Interviewee: G. Floyd Steele
Interviewer: Robina Mapstone
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MAPSTONE:

This is Robina Mapstone and I'm interviewing Mr. Floyd Steele at his home in Otter Rock, Oregon on January 16th, 1973. Let's start this tape by talking about your education, when you went into the navy, your radar experience and events leading up to Northrop.

STEELE:

I got a BA degree in physics at the University of Colorado in 1939. I was the only senior physics major and there was one junior physics major, no others, so we had the department to ourselves. At the end of the year I put out applications for jobs and got on offer to calibratethermometers because no one had ever heard of a physicist. I put out applications to get more education, and I got a working fellowship at Cal Tech in the aeronautical engineering department. I put in two years doing all the work towards a doctorate in aeronautical engineering, and then the war came so I went to Douglas. Instead of going into aeronautical engineering I went into an early version of operational research.

MAPSTONE:

When did you go to Douglas?

STEELE:

In the fall of 1941, not too long before Pearl Harbor. This work was on learning curves, growth curves, things of that sort. Highly interesting, but since gotten a great deal more advanced than when we were there. By 1944 I'd decided I wasn't accomplishing enough at Douglas, volunteered for the Navy and got into Captain Eddy's Radar School. Just as I got through training the war was over and I got out again.

MAPSTONE:

Where was Captain Eddy's Radar School?

STEELE:

In 190 North State Street, Chicago.

MAPSTONE:

He's sort of a famous character, isn't he?

STEELE:

Yes. The schools that he planted proliferated and he was in charge of them all. The reasons I was picked was by luck because during my last year at Douglas I was doing slide ruling, adding, subtracting, multiplying, dividing, trying to do multiple correlations, and I was really hot on ordinary arithmetic. This is what they taught at this primary school; everybody took a brief course to weed out those people who couldn't do those things. Everyday they'd give you material and a ten minute blitz at the end of the day. I sat in the back and read. I thought the instructor didn't know I was reading, but he knew all the time. He pulled me out and said it was okay to read all the time, and I could do it where everyone could see, but if I missed one problem out of one test, I was out. Then I had to sit there pretending to read, at least, and hoping I didn't miss one. That way I got identified as a character in 190 North State Street School. There were all kinds of interesting people in the navy radar program.

One of the fellows at my bench helped develop the electron microscope; another was the chief geologist for some oil company. The background in electronics was quite different than taught in physics. It was really what Northrop wanted when I applied there after the war. The group that I had worked with at Douglas became RAND. At the very early date Douglas set it aside as a special branch.

MAPSTONE:

Was this the RAND project at Douglas?

STEELE:

Yes, they'd get certain people out of every group, including this operations research group, and finally they set up the separate non-profit corporation, I don't know how it evolved. Had I gone back to my old job at Douglas, I may have gone to RAND. Instead I decided to go to Northrop which had an excellent reputation in the aeronautical engineering field. I worked in the test area for two months before they were ready for me on the missile project. Finally I was interviewed by Dr. Ackerlund, head of the computer group. Have you run into him at all?

MAPSTONE:

No, I haven't.

STEELE:

I wonder where he is.

MAPSTONE:

Would you have any clues?

STEELE:

No, except that he lived in the Pasadena area about five or six years ago.

MAPSTONE:

Was he working at the time?

STEELE:

I think he was a consultant for American Astrophysics. He officially ran the computer group; and I was the technical head.

MAPSTONE:

You're talking about 1946. Was this already the computer group that had been set up to work on the SNARK missile?

STEELE:

Yes, when I got there they had directions they were vaguely thinking of going. One was to develop the gigantic tin can to solve an apparently fairly simple problem in trigonometric functions. They wanted to measure altitude and the angle of a star, and it had to be done to high accuracy. The project then consisted of a theoretical mathematician or two. I guess I was about the third of fourth person on it. Other people were ahead of me, but so many transferred between departments or between sections that I forgot who they were before I got to know them.

I was supposed to open the labs for the computer group. Interestingly enough, no one who was on the SNARK wanted to be in the computer group. This was the bottom of the totem pole, which seemed incredible to me because I thought there would be a big rush to get into it. It seemed self-evident that the tying together of the electronic brain, the A-Bomb and B-2 rocket, were going to be the next experimental things. Of these, the hard thing, the one that hadn't been done was the electronic brain—which was in the work we used in those days.

MAPSTONE:

Apparently you were seeing something that none of these people could yet see.

STEELE:

Yes, well, they could see it easy enough, but they couldn't also see that it's possible to make up things, and it didn't sound convincing for a while. It certainly didn't seem self-evident to them that this was going to happen. It also seemed self-evident that the problem was going to be very much larger than one though from simply trying to solve the tin can problem, but we needed an airborne computer and that ultimately the computer had to, just like the control system on anything else, tie into the sensors and run all the actuators and then compute somehow, and no one knew how to do it.

The first work then for many months was oriented around the star function for very precise accuracy. The SNARK contract went out ahead of all the other competitors because it actually had a system that would give the accuracy that the Air Force wanted in principle, at least, although whether it would in detail was not known. There was some talk of putting an IBM punched card system on board, feeding the cards and interpolating them to get what was wanted.

MAPSTONE: Was it seriously considered?

STEELE:

Yes, I suggested they use an incremental recording on tape and record the changes instead of the numbers. That simple beginning led to sequence of computing devices that more and more always behaved in an incremental manner. There was a man by the name of Rob Rawlins who was project engineer. I remember how absolutely terrified I was of my first assignment. Everybody on the project seemed to know what they were doing, except me. Everyone looked confident, everyone seemed to know just what to do, and I hadn't the slightest idea. I joined Northrop in September or October, and Rawlins calmly told me that in February they would have a static test up in the desert with the star tracker ready to go, and this computer group had to drive the tracker in some fashion so that it would track where the star was supposed to be. The actual sensing thing on the tracker would close the loop and simulate what the guidance system would be later on. All this was supposed to be done between September, October and February. The labs hadn't even opened, and first he wanted some method of getting a function in the air, and then he wanted a computer buildup to supply the function on the ground.

MAPSTONE:

Just like that.

STEELE:

Yes. I thought, "Gee, this is what everybody does; they know what they're doing." I opened the first labs in the old clockhouse. A fellow named Root was the purchasing agent. He and I went up town in a company car to an electronic place and bought sacks of different kinds of parts. This room, which had been an office, had one plug in the wall, so I came back with a gigantic extension cord and a series of fan-off plugs to go around the room. They repaid all this by writing out purchase orders for a handful of resistors, and a handful of this and that. I didn't know what I wanted, but I did know I needed a soldering iron. There wasn't enough money at the time to ask for as scope, which was quite a big deal. We were going to get ten, fifteen, twenty dollars worth of equipment and the idea of getting a scope and paying thousands for it was paralyzing. Little by little this lab grew and gradually acquired more people.

MAPSTONE:

Were you doing the hiring?

STEELE:

No, not then. Originally I was put in the lab and I guess I moved upward in the ranks because, I was so bad, I don't know why else.

Eric Ackerlund had years of experience in radio and communication of all sorts, and apparently one thing that everyone learned in that field was a law which was never to be broken, you must have very good grounds to cut noise out. He decided we ought to drive a copper pipe in the parking lot outside the window. Of course, none of us had a hammer, let alone a copper pipe. I went all around Northrop trying to get very ordinary tools. I'd go to a tool bin where they had special tools for milling machines, and cutters, and all sorts, and ask them for a heavy single jack-hammer or something. They didn't know where to get it. Finally we got all the stuff and drove it down in somebody's flower bed at the edge of the parking lot. It went down a certain way and then stuck bedrock and just mashed over and bent. There it stood outside the window, really tasty looking thing with a flared out top. They wanted that filled with salt, so we put salt in the upper end, poured water in it, put a ground strap on it, brought it in and we had a very good ground. It wasn't a bad idea except it took a world of time to make it like that.

Finally, the labs began to grow, we moved into a better quarters, and little by little it went. I really had a paralyzing time until about November when I discovered that no one else in the project was anywhere closer to the desert test than I was. I wasn't really making any headway. It turned out that by the time the lab had built up and we had twenty people, it took about two years to accomplish that particular goal. Finally the test was carried off about two years later.

In the meantime I got the wire recorder I mentioned, which was a hot item. It was all modified and fixed up, but it didn't have a control motor. I borrowed a fan motor from home and set it up with a couple of thyratrons, we had a side drive on the wire, a whole bunch of real Rube Goldberg things. It didn't have a chassis, so we nailed components on pieces of board; it really looked terrible.

I think the next person who came in was Dick Sprague. I forget the order after that. Al Wolf came in, and Will Dobbins.

MAPSTONE:

Don Eckdahl?

STEELE:

No, he was a little bit later, not much. We'd gotten our third site, I think, before he was hired.

We began to build what was called an Incremental Slope Computer to divide the function for this wire. We were going to record increments on the wire. The star function would be a curve and they wanted it to be accurate to 1 part 5500 overall. So they wanted us to supply 1 part in 55,000 which, of course, is preposterous. If one fits the curve with a large number of straight lines, you could then put in a linear interpolator and it out of this linear interpolator which I got started on, that the integrator idea evolved. The idea of this tape would be that each time it changed by a fixed amount, it would put out an increment, and the increment would go into a stepping motor which would just keep turning the angle up. It really made a very simple way of getting the accuracy very clean on the ground, and then putting it back in the air and servoing the whole missile to follow the recording. Hughes had the opposite deal; they had a computer tied into their missile sensing and it was supposed to deduce where they were. It was an analog computer and they simple could not get accuracies of the sort they needed. This system had a whole lot of defects as far as promising high accuracy.

The next step was to get a reasonable one of these and an attempt was made to get it ENIAC style by using rings of ten, to five decimal places. This meant there were five relay racks, and a great big room was built up. It never did work, although once or twice we got a short running function. It was Eric Ackerlund's opinion that once you started something in the electronic business, you never abandoned it, you never admitted defeat, you never let on that you goofed, and you always pulled it through somehow. Eric was an extremely nice guy and all he would do was suggest and hem and haw and hope. He would have at least a little edge over every new man that he hired so he would put him on this machine to get it checked out and going. After about three months the fellow would get highly interested in new things and drift off and start something new. So it never really did actually work.

MAPSTONE:

It must have trained an awful lot of people.

STEELE:

Yes, it was the ENIAC of our project. One interesting episode that happened, I think it happened on the Incremental Slope Computer, is typical of crazy things when the top brass come through. They did periodically make a survey, and everyone would be alarmed and uptight and stay up half the night hoping something would work. Generally speaking nothing did.

This particular day I was certain nothing was going to work so I put a very elaborate pattern on the slope. By taking the digital voltage on both axes I got the equivalent of a figure-of-eight Lissajous pattern, only it had notches on it. On my scope it looked just like a pair of gears with teeth, and as the thing processed slowly, the gears would run one way and then run the other way. I put these wires around and stuck that on the scope. This set of people came through and stared at that in fascination and didn't watch this wire I was servoing. When we were done, the Air Force officer with them said, "Well, thank you very much," and reached around and turned off the equipment. He turned off the bias voltage on these tubes before he turned off anything else and bang, the whole set of tubes went. I had hundreds of bucks worth of tubes. Everyone just stood there for about five minutes. Looking at the guy, looking at each other, wondering what really happened. I went over and started turning knobs on the computer for a while and then gave up that day. It was a reflex on his part; some people have switch reflexes. But it was a low period when nothing was really working, nothing underway, and what we did have looked awful.

MAPSTONE:

When you think about it, when you start from scratch with no real direction, it's rather difficult to have something beautifully put together at the first attempt.

STEELE:

It was also true that no on had ever done any of this before and we had to learn. It was a great comfort to find out that nearly everybody else was in the same boat too; nobody was able to do the impossible things.

The next step, as I recall, was to get the computer cleaned up. I designed a binary unit for it, which actually was built up and worked. For that I got moved up to office work, which at least was helpful. A very interesting thing about a binary counter is that if the clock be input to a counter, and if the times at which the flip-flops of the various stages change from 0 to 1, it is quickly seen that only one flip-flop changes at any given time. All you have to do is to mix these. Here, if the clock is a rate of one, you get rates of half, a fourth, an eighth, clear on out to an Nth of the clock. By selecting these outputs and putting them on a common line, for each N input, of the clock, you can get M pulses out. Where the number of counter stages is large, you can get a very high degree of precision. If you connect two sets of switches to the carry-ups of the counter and switch the mixed outputs alternately to the final output, it is possible to manually change one set of switches while using the other to generate a slope (a straight line). You end up programming the thing by hand, sort of like a card program only with switches in your hand. That worked interestingly.

I was naïve; I thought I'd invented the binary system. Really I did. It didn't ever occur to me that you'd use a base two number. One time I'd seen something about base twelve numbers and I thought base two would be ideal for this. I dutifully worked away at it to see how to go about adding and I got very excited. It was sort of a comedown to find out that Leibniz had done it, and that people had been using them in computers for a long time.

MAPSTONE:

Which machine did this binary adder go into?
STEELE:

It was called the Incremental Slope Computer. The next thing was that somehow we wanted to get some computation. I kept looking around for any means whatsoever to begin to modify this from the straight line into a curve of any kind. I developed a reset counter, in which you'd count down in this counter until you had an overflow, and then the overflow would come back and reset some constant. This gave a large scale down, so if you put in a very high rate and scaled down to a low rate, which is all you needed for this kind of thing, you could select it very finely. This was closely related, but not quite the same, as selecting the switches by hand. Then, I tried all sorts of versions of making this a variable. If you make that a Y, you always tend to get things of this sort; Z is equal to some constant, N over Y, Y always ends up as the denominator. So that wasn't a very fertile thing, but I had some luck at working up curves from this.

One day when I was on the L.A. streetcar—I can always remember days when an idea hit me—I thought of turning this over and instead of counting in here, (diagramming), putting a transfer in here, and immediately dropping the Y and the denominator up to the numerator. So now I got the rate out as Z =Y the rate in, the constant. Then I went through three wonderful weeks of finding that I could do more and more and more with this. An interesting thing was that this is the integrator, and the problem that this was to be applied to was a set of sinusoids almost exactly like the tide predicting thing. Originally it was an inverted Incremental Slope Computer, and I gradually found I could do marvels with it. It was very, very exciting. Little by little we could expand it to get exponentials and sines and cosines, everything we wanted. Finally it dawned on me that it might be a differential analyzer, which I'd never looked into.

At that time anyone who was the digital field really looked his nose at anything that was analog, or at anyone reading the material. I dug it all out and it proved to be invaluable because there was a one-to-one correspondence between that mechanical integrator and this, and one could right away use all of Bush and Hartree's connections, work, techniques, function generating and everything. It advanced it a very great deal although it was kind of disappointing to find out that it was a differential analyzer after all. That's the way that popped up. Finally Eric asked permission to try one of these. I might say that the reception was extremely lukewarm except from two guys in the computer group who were interested in the project and very much impressed. They were especially impressed with the idea of an airborne computer. We ran around like a bunch of cheerful nuts saying it's got to be in the air, can get it in the air. Everyone read articles on ENIAC, which about that time was moved to Aberdeen and went out of commission for some time.

MAPSTONE:

You can't move a computer, let alone fly it.

STEELE:

I moved on a bit and gave some thought to serializing, which one had to do to get it in the air. I was reading Charles Pliny's Candida, and I noticed DIDA stood for digital analyzer; also everybody talked about these acoustical lines as canning; canning material. I thought I'd borrow the word from the play to describe the machine. Everyone started calling it Candida (as in die) right away, and I never did find out the right way to pronounce it.

The acoustic lines didn't come along very fast, so Dick Sprague started a slightly different principal in the integrator that saved quite a lot of diodes and tubes at the time. It was a twenty-four [place integrator, which would be forty-eight flip-flops per integrator, and of course we had to have eighteen; the magic number because of Bush. Dick and some technician built up two of these rays and later on eighteen were farmed out, to Hewlitt Packard to assemble. In the meantime the serial one came along faster. Although DIDA got started a year earlier, MADDIDA finally overran it. DIDA never was finished. It was wired up but I don't think it was checked out.

Through this period, there was a much larger program I was trying to get underway, which I called the Automatic Attack, and the airborne computer was just a piece of it. The general idea was that there would be an automatic colonel which would be a flight of these at one site; an automatic general; and I think I had a few words about an automatic president. In those days we had about a half-hour warning on a possible bombing raid, and we had to get these things off the ground if we didn't want them wiped out. The attack had to be fully planned. That's the problem people still have. Because there is almost no warning at all, we must can our whole thinking in advance. It seemed mandatory. In fact, within the computer group, we got everybody fired up and thinking how we were going to use the darn thing. No one had given any thought on how to use the missile, or any thought of putting it together.

The star tracking group had worked up a sort of greenhouse on top of the missile, which was a series of special glass panes that the tracker could look out of conveniently. I believe originally they thought of a hemisphere about four feet in diameter.

No one had ever talked to the aeronautical engineers who were designing the frame, so I used to go over to talk to them to see what the problem was. These guys never heard that this was going on and they were thunderstruck because to them the all important job, and it really was a big one, was to get the most range out of a moderate sized jet airframe. Jets had poor ranges in those days, and they especially wanted to have it streamlined and make very little thing count. They wanted a very optimum flight pattern, which is essentially a cruise rise kind of deal, to fly the programmed altitude to get the most out of your fuel. At the same time, the bubble people didn't want to put a vertical accelerometer in so they were assuming a level flight.

No one had given any thought to how accurate the maps were, or such questions as "what if the enemy bombs these things on the ground?" or, to get around that, if we are goin g to get them off early, how are we going to get enough automatic control to bring them back in case we were wrong? Even today these air breathers would be an advantage in that particular way if things were that tight, or if people were antsy. We used to think that we could always respond to what we fancied might be an attack by getting a counter attack off the ground, in the air and underway and then bring it back. The air breather conceivably could be landed and used, so the whole flight wouldn't be lost. On the other hand, you'd think that one through because the enemy might track you on that. Anyway, it took a whole lot of thinking.

One of these guys who worked with me for awhile was Herman Kahn, and that's where Herman got started on this business of planning weapons. He and Irv Reed worked together as mathematicians. When Herman went over to RAND he, more than anyone else, got RAND geared up to thinking about to really use these things.

