; it's the remainder the matters. The characteristic of many of the flowering contemporary machines is: they develop a quotient and you don't get the remainder. Sometimes the remainder is all you want. For example, working with rotation in physics, but working with units conversion, gross, dozens and so on, remainders are always part of the problem.

I was looking for a proper prescription for how to do arithmetic between these two systems, and something occurred to me that I wrote out and subsequently modeled when I went back to work for Friden. At first, all I did was write a disclosure of how to organize such a machine and later, on seeing IBM's APL , I saw some of this spelled out. Particularly the part where you could spell out the operations you wished to perform on the numbers, and it would save these operations so that you had a program written in the machine which you could then test. Now, I had a little different approach. I was going to make it so that it stored the program you were evolving while you did the examples with the actual numbers. You could indicate whether a number was a coefficient which should stay, or was a place at which you wished it to stop and ask you, or the card reader, or the disc storage, or the tape, or whatever for the entry to continue the recursion. Much of this was inherent in the APL language, but on the other hand I was thinking in terms of mechanisms, and in terms of implementing mechanisms, not in terms of building a program to operate the machine and do it in kind of a round-about way. I had an APL tape down there that got passed to me at no charge from IBM, And I'm not sure how come. Maybe they know all this for all I know.

MAPSTONE:

What did you do with this disclosure?

WOODBURY:

I just got it dated and signed and I submitted it to IBM, and I reserved it against any possible work I might do in this territory at Friden. If I used some part of this material which wasn't theirs by virtue of my consulting agreement, it would be recognized that it was not conceived by Friden and I would make a claim on it. This is a hazard when you come in and say, "Look, I've got a lot of work here and it's not paid for yet. If you decide you want to use it, I'd like to have some arrangement on it." I had expected to work at IBM indefinitely, but after what happened I've thought I had better establish property rights on these things instead of letting them go.

MAPSTONE:

We talked earlier about the patent on the Wooden Wheel. Do you have other patents that are important that I should know about?

WOODBURY:

Well, there were those two first patents for the Wheel itself. As I said, the claims of the patent were really Truman Wheelock's work; the disclosure of the patent was my work. There was a cathode ray patent which was taken out in three names but the suggestion was a flash I had--just the kind of flash the patent laws described--supposedly taking place one evening at Greg Toben's house. I felt so indebted to them for putting me in a position to do this work that all three names should be on it. I think maybe they should have been interchanged with the Wheel patent, but there's nothing I can do about that now.

Subsequent to that I was named on the patent on RAMAC. The original attempt to patent RAMAC ran afoul of the Wooden Wheel patent. It turned out that patent was already described, so they rewrote it and this, of course, had to do with the taking of claims from the Wooden Wheel patent over to Remington Rand, and the IBM having to pay seven million dollars to Remington Rand. It wasn't just for that; many other things were involved.

By the way, this is all kind of hearsay. I have no immediate knowledge of the negotiations between Remington Rand and IBM and the patent claims. I just know what Baron [?] said to me, and for all I know I may have embellished that quite a bit in my own mind since it's been a few years.

The subsequent patent on the RAMAC had to do with the organization of information transfer, which was based on Wooden Wheel. Now, I'm talking about transferring miscellaneously long vectors of information which would be card images, alphabetic data, processing for writing paychecks and that sort of thing, as against the disc file, the same kind of information to work it out. The simple calculations--add, subtract, multiply and divide with a single address out here in the discs you get part of the information. John Hanstra and Lou Stevens were in on that. In fact it was a great conglomerate patent and the only part of that patent I had anything to do with was a little bit of systems work that really belonged in the 705. I didn't add anything new when I brought it over except insofar as to present it new circumstances, in alphabetical data record circumstances, instead of pure numerical arithmetic circumstances.

MAPSTONE:

Was your name on the patent?

WOODBURY:

Yes, my name is on that patent. Then there were two high-speed punch patents. They did a lot of talking to me after I had left about the patent, which I assumed was the original high-speed punch with the swinging die punch. That was certainly my work along with Norm Vogel's. They were trying to get a second punch, so I suggested moving the intervals in lateral. I had forgotten I had made the suggestion until I saw the patent. I said, "Oh, sure, I argued right, I did add that." But they did not make clear to me at all what I was arguing about. I was almost certain from what they were telling me that I was arguing still with the representative and had forgotten all about suggesting that you could cut down the [?] and distance and everything by sliding that a little bit sideways.