The program that was sort of heading up and pulling together in the Northrop computer group was one that involved the whole nation. It involved all the ground support, it involved the entire system and it almost took place automatically because of the question, "Why do you want a computer in the air?" If you took the thing they had in mind then, you would just put a simple program on the ground, but as soon as you wanted flexibility, which would be the next problem, there's your reason.

But people wouldn't take the next step mentally and it was hard to sell them on an airborne computer.

Anyway, the whole computer program went over like a lead balloon, believe me. Opposition around it was pretty great. People thought we were way out of place, and actually we were. We began to get cocky; we were all fired up and nobody else was. We were considered the odd group or something. The opposition began to get gradually stronger and stronger, people were just dragging their feet more and more. It was very easy to see what was really happening, because as far as the star program was concerned, the Air Force and the Northrop Company looked upon this as air frame with an improved autopilot. Neither the people actually doing the procuring or those with the final say, had given much thought to this guidance system; it was something that had just come along and would be tacked on. Not did they think that you had to integrate the airplane with the guidance system, it would be a piece of equipment like radar. As soon as the plane got off the boards and looked pretty good, they got a prototype which worked, and they decided to produce twenty-four of them. Well, the first one hadn't been invented. That's how these things really happen. This gets us to 1946-1947 and maybe up to 1948.

Now I'll have to back track a little bit. In the meantime, Stan Frankel had been a consultant to Northrop. I have all this second hand and Stan can tell you about it but I'm quite certain, knowing his ability, that he had done the analysis equations. I was told he had by a guy who knew him. He had come up with a fact, which the Germans had already discovered in the B-2, which is an unalterable law of physics. It falls our quite simply. If you have a plain pendulum or anything that tells you which way is supposed to be down, let's say the earth, and you have a stable platform that's tracking the stars, you can either be on the course you think you are, or you can be oscillating around it in an 84-minute oscillation. The oscillation of 84-minutes comes in without regard to the instrument itself; it is related to the radius of the earth and its gravitational consent. If this weren't so, there's been something defective in primary physics. Stan was the first to write the differential equations and that pulled it into shape. This meant that the airborne problem was much more severe than they had thought. It's my impression that Stan was shoved out of the way, very unfortunately, and there's no question that Frank Bell took glorious personal credit for these equations—he was doing ordinary navigation and trigonometric geometry—so it was astonishing to me when he came up with this. This discovery meant modifying the system rather considerably, at least, a lot of conceptual things. The fact that Northrop brought this to the attention of the Air Force helped a good deal.

The Air Force navigational group at Wright Field thought there something funny about this. They'd never seen an 84-minute oscillation doing celestial navigation, so this became a rather furious controversy. Therefore, it was decided that we should throw together a batch of hardware, get it in the air somehow, and prove that this law of physics existed. This is the way many of these programs really get underway. Someone says, "I believe you but my boss won't believe me. If we could just get something in the air within just a few months. It doesn't matter how it looks or what you do, don't waste any time on anything that you can buy off the shelf, if you can stick that in and just prove the point, then you're all set, everyone's sold and ready to go." This way the program was dragged on into setting everything aside and building a test unit that could be airborne to prove this point of physics.

As usually happens, it took about two years to do this; two knock-down drag-out years of everyone's time. We picked up anything that could work. We used sixty cycle equipment all the way through, nothing was designed that could be bought. Some of the weirdest Kluges were thrown together just to get the thing up and get it tested. Then we kept having to go back and do a little and a little better because we didn't realize in the beginning how stiff the requirements are to get anything airborne. Somebody has to fly along with it. It took time and a lot of people, and it took everyone away from the job of actually manning the SNARK guidance system.

Nearly everyone was doing his thinking at home. I know that's where I laid out most of the MADDIDA, laid out the whole wave range and laid out the ground computer to fill it. I had a whole series of these things laid out at home because of the work we were doing on this airborne tape.

By that time Harold Sarkissian had been hired. He originally worked on the tape, and there was Carl Isborne under him who was good at magnetic.

The missile took eight hours to fly its extreme range. At the time the tape recorders one could buy were running at about eight inches a second; eight inches a second for eight hours required a very large roll.

MAPSTONE:

Was this magnetic tape or were you still into wire?

STEELE:

No, finally the tapes became available. We ran about a half hour's direct function on that wire once. The wire had to be synchronized, and to show you how tough this was to throw into the air instantly, this incremental function had to be played back to a very, very high accuracy in time. It had to be much more accurate than the drive mechanism of the wire, which will slip half of one percent of something like that. If it is necessary to record a sine wave on the wire, servo the sine wave against a crystal clock and drop the impulses into the dips in the sine wave for counting, then this all had to be started within a hundredth-of-a-second of real sidereal time. So, we had to put together receivers for WWB (world wide broadcasting?). Somebody got us a diversified receiver for 05 channels. We had a scheme of starting against WWB, and cranking in the difference. What you had to do was watch the clock very carefully, and hope that it came in very, very close. When we got the time beep from WWB, these would run into the digital counters. For each second missed you had to turn a crank sixty times to recover sixty cycles; a few seconds would mean five or six hundred turns.

Fortunately, we began to get something better to work with. Finally I was able to run down some movie tape with magnetic coating. They used it for playing recording sound and magnetized sound track. Because it was socket punches, it was possible to have a synchronous motor driving the sprocket, which was much nicer. Then we got several channels on the tape. Carl Isborne was working for Sarkissian—Carl and Harold were very good friends, I think they worked together before or in the army—and Carl kept working up better and better heads. Finally he got the tape down till we got excellent pulses running at a half inch a second. The one he had in the lab was doing a quarter inch a second. That began to get the things down to a usable point.

Al Wolfe worked on the digital clock; it actually had seconds, minutes and hours for direct reading as a clock. All we needed was a binary counter, and to interpolate it, but I was getting very cynical. I said, "I bet if we lay this out to look like a clock and put it on the front of the machine, we'll get more attention for this than for the computer." Sure enough, we did. Every time they'd start up the missile, everything would be running with the lights flashing and everyone would gather around. It justified our cynicism, but it made us kind of mad. As far as I know, that was the first digital clock. Al wrote an article on it which was published.

We finally settled on movie-magnetized sprocket-punched tape and we got that in the air. All this took a lot of time. The program would roll on and finally the computer group got up to thirty-four people.

MAPSTONE:

Predominantly working on this tape.

STEELE:

Yes. One set of people were doing the guidance equations, they were mathematicians primarily, and a little more than half the group were in the lab. To make short story long, we got it in the air and it did work.

MAPSTONE:

What did it fly in?

STEELE:

The old black widow, the old night fighter. It was spliced into that and four men had to go along to make it work. Phil Taylor was in charge of that and it was far from being automatic. To show you who these things gradually grow up, they began to take night flights about once month when they were squared away, from L.A. and Phoenix. The first time they showed up for the flight in their business suits. I happen to know that country since I worked on a ranch over there, so the next day I went around and gave them a stern lecture about what in heck would happen to them if they bailed out in winter in the desert. They could have a hundred miles to walk. They ended up getting some flying suits and equipment and one of them even carried a gun.

The problems with the tests were about over. No one was interested in this 84-minute oscillation at all. What always happens when you try to prove scientific points by building hardware is that people who originally didn't believe it, convinced themselves it must be true long before they can build the hardware because every physicist and mathematician who looks at it agrees. The people who were going to be dragged to see it immediately already assumed it was going to work and weren't interested.

During one of these tests the thing flew right over Phoenix, on the button. Once they got the plane in the air, got the stars picked, and twiddled the gauge on the trackers occasionally it ran automatically from L.A. to Phoenix. The Air Force decided then that it was ready to put into production. Of course, they weren't aware of the fact that the 84-minute oscillation occurs along the track as well as left and right; the forward-backward errors don't show on the graph. It had flown over the center at the time. At any rate, this stimulated the production of twenty-four of these tape devices, and the company genuinely believed the problem was done. Everyone acted as if it was ready to go. The big mistake they made, and I put up a big fight to head off with only partial success because no one could see it, they hired people into the program to build these twenty-four units; they began to build up under the inventing program. They kept hiring people in and the program began to swell and swell and get bigger and bigger. It turned out that the people like Ralph Olstregin with the daylight tracker, and Phil Taylor with the optical system, were not the right guys to build twenty-four units because more than anyone else in the world, they were aware that the first one hadn't been invented. There was a difference between having someone say, "Take the first thing you lay you hands on, shove it in the plane, we don't care how it looks." And, "Oh, it's all done, let's manufacture it."

At the same time, everyone along the way had put in a lot of time. Ralph Olstregin did daylight tracking in his garage; he had a really good setup there. That was the first daylight tracker in the world. Everyone was working hard at home and had a good idea of what to do next. It wouldn't hurt to build up to thirty-four, but they should have trained the next set of people to carry on. In the process of building up, they simply ousted all of the people who could invent the next one to carry on; who could get the next one going. These people should have been moved into a separate section. That was the chief error. No one responded because it wasn't done in the aeronautical field at that time. As a matter of politics, or a matter of salesmanship, or something, no one wanted to talk about the fact that this first one wasn't the final thing. The Air Force was in the habit of going out and purchasing something, a prototype would be made, it would be tested, modified a little, and that's it, you can put in production. Nobody ever dreamed or saying, "This really isn't it, we've got another coming along and maybe a third one."

MAPSTONE:

What did you do, write a memo or talk to people? Because you knew this was not the final product?

STEELE:

It was after I left after talking to General Seville and thinking it over that what actually had happened began to make sense. At the time, everybody would get frightfully upset, because everyone thinks it's their fault somehow, something is the matter with me. Ralph Olstregin was finally shoved out of his labs and made an advisor. He felt terrible; he thought he was a failure. He had been running a group of three or four men getting out these wonders; the next thing he knew he had a lab of thirty or forty men. He didn't make an effort to run them; he just went on doing the work so he was a poor administrator of such a big lab. They got somebody else and moved him out of the labs.

This was how MADDIDA got started; feelings were running very high and they reorganized and decided to put the labs under a different administration. All those who had the title of engineer would have desks upstairs and would work on paper; those who were technicians were supposed to work downstairs. All the technicians were to be put in a common pool. This meant all of those out of the servo group, out of the gyro group, out of the star tracker group, some optical technicians; all would be put in the technician pool. The engineers would write a work order of what they wanted, get to the pool and to which ever technician was handy and say, "Here boy, go and wire this counter."

Of course everyone knew this wasn't going to work at all. Not only that, there were these little groups working together all the time and the fact that some of them were called technicians and others engineers didn't make any difference in what was going on. Right in the middle of when they were trying to enforce this system, it got so bad I moved my desk downstairs into the lab. I said," That's all right, just make me a lab man, I'm not that keen about an upstairs job, but I do have to be down here." Also, it would have been preposterous to put some of the technicians off on different kinds of jobs. Some of them, you see, were better than engineers.

It got worse and worse. There was space all over that area but we just couldn't get it. No matter what we wanted, they wouldn't do it. We thought this was personal after awhile, maybe it was. We wanted a place where we could have some desks and blackboards, so that we could design the computer on the blackboard. And we wanted the technician to be able to have a desk there with his odds and ends, so he could go over and sit down and check off some of the latest catalogs and then go back to soldering iron. No one could see this at all; they wanted to do it a totally different way. The guy that was to head it all up was an ex-draftsman.

We finally got so mad, swiped some plywood, and brought it in to build ourselves an office. This really stirred up a tremendous ruckus. At least we had an office. Then we carted our desks down from upstairs, grabbed the blackboard and we were hard at work. It looked terrible with rough plywood sheets propped up and nailed.

MAPSTONE:

They didn't at least knock them down.

STEELE:

No, but there was no reasons to. There was a lot of lab space and all we wanted was to work together. However, it didn't fit into the organization at all. That's where we hacked out that MADDIDA design. I had originally done what I called the tactical at home, and we all got together and did the Boolean equations. That was the first computer designed by Boolean Algebra.

MAPSTONE:

How did you get the idea of the Boolean equations?

STEELE:

I ran into it by readin a reference to it in Stibitz. He used it on relays and mentioned that Claude Shannon had done the original application of relays. I don't think there was anything earlier than that. I fiddled around for several weeks trying to apply this to the electronics in a way that could work. Finally, driving home one night, it suddenly dawned on me what the trouble was. In those days everyone was trying to use pulses, we used to call this the Eastern school of computers, kind of radar oriented. They used pulse and no pulse for one to zero. Well, it was evident that no pulse was a lack of information rather than being a second state and this was the reason, as long as you did it that way, the logic didn't do you a bit of good. You can make little tables of what you want, but you can figure out quicker ways. Somehow it wouldn't pull together because every place where you need the prime, the absence of a pulse will not do something for you. Therefore, you had to work up a special circuit where the absence of a pulse gated through a clock pulse to do something. These special circuits overrode what was going on to such a degree that you couldn't see what was happening.

I decided to try high and low voltages. We were living down at Surfside and one night I sat down and tried the diodes on that; it all worked marvelously. The largest single layout I ever tried I did on the first night. In October/Nov, 1947 I got a piece of shelf lining paper and started to lay out the diodes for a one stroke multiplier. Presently, I got far enough along that I could calculate that there would be 8,000 diodes. I was somewhat depressed that it didn't turn out a little better than that.

There was a period of about eight months when I couldn't interest anybody at all. It's easy enough to see why an electronics man would turn up his nose at it. Up to that time everything in electronics had been special circuits, and to such a degree, that if you had decided you were going to add, you could work up an adder to see if it was possible to add. Then, if you decided you'd also like to subtract, you had to reword it to see if it really was possible to subtract. Then the question would come up of whether you could combine these two. Generally the answer was its better not to mix circuits like that. It was against all sound ethics.

I fiddled with it for eight months before I could get anyone to pay attention. Al Williams built up the first. He had a little time available so I laid out an up-down counter and had him wire that. I'll be a son of a gun if it didn't go into operation immediately which never happens with pulse circuits. Everyone got quite enthusiastic. Then there was another bit of finagling around before I found the two logic steps it takes to do one complete step of logic. Actually, that isn't too well understood yet. There are as many people who goof on that as don't. Every switching system principal required two distinct steps. Most switching systems are designed to do it in one. The reason is simply the ambiguity. If you have a set of flip-flops that are controlling themselves out of the net, they both select the next value and then move the value into themselves. If they're trying to do this in one step, they're both selecting the next value and changing themselves at the same time. Whichever beats the other has altered the combination for all the rest and immediately goofs up the thing. The reason it can always be compensated is that there's always a little bit of local memory, a little residue in every flip-flop. You can always tell when you've got a circuit where the extra step is missing, because the clock that you use has to be of a very precise width. If it's too wide or too narrow, or if the amplitude is too high or too low, you have troubles. It's very easy to get two steps by using a capacitator gate. That worked out, and we began to design the whole computer in Boolean Algebra. Apparently, this got started on the last post and made headway although it constantly had set backs. The Eastern approach has invariably been to maintain special circuits, which is what's going on in the integrated circuit field today. As an example of why you shouldn't dot it, if you simply look at the computer as a set of flip-flops which select and change themselves, then you're using too many combinations. If you add one more flip-flop to the system, it will do twice as many things, not just one more thing; but if you have a bunch of special circuits like an adder and a subtracter and enable flip-flops for each thing, and then a sequence flip=flop to go to the next thing, each one of these has one assignment and it's very easy to get scores of them doing very little work. If you put them all into one net then you can design everything. In fact, since then I pulled everything into the logic; the push buttons, the front panel switches, all sorts of odds and ends, servos, inputs, outputs, everything. On this basis, you don't have any interfaces; the interface idea disappears, and the inputs and the outputs are part of the net. You know how much stuff is wasted on interface these days.

MAPSTONE:
Is it still a West Coast/East coast dilemma?

STEELE:

I haven't followed it for quite awhile, but the Boolean algebra done on the MAADIDA, the thing was wired into the equations. A curious thing happened. About the time MADDIDA was designed and reduced to the equations, the opposition really began to get intense to the whole idea of a computer. My opinion at the time was that it was an attempt to assign credit for all of the SNARK Program to people who had nothing to do with it. Suddenly, overnight, the computer group was out of business. There wasn't any; parts of it moved into other groups.

To show some of the opposition, I had originally put in as memo hoping that we would get an airborne computer, and saying that we could put roughly thirty integrators in the air for fifty flip-flops; a digital differential analyzer, and that we could probably do an airborne guidance with it, at the time. That memo was taken by Frank Bell and shown to a computer authority in the East; I never found out who. They were paid to evaluate it. Their evaluation was that there wasn't ay such thing as a digital differential analyzer, and that if there was thirty integrators would use 2,000 flip-flops.

MAPSTONE:

You have no idea who made this analysis?

STEELE:

No, I don't, although I have suspicions. I heard from the secretary that such an analysis existed. In fact, the response said, whoever had written the original memo apparently didn't know much about computers because it was impossible. That didn't help, although at the time I didn't know what had happened.

In the meantime we let the contract to Eckert and Mauchly, and that was a good idea too. I always thought they (Northrop or Eckert and Mauchly) got kind of a bum deal out of that. In a way it was their own fault, except they didn't know any better and they did wait too long. Yet, I think they were coerced to make it low. On BINAC they bid $80,000 but with lots of overruns it came to about $120,000. They built two of them and every attempt was made to make it plausibly airborne, although it was evident early in the game that it wasn't going to be. A great ruckus came about finally. BINAC cropped up as the first running GP and Eckert and Mauchly started an agitation to hang onto it. They were certainly justified because Northup wasn't ready to use it at all, there wasn't anybody who could have kept it running and serviced. The computer group had its own computers going and we didn't really want to get involved with it. E and M should have kept it and put it to work. They knew how to run it and they would have kept it going.

As soon as Bell heard that they were giving him some troubles, he immediately grabbed the machine and had it shipped out. It lay in crates for a long time after it arrived. It had mercury memories, which Pres. Eckert had done such a good job on. I think he got them up to about six megacycles. Later on when Raytheon started, they had to drop back to about one-tenth of that. Did you ever talk to Dick Baker?

MAPSTONE:

I can't talk to Dick Baker. Unfortunately, he's locked into some legal hassles as a witness.

STEELE:

That's just Dick. Dick was a fantastic worrier and has probably gotten worse. You know, every once in awhile, and there are several in every company, you run into a guy who is paranoid, he is of the opinion that everything around is a sort of conspiracy. He has a very high opinion of the capability of management in one respect; he thinks they plan everything, but he thinks they have some subtle reason why things go wrong. He's always trying to figure out how they make money on this or how they do things and they constantly outmaneuver themselves. Dick was a great, great worrier and something like a suit would drive him up the wall.