Norm saw that drawn up as part of the patent, and there was a big argument. IBM patent didn't think I had anything to do with it. Norm kept insisting that I did and I was kind of going along assuming Norm was insisting on this other thing which had gotten embodied and on which I had really worked hard.

That was a very odd circumstance and it was kind of a mess. I'm giving you some information that in a sense should just be forgotten, because IBM was peeking around to see if I had really done it or not, as though I had some kind of a photographic record in my mind of precisely what was suggested. Yet, when I saw the patent, I said, "Yes, that's all right. I have no apology to make on that." I had an apology to make on that first patent to Truman Wheelock, but not on this one to Norm Vogel. I didn't think Norm Vogel even intended to generate patent material. He always had to have someone ignite him. An excellent mechanical engineer and skilled at machine work and all that. All that facility that I gained there, Norm taught me. On the other hand, I lit the little fire that made these fast punches that Norman then executed so beautifully.

MAPSTONE:

Well, that's part of what communal working is all about, isn't it?

WOODBURY:

Yes, but this isn't a great big mass of people, you know. There's very specific spaces we're occupying. Everybody's got a very specific niche that interchanges information and foils with somebody else. We had a terrible fight at Poughkeepsie over the cathode ray tube storage. Finally Truman and I overruled Greg, and we broke his enthusiasm to some measure on that decision.

MAPSTONE:

What was the fight over?

WOODBURY:

How to build the amplifiers for the cathode ray tubes. He took a regular box that people build electronics in, he put a cathode ray tube down inside, he built the amplifier in it, he put it all together; then he had a little plug in the back so this whole box plugged in at once. We said, "Greg, it can't be that way. The tubes have got to plug in, and the amplifiers have got to push in between them, so you can pull one amplifier our and exchange another, or pull tubes out to change them. We cannot go in there, take the lid off and burrow in a place that even a mouse couldn't get parts in and out of. They've got to be in the open so you can work with them." Greg almost cried when we took this thing and just tore it apart.

We made a t-shape section that shielded the front of the tubes, and a long slide with the amplifier and the tubes plugged into it, and the plugs are in the back. The front screen of the tubes had a swing contact coming out, which was the first stage of the amplifier, and at the back is a plug for the amplifier that gets the signal back. Up here are the circuits that run this way to all the tubes in parallel because we read all the tubes above and below and the ones we write one and zero that's driven from below to it's a three dimensional picture. Here are the tubes across here from high to low order and sine in pairs. The cover for the tubes with the shielding glass and the pick-up screen for the tube is attached to the amplifier which cools out, and it turns out to be the T extending over the tube and between the tubes back to the base into which these tubes are plugged and on which the shields for the tubes are mounted. The tubes are also magnetic shields.

Now the base from below come the signals saying write ones, write zeros or the read signal coming back from the amplifier. From the end come the signals that deflect the tubes, because all tubes are deflected into the same place at the same time, and the signals that turn the beams on and off, except for that choice of signal for write one versus write zero. Only the upper or lower tube is turned on during the write process. You make a mark on one tube or the other and then at read time you turn them both on and amplify the difference.

This is one of the things that made lots of money, but it was a real overruling by two of us against one, and it was too bad. We were never able to convince Greg that this thing shouldn't be built inside of a bottle. He literally had a ship in a bottle situation for us. We tried to persuade him that we couldn't work with that, there were problems with it, that we couldn't get at it and that we were not willing to let him have a private bailiwick. None of the rest of us had any private bailiwicks. Ha could work with our circuits; we had to be able to work with his.

MAPSTONE:

That's fascinating, because in talking with Toben and Rex Rice and you, it appears that Toben was the one at Northrop who said, "If you want to use this machine, you're going to learn how and you're going to know what every plug in that machine is for. Yet here he was breaking his own philosophy.

WOODBURY:

Well, in a sense. I don't think he saw it that way. He thought of the whole thing as having to be a unit package. I believe he was thinking radar and electronic radio, where you couldn't break these things up without interfering. What we saw here was a high voltage turn-on/turn-off coming across this way on the tube, here is this amplifier going back between the two and delivering a simple signal for that, here is a common power supply line, and here is the line that comes up here that says turn on one or turn on both; or turn on upper. Turn on lower. I guess there were two lines; turn on upper, turn on lower, and a thing controlled from below, turn on both.