MAPSTONE:

When I talked to him he was very nervous about the whole thing.

STEELE:

Dick was a good electronic technician, but an extraordinary worrier. He inherited the BINAC for a time; at least he was the technician who went back and was trained by Eckert and Mauchly. The poor guy was always of the opinion that the whole onus wasn't set up for use, was going to collapse on him and that he was going to be totally ruined for life. He genuinely worried about that.

When these crates came with these mercury lines—it was monatomic mercury; they had especially gotten the isotopes out of it so the transmission rate would very standard—some of the clods in the computer group got themselves a beaker of mercury and poured it on the ground by the crates. It took them a long time to convince him it was a gag; he thought they were just trying to make it easier for him. That was about all that happened on BINAC for a number of months. Finally it got going again.

MAPSTONE:

Did it ever really operate?

STEELE:

Yes, it might have. I don't know the true story but Dick would know. The company of course was under pressure to make it work and after a lot of time went by he got to do some. It never did work well for Eckert and Mauchly, but since they had two machines, one checking the other, they would feel their way through quite a lot of computation.

Ike Auerbach worked on the memory at E and M too. They could have done a lot with it, but they ran out of money and the computer was gone. It was all too bad. All this was coming to a head at the same time as MADDIDA. The Air Force had declassified the BINAC; they had ruled that although its applications could not be discussed nor could one indicate in any way what you were going to do with it, the way it went about solving equations need not be classified. Consequently, I arranged with Aiken to give a paper on MADDIDA at a conference he was holding.

MAPSTONE:

Howard Aiken?

STEELE:

Yes. I wrote up the paper, and about that time the computer group suddenly dissolved. I was informed that Bell was bringing security violations proceeding against me, so I cancelled the paper. Bell was finally fired so he never made that one stick.

MAPSTONE:

What was the security violation?

STEELE:

The fact that I was going to give a paper. Originally the company had okayed it, so that didn't make me too happy.

MAPSTONE:

Did anybody give the paper?

STEELE:

No, and then the original of it was stolen. I never did find out what happened to it; it as just swiped from me.

MAPSTONE: Sounds like the dark ages. . .

STEELE:
All of a sudden Dick Sprague, Don Eckdahl, Harold Sarkissian and myself were booted clear off the SNARK program and put under Ohlinger. The reason that came about in part is that I'd been trying to push the idea to Northrop that they should set aside a group, or some kind of group, to go on with

the design of the SNARK to get the next one. I knew pretty well which guys on the program were worth anything. If they'd pick about the twenty or thirty of those, they'd have been all set, because these were the people who knew how to do the next one. Not knowing anyone else around the company to plug for, I plugged for Ohlinger to head up the group. Supposedly he was an atomic scientist. It later turned out that he didn't have a background in science particularly. At any rate, he was willing to help. Sp suddenly, bang, we were parked under him and we were supposed to help him handle the BINAC when it came.

MAPSTONE:

You were taken off MADDIDA?

STEELE:

Yes, completely and it left Isborne and Wolfe to finish it up. The miracle was that they wired up the thing, plugged in the diodes and with little clips and it went right into operation. There were a couple of funny things that happened, and it took about two or three days of talking to the technicians over the phone trying to deduce what was wrong. In short order, it simply started and ran. Just as the time we had all been thrown off the project, the computer group had been dissolved, the fellows who were working on it had moved over into another group and supposedly were being put this pool, an Air Force Colonel and Major came through to have a conference at Northrop about the long range possibilities of the SNARK. In other words, what did they envisage would be possible ion ten years?

Both of these men had been trained at MIT and knew Bush's differential analyzer and they'd actually run problems on it as part of their training. After they'd talked upstairs and been given the usual treatment, somebody brought up the fact of the possibility of an airborne computer. The colonel was quite disturbed. He said, "I don't know how the notion of an airborne computer came up, and something like that is years off." Whoever it was, and I never found out who, said, "Why don't you come down and have a look?" He took them down and showed them the whole thing running around on the teacart. It just happened by great coincidence. The Air Force people said, "What is it?" "It's a differential analyzer." "What do you mean?" "It's a digital differentia analyzer." "How many integrators?" "Eighteen." "That's as big as Bush's."

All of a sudden we were escorted royally back onto the project. About a week later Charles Lindbergh from the Lindbergh committee came through, he was working for the Pentagon; Jimmy Doolittle came through and Jack Northrop came over there, in fact, all the company big wheels came over to hear about this computer. I used to suffer from stage fright when I was a kid and I had to get up and give this speech before all these people. All of the Northup personnel that attended sat there and glowered at me. I can't remember what I said at all, but we were back in operation temporarily. The rest of the time we were there, we weren't able to get a desk, or a place to sit down. We had a room and we had the computer in it finally. We had the same chalk board that we liberated upstairs. I hope I don't have all these things out of order, they happened so quickly.

I had several times talked to Jack Northrop on the problem of getting the MADDIDA patented, and it had a very severe need for this, but I thought that they ought to know that they had valuable patents and that they ought to get them. The head of the patent department was dragged into this and he advised Northrop that it wasn't patentable.

MAPSTONE:

Who was this?

STEELE:

Colonel Metcalfe. His first advice was something to that effect, which set it back a bit. Then Northrop sought the advice of Frank Bell and some of the other people on the project, and their advice was that all kinds of valuable discoveries had been made on this project, and that they certainly ought to take every pain to patent them. The computer group thought too highly of itself; theirs was just one among thirty things that ought to be patented. It gets pretty hard to keep pushing on people who felt this way, but I still thought they ought to patent it and that's where John Matlago came in. Finally, I asked if they wouldn't get a younger engineer or somebody to do something about the patent end of it, and send him over and let me train him. I didn't see any other way to get it done. John came over and got assigned to computer work only.

MAPSTONE:

Was John a patent engineer?

STEELE:

He'd been a regular engineer of some sort, and then Metcalfe hired him for this job. I think he had some background, and was working to be a patent agent by going to night school. That was his interest. He came over and we took care of filling him in on the computer. Colonel Metcalfe had given him strong advice and it took quite awhile to shake him off of it. The advice went this way, "All right, the way to go about it is to have them show you what happens on the way through before you push the switch." Of course, you can't think from the front panel, you've got to start from the computer and think outward. We finally got him squared away, and he reached the point where he could put the patents together, which was no easy matter. All of that was just coming to a head at that point.

MAPSTONE:

What was Jack Northrop's reaction to the computer and what you were doing? Did you get any reaction from him personally?

STEELE:

Yes, I was always very impressed by him. I don't think he understood the problem. I tried not to go to him to complain, I always wanted to give him some information. I don't think he could figure out what I wanted because I never was asking for something for myself, particularly, I was just trying to inform him. But we always got along well.

STEELE:

I had gone to Jack Northrop previously about the patent problem, and I went to him again about MADDIDA. There wasn't a great deal of interest in it and he couldn't decide what to do. He got such heavily conflicting reports. He talked to some people that told him it wasn't worth anything; others thought highly of it.

MAPSTONE:

Yet the two military people had reported highly and that got you put back onto the project.

STEELE:

Yes, that's right.

MAPSTONE:

Do you remember the name of the two military personnel?

STEELE:

I think I will remember them as we go on. I asked Northrop if he would get a consultant to appraise the machines so we could go on that basis. I was suggesting that he undertake the manufacture. I didn't want to do it myself, but I thought it might be a good idea. I proposed once that they set up three different computer activities. The one we had using the BINAC and the IBM set-up—Bill Woodbury helped get a computation center going—get a commercial manufacturer outlet going, and at the same time keep going on the SNARK and digitalizing it. I asked if he'd get an evaluator. He thought that a very workable suggestion and asked who would be the best person on the country. I said, the best man in the country in computers was John von Neumann. He called him up and asked if he'd be available to evaluate a computer. The way he described it sounded to von Neumann like a small analog computer built on a little wooden tea cart with wheels with a power supply in the bottom and feed in the top. He said he couldn't leave during the school year, but why didn't we send it down there and he could run it in the Princeton labs.

We got the carpentry department to work up a case for MADDIDA and got ready to ship it east. Then he called and said that he didn't think it was ethical to use the Princeton labs for his own consulting work, and would we mind demonstrating it in his basement. Since the power outlet required only 30 amps, the normal fuse would be sufficient, which again gave him the idea of a very small device. It seemed plausible, so I said, "We can try it." We were ready to ship it when he called again and said that his wife didn't like the idea at all, and she wouldn't let us near the place. We said, "Well, we'll find some place around there," and we sent it to the hotel. Then I couldn't get the company to send technicians who knew the circuitry code. Al Wolfe knew all the flip-flop circuitry. They decided they'd send Irving Reed because he was a mathematician and could talk to John von Neumann. So Reed, Eckdahl and Sprague and I checked in at the Princeton Inn. Our room was on the top floor, I think it was the fourth floor. It was bitter cold; it must have been twenty below zero. It was January 1950.

MAPSTONE:

He wrote the letter in March, 1950.

STEELE:

Okay. At any rate, it was awfully cold. The man who serviced the elevators said we couldn't possibly plug the machine into the wall because it was fused for about ten amps; a tiny bed lamp was about all it could take. He loaned us a gigantic extension cord, it was the meatiest thing I've ever seen, and we stuck that out the window down four stories across the whole front of the hotel and plugged it in, in the basement. Then we couldn't shut the window. We darn near froze.

We got MADDIDA plugged in but she didn't work. We were afraid of that. We stayed up all night long and finally traced the problem down. You see, all of us were fairly rusty on the details; we knew theory, but not the details or where to find things. We poked around and finally I isolated it as being a rectifier in the power supply. We were supposed to demonstrate it, or talk to von Neumann at noon. Reed took off in a taxi running around the Princeton area to see if he could locate this rather exotic electronic part at an electronic store or outlet, which of course he had no chance. In the meantime Eckdahl and I discovered it was merely a colder solder joint, so we re-soldered the joint and the machine started to run. We demonstrated it for John. We got off the J zero Bessel function for him, and it gave the right answer. He was very pleased and spent a couple of days going over this theory and so on, and wrote the letter.

MAPSTONE:

Did he play with the machine himself?

STEELE:

No, we punched the buttons. He stood there and indicated what he wanted entered and he would check the scope. It was wide open, if you punched the wrong place you punched the wire and it was completely spidered. We got a good write-up on that.

In the meantime things began to really go from bad to worse. I'd recommended two things to Northrop. There was a conference on small computers being held at Rutgers in the East. I'd gotten some tentative noises from Northrop that they would consider manufacturing MADDIDA commercially, so this would be an ideal place to demonstrate it. Ike Auerbach was on the conference committee and he got me on the program to give an un-posted paper. We really couldn't get permission from Northrop to do this; we couldn't get anyone to say yes or no, so we took it up there anyway and demonstrated it anyway. Everyone was really beginning to get mad.

We stuck it in a big gymnasium and there was a huge IBM set-up down the way. Apparently they had a standard demonstration with the big back drop stand-ups that went around, a big machine set up in front, a piece of carpeting on the floor, a railing along the front, and a very neat sign that said, "Thing." We had a sign painter do a little sign that said, "Relax", and hung it on the wall. It was a terrible looking thing. Then we had to take turns watching it. I never have figured out just what I gave in the way of a paper. At four in the morning, after we got installed, I started to work up the paper I gave there. It was never written.

MAPSTONE:

I think this is an abstract of the paper.

STEELE:

I must have written that later. I must have sent it back to them. Do you suppose I did write up something and leave it there?

MAPSTONE:

You might have, because the abstract was in the proceedings.

STEELE:

I knew they were going to get out a proceeding. I'll have to read that later, I've forgotten all about it. It's possible, because I did try and make notes. I was getting pretty blurry by that time. In fact, when we weren't setting up, debugging or running the computer, we were drinking beer. That was quite a time.

MAPSTONE:

What was the reaction?

STEELE:

A general brush-off from everybody, and yet there was a great deal of excitement.

MAPSTONE:

General brush-off for what reasons?

STEELE:

Because we really hadn't warned anybody, we hadn't tied in with anybody, we cam in cold. One young man came around, a very earnest sort, who was talking to Irv Reed when he was looking after the machine. This young gentleman was very indignant. He had just finished a report, a summary of all the computer efforts in the country, and he had never heard of this. Apparently, he got quite upset and almost with tears in his voice started accusing us of trying to get away with something. He was very belligerent about it all. We were quite amazed and we couldn't figure out what he was talking about. No one knew about MADDIDA or what it was. I don't think anyone liked the way we were behaving. Putting that sign up took the dignity out of the meetings.

MAPSTONE:

Did the machine itself have an impact?

STEELE:

Yes, it ran. We would show it to people. However, none of the big names in the computer field would pay any attention. Aiken wouldn't look at it, neither would Stibitz. Some of the younger people would come around and ask what it was and get an explanation; the others wouldn't.

MAPSTONE:

It's interesting that Stibitz wouldn't, because if he had come and listened he would have found some things that he'd done in his machine.

STEELE:

They may have been nervous, but they wouldn't come around the computer itself. The reception was very good outside of that. It was so much better than at Northrop. Someone said, "It's as if you've been raised in Wyoming all your life and the wind never ceases blowing so you walk around pitched forward. One day the wind ceases to blow and you fall on your face." We had so much opposition at Northrop that we didn't know what to say when people agreed to cooperate.

During the lunch hour, I met Eldred Nelson and I showed him the Boolean algebra, how to apply it, how to set it up. He took that over to Hughes and started the MX 1179 program. That was one direct thing that we did help get started.

The next thing we arranged on our own was to show it at the Pentagon. They had an educational program once a week run by some colonel. Special things were brought in that would be of interest to the military men themselves, mostly high level people. Somehow we got on that program, and we crated up the MADDIDA and sent it down to Washington. We went around the back of the Pentagon and wheeled it up a little ramp and finally got it into the colonel's office. We dragged in our scoped and tools and he said, "My, I don't think you can leave it here, it might be stolen." It turned out this is true. Things are stolen all over the Pentagon. We had to lock everything up. We finally found a little closet that we could lock it in. They leave all the rooms unlocked at night, and apparently the janitors liberate everything they can.

Just at the time we were to go on the program, Northrop flew back two of our staunchest enemies to take charge of the presentation. George Fenn was one of them. He was the fellow that was giving us trouble. He was very competent, which was why he was so troublesome. He was the only one that was really first class opposition. These two were sent to Washington and they were going to give the presentation instead. Just before the meeting Irv told them if either of them got up he'd punch them in the nose before the assembled people. They didn't respond. The colonel heard this and was flabbergasted.

We presented it. I was told to give a very brief five minute summary of what I was talking about, because the top brass would drop in for just a few minutes but they could never stay. They'd get the summary first and then get up and go. I was told not to mind if the top brass got up in a few minutes and left because their assistants and other people would be staying. I went straight into Automatic Attack. I talked for an hour-and-a-half and nobody left. I tried to get the point across that the airborne computer was here, and here was one of the sizes to be; also the fact that missiles were going to have to get automatic response. That reception was very good.

We also arranged to stop it off at Wright Field on the way back, and take it into the air labs. Colonel Boden was there at the time. The first day we took it in and talked to people about it, and explained what it was about. I don't recall that we ran it. It's pretty hard for people to see whether it's running anything or not. Everyone was extraordinarily sympathetic. The next day when we went in, everyone was belligerent. In no uncertain terms they handed us equations and actually defied us to solve them. I found out, a year or so later, that there had been a major scene after we left. A professor from some university had just finished selling Wright Field on funding him to build a mechanical differential analyzer. He'd been shown the MADDIDA and pronounced it an out-and-out fake and a fraud. He was convincing enough so everyone got a little hostile at the thought of being taken.

The equation they gave us was a tough one. It was an equation that intelligence had brought out of Germany during the war. In fact, they were working on a magnetic cannon. The idea was a series of solenoids accelerating an iron slug at high enough speed. The equation was highly non-linear and quite a mess. This was coded up and run on one of the big machines.

MAPSTONE:

Was it a Mark I?

STEELE:

Probably was. It took the machine a couple of weeks to code it and get it through the machine and the results indicated that the cannon couldn't work. At any rate, the equation was available. Doggone it, it happened that I had worked up a little trick with the machine based on transferring the new value of Y from the integrator. Eckdahl didn't know about this, so when we wired it up he transferred the old value of Y, which didn't make any difference if you compensated it right, but I couldn't play this trick which enabled me to get two DX connections. So here this thing was stuck and couldn't use this hard servoing trick to get to the X inputs. This equation had about three or four of them so I had to improvise. Irv and I worked for a long time without sleep in the hotel room. Finally we servoed it with an equational function and just hoped against hope that it would work. It does servo that way, bit how well and what would happen to the equation, we didn't know. We got closed and it took every integrator in the machine; oh there may have been one left. The first time we ran it through it turned out that Harold was reading the table backwards, so the results were wrong. We got that straightened out and suddenly it began to come right, which wasn't bad, since I guess we did it in two days.

MAPSTONE:
It felt like a year.

STEELE:

Yes. The result came out and it was the same one they'd gotten. Everybody was bright and happy and cheerful and loved us dearly. We dragged off to the West Coast. That was a tough one.

MAPSTONE:

Who was the professor, do you know?

STEELE:

No, I never did know his name. I was told indirectly of everything that happened. It was a very strange episode.

MAPSTONE:

Was Wright Air Force interested in buying the MADDIDA?

STEELE:

Well, they'd already bought it. It had popped up from the SNARK program for free, you see. In fact, we'd bootlegged it, because among other things it had been kept out of the company reports.

MAPSTONE:

The SNARK contract was with the Patterson Air Force base.

STEELE:

Yes, Wright-Patterson had been given the SNARK contract, at least, for the guidance part. That long trip through the East was when some of the funny interview that you've got clippings of, were held. I remember that was when I discovered that you must be careful what you tell a reporter. I would tell them this was a digital computer and what that meant. This was the kind of thing that could eventually produce an automatic factory. This was the kind of way to go about that and this was a piece of control electronics. The next day's headlines said, "Inventor says his machine will run automatically."

(Laughter) I felt discouraged. The only reporter that got it straight was from an Australian paper. And I never got a print of it. Off-hand I can't think of any other little episodes that happened on that trip.