MAPSTONE:

I talked to Greg Toben and he's a very retiring man.

WOODBURY:

Yes, he's very shy.

MAPSTONE:

We also interviewed Roy Harper, Jim Smith and Dave Montgomery. They were a great help because they brought Greg out. He would humbly say that he hadn't been involved with such and such, and they would point out that he had. What you could do is tell me what you thought Greg's contributions were. And what his role was.

WOODBURY:

Somehow Greg was not technically into it, even though he was technically part of it. I don't know how to describe the way in which he made a place, in which he kept me convinced that just because I was a poor mathematician from Princeton I shouldn't be afraid of electronics, and that compared to the things I knew about mathematics, this was simple stuff. Really his contribution is a much deeper contribution than mine.

At Northrop he was in charge of all the machines. Bernie Shore kept telling me not to look to Greg so much for whether I could or couldn't do something with these things. He said, "You're over-rating his process of thought on these things." I think probably that was honest. I don't think it would be fair to expect Greg to come through with machine structures; but he came through with sense, lots of it. Do you follow what I'm trying to say here?

When we put the 601 and the 405 together, I did it. However, somehow he had observed that by wiring through the top counters of the 405 to the summary punch, you could get those signals across there. To me, the fact that there must be interlocks and stops and starts on these machines, because they were always stopping and starting, made it obvious that you could interlock the two machines with each other, but it took me a little while to find out how. But I did.

Greg was busy doing the computing and getting it out; the dirty work. He didn't really want to come over much; he seemed to want to leave me to this process of doing these things. Jim Smith, of course, saw us together and he was the man who had to turn his back while we were wiring machines together. Subsequently he did something that I couldn't figure out how to undo. Through him, we made a suggestion of how to eliminate the remainder when we didn't want the remainder in a divide, because this would save a cycle on the 604. You divide the…you put an extra hub on the machine to accumulate at the end of divide. Finally, a $50.00 or $100.00 award came through on it and Jim split it with me. I didn't expect to see it, and didn't want to see it. There was more to it than that. Since I went back, nothing came of it, and about a year or so later I remembered it and I mentioned it to John McPherson, I believe. Then in a few months this check came. I didn't mention it to John McPherson for me; I wanted Jim Smith, who had been so damn helpful to get it all.

Dave Montgomery, of course, is CE. He was there and got educated in operating it long after the machine is all built and all this process had taken place. Greg was housekeeper. He kept the thing in order, he knew what you were doing all the way down, every part and parcel of it, and he was almost the spirit of it. But he was retiring about putting forward ideas and this was one of the reasons, I think, for his box. He didn't feel he projected himself into other things as much, so here's a little kink he wanted in it.

MAPSTONE:

Did he go to IBM before you?

WOODBURY:

This was in conjunction. He tried to go to IBM before, but because he was diabetic they wouldn't have him. Then, after all this happened, and I asked could he come to work, they snapped him up. Subsequently, I suggested to Bud Beaty that Murray Lesser come to work for IBM, and they promptly snapped him up, too. The others went to work for IBM and went their own way. Rex Rice was more of a promoter than a thinker-outer of something, though for all I know he may have done some excellent work at Fairchild, and at IBM. At Northrop, we saw him bringing down structures problems and doing them in kind of a pedestrian way. I'm not sure whether a man that does stress analysis can be anything but pedestrian about it. Our experience with structures analysts at Northrop was that "stress man" was a dirty word.

MAPSTONE:

Rex Rice was one of the people who was probably most turned on by the CPC and by what it made available.

WOODBURY:

Yes, for that tremendous pedestrian job they had, it was a miracle.

MAPSTONE:

He has recently built a machine.

WOODBURY:

I didn't know what he had been doing. He's at Fairchild.

MAPSTONE:

It's called SYMBOL and it was built for the University. It's a machine based on bringing as much to the front of the machine as it is possible to do. In other words, it's not all buried within the machine.

WOODBURY:

I would have to see the machine to know what was meant by that.

MAPSTONE:

He says the machine is a pot full of parts and that you, the users, should be able to use this pot full of the parts to do what you want done.

WOODBURY:

That's the same with the 795.

MAPSTONE:

Exactly.

WOODBURY:

I realize this permeated all those people from Northrop. Rex came from stress. Do you know what it's like to do structures analysis? You have these great pages. In the left hand column you have the configuration values set out; in fact there would be several different columns because there would be several different stress configurations. Then you start fil1ing this out across. You carry numbers from one page to the next page. You integrate the columns by adding along, you differentiate columns by taking the differences between, and you just fill this out, fill this out, fill this out. Then you get books and books of this covering all the landing shock configurations of the airplane, the in-flight surges of air pressure and so on, and you wind up with just a great bible of structures information. Having girls punching Fridens to calculating all this stuff, was just an infinite chore.

A fellow named Welch preceded Rex Rice, and we used to speak of being Welched. Being Welched was when you took a bunch of answers up and he opened this big tabulation in the middle, "That number, how did you get that?" Just like it was in red ink. He just had an eye for the mistakes. Then, when it wasn't our mistake, he would come down and revise the calculation prescription after we were three-quarters of the way through, so we would be back in the first quarter.

The CPC made this much easier because it ground it out. If you had a small change in configuration, your prescription for the works was all made. You could just grind that change in and run it through again. I shouldn't be unfair; I enjoyed Rex, having meals with him and so on.

MAPSTONE:

I would like to mention another name, and that is Murray Lesser.

WOODBURY:

Murray Lesser was one of the sharp analysts.

MAPSTONE:

Was he at Northrop?

WOODBURY:

He was at Northrop and he was in structures, aerodynamics. I recall he got permission to go back to Oakridge. He was still a bachelor when he went there, and they presented him with a fur-lined leather G string with sliver straps to hold it up. He came back married to a girl, Millicent Lesser. Who I still see once in a while. He left her. She liked it out here and when he suggested going back to work in New York, she said good bye and they got divorced.

He was at San Jose thinking hard about the problems of the RAMAC when we were there. He could hold still and think carefully about the kind of things that get written down. You know, express the kind of things that lie underneath the programming theory and so on, that I never could bother with. I could get a program together all right, and once in a while I had to come to terms with some of the theoretical difficulties, but usually not in such a way that I wanted to stop and think about it and write an essay on the generalities of it.

MAPSTONE:

Was he with you in your IBM work on the 795?

WOODBURY:

No. The summer I came out to Northrop for the three month stint, I outlined what I intended to do when I want back to IBM. This was when I argued with John von Neumann. I described this hypothetical electronic machine to Murray and to Greg. I got the impression when I arrived with it that they hadn't understood it all, because they were quite taken with the machine when it arrived. Greg, of course, was with it; but when it arrived Murray and Rex were quite pleased. So I got the impression that I had not managed to describe the machine very visibly to Murray or Rex. I thought Murray had grasped the idea completely, but I had talked too fast and there were too many gaps. This picture of how it would operate, and how it would cover all fronts as it were, were all developed in my mind.

MAPSTONE:

They were pleasantly surprised when they were able to get in and do some real problems.

WOODBURY:

Oh, yes. They immediately went to work. Many people did lots of problems on it just like that. Murray devised a form for laying out the use of the plug board to do planning and that was quite helpful--an excellent form.

MAPSTONE:

Did you do a lot of work on the 705?

WOODBURY:

I did that one large job of the [?] Stokes equation with Murray. We did other problems. We worked on the equations of the Eccles Jordan trigger circuit for the vacuum tubes, to discover what were the significant factors in the design, and what were not significant. We learned a little bit about where to improve it. We found some of the things that were being done in the name of [?] weren't really very much help, at least as far as our mathematical model went.

I don't remember just what all I did do because I was there only as an IBM employee, from August to December. Basically I was concerned with getting the thing going and telling people how to operate it. I wasn't really concerned much with problems, and so it was just teaming up with Murray accidentally that I did any computations.

MAPSTONE:

Were people from other companies coming to see?

WOODBURY:

I don't remember how much other people came to see it. I'm not sure that IBM showed it very much. I had the feeling that IBM didn't want it to be seen because they didn't want to get into a problem with that versus the 650 and possible demands for more machines. If the customers wanted a big machine, they were encouraged to buy a 701. The 701 cost around $15,000 a month or thereabouts. The 705 cost $2,500 per month; IBM could have paid the freight on its 604 scale of operations at probably $1,500 a month per machine. This is quite different from the 650 price of $3,500 to $4.000 a month which, of course, was competitive with ElectroData and so on. Here again, the 650 was possibly not as expensive to build as the 705, except for the cost of the drum. The 705 probably had a few more "nuts and bolts" in it; certainly the cathode ray tube technology was a little more expensive than the 650 vacuum tube technology.