Oh, yes. I talked to Dr. Clifton at the Bureau of Standards. He was very enthusiastic about the electronics, the drum work and the Boolean algebra design. He had left out a contract, or was about to, for the Bureau of Standards drum GP computer. He gave me his personal copy of it, sort of sketched in what I should bid and we (Northrop) had the contract. That looked marvelous; I was in business for Northrop, including digitalizing the SNARK, building a Bureau of Standards GP computer. Northrop turned all of it down, refused to digitalize at all, and simply put an end to any further computer work. Northrop did decide that wee were going to market the MADDIDA, and that's all.

MAPSTONE:

They did?

STEELE:

They did, and that's the only thing they were going to do. They wanted to move us off of the SNARK so we could get MADDIDA onto the market. That was all we were going to do. This was, among other things, poor business.

MAPSTONE:

That's interesting because the story as I've heard it up to now, was that they weren't going to market it, and after you left, Hagen convinced them to do so.

STEELE:

When I left Jack Northrop called me in and told me this is what they had in mind. He'd been so misinformed that it was awfully hard to straighten him out. Apparently we'd been painted as people who had commercial ambitions, and that we were selling out the SNARK. This was exactly the reverse to where we stood, and it doesn't really concern the things going down the drain for the reasons they did. It was simply that there were too many parts to it, and these should all be pulled into the computer. The misunderstandings had gotten so severe that the rest of the people began to leave. With all that training and knowledge, it seems incredible that they let it vanish.

The next thing I knew I'd started CRC without any desire to do such a thing. In fact, I went around the entire area to see if anyone would hire the whole group as a unit, but no one was interested.

MAPSTONE:

Really a terrible waste in that Northrop actually had a great group of people and could have probably put that whole area of computing on the map, and probably would have had financial benefit if they'd gone through.

STEELE:

Litton patent writers later on said that had Northrop pushed what it had right from the beginning, they could have beaten out IBM, eventually. They had the patents to do it.

MAPSTONE:

What about the patent business, you started to say that John Matlago worked on writing up a patent. Wasn't there some mess up about putting names on the patent?

STEELE:

Actually all sorts of people were put on the patent who didn't belong on there, just because they worked on the electronics. On the DIDA, for example, even the guy that worked under Sprague was included. He built the power supply. Collison's name got onto the MADDIDA, which made all the guys mad, though he had as much to do with as the other fellows did.

MAPSTONE:

Do you have copies of the patents?

STEELE:

No, in fact, I have never read the darn thing. I edited and signed it, but I never edited the introduction. There's something crazy about the introduction, apparently, because several people have said, "Did you write the introduction of that?" I said, "No, I didn't even read it." They've replied, "That's good," or "That's a relief," or something. Have you ever read it? What's the matter with the introduction?

MAPSTONE:

No, I haven't read it. I think I'll have to get John Matlago to give me the numbers so I can get a copy.

STEELE:

Apparently, there's something funny about it. He wrote it up. Then, of course, Northrop had a little skullduggery going. They had tried to file in foreign countries under Hagen's name and almost crossed themselves up on that. At any rate, they sold the patents to Bendix.

MAPSTONE:

What do you know about Glen Hagen's work in neon tubes, because this was going on while you were working on MADDIDA?

STEELE:

I was the one who hired him. Before that we had farmed out to a normal neon sign maker some little test neon tubes. They were a mess and they didn't work out very well. You could actually take these tiny tubes and you make them switch. They were very reliable and very cheap. The trouble was that occasionally they would be fired by cosmic rays in the simulation counter. I got the idea that if there was a three electrode deal in there which was always kept hot on one side that maybe this would cancel out the premature triggering. This was finally worked up into something we called the Trionode. It's too bad that we didn't go on farther. I think we could have worked out a pattern of wires on a single unit, a little bit like integrated circuitry and small wire units.

Hagen ran into the Nixie principal while he was working on the tubes. You have to pump the vacuum up quite high and get the voltage just right then instead of the glow all the way across, you get a little sheath around the cathode, just like an illuminated piece of wire. He built up these units that had the numerals 0 through 9 and any one numeral would shine through the rest. Northrop let that go, I think, to a cash register company. It wasn't National Cash, but it was a Dayton outfit. I don't know how it got to Burroughs, or whether Burroughs hit on it independently.

MAPSTONE:

Did the Nixie tube come from Hagen? I thought it came from somewhere else.

STEELE:

No, when Hagen joined us in Washington he demonstrated some nixies.

MAPSTONE:

When you went to the Pentagon?

STEELE:

Yes. To show some readouts.

MAPSTONE:

Did you at any time seriously consider neon tubes as a way to go?

STEELE:

Not for airborne devices. They looked as if they might do well with desk calculators, things that didn't have too many units. It looked as if the airborne problem would probably end up taking 20,000 to 50,000 bits. It didn't seem to be a good idea to do that with neon tubes. I certainly think that of desk calculators that would be done today and you just have a little pattern of wires, with neon memory, switches, nixie readout, the whole works could probably have been done as compactly as today, long ago. I'd almost forgotten about that. It's kind of interesting. We had some digital servos too. We never got around to filing on them; North Americans filed on them. I don't know whether there was conflict between Northrop and North American. Carl Isborne was making transistors for us.

MAPSTONE:

He was?

STEELE:

Yes, he'd take diodes apart and put extra points on them to see if they would work. He'd take a diode back bias and all kinds of things and make the back resistance vary all over the place. He checked the effects of heat, age and over voltage and everything else. The question is can you deliberately reject something or do something? He put in a probe and found that you could actually get an amplifying effect.

MAPSTONE:

Did you use them in any way?

STEELE:

No, Bell Labs was way ahead on that. We began to buy them.

MAPSTONE:

Why don't we finish up on Northrop?

STEELE:

Later on, after I was out of CRC, I consulted for Northrop for while. I was in kind of a financial jam at that point and Jack Northrop put me on as a consultant. I ended up temporarily consulting for Hagen. I had to do it in secret, which was interesting, because the CRC release on me was so rigorous that I would have had to lose all rights coming from them if I did anything for any competitors of any kinds for quite a long time. It was a rough contract. Jack Northrop was willing to pay me on a private contract.

I was a consultant for Hagen, and I trained a couple of his guys in designing a GP. That's where the ALWAC got started.

MAPSTONE:

Who did you train?

STEELE:

I've forgotten their names now. Jack Carney would know; we trained them at his house. I had the same blackboard; we dragged it up to his house. The fello9ws would come around and we started the layout of a GP (general purpose computer). I did necessarily end up being used, but it got them all going systematically on the design of the GP. What it was supposed to do, and did, was teach Boolean algebra design approach, what it did, and how to go about it.

MAPSTONE:

I haven't head Jack Carney's name.

STEELE:

He was the one who raised the money that got CRC going. He was a good friend of mine, quite an enthusiast, and he was in DICO later on.
MAPSTONE:

You also influenced Stan Frankel with the Boolean algebra design approach. He went on to Cal Tech and then the LGP-30 machine came out of that train of thought. He gives you credit for the Boolean algebra approach. There's a whole string of machines that followed out of that one.

STEELE:

Yes. Also I got the Librascope on a differential analyzer for the Navy.

MAPSTONE:

Were you involved in that?

STEELE:

Yes, I sold the contract to the Navy. That was when I was at DICO. I really got hooked on that one and, unfortunately, so did the Navy. A differential analyzer is inadequate for all control, by any means, and it should have been appraised in a different way than it originally was. Here's what I've been trying to do since the beginning of the SNARK and it's still going on. The thing that ruined the SNARK has really been that setback for all automatic control. The fact that if you take any control device whatsoever, it doesn't matter what kind of pieces you make it out of, if you can get the numbers, then plot the number of pieces against the amount accomplished, whatever you mean by that, and you'll find that the number of pieces goes up faster than the square and less than a cube in the ways your connections show. This rule of thumb curve, is a very true one. It says that any time you've got an automatic device of any sort that's doing so much, and you decided to do a bit more, the number of pieces begins to go up very, very fast.

It was evident from the beginning of the SNARK, if one took the whole problem and thought it through, that the main problem was all of the problems; there wasn't just one, there were a myriad of things, large, medium and little. Everyone was concerned with just one, the guidance, and everything else was ignored. Yet that really was a small part of all that was needed. It seemed without question that it was destined to fail out of sheer complexity; and it did, for this very reason.

When I got these tape loops going, the predecessor of the drum, it seemed that there was one kind of mechanism that was quite different from the other kind, and that was a serial memory. You take a single loop on the tape, in the same sense that you actually record on a drum, pick it up, move along, re-record and run it in a circle. Nowadays you can easily got up to 10,000 bits. Without deciding exactly what you mean by pieces, amplifiers, etc., you have an investment of ten components. So here's a device that has the potential of a thousand times the response to investment of pieces. As you get still longer loops, which are possible, the figure gets higher and higher. The great question at the beginning was not whether one could get a differential analyzer or anything else, the great question is can you possibly take such a mechanism, just this plain recording loop, and a serial memory, and make it act like any mechanism you wish without having the complexity come back. The answer GP was supplying was no, you can't. In fact, when building the GP, people were saying that it was just a passive store. We will pick things out of it, bring them into the rather vast complex that we've been cooking on the side, manipulate them and put them back. By the time you get this investment, you've for all the complexity back and you haven't saved anything. Everything that was showing up in the computer field seemed to prove that you couldn't take advantage of this, because if all you can see if the bits at one time, how are you going to get them into affiliation with each other without getting them out of memory. MADDIDA should have proved the point, because with two loops on the drum and less equipment than was on one of Bush's integrators, it accomplished the mechanism from his last machine, at somewhat higher speeds and higher accuracy. He had electro-mechanical; electronic relays, cross bars, telephone exchange and all of this went into his memory. He had the two things on the other side of the wall and the wires coming through and you couldn't tell which was which.

MADDIDA should have proved the point that the simplification needed for automatic control of every kind was at least possible. The differential analyzer per se, is not the final answer because there are lots of things it won't do. It's fine for inertial guidance because you have to integrate with the sensors themselves, but there are other kinds of things which it just isn't willing to do. I worked up a whole series of other things to go with this to try and make it a complete system, almost none of which have gotten out for one reason or another.

The next step along the way was to work up a process unit, in the way that an integrator is a process unit, units could affiliate, that will do process without integrating, it will just sense rates of instruments and run them straight so that in this way it will behave like a servo multiplier. I worked up a method of getting about thirty or forty of those, each one were somewhat better than a servo multiplier. You could put forty of them in a double loop and use airborne fire control with fourteen servo multipliers. This would have been the way, way more. This was quite a different control than the integrators. At least I had the answer of how to do bombsite problems calculating.

Librascope wanted to get into the business and I was just getting going with DICO, a small company. We agreed to work up a joint contract, I would design the device and they would build it and do the flight testing and so on, which is a big job. The reason the Navy was doing this, apparently, was that they had just discovered the advanced Norden system didn't work at all. They'd gotten too much stuff in it and they'd improved it a few times to the point where it didn't work at all. The Air Force had found out about this and challenged them to night bombing. Of all the Navy planes that were entered, not one performed. The Navy was really in a jam, so they rushed out and ordered a digitalizer. I sold the contract technically, and I guess I was presented as a consultant for Librascope. I forget what the deal was. They got the contract to do the bombsite and told me to whistle.

The curious thing was that the Navy office was by no means prepared to contemplate anything new and untried, and, I explained, I thought, to the Librascope people what was really needed. To sell the Navy, I gave them a for instance, the digital analyzer, and gave the example which later turned into the Litton-20, then called the DICO-20. Here is the amount of computation you get in the air for this number of parts, and it will do a heck of a lot. He accepted it on that basis, although it was just an analogy, somewhat like the analogy I've given you here. As soon as Librascope got the contract, they decided to do it as a digital differential analyzer. Somebody was telling me long after, that when they were running into real jams they were blaming me because they said I told them it was easy. It didn't get into the Navy job, but it got them into the digital business. That was before Stan Frankel got going.

MAPSTONE:

Yes, that's right.

STEELE:

That particular event was a numerical bridge; I guess you could call it. Then there was all the non-numerical computation that came up next. That's a method whereby you do digital computation without using numbers. This must be analogous to what the nervous system uses.

MAPSTONE:

You'll have to explain that.

STEELE:

That always creates a reaction. The best place to start is to note that when you balance yourself, you actually keep a running coordinate system going in some fashion that you modify. You have to do at least the same kinds of operations that you would in coordinate transformation, which is a mess of equations. It's for certain that you don't have gray codes in your semi-circular canals and pick them off and process them through. We have intersecting selections of nerve rates which modify each other in a way that is the other. This brings on an impasse, because as soon as you get a serial machine and apply it to a parallel situation, which automatic control presents, you are terribly cramped for time; as soon as you get that cramped for time, you find you are compelled to go to parallel memory. Consequently, everyone decides that a GP with core memory is parallel, but it isn't, it's serial in nature and parallel in mechanization to make up for this defect. It is impossible, so it's amazing that they've been able to make any headway at all, putting a serial machine on a parallel problem, which automatic control is.

Automatic control is obviously the slowest thing that goes on in electronics. Five of ten cycles is very fast for a control servo, whereas 500 cycles is the worst audio. If you think of a set of inputs and a set of outputs all going at the same time, the numerical form of the computer has to read an input, chug across a bunch of steps one after the other, and dump the result in some kind of actuator. Not only is there a general pattern from input to there but there are cross-coupling patterns in the action. It's really not difficult at all to get up to a thousand instructions in quite a small control problem. In fact, you get a lot more than that without trouble. If you take the fast servo, you find that you have to come around about ten times a second in order to keep it going, so it doesn't know you've been away. That gets you roughly 10,000 operations a second, which is very slow and for a very small problem. Whirlwind faced with this, then decided to do 10,000 a second, including the parallel memory, which is absolute brute force.

What is needed is a machine that is entirely parallel in nature, in which case it has no trouble with time, and then you can make use of this serial memory. Considering I had been trying to put the serial memory to work in those loops, it was necessary to get a parallel organization. MADDIDA is a parallel machine by nature, but for a curious reason, it doesn't show very much. At times people have speculated on the fact that the speeds are higher than they thought, because they always think of it as being serial. The reason is each time the drum turns once, everything is updated; whereas in the GP you've got a third of one instruction carried out in one turn of the drum. As you add more and more integrators, the relative speed gets higher and higher till a few people like George Forbes and others beat the fastest GP. As the old Bendix DDA's got fully loaded they began to get a little faster.

The reason it doesn't show up normally is that the DDA takes little tiny steps all the way through; it is its own interpolator. Whereas the DDA takes little steps, the GP takes big jumps and gets there quickly. For automatic control, you need all the little steps because they have to be filled in; for computation you can jump to the end point. Other processes though, that don't accidentally have this off-set, can get very fast in parallel, so that using a modal computer—or the one I'm putting together now; a tiny, simple thing, the equivalent, roughly, of 16 GP's running in parallel—it does control very nicely. Control requires a bunch of different things; it requires a different process entirely and you don't particularly want to use numbers.

Look at the basic difference that arises in all this. The GP is an algebraic machine. When you think back to algebra, you recall that it is a science of relations that supposedly are drawn from arithmetic, but drawn from a completed arithmetic in the sense that you have X equal B. That means there is some process where you multiply an A by X and when you're all done it equals B. It is the science of the relationships that result after the processes are over, sort of an end point approach to things. In automatic control, you want a process that's going on all the time, is never over. You don't want to break it up into end point pieces all the time, you want it continuously running. So many processes just flow all the time. It's somewhat like an analog but it's truly digital. One can evolve, and it's perfectly general by programming alone till you can adapt the problem and change it to anything you want. That has been the evolution of MADDIDA, although most of this hadn't gotten out. Quite a lot of it was available for Litton but they didn't pursue it.

MAPSTONE:

So, it's still really yours to play with.

STEELE:

Yes. For a long time I was off it and onto digital physics and math. Since we are about to wind up here. I'll give you the dinary system. There's really nothing to it, except the implications of it as so fascinating. Supposing you have to go through a series of steps on one of these deals that it actually involves. Suppose you take the power of two and put them on column headings. Normally in the binary system you fill in a 'one' to indicate the presence of power, and 'zero' to indicate the absence of it. Suppose instead that you fill in every column either with a plus or a minus, you either include it positively, or you include it negatively, you don't put zero, you don't omit anything. Now you find out this gives you a perfectly workable number and that you can work up a system of adding them. This will give you either a positive or negative number, without bothering to have a sign. It turns out to be curiously true that the binary number system is not truly binary; it is a reduced trinary. It you take a particular power of two to the Nth, you'll find that there are three possible values that occur depending on whether its positive, negative or zero, and that you reduce it by saying if you have a plus 'one' then they're all pluses; if you have a minus 'one' they're all minuses, and put the sign out front. As soon as you try to transmit increments between these, you always have to transfer 3 values, not 2. The original MADDIDA had the dinary system in it, but I didn't know it.

MAPSTONE:

That's marvelous.

STEELE:

It was an offset binary but I got it reduced to a two-value system. You had only to transmit a plus or minus for the increment, not a zero. It took a lot of work to figure that one out.

MAPSTONE:

When did you realize that you had used the dinary system in the MADDIDA?

STEELE:

Several times when I gave papers I mentioned that it wasn't off-set binary, but that it was a new kind of number which expressed zero. I finally had to look at it. It took quite awhile to get started at all. Then I discovered that the instant you suppress the zero one place, it has to be suppressed every place. If you start adding two numbers, in no column can you have a carry of nothing? Every carry has to be plus or minus one, and that means the original carry in has to be plus or minus one; you can't have a carry in of nothing.

On top of that, if you take a limited dinary, you find it gives you only the odd values. There are specific theorems in modern algebra where the odd values can't form a closed system if you add two odd numbers, it gives you an even number in the Arab system; it doesn't so in dinary. This actually overthrows one of the basic theorems in modern algebra. It begins to overthrow a lot of them when you see that modern algebra is based on an Arab number system with zeroes in it.
The next fascinating thing is that if you carry it through correctly, the way you really should, you find that zero occurs in numbers not only on the column places and in the carries, but als you use the implicit zeroes off to the right or off to the left. You can't do that in dinary. You have to assume that you have an infinitude of either pluses of minuses off here. If you put a plus here and a

minus there, and that's the closest you can come to having nothing this way, you have to assume that this goes to infinity here. It has a plus of infinity; no minus especially is plugged into this new number.

It gets even more fascinating. If you turn the things around in a circle and down here is the infinitesimal, the least significant place, where you have the most significance, if you cause them to overlap in a loop they close on themselves exactly and you now have a peculiar number that feeds from infinity, back to its infinitesimal self and that gives a system that among other things has no integers. You can get as close as you please to an integer, you cannot write a zero on it; you can get as close as you want to zero, but you can never reach it. So there's no number more sacred than another. Integers are sacred in that everything begins with them and then you work up correction. With this the integers don't exist any more than any other number at all. It is somewhat easier to mechanize than binary, so it's a very practical system.

If you apply it to geometry, it turns out that this is the number system that you want to use to tie in algebra and geometry together. As you lay out a scale, it applies to the interval, not to the point. Supposing you take the decimal system and on your ruler you decide that you will let 1 apply to the whole interval. You place the 1 at the middle of the interval, and that represents the whole interval. On the other hand, for the system to work, you have to have such a relationship that 1 is equal to 1.0, is equal to 1.00, is equal to 1.000. Divide this up into ten parts, 1.0 would have to be in the middle of this one. 1.00 would have to be the first tenth of that; you're automatically driven to being the 1 back here.

Lord Kelvin did a demonstration long ago where 1.000 carried out to infinity is the same as .0009 carried to the infinite; they're the same number. Therefore, the one isn't just that it gets infinitely close, it's exactly that way, exactly on the crack. Any number system that has zero in it, you're forces to number the cracks. This forces you to number the points, and the points having no size at all, and being restricted from having any size, it is an impossible to make a length. It's actually a contradiction of thinking. You can't have a class of things where the individual objects specifically don't have a property, and the class does have that property. Otherwise, if you slice salami thin enough, it would have no size at all, or if you had an infinite amount of it, you'd have chicken, or some other properties. As weird as this is, one of the great problems of all modern mathematics is to try to square away the number system. It's the problem of trying to tie in the number of points in geometry. Everyone assumes that there must be points, and there really needn't be. You can get around the point and this is the best way to approach it. You get to labeling the squares now. If you start applying it to a number, it applies to a little, tiny intervals and as you carry it out to more and more places, the interval gets smaller and smaller.

Anyhow, this number system kind of shakes up all the basis of ordinary algebra. It's surprising to pry the zero out of one place, so you can remove it from all places, it changes infinity and you don't touch them that way, it's quite a different crack at it.

MAPSTONE:

I hope you're going to do something with it.

STEELE:

I wrote a paper on it, but I haven't sent it in yet.

MAPSTONE:

Have you any papers from your Northrop or CRC days?

STEELE:

The one I did on the MADDIDA disappeared.

MAPSTONE:

That's the only one?

STEELE:

Yes. Among other things, when I heard I was going to be accused of security violations, since I'd been doing all my work at home, I burned all of my papers. It ended so visibly badly with the SNARK being non-digitalized; I didn't have the morale right then to write the paper. Somebody in the company had told the Victor Adding Machine people that maybe they had the information. There were a lot of good digital things going on, things that influenced the work on the SNARK program. Someone invited them to come and go over it. It happened that one of these designs that I'd run off—-I used to run off a lot of test designs of simple machines to work out the tactics—was for a desk adding machine, and this kind of stuff. I couldn't see Northrop behaving that way at all. They weren't coming in quite legally through the company. How did that go? I went to Jack Northrop? I don't know. Ohlinger? One of the other impressed upon the group that they should not let this go on informally, that if they wanted to make arrangements with Victor Adding Machine that they should get it in writing, put the documents together and get paid for it. I wasn't going to get paid, and it didn't seem like good business just to hand the designs over to them. Of course, they probably would have called on me immediately to get everything protected in a hurry. Anyway, the Victor people didn't come. It might have been a good idea but it looked as if the stuff was being leaked out. I do think an adding machine company could have gotten a tie-in with lots of useful things. In fact, I would have liked to talk to them about that. I've forgotten most of the episode. It certainly wasn't Jack Northrop himself. There seemed to be a bit of skullduggery because when it got to the top level of the company there was an apparent uproar. I got to the point where everything I did, even the simplest thing, was always done with an uproar.

MAPSTONE:

Controversial young man.

STEELE:

Yes. All I wanted was to work; I had all these thing to do. Every simple thing, like why don't we send it to Rutgers. They had a board of Directors meeting, and they decided they might be interested in marketing computers. They wanted to look into them; they weren't certain but they were highly interested. Here was a perfect way to get some attention in the computer field free. It reached the point where I had to threaten to quit once a week to get some of the things done. That doesn't work too well.

MAPSTONE:

This document, *The New Machine*, when did you write it?

STEELE:

I noticed it's heavily underlined which is the way I usually write thing so it must have been an informal one. It must have been among my notes at National Cash; it was in a bunch of folders.

MAPSTONE:

It was. It was in Matlago's files at CRC.

STEELE:

That would have been mid-1950s.

MAPSTONE:

You refer to your machine, whichever machine it was, as Organo. Was that a conceptual name for the type of machine?

STEELE:

I was trying to get a name for a machine that had the data flowing through continuously in parallel. Then I discovered this steer manure on the market called Organo so that killed it.

MAPSTONE:

Let's talk about some of the people who you were working with at Northrop. Last night, you mentioned some names like Al Wolfe, Carl Isborne, and Whitehorn.

STEELE:

He was at DICO. Herman Kahn was there; he and Irv Reed worked together.

STEELE:

What was Irv Reed's contribution and position?

MAPSTONE:

He was rather a late comer. MADDIDA was under construction when he arrived but he had kind of kept track. He was going to Cal Tech and one summer he worked as a mathematician with Eric Ackerlund doing guidance equations. When he went back to school, I arranged to get him a small consulting contract and he did the analysis of the gravitational anomalies. When he finished up, he came right back to work and he was one of the most solid supporters that we had.

MAPSTONE:

Who wrote the equations from MADDIDA?

STEELE:

The Boolean equations or the guidance equations?

MAPSTONE:

The Boolean equations.

STEELE:

I did. I laid out the tactics. Has anybody shown you how to go about this? There's a lot of design things that people still don't do.

First you have to design the strategy of the machine; how it goes about doing something new, and how you fit it together. That job ends when you have a work structure and you have arranged the memory. These are all methods that apply to putting material into memory. Then we begin the tactical chart, it's my coined term, and this is really where the design takes place. You go down filling it in, line by line, to determine each separate thing directly down the columns. Writing equations can be done by rote. You can go from equations to diodes to rote, and you actually shouldn't do this because you cane save a great deal all the way along. The way you really save diodes is not by polishing equations, but by going back and considering alternate strategy and tactics to make certain dissimilar things become similar.

I had a very good work structure for a long time, a strategy for serializing the computer. For a long time we didn't have a serial memory that was near ready, and we all supposed the acoustic memory would come along the quickest. There was an acoustic line underway, a liquid line, and a solid line too and there was a Williams tube. When these didn't materialize very rapidly, we decided that perhaps we could simulate them with just a loop of tape, like a tape belt, so that we could get the effect of re-circulating line and get the computer going. We started originally to lay out a loop of tape, and this lead to the idea of pasting it around the rim of a wheel. Then, because of the crack in the tape, it seemed best just to spray it on. This led effectively to the disk.

At that time Aiken was doing drums that had bearings on both ends and I was trying to do a simple disc, just a single loop. The drum ended up as a simulator, with one end of the drum a simulator for the loop, actually. It had about an inch of space on the face. I think we were the first two to do a clock channel. It was recorded on the drum as a pulse rate. That came independently. It seemed self-evident at the time that you couldn't record a clock on the channel and use it, because anyone would anticipate that if you constantly reread a record you would use a little energy each time and the record would gradually fade and disappear. Somewhere I read about a paper that someone had given on voice recording, based on running voice loops. Short record loops were used in World War II to get quick fix on airborne commands that came over communications systems that were very vague. The person receiving the information would want to replay it a few times to hear what they said. Somebody had spun the loop many millions of time playing it, and the record hadn't appreciably diminished. Instead of departing entirely, it fell from a 100 percent down exponentially toward 80 percent. It was apparent that the unexpected had happened, all of the energies came into the motion of the drum versus the head and none of it was taken out of the magnetism. I decided we'd record the loop. We were going to engrave it, or photograph one and read it with a photo cell or something like that. We recorded it instead. We put it on with an indexer and I think Al Wolfe did that.

Al Wolfe had worked for Tektile and was a late-comer. Dick Sprague was an early-comer; he came into the computer group about the same time I did, and as we got several airborne things going, dick for awhile was working on the ground film. He first worked on the Incremental Slope Computer, the decimal one, based on a ring of ten. It was kind of brute force, and it never did exactly work, but it got the first digital things going. While he was working on that, Will dobbins was working on this binary version with the two sets of hand switches, which finally replaced it. Dobbins came along next. There were some intermediate computers that did special computation, hopefully for airborne use, and one of these was called 'Path'. It had magnetic tape, and it had some special digital circuitry. Don Eckdahl was hired to start out and build this unit, 'Path'. It was finally obsoleted and never did get done. That, as I recall, was his first job. Presently he acquired a technician, Al Wolfe. His first job was to build 'Path'. At that time each computer design was a special problem all by itself, each circuit had to be picked differently; the circuitry being much like radar. Radar and circuitry, early digital circuitry, is such that you don't spend much time on paper designing. You just sketch out a rough guess, wire it, check it out and see to other things around it.

The whole problem is it's interaction with the things around it, like pulses feeding where they shouldn't, so there really isn't much to be done on paper.

MAPSTONE:

More trial and error.

STEELE:

Yes. You spend ten percent of your time on paper and ninety percent debugging. These early computers were all of that sort. I kept trying to find some process to repeat one circuit over a number of times, based on the analogy that since the brain does it, it ought to be possible, and that it would save a heck of a lot of work. It seemed if you could just once work out the logic so that you knew what was possible, then, you could decide what you wanted to do before you looked at the circuitry to see if you could do it. The intersection of these things was so great that you had to keep all of it in mind at the same time; you couldn't focus on one step at a time.

Through that period I kept trying. First I'd lay out a computer like the Path, sketch the circuits, work them up, and de-bug them, which was a big problem. Primarily I was trying to get standard circuitry and a computer that would do fairly general things and remain in a loop. I had the loop design for MADDIDA laid out long before there was a loop available. The detail design wasn't worth doing until I knew which loop was going to be used, because the acoustic lines are very high and I would want to do the circuitry quite differently.

The drum came along and beat out all the other things. That was partly due to Isborne's talent. Harold Sarkissian primarily managed Carl; they came in as buddies. I think that Carl worked under Harold during the War. Harold was an officer and Carl was an enlisted man, or something, and they were in the Signal Corp. communications and Radar. Carl always liked ton work under Harold, and he would actually work on the bench and pay no attention to what was going on. Harold started by administering Carl, and Carl got all the all the magnetic things going.

MAPSTONE:

What about the work that was being done on the ERA and how did this tie in with what you did and the directions you took? They already had some drum work done by this time, didn't they?

STEELE:

I think do. For a long time I wasn't sure of any drum work except Aiken's. The first work that we did, because it evolved out of the loop, was really quite different. Aiken used his drum as a true memory. You could record and presently, if you wanted something, you'd play it back and move elsewhere. The drum that I set up was to actually simulate the acoustic line that endlessly ran around in a circle. This gave a completely different sort of computer. If you had a drum going this way, you might record here, it would play back part of the way around the loop and erase in here, and then you'd have a set of flip—flops. It was a delay line, it wasn't true memory. Every bit of material on the drum was removed and re-recorded at each turn of the drum, so that nothing was stored as much as one full turn. The information was endlessly being updated. That, in effect, is why it was a parallel machine.

The MADDIDA was, in effect, two loops. If one draws a slightly more schematic picture. Here's your computer set of flip-flops, here's the long loop delay a short loop, here's the real world coming in and going out, and this tremendous flow-through. We were using the drum entirely as a delay line—as a register as they later began to call them—whereas Aiken was actually recording and playing back. We couldn't or didn't use his circuits. In fact, I didn't know what they were, but I did know he was doing a drum. When this was started out to do the tape loop, I was surprised to see that we evolved back to the drum. That started the one-ended drum, or disk. Maddida actually had 2 long loops and 1 short loop. The drum just overrode all of the other possibilities and was there first and worked the best. That's why MADDIDA was detailed first.

What I used to do was to get people together who were to work on it, and detail the design with them so that they knew all the steps that you didn't take as well as those you did. This still works well, because the design of these serialized machines with the memory tucked away in itself, is like a game of chess by analogy. An outsider who had never played chess and gets in the middle of the game and sees the whole board set up and the strange motions, can't make heads or tails of it. If you see a finished design on one of these highly serialized machines with things jumping over each other, it's kind of impossible to figure out which comes first, which second and what influences what. If you start right at the beginning and go through each step—the same as I did when I trained John Matlago so he would write the patterns—as you go along you see all the technical matters, at least all that relate to computers.

There are too many choices with digital devices, not too few. Analog devices always find a way of multiplying; it was great and was worth something, because it could be a shorter way with digital devices there were an infinite number of ways to do that. After you've gone along a little way and are making steps forward, you come to a spot where you have several possible branches and you have to stop and reduce these alternatives to words. First you must cast around to determine just what these ways are, and then you have to look for some means of deducing which one you want to follow without going down all those paths. If you can pick one of these and go a few steps and break out again and so on, you only have to go through 4 or 5 of those and the design's essentially done. The work is really in finding out why you want to take a path; but you've eliminated a vast number of directions you might have gone.

It often happens that the reason you take one path over another is a certain assumption one makes that things will probably be a certain way. Later on if it turns out that things are different, if you remember how the decisions were made, you know right away that you can branch another way back there and do better. If the people who are actually wiring the device and checking it out follow this and know every one of the steps, it's no problem for them to go back and modify the whole machine very drastically. Otherwise, if people just see the last design and believe it so firmly they don't know where to begin to alter the innards, they will tack things around the periphery and make changes.

When Bendix took over MADDIDA it was really a very rushed and poor job, with 41 flip-flops and nearly 900 diodes. The first time I got the chance to polish here, it dropped to about eighteen flip-flops and 400 diodes, and now it has come down to 4, which is preposterous. I would have never guess at that.

MAPSTONE:

It's been said that you're the man who wants to design the one flip-flop machine.

STEELE:

Yes, if I could. Not because I'm nutty, but I want to find out if the thing would work with one flip-flop, because then you'd know what the basic processes were. When you know exactly what to do with one flip-flop, then you'd know exactly what to do with two, three, and four. Now if you want to go to a decimal system, in fact you can go to the plushest computer going from four to seven to eight. You find the elementary thing, the things that come first by holding down the flip-flops, and also this is the question of how much can you put into these serial memories to clean things up. It's really going to be true, no matter what people think, that the number one problem in automatic control is complexity. The astonishing thing about automatic control is that no one believes, when they first get into the situation, that there can be as many little things to be done.

MAPSTONE:

This is Tape #2, Side 1 of my interview with Mr. Floyd Steele.

STEELE:

Hancock can give you the inside story of the gas tube detail.

MAPSTONE:

What's his first name?

STEELE:

All I can think of is John. I'll remember in a minute. He was down in San Diego recently.

MAPSTONE:

Do you know who he might be with?

STEELE:

He has a little company of his own, I think, or he did. He was the actual technician on the gas tubes.

MAPSTONE:

How about Bill Collison? What was his responsibility and contribution?

STEELE:

I had known him at Cal Tech. He had been going to Pasadena Junior College, and we stayed at the same boarding house. Later on, during the War, I ran into him at Treasure Island. He was instructor in Radar. He went into selling courses in television servicing or something for awhile and then ended up in the L.A. region. I hired him. He was somewhat of a late comer, like Irv Reed, and got in when we were quite a long way along. He was certainly upstairs. His chief contribution was that he plunked solidly behind the symbolic logic from the very beginning. The others resisted it so heavily it nearly lost out. About seven or eight months passed before they'd pay any attention at all. They couldn't see using dumb circuitry. He plugged at it, constantly, and was actually the first person in the nation who worked with the NAND/NOR approach.

There is the Schaeffer stroke, as it is called in Logic. George Boole used a times, a plus, a minus, and several other things. Russell and Whitehead had slightly modified Boole's operation and cut it down, essentially, to a plus, times and the prime, and brought out the *Principia* in 1910. Schaeffer pointed out that the whole table of tautologies could be deduced from a single operation, which I believe can actually be redone from two different ones, NAND and NOR as they're now called. He called this the stroke operation. It was evident that if you really wanted to have minimal *Principia Mathematica*, you had to do it all over again because this stroke operation is not direct. Somehow it's not logically a direct one. Bill plugged away on that for quite awhile; it was his private thing to do.

At that time long before transistors began to replace diodes, it didn't seem very profitable to use amplifiers instead of diodes. As long as you rely on the diodes, you can use the plus and the minus and times. Bill was highly interested in the logic and gave it a lot of moral support. Later on, when Irv Reed came, he too gave his support.

By and large things really went this way: of all the people around there who gave full and enthusiastic support to the idea of airborne computer and its necessity, I think Don Eckdahl was probably number one. The people who actually got involved in MADDIDA shouldn't have been organizationally; they were all hired just for electronics work. None of them had any math background to speak of, but it turned out they were cooperative and willing. I was supposed to have used Chester Stone, Herman Kahn and the bunch upstairs who had degrees with mathematical backgrounds. They were the ones who should have done all the design work, and the guys downstairs should have worked it up from paper. For a long time I ate lunch every noon with Don. He was just a lowly technician when he came in, but immediately saw the need of the airborne computer and was a wholehearted subscriber to it. We worked it out and constantly talked about it during lunch. Then several others joined in. Harold had lunch with a bit later, and so did Al Wolfe. Dick Sprague spent lunch playing bridge. He never really was very enthusiastic about any of the stuff. Don, and you might call it the lab gang, became great supporters of the airborne computer, the serial computer, the small computer, the MADDIDA thing. Dick Sprague was heavily opposed to it because he was doing the DIDA, and the MADDIDA overran it. He was always a cooperative guy and that didn't bother him that much. He switched over and toward the end he chipped in and helped Don. He sort of shelved DIDA and Eric could never get him back on it again. It really was true that MADDIDA did a little bit more with about twentieth of the equipment. The lab gang did champion a serial computer, especially.