MAPSTONE:

Did the 795 have abilities that the 650 didn't have?

WOODBURY:

Well, if the thing will multiply, divide, add and subtract, and do it from now on forever, what more can you ask of it? What could any of these things do that a man can't do with a stick in the sand? You are talking like the Turing machine, which is one of the more ridiculous things that have been circulated in the computer field.

MAPSTONE:

I'm trying to understand the reason why IBM didn't want people to see this machine. They wanted to rent the 650, but this machine was renting for a lot less.

WOODBURY:

This one was, but only in this particular example. I believe the ones that had the storage multiplied by tens that's a thousand words of storage--797s--they rented them for a price that was up toward $7-9,000 a month.

MAPSTONE:

It wasn't even competitive.

WOODBURY:

No. I'm sure two 650s would do as well provided you had the right kind of work. We are discussing the same kind of thing when determining whether it is better to have a great big IBM machine or a small model 30 in the present 360 series. I have two experiences.

First, let's take the original. In 1939, Douglas Aircraft had 500 employees. Mrs. Don Bosio, wife of the director of engineering, and two other girls got out the payroll with Comptometers. Four years later, Douglas had five thousand employees, that's just ten times as many--and they had about a half million dollars of Remington Rand punched card tabulating machines and they had thirty people getting out the payroll. The ratio of dollars per hour for those hand machines versus comptometers is astronomical. There is just no comparison in cost per check to get out the payroll. It's costing them just as much or more to get out the payroll per person per check as it did when they had five hundred and no machinery at all. But they get it out, and who can make thirty girls put out a perfect payroll unless they put machinery in for all the bookkeeping and transcribing, whereas three girls and five hundred checks to put out never made a mistake. There was no gain of efficiency from this multiplication of machinery it was just the possibility of managing a larger organization without complete chaos.

We've got the same thing now. Friden had two Honeywell machines with discs and tapes on them, and they were using them for manufacturing control and so on. The question was: now to expand because they wanted to get deeper into manufacturing control. Joe Bogart suggested a 360 Mod 40, which, I suppose, would be rated as having the capacity of about four to six Honeywell machines. Not only did the Mod 40 get completely filled with work, and was replaced almost immediately with a Mod 50, but it didn't[?] life a single thing off the Honeywell machine. Eventually, with the Mod 50 and 65, the Honeywell machines were first discontinued at University Computing Service, where we did the Honeywell computation and eventually it got transferred onto the other machine. But the computing bill for Friden had been multiplied many times over. There’s a lot being done now that wasn't being done before, but I would not want to have to defend their value in court.

I just observed that larger machines entail more software, more churning of IBM's supply programs, doing only processing--the succession of jobs flowing through the machines, and had nothing to do with the actual computations being made. Small machines working directly on the needed computations can hardly help but be more economical than a large machine, churning a lot of systems programs to decide which of several programs, which happen to be resident in its core of storage at the same time, will do the job. This all takes time on the machine and programming space.

It's a little hard in the face of all this to work up a deep enthusiasm for building some new type of computing machine, except as an artistic effort. I would rather go play the piano for an artistic effort unless somebody gives me a real free hand with a brush and pallet and a canvas.

MAPSTONE:

We talked about ElectroData earlier, and then you mentioned Floyd Steele and that you would have some things to say about his work. Do you?

WOODBURY:

I can't remember what I had in mind. I don't really know a lot about Floyd's work, or even what was his versus other people. I can remember in his digital circuit right out over the end a ten megohm resistor, and I know that a ten megohm resistor has got nothing to do w1th germane circuitry in digital work. That alone was kind of a mark of somebody who thinks he's got devils in here.

MAPSTONE:

Let me refresh your memory. In your initial paper, you talked about a group who left Northrop to form CRC: "Floyd Steele was reputed to be the guiding genius. Of their work and that at ElectroData, I'll have quite a bit to say later." You talked about ElectroData. I wondered if there was anything you wanted to add re Steele.

WOODBURY:

Okay. First off, they ascribed an importance to this digital integration process, that was out of all proportion to its value. Several efforts were made by Hagen and so on, to make these magnetic drum digital differential analyzer mechanisms, and I don't think they were of great interest to anybody, although IBM was excited about them. I think what I said about ElectroData and the cost of construction applies precisely to them. They never looked inside the IBM 604, or the 603, to see how these people who had been working with digital pulse circuitry for a generation before the electronic people even discovered it had come to deal with it. They weren't taking any advantage of a well developed technology and accordingly they spent three dollars for every one they should have spent. I think that's all I had in mind, that both the group that went to work for National Cash via Computer Research Corporation, and the ElectroData group, fell into the radar data electronics trap. They had this illusion that there were spooks and ghosts and dust in the vacuum tubes and all these funny things that just didn't exist.

Did I discuss the BINAC dust problem and how very real the real problem was; the difference between the servo pistol oscillator mercury delay lines versus the electric delay lines that did arithmetic? This was one of the sources of the dust theory; another was the wool from the girls' sweaters getting in the vacuum tubes and making occasional slips. The machine was entirely serial, so synchronism of pulses in the main register as they flowed by serially had to be maintained, and they had to stay synchronized to the mercury delay lines, which were the main storage. The mercury delay lines are kept in synchronism with quartz crystal by means of a heating element to keep the mercury at just the right temperature. If it slows down just a little bit you heat the mercury a little bit or vice versa. It is monitored so the frequency of that delay line is monitored by the quartz crystal and is precise at all times.

Now, we also have electrical delay line registers. Bhc? pieces of phenolic. The delay lines delay constant has to do with its deductance, capacity and resistance. In any environment, outside something in outer space, there is a certain amount of dust and atmosphere around, and there are changes in temperature, especially in this thing which is not completely enclosed in a bottle and couldn't be because of all these fifty volt tubes and so on. It was a terribly hot machine. There were lots of cool tubes they could have used. At any rate, these electrical delay lines had nothing to keep their frequency absolutely constant. A little change in the humidity, the resistance goes down, the delay changes; a dry day it goes up. They would tune the lines carefully, but if they got slightly out of synchronism and one pulse was lost, it was blamed on dust in the vacuum tube--spooks and ghosts. It was just bad engineering by Eckert and Mauchly. Everyone of those had to have its own monitor with a quartz crystal on it.

MAPSTONE:

You actually did do some playing around with BINAC?

WOODBURY:

I studied the BINAC very carefully to see what had been done. I saw this gap in it, inquired and discovered there was nothing to keep those tubes together. I had already been worrying about some short delays we had to have to monitor the cathode ray tubes in the 795.

How did I get into this? I was in there quizzing him but I guess it was during the summer. That must have been during the in-between summer when I worked at Northrop. I had only had this experience with the Institute computer. That's right, because the BINAC was gone two years later when I came out with the new 795 machine. This was during that summer when I came back after the year at the Princeton Institute, looking at their machine.

MAPSTONE:

This was before you went to IBM?

WOODBURY:

Yes, the end of my first-and-a-half year at Princeton.

MAPSTONE:

You were actually quite familiar with the work on the BINAC. It was a very controversial machine in many ways.

WOODBURY:

It sure was.

MAPSTONE:

Did Eckert and Mauchly push the technology too far; i.e.: four megacycles?

WOODBURY:

Yes. Four megacycles probably wasn't as bad as failing to observe that any place where they had a delay line of any length they had to do something to make sure it held still. It obviously added and subtracted at four megacycles all right, and that was fine, no question. On the other hand, the only reasons for the slower speeds on other machines was the cathode ray tube storage. Let me describe it on the 701.

I don't know whether you can imagine a differential amplifier switch, where you only hear the difference between the two things. When you try to find what's coming out at the end of the cathode ray tube, you are going something like hitting an iron plate with a sledge hammer and trying to hear whether a piece of gum is stuck on it or not. If you had two plates perfectly tuned, one to the other, so that the tones emitted were identical, and you put a piece of gum on one of them, you would hear the difference in tone. You would hear which was high and which was low, and the one with the piece of gum on it would be low. If you hear it by itself, then that difference in frequency wouldn't even have a wo-wo-wo to go on, which is what you get from the interference of two notes. Well, that's the same difference on cathode ray tubes. With just one to listen to, it was very hard to decide whether you were hearing one that does or doesn't have the extra electrons at the place you hit it. As soon as you have two to compare, then it becomes quite audible. In this case, not as an interference frequency, but still as a difference in the rate of rise of voltage on one versus the other.