There were many pressures from lots of people in the computer group to go along hacking out scores of flip-flops, hundreds of flip-flops, and brute force. On the other hand, it was extremely hard to convince or people not to rush in and build something before it was time. That was the trouble on MADDIDA. It was really premature to rush in and build it. Dick was so eager to get it built that when I sketched out my idea of an integrator to him, he ran downstairs, had a chassis punched up and an integrator underway. I thought he was going to come up the next day so we could continue to work on it. It turned out he had one integrator almost done. He had used five double triodes per stage in the integrator; with just a few days work on paper, I had cut that to two and he had to start over again. He didn't do it with too good grace, but he did it.

He built up two integrators of the DIDA kind and couples them up. The first time these things ran, we were going to run off a sine/cosine to see how they worked. McClary, Dick's technician, was running them. George Fenn and Jerry Mendelson were snooping around as usual—they gave us a rough time—and I think one of them came around asking how it was going, what it was about and copied down the results. McClary had gotten the wrong initial conditions and it turned out that he was getting an accuracy of about 1 part in 20. It didn't take more than fifteen minutes for the word to spread through the whole Northrop plant although it never got to me. It was a long time before I knew. Everyone spread the word around that the integrator only gave accuracy of 1 part in 20. A couple of hours later, when we got the initial conditions right, we got accuracy of one part in four million. Not a word.

MAPSTONE:
Nobody was listening.

STEELE:

That was typical. The lab gang was the most cooperative in the whole airborne thing, in the small ground device wanting to keep it simple, and who pushed the MADDIDA was essentially Don's gang. Al Wolfe was hired in under Don and he checked out the flip-flops for MADDIDA. Harold worked for Don and Carl worked under Harold. All this was sort of informal; no one was ever told exactly what to do.

MAPSTONE:

Were you theoretically in this group?

STEELE:

I was in technical charge. One of the great sore trouble spots was that each group ended up having an administrative head and a technical head. In cases like the computer group where we had someone like Ackerlund, we were lucky, very lucky. He was very competent, careful and a worrier type. He worried about people and he liked to see that everything was done for them. He worried about

Their parking spaces and similar personnel detail and at the same time, he made little or no effort to compete. He really wasn't good at the new stuff and he didn't make any pretense. He had an excellent theoretical background so he could follow it right away, but he didn't attempt to take it, sell it, push it and promote it. In some of the groups the technical and administrative person was the same person, pretty much like Taylor's group. Taylor was the head of it and also the chief theoretician. As the groups grew larger, this became more and more difficult. The computer group finally had thirty-four people in it, and if I had had to handle the administration I would have gone nuts.

At the same time, since this was never formally recognized in the project, the administrative heads would all be called into conferences where the decisions were made of what to do. There was a small group sharp shooting for Frank Bell, who was under George Fenn. Taylor was head of the group but I don't think he had more than two or three people under him. George Fenn was always there and he always waded into other people technically. Fenn was a sort of systems engineer. He was competent and he knew the missile. These other people were hard put often to know their own group. In fact, the administrative heads often didn't know exactly what was going on in their own area, so they were easily putting everyone down. Fenn was always running amuck at these meetings putting everyone down. He was young, enthusiastic, and highly belligerent. A lot of them were quite sensitive, so poor old Eric could hardly stand this and he didn't like the idea of a public fight. He had a stutter too, which was another problem. It was a long flight to try and things sold technically and these decisions were constantly being made.

Then, of course, there are the air benders, the way the aluminum benders made the greater decisions. There came a situation where those on the bottom could see as clearly as anything just which way to go and exactly what was needed. Everything seemed simple, everything seemed crystal clear outside of the details which needed doing, and the branch points and how they came out, it was evident which way to go. It seemed no matter what we said of what we did, all the decisions headed off in the opposite direction. It would seem that any fool could see this was what was happening. Finally we reached the point of thinking this must be deliberate; the feeling was people can't really be this stupid, they're doing this on purpose. We had spent several years working out all these ideas. Originally they seemed very difficult; later on they began to seem so simple it just didn't occur to us that the ideas wouldn't register to people who heard them for the first time. That was the chronic problem that arose.

Getting back to the computer group, Eric made a heck of a good administrator and deserves more credit than he's gotten. I was technical head and there were the two lab gangs that counted. As the group got bigger and bigger there were thirty-four people. One of the problems was to find something that looked like work for some of the people who were more in the way than involved. There are a few who really were highly productive. I would say that Don and the guys under him were the ones always plugging for the airborne thing.

MAPSTONE:

Who was Bernie Wilson?

STEELE:

I remember the name now. He was a technician for either Don or Dick; Wilson did the power supply for the DIDA. I think he got on the DIDA patent too.

MAPSTONE:

I haven't seen the DIDA patent.

STEELE:

Everybody who worked on the DIDA had his name on the patent. According to Litton patent writers, that would have invalidated the patent. One guy just built the power supplies. At any rate, Bernie Wilson was a technician, quite a late comer, and we were really humming when he joined. With this gang we were pushing the computer and so on, primarily getting the most support from Eckdahl. Bill Collison gave the logic work support and it was really hard sledding for about eight months. I'd about given up on it.

MAPSTONE:

On Boolean logic, as far as you know, were you the first person on the West Coast, who conceived of the idea of using Boolean algebra for designing a computer?

STEELE:

Yes. I remember all the steps too. It was obvious once it was done in relays, and you'd seen how it was done, that you ought to be able to do it, with flip-flops. The question was: how to go about it in detail. The thing that really held it up for a long time, and actually held up the East coast people, was this business of pulse and no pulse left over from radar. As I say, no pulse is not a prime, it's nothing. This is especially rendered confusing by a choice of one and zero; zero really isn't. If you tried pulse and no pulse, you could work up the logic in a formal way but it was almost useless. You could make up a truth table for an adder, and it was a slight advantage I guess, but it wasn't anything you couldn't sit down and work out with circuits.

There really wasn't any way to pull the system together. I wanted to pull everything into a single system for the following reason: the whole point was to be able to work out a written system, in such a way that you could concentrate full effort, one step at a time, as you do in algebra. You concentrate line by line as you work your problem in algebra. Each line preserves the essentials for the next line. All you have to do is add one line at a time. You can take the old word problems from high school that you can't conceivably solve in your head without algebra, break it down, concentrate one step at a time, and it's a cinch. The original problem computers gave is that you're confronted with having to think about everything at once. You couldn't just decide that you were going to integrate, or that you were going to add. You had to stop, you had to see that these were circuits where you could re-circulate the information, you had to see that these was a circuit to add, you had to see there was a circuit to stop adding when you got to the end.

The grave trouble—they still have part of this because in many cases they haven't yet done the logic completely; it's one reason no one has gone into elaborate arrangements in serial memory—is that at each place along the way where you have an impulse, the question is: is it coming or going? Is the relationship that you set up then going to appear during that time or is it going to appear in the next time? You get this constant overlapping so there is grave mental confusion in each little simple case, and you have to go through and trace the pulses and decide. In every case you've traced pulses, you find there is a slight time delay.

It was a really great feat in the early computers just to keep it all in your head because having found that you could ass, then you had to keep in mind that your adder ought to have an extra grid in it to shut off at the end without throwing an extra pulse into the circuit which you didn't want. Having finished all that, then you take up subtraction. You didn't dare combine the subtracter and the adder, because the circuitry might go a little haywire, so you put in a separate one down a different path. Then you had to have a switch that would either send the material running through the adder of through the subtracter; separate routes. Then you had to look to see that you had a switch that could do this without itself throwing a pulse. There just wasn't any way in advance of knowing how it would come out. You'd make-up one little bit of something you wanted to do and you'd have to look like mad to see if you could do it, then you'd do all these fix-ups. It was very uncertain at the beginning of the pass just what was going to happen electronically or what was going to happen at the end, whether there would be an impulse left over of would there be a transient.

That's why people found it so convenient to simply put all the numbers in memory, put a few extra pulses ahead of each number, draw it out and run it into the register, finish it and then operate. They could sort of break up their problems and think about it. However breaking up their problem cost them equipment. It completely blocked the possibility of doing everything in combination. All these things had to be put into a simple system.

I got a tactical chart such that you could train people who knew nothing whatsoever about the circuitry to fill it in. Different columns had different rules, and these rules are carefully selected. For example, you can put the ordinary push button at the head of the column, where certain rules govern the column. If you do that,, the push button will go right smack in the middle of the circuitry, right on the diode nets. At present people use two flip-slops to get it in. If you push it, here comes an on proposition in between the clock perhaps, it's not synchronized. If you're not careful, of course, it could throw all the circuitry into the opposite state. Consequently, nobody puts any of these switches right into the net; but you can do it. The rules are such that if you do this, you'd probably end up in the equations, you have the brought the transients, the diodes themselves have taken them out. If you notice people go around later and put a diode into the net. A lot of these things had to be worked out, and the whole problem was to find a system so that you could take just one step, concentrate everything on it, write down the result, move to the next step, concentrate on that, like you do in algebra. Then you could plan each step very elaborately because you could focus all your thoughts on that and not have your thoughts wander into every corner of the circuitry. That really is supplied by the tactical chart.

The Boolean equations are kind of detail; they are not sufficient to design a machine. The all important thing about them is that once you have them, and once you have circuitry that works well with them so you know you're not getting lousy circuits, then, you know in advance that you can do whatever you please. You don't have to stop and look at the circuit data, you can put you full attention on finding out what you want done. This would be the strategy chart.

The tactical chart is the most flexible design technique if it is worked up carefully. It has separate rules governing separate columns. You can train people to fill in these charts and it will integrate all the peripheral things into the logic too. Then you write the equations down the column and then you start your equations. I found some people actually think better looking right at the diodes. Instead of trying to optimize factor work with the equations, Jim Price used to draw out the diodes, look at them and see that he could take three of them from one place, detach them, move them around and put them in a better place. He liked this better than the algebra. Some people are good at the strategic chart, some at the tactical chart, some at the equations, and some at the diode level.

MAPSTONE:

You mentioned that you were familiar with the work that Shannon had done. Did you learn the logic at school?

STEELE:

No. I got a whiff of it in a report by Stibitz. It was evident to me right away in this brief description what you could do with relays, and that I could easily reconstruct what's going on in relays. I didn't find Shannon's article for awhile, and that, of course, was the definitive one. Stibitz had gotten his from Shannon and I got it from Stibitz.

The problem was how to make the electronics work this way. I went through a few steps; the first one was actually discovering that it's the high low element. That suddenly broke loose so much that MADDIDA had a one-stroke multiplier. The first step was: if you start with a pair of flip-flops and you want to affiliate them with diodes, you find there are only two ways possible to tie them together in any fashion. The diodes have to point the same way, otherwise one of the flip-flops is shorted onto the other in certain positions. This is the only way you can possibly put them together. For awhile, I suppose several days, I worked up the circuitry of compiling them through capacitors. You bring these two out and run them through a capacitor with a clock so that you get a pulse coming over here logically based. It finally evolved later into the upper end of the clock capacitor, instead of putting it here. The next step was if these went high or low, and this went high or low, this common point; if it drifted up to high, let's say you put a resistor in here, if you turned this to high, then you couldn't really tie any load into this at all to pull it off of high; the same with low, so you couldn't drive anything else. That was a worrisome problem.

When I hit upon the right circuitry so I knew I could go ahead, I knew there was no point in just jumping ahead and laying out things which I didn't know would work. It take a long time to get the obvious. If you turn this to a very high voltage, then it has all the reserve it needs to pull this one up or turn the diodes over and pull it down. Suddenly one was able to go through several levels, and the capacitor that I was originally trying to work right off of the flip-flops, got shoved up to the levels and ended up with a clock gate. The capacitor then provides the other half.

In the present J, K flip-flops, you have to go through two steps. You have to get the new value, relate it to these two and in effect store it. To finish it, it has to be brought to a finish and lo0cked in somehow so it's no longer dependent on these, then it has to be stepped back into them. Once being moved back into them, no matter how they rattle around, they can't wreck it. First then new value has to be found, then it has to be moved in. By finding you have to set it up in something stable. It's much easier and therefore economical to store it in a capacitor, than it is to add extra flip-flops. The field has always been ready to waste electronics to an enormous degree. If they want to store a new value and move it in, they take two flip-flops, J and K first move into this, and then copy this one from over here. That of course causes a lot of extra equipment. The clock gate does it equally well.

There is one thing that I have been wrong on so far; I assumed, without question, it was always going to be cheaper to make a diode than a transistor, to have one junction instead of two.

Everyone found it easier to use transistors because you don't have to think as much. As yet you can't get a diode as cheap as a transistor.

It became very easy first to work out how you write the equations. However, even this gets to be a problem, because in effect you have to loop around here, where you've got a net starting and that goes off in a more elaborate way and it comes back here; it's going around in a circle where you start wiring it. I started wiring the equations in four; the new voltage amounted to the capacitor. Largely because it grew up as I went along. I started the capacitor clear down here and kept putting extra layers of diodes on. Finally it was around here and I kept driving it for this. It started out that you take the voltage you wanted here, high-low, or true-false, whatever you wanted to call it, and write the equations with these things as input. Then you had to conceive that in some separate fashion and I always started writing in clock. I just wrote clock, CL, but this really isn't adequately treated in the logic exactly. It covers the diodes, but then the extra step takes place when the clock comes along and moves it into here, and there is a transition. If you really inspected the logic carefully, you'd find that during the transition time where these things are changing, none of the equations are quite true. Some things have gone to opposite value, others haven't, in the brief period in which the equation really isn't applicable.

You opened the loop up at the top where you copy the value in, and then you put it in the left hand column. I used to use set, zero, S and Z and then if this were an X then X's over here and you'd write your terms across here as sums of the product.

This is about what they were doing with relays in a sense although nowhere in the whole relay logic did they ever do it right in that they never once put in two steps necessary to move the next value back without transients. The new value, however, is fixed, stored, because the contacts in every relay have a little spring to them and it means that in every relay the new value can get underway and the things in motion before they cut each other off and stop driving them any more because of suddenly changing. The inertia of the thing getting under motion carries it through and closes the new contact. Its kind of hairline whether all of them make it all the time. As far as I know, no one has cleaned up the relay logic to two steps and it's really dropped out.

In relay logic too, they wrote the equation about the final current through each flip-flop and through each really, and then went back to the connections and back to the other set of relays which were running it, which might be itself, if we're talking about a machine running itself. Most of this sort of stuck, as far as I know. People continued doing this just about the same way until the NAND and NOR work came in.

In effect Boole's propositions were better than anyone's since. All of Boole's work was really better than his followers; better than Russell and Whitehead's. They got carried off in rather hairsplitting problems and applying them to language. Boole's work is a genuine masterpiece. If you take any two processors whatsoever, any two pieces of machinery, any two operations, any two diodes, any two contacts, there are only two possible ways they cane be related; side by side, or end on end. This gives AND and OR through a fact of nature. The question with OR is, should it be an exclusive OR, or an inclusive OR. Diodes make it inclusive. Boole used an exclusive, which is much handier in working equations, yet it gave him trouble interpreting it; you have to be careful how you interpret it. This gives trouble too as you interpret. Have you gotten into Boolean algebra at all?

MAPSTONE:

Not really. I've read very brief introductions and I know what you're talking about. When you really got into this, did you go directly back to Boole and use him as your source, and not Whitehead and Russell?

STEELE:

Yes. They're not terribly applicable because they developed a new notation implication, and the straight analogy to algebra is more direct. The reason that NAND came in is. . .

MAPSTONE:

Whose work is that really?

STEELE:

I don't know, I think it got going at Hughes. It got going for this reason, which is moderately persuasive in a way. If you put in resistors, instead of diodes, making these fairly small and this quite large, and turn this to the quite high voltage between high and low, you'll find that the voltage here is a selection function. If one of these is low and two of them are high, you pull it down a little step. If you don't have too many resistors and you re-amplify this, you put in a transistor, if one of these goes low, you could have just that amount of drop which is one fourth of the total which is sufficient to cut this off. The transistor by re-amplifying restores the whole thing, so this now goes to high.

Somebody at Hughes was sold on the fact that resistors ought to be cheaper than diodes, but they really aren't. It always looks like a good idea, but as soon as you begin to take really cheap resistors they have a ten percent tolerance, drift of time, a drift of temperature, and you begin to get such a loss, especially if you want to use up four of them. People then start using precision resistors, which begin to cost more than diodes. I think it was in pursuit of this, because the first circuits they used in which this was pushed were using resistance selections. People began to go back and put used diodes in place of resistors. Since diodes give you the full swing of the net, instead of having this form of collector amplification, they get an emitter follower.

The factor is that this little piece not only produces the AND and OR, depending on which way you aim these, but in putting the amplifier in it inverts this, turns it upside down, and this is your stroke operator. That gets one very enthusiastic because now you don't have to run one operation over and over again. Logic, however, is probably not quite as flexible because it's very hard to always interpret what it means. As a matter of fact, you could do this circuitry if you wanted without bothering with the stroke operator, but its fun to do it that way.

MAPSTONE:

Let's push on now and go to setting up CRC. As I understand it, Eckdahl and Sarkissien wanted to leave.

STEELE:

Sprague also had gotten a job lined up. I had talked to the Librascope people about hiring the whole group. At that time they weren't interested. I talked to Telecomputer Corp. Somebody said they might be interested and that I ought to talk to them. I did, but I forget who it was. It seems like I talked to hundred of people during that period. It would have been impossible to pick out the key people, but they wouldn't hire them. It really was quite an oversight.

I didn't want to start a company, so Jack Carney put a little money which he got from widows and orphans and acquaintances of his. We rented the upstairs floor in a building in Manhattan Beach and that got CRC going. I had to keep the books, we did our own janitor work and waxed the floors. We got a lab going. We got enough equipment so that Carl Isborne could work at home while he stayed on at Northrop. Several of them stayed on at Northrop for awhile and would work for us at home. Finally we got it going.

MAPSTONE:

Before you left Northrop, had you any feel from people about whether you should be able to sell your product?

STEELE:

I really wasn't interested in selling the product; I was interested in getting a setup so I could go on with the SNARK. What I wanted to do was get an organization, a place, a balance sheet, some money and try and get a contract to digitalize the whole SNARK. I wasn't interested in commercial sales at all. The great thing that was lost, you see, more was lost at Northrop than ever got out. The MADDIDA was just a small piece, it was really a minor part of everything that was going on. I had some digital servos that North American swiped. I presented the idea at Wright Field and they were there. We went back and in three or four weeks they had filed on them. Quite a lot of the different pieces that I needed were ready to go. The ground support for the SNARK was all sketched out, the strategy had been done, I had tentative layout of the tactics and was ready to go on that. The MADDIDA, which was a prototype, had half of it left off just to get it done and get a little experience with integrators. Also to make sure it really worked at all.

MAPSTONE:

Did you personally go to the Air Force and talk to them about what you could do.

STEELE:

Yes, many time. I made many trips to the Pentagon and Wright Field. Somehow I always ended up selling contracts for other people. I would find out there was something to do, go back and show them it was all ready to go and how it would be done. They'd give the contract to somebody else, and they wouldn't know how to do it and would goof it terribly. It would come out twenty, thirty, fifty times bigger than I planned actually. Everyone would decide that this was a mistake and that it wasn't ready to be done. I got J.B. Rea in the computer business that way and the Bill Jack Co., and then that non-linear digital business, Librascope, North American. I made a set-up for Hughes. All these things were ready to go. From 1952 on, all the rest of it was done; digital servos, the actual servo logic, non-numerical computation was ready, all of the things that were the generalization of general purpose computers for automatic control. From that day to this, it hasn't gotten out. Each time that I'd find some possibility, they'd get very excited, pour out a huge amount of money and it just didn't get going. This still remains to be done. At Northrop, most of this was ready to get underway, way in advance of MADDIDA.

MAPSTONE:

The contract that you got from the Air Force at CRC, was this part of your major plan?

STEELE:

No, I got it from John Marquetti at the Cambridge Air Force Research Lab. He said he'd make a deal with me: if I'd give up carrying a torch for this SNARK and get to work on what turned into the Dew line, the distant early warning radar approach. He represented one of two approaches: the decentralized one. He had CW radar that was small enough, it only had nine tubes, but there were a vast number of little ones stashed around and he needed a centralized computer that could go with them which was small and simple and could be unattended. The rule is if you have to have one man in attendance at a radar installation, you have to have thirty; it grows that much. So it had to be literally unattended and occasionally some ground attendant would fix it up. To do this a computer was needed.

When I started out to do this job for CRC I didn't have a thing invented, so I started on a GP to see how much could be trimmed and cut back and worked up. That's how I first got going with CRC. Sprague and Eckdahl primarily started detailing the GP, and mostly I was out raising money. I had some marvelous times banging around Wall Street and the strangest experiences. I was such an utter amateur, it must have been amusing to these people. Finally I got CRC financed with some money my dad cashed in on an insurance policy, my father-in-law put in some money, my friend

Jack Carney put in money; that's what got CRC going.

I finally found the computer John Marquetti wanted. It would have actually given a decentralized DEW line approach because it was real simple. It would have been a form of GP. I got down to sixteen flip=flops at a time. By golly, neither Sprague nor Eckdahl would consider giving up the one they were on which was growing huge.

MAPSTONE:

I see, there were two different machines; they were going on one design philosophy and you were going on another.

STEELE:

Yes, they'd detailed quite a lot on paper, and they had two or three months work invested. I finally found the Tabula Rosa but they just wouldn't pay attention to it.

MAPSTONE:
Tell me about the Tabula Rasa.

STEELE:

This was a computer designed as a for-instance for John Marquetti and that would have marketed nicely. It was a drum GP, it had only sixteen flip-flops and I think I could have cut out about four more. It was micro-programmed so it was quite plush as far as the user was concerned. It had a floating decimal point, and used the decimal system. It tied right into an input and output printer and everything for twenty-four flip-flops. It took quite a bit of careful detailing, especially the micro-programming.

MAPSTONE:

Yes, this is ahead of the public knowledge of microprogramming.

STEELE:

Way ahead.

MAPSTONE:

How did you get it?

STEELE:

I can tell you the day in which I found it. It was the day of my birthday, I was walking down to the office and I hit on it and I thought it was a good omen.

MAPSTONE:

It was an original idea?

STEELE:

Oh, sure. No one was trying to build computers at sixteen flip-flops in those days. No one seemed to care if they used a few hundred, no one bothered. The way the other CRC guys were going, they were getting into a big GP that wasn't going to be decentralized. That is what Marquetti wanted, his neck at stake in getting a decentralized computer. He didn't give a darn whether he got a GP or anything else, but he wanted one that was so simple and reliable that, with occasional attention given to it, would be run week in and week out. He was certainly willing to pay for this, and that's what the contract was about.

MAPSTONE:

In 1950 that was quite a tall order, wasn't it?

STEELE:

Yes. At the same time, I was supposed to finance the company, which I did—I brought in every dime that came in—got contracts together to do the work. It seemed preposterous, so finally I told my problems to Dick Dabney, a guy that I'd known in my wanderings, who seemed to have some good sense of business. I'd gone through Navy Radar School with him. I'd known him at Douglas, and he was always primarily interested in management and money. I decided to bring him in so he could be president of the firm, run the billing and the business end of CRC because it was very necessary to move on with this R & D. The problem with the other computer was that the engineers in a sense were getting the bit in their teeth and were just shoving it through anyway. This was going to put Marquetti in a jam personally. I was very much afraid that the big piece of hardware delivered to him would wreck his program, because he was in an all out fight with MIT and Whirlwind. Their idea was to have a gigantic computer and centralize everything for hundreds of miles around and put it through a building. This was the competition, and I had the only contract going to do a small computer, the decentralized one.

No one thought it was possible, but it was. It was mocked up and ready to go when complete rebellion broke out. The engineers just wouldn't do it. So along with Dabney's cooperation, they seized the company and ousted me. I was just tossed out in the street. I was completely helpless because they cooperated with a fellow named Gordon Turnbull to take over the company.

MAPSTONE:

This is where Turnbull came in.

STEELE:

Yes, he was a hard-boiled contractor who had what they thought was an idea—he was their knight in armor. I had to get my father's and father-in-law's money back somehow, because that was their life savings, so I was really over a barrel. They wanted me to sign a contract for return of that money stating that I wouldn't practice computers for five years. That was part of the deal they bounced me out on and Turnbull was the one who turned that down, so I always felt kindly disposed toward towards him. There was a period of several months during which the contract wasn't wound up yet and they still had the power to turn down the release of money. Consequently, I was immobilized because I couldn't offend them in any way. I couldn't even go to Marquetti to tell him that I had this design done. Irv Reed stuck with me, and we started going on with the design anyway at the Real Estate office. He decided he'd rather go off and work at MIT so he left too; it got to too much pressure and hanky panky. That finally ended it. We never did get wound up again, but it was pretty well detailed and about ready to go.

The result was that Marquetti finally got this huge computer. These guys were sure that their two or three detail was sacred, they couldn't possibly see there wasn't that amount of time on paper. I tired to tell them that they were going to be stuck for several years with what they were laying out, and they were. They finally delivered it. I don't know if it was shoved in the corner. By that time Marquetti had lost. By default he had simply proved that whirlwind was right, that you couldn't have a decentralized computer. That took CRC out of the research business.

MAPSTONE:

The machine they delivered was the CADAC, wasn't it?

STEELE:

Yes.

MAPSTONE:

It wasn't a large computer.

STEELE:

No, it was a drum computer. All drum computers were about the same size; 1,000-2,000 words, and one complete instruction every couple of turns of the drum. It ran into several hundred flip-flops and this brought it to the point where it constantly needed attention to keep it running. Then, of course, it still had to be tied into radar. I had that done, too; I had a way of tying into radar that would have worked pretty well.

MAPSTONE:

What did you do with the design of this computer? Do you have it, did you keep anything in documentation?

STEELE:

Yes, it's around somewhere. Maybe D.J. knows. When I finally left CRC it was on the basis, if you guys don't want this computer will you release it to me? And they finally agreed to. By the time I was free I had started a new organization to get going all over again. I got DICO going based on my father-in-law's money. Jack Carney didn't get back the money he brought in for quite a long time.

MAPSTONE:

Was Carney an officer of CRC or was he just a money contributor?

STEELE:

He did something; I don't know what it was. I've forgotten. He had some kind of position there. He definitely was an officer at DICO.

MAPSTONE:

He did follow you to DICO?

STEELE:

Yes.

MAPSTONE:

Was DICO set up based on the Tabula Rasa computer?

STEELE:

The reason was there wasn't anything else to do. I have never founded a company because I wanted to or planned to. At the time it just happened because there was no other way to go. One of the things the CRC guys brought on themselves is they threw themselves 100 percent into Turnbull's hands. Once again, I tried to tell them that this kind of in-fighting would simply dump the whole thing in his hand; that he would be calling the shots, and they would be worse off than they had been at Northrop.

The day this was all wound up in Turnbull's lawyer's office, it hit me as really funny in one respect, if it hadn't been for Turnbull himself they would have tried to get me into signing a contract to quit the computer business for five years. So there's a lot of hostility there. At the same time, my contract was wound up, Turnbull indicated that he would put out the money that was being returned to my outfit and that he would supply it to himself. He led them on that he was going to be the money man instead of my family, and it turned out that part of the contract was that he was going to supply the same amount of money in capital to CRC on a ninety-day demand note, a short term note. The only one there who actually caught on to what this meant was Dabney, whose face fell clear down to the floor. He was completely struck. Turnbull took it over and, in a way, he had a pig in a poke. He was $700,000 in before he unloaded it to National Cash.

Again, the thing that was lost in that foolish move was that the decentralized early warning system was possible, was in fact provable, and the possibility of proving it was lost. That particular direction, which would have been a small, simple GP, never did get realized.

MAPSTONE:

Marquetti had lost face.

STEELE:

Yes, he was simply unable to prove that this was the way to go, so the approach went out. Marquetti finally was moved out of existence. With the computer becoming more and more a central part, the Lincoln Labs were built up, too, and they took over the radar work.

MAPSTONE:

That was Project Sage, wasn't it?

STEELE:

Yes. They began to string these five-star buildings full of computer stuff all over the artic. I guess they never did work, at least reliably. That was a loss; a lot of money went into it. Again, it was one of the steps along the way, in which everyone decided that was impossible to get simple, cheap computers; therefore they would keep plowing money into getting the reliability higher. This is still the direction they are pursuing. Everyone still goes to the GP, everyone still decides they have to be complex, they've got to be fast, and therefore they keep pushing components that can be made very cheaply and can be run in great quantities. Their success has been much better than once predicted.

At the same time, the other direction remains unopened. It's kind of a hazard because they tackle their own experience and extrapolate behavior of say the Chinese on the assumption it cost them this giant sum of money to digitalize long range guidance, and it really wouldn't. So sometimes that's a hazardous approach.

During the CRC era, Harold Sarkissien came across a very interesting magnetic drum flip-flop. He came upon it by accident, that was rather remarkable. I'd started him on a magnetic flip-flop. The approach is pretty obvious: it's just a small, magnetic amplifier with a pair of them cross-coupled. The magnetic amplifiers needed very little things like cores of magnetically heat treated material which was fairly expensive. Normally these things had to be annealed under hydrogen atmosphere to take stress away and get much higher permaeability. We took a pair of diagonals and stuck them in here and squeezed this. It didn't seem to produce any effect that we could see, we squeezed harder and harder, finally, when we cut it in two, the thing was somewhat better. We bent it out straight and it was better than ever. Of course, the permeability was lousy, but what it meant was that this was taking great advantage of the leakage.

He worked up a pair of these into a flip-flop that could have also worked for adding machines and desk calculators easily. That's effectively what I had in mind. If you kept the speed way down, you could have built it up using a pair of shingle nails for the iron core, a piece of iron wire would have done, compound lining (??) and so on. The flip-flop could easily have done for between a nickel and a dime and should have lasted twenty or thirty years without trouble, really reliable. A set of those as stepping registers, and you would have a desk calculator.

There were two fellows who were forming a little company, Bill Nugent and Frank Jonane and they were acquainted with Jack Carney. He brought it to my attention that they were very anxious to get started and they had been working in their spare time at home, for almost nothing since the beginning. I got them winding and plotting one of these little magnetic amplifiers, and they got underway with these. At the same time I was canned, Dabney cut off the contract to them, although they weren't charging much.

They were highly dedicated types, very idealistic, and they very much wanted to keep going in the field. They came to me and said that if I'd help them get going, they'd work for nothing. Jack Carney offered to work for nothing and one other fellow named Ralph Short said if we could get started, he'd put in time for nothing to get something going. It was their enthusiasm which made me decide, Okay, I'll see what I can do. I got going and trained up a fresh group, and that's how DICO got underway. We rented another real estate office in Playa del Rey, had a little lab there, and built the DICO-20, which later became the Litton-20, a version of it. We built up out of drums. It was a DDA. I selected it as a trainer problem because it had relatively few flip-flops and was rather straightforward. I wanted to train them on the design, logic and so on, so I picked something to start that wasn't too complicated and trained them on the tactics. First I gave people a course in Boolean algebra and gave them something simple like a counter, then I gave them something a little more elaborate like a set of counters and then taught the tactical chart, which is the actual memory, as well as integrating it all the way through. One really needs a very simple, straight forward setup to do this. After they've gone from Boolean algebra to tactical chart, then they pick up the strategy of the new machines.

At Play del Rey we began with a desk calculator and this DDA. Bill and Frank worked up a plastic drum that actually worked. Bill Nugent knew plastics well, and he said there was a plastic that would be dimensionally stable. He was right. It was cheap, it worked much better than aluminum; it was somewhat more stable and so easy to bond the coating on. He used a plastic solvent to mix the magnetic paint, sprayed it on and it bonded tight onto the surface. We had hoped we'd get a drum for about twenty dollars. We borrowed a diamond saw from someone who had been an amateur stone cutter, got a box of ferrite and began to make our own heads. We had magnetic heads underway, the drum underway and the electronics were re-circulating.

By the time I'd gotten a contract from Convair. Originally, I had talked them into what I thought and hoped they would really do, and that was to digitalize the Hustler, the big semi-automatic monitored bomber which was to be as automatic as possible with a small crew.

MAPSTONE:

Harry Huskey mentioned a proposal you submitted to Convair on which he had done some evaluation work. Could that have been the one?

STEELE:

It could have been, yes, it certainly could have been. I'm never sure whether they were using this work to help sell the program to the Air Force. Later on they had represented in effect to General Seville that they were going to digitalize the Hustler, because I had sold Wright Field enough so that every body else that gave it to them, it went to Fort Worth finally, was the fact that it was going to be digitalized in a new way. They didn't bother to go on with it after that.

In the meantime, I had contract from them for small amounts as computers go. I decided that since there were so few of use and we were all in agreement, we'd move down to La Jolla. So we decamped down there, Bill Nugent, Frank Jonane, Jack Carney, and myself. Ralph Short was finishing up at USC and a few months later he moved down. Finally I was able to have a payroll.

One of the things in this contract with Convair was they very much wanted something in the way of hardware--some things to show—so I wrote in that we'd do the DDA for them. The desk calculator was temporarily shelved and we never really got back on it. We got going on two different DDAs; and one was more a control DDA, about forty integrators, and the DICO-20 was a little less top heavy. We had hopes that it could be sold for $200, for people to buy and use as a desk calculator. We got a long way on the 'Convair contract and then we got a contract from NEL, Naval Electronics Lab, to do the MOAT flyer. Do you know that?

MAPSTONE:

No, I don't.

STEELE:

That would have been about 1951. This was a mortar trajectory extrapolator. They were still using them in Vietnam. Radar will track a mortar shell quite a ways, maybe 6,000-7,000 yards; pick it up and follow it. If you run this at real time into a computer, you can fit the ballistic equations to the trajectory you're following, you can catch the shell going outwards, fit the equation and extrapolate it back to where it came from, and you can locate the mortar that's firing it. I worked up a one-channel deal on that; didn't even take two loops, just one; and it had fourteen flip-flops and would extrapolate back in real time. You can find the location of mortar from where the mortar shell had landed. That was called the Moat 5 and NEL was very interested.

There were big problems going on all over the country. MIT had done a study on it and come up with 500 flip-flops. NEL had an analog computer going, and they had 18 to 20 special servo multipliers in it. It seemed to me it cost about $3,000 per multiplier, in terms of special cost. G.E. was in it very heavily and built a gigantic radar system, which was very expensive. So this little computer, it really was quite small, had a drum going on it, just a little drum with one channel, and it was transistorized. As far as I know we were the first people to re-circulate a drum with transistors. Transistors were really lousy then. The ones we could get would only run to about 30 KC.

MAPSTONE:

When was this?

STEELE:

This was in 1951. We had transistor flip-flops underway and we did the first commercial etched circuitry on the West coast. The fellow that had been working on the etching at Hughes and knew the exact details and could do it himself, came to work for us. We started making up etched circuits for our components.

MAPSTONE:

They were first used in this small machine?

STEELE:

Yes, in a way they were. The prototype was spliced up because everything was in a huge rush. At the same time, we worked up the flip-flops in the etched circuitry and used rolled up etched circuitry with flexible backing. We etched it, put the components on, rolled it up, and put it right under the plug. The first transistors really weren't reliable enough yet, but this small device could be run slow enough because it was effectively a parallel machine. We could run the drum at 30 KC and the transistors worked at any rate, we did re-circulate the drum and we fell back to tubes.

We worked up a prototype, tied it into a simulator and demonstrated it. The Secretary of the Army was going to fly out personally to see this. The Army invested a lot of money in G.E. and MIT and this looked like it would work. Also, there was a plan underway to move a good deal of the radar into the computer. The Army Radar Headquarters at Redstone Arsenal had $7,000,000 they were going to contract to G. E., so they were going to see whether or not to contract with DICO. Almost on the day they were supposed to come and see the machine demonstrated, McCarthy moved up there to investigate them which just closed them down and we didn't hear from them for a long time. They never did show up, and there was a real up-roar for quite awhile.

With this $7,000,000 at stake, we did demonstrate to NEL and to one guy from the Air Force. The simulator had cams cut that would actually simulate the output radar would have for certain trajectories. You'd run it into the computer, run the computer back, it was in decimal form, and it would give you the x-y coordinates on the map. The next thing which could have been done but never was, was to put a power out in such a way that you run the cross hairs right up to where the guy was located. NEL then came over when I was back East and seized the computer prototype, made quite a big thing of the fact that it was hay-wired, and destroyed it on the spot.

STEELE:

I've had three prototypes destroyed. Then they rebuilt it. I'd been doing the work but they had the prints and everything. They rebuilt it stylishly. Their objection was that the electronics were awful although they copied the electronics right down to the last detail.