When we started searching for optimum operating here, we found that we had a forty volt rise of our pulse. You can only drive it so hard, so you look at the slope up and slope down, and when it got to where it was shimmying a little bit against our clamp--we had a clamp set at an exact height and the shorter we could make it so it still always hit the clamp--this was the most precise operation of the tube. We didn't build the circuitry to do it--it would have been expensive to do so. Now we have gotten over the radar trying to do this. To cut that pulse down and speed it up to the place where it operated the best it could, it was always on the edge of the clamp to find this pulse. It really wanted shorter than one-tenth of a microsecond pulses for its operation. We suspected it would have probably been more like a nanosecond or ten nanoseconds was probably all the pulse that was wanted on one of those electron beams to do the whole charge and recharge and signal job. To do that with the voltages on cathode ray tubes was quite a chore. Of course, you can't do it with transistors because they don't have a thousand volt pulses. From my observation it was faster by far than anything I've seen since.

MAPSTONE:

BINAC was up and running, or not running, as the case may be. Were you ever aware of both computers running in synchronization?

WOODBURY:

I never knew them to do a job. They may have, but I never knew of it. I think some test things were run through, but I don't think they ever managed to make it hold still long enough to get an answer out. Granted, the task of getting data in through a typewriter and out through a typewriter, suggests that maybe you couldn't have gotten much through it even so. Do you follow? See, input/output was a problem. At the time they got it, I talked about that. I said, "Look at these machines with the cards flying through them, we have a tremendous capability of bringing information to them and feeding away on these printers. You've got a mechanism that's going to multiply and divide a thousand times as fast, and you can't get data into or out of it any faster than you can through a typewriter. Something's wrong here." People hadn't thought about the essence of this process. That's not what's going on in the computing world; that's not mass production. If it's run off like that, usually it's faster to do it by hand than it is to put it in the machine, because the effort of planning for the machine will entail executing the calculations to get the answer.

There has to be some kind of recursion, so..big recursion to make the machine pay. If it's a big recursion you get the two to one answer, but if the recursion is simple and many times round, all right then you can take it to the machine. If you have thousands and thousands of numbers to put through with simple calculations, and you have a way to get the numbers into machine-readable form and take the answers away on a printed page, there again you have a useful machine. You must have some kind of mass production otherwise you can't go to the machine with it, except as an exercise.

MAPSTONE:

Did you keep any documentation at all, apart from your patents?

WOODBURY:

No, when the second wife moved out on black Friday, I went and dumped it all in the fireplace. I was tired of the whole thing. I didn't have a lot of documentation anyway. I wrote the paper describing the 794 which is extant in IBM I am sure, and you could refer to Greg Toben for that. I am sure it's available and he would have copies.

MAPSTONE:

No. he doesn't, unfortunately.

WOODBURY:

They are not around in IBM?

MAPSTONE:

IBM archives is a difficult place to get information from. Over the years they have had to rely on people presenting their files to the archives. Only recently did they set up very strict rules.

WOODBURY:

This was in somebody's archives, as it were. I never had these papers.

MAPSTONE:

That somebody else's records might never have reached the archives.

WOODBURY:

It was accessible for a long time because I would ask for copies of it and they could come boiling. I don't remember who I asked.

MAPSTONE:

That's too bad. I wish you could.

WOODBURY:

This account was twenty or thirty pages of double spaced type at the most.

MAPSTONE:

It's a description of the 795?

WOODBURY:

It's kind of why it was built that way, and how to use it. Although the question of why it was built that way wasn't fully answered. John Tukey, my faculty advisor at Princeton, asked why I built it that way. The only answer I could think of was it seemed a good thing at the time. He said, "If you can't tell why you built it that way so we can understand this machine is a member of a class of possible machines, and then you don't really have a mathematics dissertation." That was the end of the dialogue, and John Tukey has been very disappointed because I didn't come back with enthusiasm and present a great mathematical dissertation on this machine.

MAPSTONE:

Would Murray Lesser have a copy of it?

WOODBURY:

He might very well. The patent is a copy.

MAPSTONE:

Let's turn the tape off.

[End of Interview]