MAPSTONE:

Surely they understood that this was a prototype.

MAPSTONE:

They understood that there was $7,000,000 at stake. They weren't dummies. They know what prototypes are, but as far as influencing guys at the top they could say, "Look how horrible it is, it's spidered, it's all hay-wire."

MAPSTONE:

I take it you had not patented any of them.

STEELE:

Oh yes, but the patent rights would have gone to them anyway, since they put up the money. Anyway, we were just chopped out of it. They were always willing to give me credit for the design, despite the fact that later on they had to buy the circuitry from us because it was the only one available. At least they got this big chunk of money going into the program, themselves.

When they reworked the prototypes they wanted to change the servos. The prototype had seventeen flip-flops; it could have been done with fourteen, three were just to speed things up; they got their prototypes u to eight flip-flops, just tying those servos in. At the same time, when they went on with it, there were 500 cubes in the back of the radar, most of which could have been moved into the computer. The computer could have been moved in really close to the equipment, and gotten around all these other things. All of that was lost on that particular deal. That act on the part of the NEL crowded us out of the mortar business.

I believe 30 of the NEL version of my version were manufactured for the Marine Corp. They kept about 5 as trainers over here and they sent 25 over to Vietnam. They could have had hundreds if they had ever cleaned it up and it would have made a big difference. It still would in guerilla warfare. Mortar, is the chief weapon of the guerilla.

MAPSTONE:

So tracking it down is really getting it right at its source.

STEELE:

Yes. The number one reason it's so dangerous is that the guerilla can infiltrate, hide, lay up mortar, get it zeroed in on a crossroad, and the trucks begin to go by them suddenly start lobbing shells from all directions at the crossroads. They wait till it gets dark, sneak out again, and no one knows where they came from. The instant that you track back the mortar and locate him, and one doesn't even have to do it very accurately, his point of view changes entirely. He doesn't really bother to infiltrate very far, he doesn't pick worthy targets, he just goes in a little way, gets rid of his shells, and gets out in a hurry. It was quite an interesting device. It was the first digital computer, parallel in organizational structure, to use automatic control.

At the time I was trying to sell people on what I called micro miniaturizing things it was a word already being used for resistors and things. I had the idea of microminiaturizing the pieces. The example I used was that transistors sufficient just to show the volume of them. I showed that the whole MOAT 5 could be put in a coffee can. NEL thought this was ridiculous that they hung up a coffee can labeled “Steele’s Can”. We were very close to having that transistorized. In each case when one of these things was lost, a whole trend was turned off the other way. Before long instead of going from seventeen flip-flops back to 14, they go from 17 up to 80, and that’s just for field testing. When they get to the final polish job it’s 50, and then they can’t afford not to make it worthwhile so they decide that all this proves is that the time is not here when you can do it. So these programs get cleaned out and a new trend comes in. The mortar tracking device never advanced the way it should have.

MAPSTONE:

Although it did at least get some practical use.

STEELE:

Oh yes, it was actually out in use. Litton made quite a killing on that because they were the one who made the thirty computers; they got the patents.

MAPSTONE:

Is this the same machine as the Litton-20?

STEELE:

No, it used to be the DICO-20. First there was something called MADICO, which was committed to Convair. Then we made our own drum. We made a cheap simple one and had a year’s test on it. We had our own heads and they were so good that J.B. Rea, after testing all the heads on the market, bought ours. We had these packaged flip-flops and we busy getting the MADICO in shape. We ran a little over time on that, which wasn’t surprising, and I’ll be doggoned if they didn’t come and take that again when I was gone. They just arrived on the scene, they must have known I wouldn’t be there, expropriated it, took it away, but could never get it working. We had 2 or 3 weeks more work on it, but they didn’t quite understand what was needed. Eventually they cut it up for parts. This was the second prototype all ready to go which proved something in the way of simplicity, and it was grabbed.

MAPSTONE:

What would have been the reason?

STEELE:

Well, you see, anybody who could get it in their own hands, politically, then could get a program underway. The same guy who instigated this also tried to become a DDA authority on the basis of it. He founded some kind of a DDA society. For all I know it may be going yet.

MAPSTONE:

Do you recall the official organization?

STEELE:

D.J. will remember. He got in touch with some people who were using Bendix DDAs, and there were quite a few other enthusiasts about, and they formed this DDA society. Their first meeting was held down in La Jolla shores, about three blocks from my house. I was never invited to any meetings, and never have been. By getting a computer this guys became quite a computer authority and convert.

MAPSTONE:

What was his name?

STEELE:

It doesn't matter, because it didn't work out too well for him. He really wasn't able to get airborne computers going there.

MAPSTONE:

That was the end of your contractual agreement with Convair?

STEELE:

Yes. Again, this prototype really had some new and interesting things. It had cheap drums, cheap heads, and a new kind of flip-flop that made a big difference. One of the illusions people had was that transistors greatly reduced the power requirements in circuits for the electronic vacuum tube circuitry. Now this isn't really true, because one of the last thing I found is that if you cut the voltage way down, you could actually cut the voltage in the vacuum tubes down to about six volts, to transistor levels. When you do this, the power is the same as for the transistor, there isn't that big a difference. Of course, you have the filament power, but that's cheap. Everyone was used to swinging tubes in radar work; the biggest swing they'd get was 100-150 volts. When you get down to swinging at three volts, the power went down so much that something like MOAT took a one volt playback.

We had heads that were getting a one volt playback signal; our average head would give 4/10 of a volt playback. It only took one stage of amplification to re-circulate the drum. I had channels in recirculation that had two amplifiers and one double tube. Aiken's drum had fourteen amplifiers. This was going to cut cost; real cheap heads, real cheap drums, real precision, low power requirements, and very few components, so these things were all ready to breakthrough and go. In each case people had a chance to seize the program and run with it, and they did.

I started another DDA all over again, which I called the DICO-20; the one that Convair got was called the MADICO. In the meantime, because of just training people, I found a way to cut the thing way down some more. This was beginning to be very, very interesting and surprising. That started still another one and it was done with all we'd learned on MADICO: cheap drums, low-cost heads, low-cost flip-flops and it really was something that you could put on people's desk tops for a few hundred dollars. I don't care for mass production and all that, but the costs and power requirement would have come way down.

MAPSTONE:

What did you think it might have sold for?

STEELE:

Around $500, I guess.

Tape 2, Side 2

MAPSTONE: Do you recall exactly what we were saying when we left off?

STEELE:

I guess the evolution of DICO. I think I'd mentioned that with a little careful attention to a mass production it appeared that one could get these on the market for maybe $500; a notion that anyone who wanted one could have one for himself. This would be a totally different kind of computer in one sense; as soon as you do a computer for an individual, there's no real sweat about getting problems in and out of it in a hurry. The computer center always wants to move problems in and get data out fast, to get one problem out and another one in. The individual may have only one problem over several years. It's no problem, really, to provide quick change of code or anything else as long as it isn't too inconvenient. Sometimes it takes you thirty or forty days to get original numbers and get started, and that changes the time scale.

The all important aspects are: to be able to buy it, own it, and play with it, as opposed to sending the work to the computing center. Consequently, the economics should dominate until people used them and discovered the niceties; because unless people actually use things, their notions of what they want are no good at all. Everyone can always guess how nice it would be to have things, but if they have to make the choice from the hand of experience, it always changes a great deal. The hope was that it could get on the market economically and cheaply.

The rationale for marketing was to keep down to twenty-five pounds. This weight, picked right out of the sky, was the amount of weight of weight a middle0aged salesman could lug up and down stairs and talk to people. The idea was to sell them through almost any outlet that sells office machinery. When they needed servicing, the salesman would give them a loaner, take the original, crate it and send it back to the factory. There is no problem at all for him to cart one twenty-five pound machine up three flights of stairs and bring another down. If it's under twenty-five; that's fine, if it gets over fifty, he isn't going to do it. This changes things quite a lot too, so it's necessary to keep it light.

The lightness was all introduced by this low-level flip-flop. It could cut the power so drastically that the power supply, wherein all the weight lies, was very small. The original prototype weighed twenty pounds and was about the size of a mechanical desk calculator. It was coming along, just about ready to go, and at that point DICO was sold to Litton.

MAPSTONE:

DICO, the company?

STEELE:

Yes. After NEL's maneuvers and so on, we just ran out of money. In the meantime I had sold the Navy on the necessity of digitalizing their instrument program. A man named George Hoover, a Navy commander, had got a cockpit advanced instrumentation program going and he had both Army and Navy R and D money pulled together in a huge package. When I was in Washington I used to stop around and argue with him. We would argue around and around the place. I kept telling him that he couldn't just throw together these displays that were dandy for the pilots, without any regard to what went behind them. The way people were throwing vacuum tubes around, he was going to have to tow the electronics to be able to instrument his display behind him in a glider. He was very blunt, hard boiled guy, with a very good mind, and once he got convinced he never let go again. He wasn't dogmatic in the sense that you couldn't get him to change, but he was a rough old character.

He was putting this system out to bid, so I put in a bid on the computer. I found out later that 37 companies had bid, including Hughes, G.E., North American, everybody. I'll be darned, I got it. We were just a little hole in the wall company. I got it the day I had signed the sale with Litton, so all $7,000,000 went to Litton's hands. All of these things were just about to happen; often a matter of a day of two changed them.

Through that period, I had approached quite a number of people to see if I could get them interested at all. It was always the same thing. You can't get anybody to evaluate what you have in a workable way. Invariably, if an outsider who doesn't understand wants something evaluated, he tends to call in a competitor, somebody that's working for another company, to have a look at it and tell him what it's about. An example of an evaluation that took place was when we approached the man who had the money behind Marchant, I believe it was. He was a nice old gentleman who would arrive in a Cadillac limousine with a chauffer to talk to us. Our labs were set up in a World War II bomb shelter with no windows whatsoever. Finally he decided to retain Cal Tech to evaluate this.

The head of the electronics department at Cal Tech really didn't know much about computers, but one of his ex-students, Bob Johnson, was a computer designer at Hughes. It was a little bit ticklish to have a guy working as a computer designer at one place evaluating designs for another specialty that was so critical to us. Bob Johnson was a very nice guy, and when he came to look at the machine I found out some DDA background that had gone on that I'd never heard about at all. In the early days of DICO when still working with Marquetti trying to get something else going at MIT, a purchase order came through that pay my way to go back as a consultant. It was the early part of Project Lincoln and it was still on campus. I believe I arrived there Tuesday, but I got to Boston late because the shuttle plane was grounded for awhile. By the time I checked into my hotel, it was about two in the afternoon. I thought I really shouldn't talk to them that day because I normally bill them by the day—that's they way it was authorized—and it seemed like sort of a gyp to bill them for a whole day. I thought I'd log in with Bill and just not bill them for that day.

As I walked through a door, a couple of nervous guys rushed up and grabbed me, "You're late for your seminar." As I walked across the campus it didn't seem right to say, "What seminar?" It seemed I'd been billed for an MIT seminar. I was shoved into this room with hundreds and hundreds of students all lined up around the walls. I'm not exactly sure what I said. I never found out who played this trick on me, or whether it was just a complete cross up, because no one ever mentioned that I was supposed to give a seminar. I discussed the MADICO (maybe it was the DICO-20, anyway it had 14 flip-flops), and I explained that we could now do digital integrators at a good deal less than one flip-flop per integrator, and that it wouldn't be hard to push it as far as one quarter of a flip-flop per integrator. So the digital device, on this basis, compared to the analog integrators, was actually pulling ahead. Then I went over the business of simplifying the memory. Something like that was what I discussed, I'm sure.

This caused great racking around. At that time both North American and Hughes were building DDAs for Air Force missile guidance, and apparently the announcement that it only took a total of 14 flip-flops to do a complete DDA created an unexpected sensation. Somebody told me that it went all around NATO and there were arguments about it. Finally, of all the crazy stunts I ever heard of, they had North American and Hughes experts on hand. They came from Hughes; they came from North American; they came from Bendix. And they all sat around determining what was the minimum number of flip-flops needed to build a DDA. They came up with the figure 34. To let me off the hook, rather than saying I was a liar, they said it might be possible, with new and untried exotic circuitry to cut this down a little.

Bob Johnson had been at the conference, so when he came to evaluate our machine he was of the opinion that he was supposed to evaluate exotic circuitry. It turned out it was standard circuitry; Hughes or anybody else's, and by that time it was down to twelve flip-flops. He was very upset and went back and needled the Hughes boys about it.

To show how evaluations go, he fed his information back to the Cal Tech professor, who put the report together. It's always the scientific approach, a little of this, a little of that, the best and the worst. The Cal Tech prof was of the opinion that we were unwise to use unregulated power supply, which may or may not have been the case. I wanted to try it to save money. It didn't occur to him, of course, that the Marchant man knew nothing about these. The man from Marchant got a report saying there's some good and there's some bad. You go to the guy and say, "Here's a revolution," and then he gets the report back, "well, there's some good features and some bad."

The man decides it isn't a revolution, and that's all he derives from it. He decided he wouldn't go ahead with it. That was just a few weeks before Litton became interested. By chance the chief Litton patent guys got assigned to check this out. They knew digital patents so they knew what they were seeing. They were natural enthusiasts which was helpful. Otherwise, I wouldn't have been able to sell. Litton was just getting started; they were a tiny company then.

MAPSTONE:

Was this their introduction to computers?

STEELE:

Yes. Have you seen the write-up in Fortune? It had something about DICO and Litton.

MAPSTONE:

No, I don't think so.

STEELE:

I might have a copy here. DICO was their first acquisition in the computer field. Of course, the great advantage with DICO was they acquired this big Navy R & D contract, a most prestigious one.

Finally, to get back to the DICO-20 again, they came and took it back to Beverly Hills to prototype, again when I was away. It was cut up completely and started over. This is how irrational the field is. The reason they did this was that none of them had looked into the low-level flip-flop to see how important it was. It happened that the low-level flip-flops as copied by NEL were actually etched up for them by us, had a years field try out and worked just fine for a year. They were pretty well proved out and had no problems.

MAPSTONE: This was Litton?

STEELE: Yes. They never did check into the electronics. Their notion is,especially from talking to Convair and they checked all of this. They talked to Convair, they talked to NEL, and every place they'd call they'd get the same story: "Steele's a great designer, but his electronics are terrible." They grabbed the device and even copied the electronics, but they didn't know it. The people at the bottom knew, but the people at the top didn't know it was the same electronics. Their rationale was that they had to take over the electronics and do it themselves. By calling around, instead of checking for themselves, they were convinced that the electronics in DICO-20 must be very poor and needed to be done all over again. They hired an electronic character from the East who designed the first reservation computer for the airlines, a real brute force thing.

MAPSTONE: Who was that?

STEELE:

John Conway. They redid the electronics on DICO-20. Instead of using the double triode, 2 tubes in 1 economy 80 cents version which I had been using, they decided to use the most premium tubes available. These were vacuum tubes used in the transatlantic cables. I'd never heard of them. They got these at a fantastic price of something like $20.00 a piece and used them in pairs. Then they decided that the customer needed to be very impressed, so they got a great big cast aluminum case with leather panels and a lot of very impressive things. They went back up to the high level switching, so power supplies came back. When they were done they got it up to 88 pounds, and a price of $13,000 instead of $500.

As soon as they tried to sell it, only computing centers were approached, because companies won't buy a $15,000 computer for an individual. Computing centers, of course, wanted more input and output. Even so, they sold maybe 40 or 50 of them, and took them off the market. That ended the whole round of DDA activities. No attempt was ever really made to get the thing out at a low cost.

Early in 1954, the people I had in DICO all moved to the Litton payroll, they were on Navy payroll actually, and it became the La Jolla Labs.

MAPSTONE: Because this was Navy contract?

STEELE:

Yes. Then the La Jolla Labs under Litton began to gradually dwindle away. They would hire the guys to come to Beverly Hills. It dwindled down to two people besides myself. In short order, they gathered a huge mass of people hired away from Hughes primarily, and North American to spend Navy money in a hurry. They evolved almost overnight back to the GP approach, so the Hoover contract finally lost out.

I did get Hoover himself interested in micro-miniaturizing again, and as a point to try and convince him, I put a whole flip-flop in one of these things at the end of a pencil. It made a nice case, and the little prongs went in the bottom. I had the guy working for me who had been a jeweler, and he did very fine work with his hands. I had him take transistors apart, take the case right off, solder the things up, and carefully mount them in the pencil end. After two or three tries when they would sort out, we got one that would plug in. Hoover went all out on micro-miniaturizing and gave out a whole lot of contracts. He was the one on that program who started the whole direction toward integrated circuitry. With the Navy going full-bloom, then the Air Force moved in, too. That was in the beginning of the deposited etched film circuitry.

MAPSTONE:

Litton got the contract?

STEELE:

No. Because I had interested George Hoover of the Navy, Litton did some work in this direction, but several other companies which were in the program also did some work, and it began this national effort in integrated circuitry. It really began in that area, George Hoover should be given a lot of credit for having pushed it along. The Air Force, which was funded in competition with the Navy, came through in the long run, but it all began about this time.

MAPSTONE:

Who did it for the Air Force?

STEELE:

Oh, several. Texas Instrument; a company up in Santa Barbara, I forgot the name now, did the film magnetic circuitry.

MAPSTONE:

TI usually gets the majority of credit.

STEELE:

I think so. I don't know who started this Moss circuitry which is just coming in. It's another jump in the whole business. In the last two or three months, registers are available with 1024 bits all etches on in steps and circles; it's like one channel off the drum without the drum. Now the question on these things is: can you get the 1024 bits, however simply conceived as a pattern or drawing, etched into a position. Can you get all these parts to behave as if they were many parts. There's a curious difference because the expectancy of failure if they behave as if they're many is quite high, whereas if they behave as one part they may run as well as a transistor itself. It's a funny conjecture, and I'm anxious to see how it turns out, I believe they will work reliably. That begins to open up the loops.

That really gets to about 1954 and covers some of the high points.

MAPSTONE:

Did you stay with Litton?

STEELE:

Yes, I stayed with them until 1960, although the program in La Jolla was running down. I kept pushing work along on this modal computer. It could have started in 1952, but by 1962 I had it to the point where it would do almost all the things. I checked out a lot in the lab and it certainly was ready to go. I could never get Litton to back it.

They wanted to use me as a technical salesman and I got hooked on this several times. I'd go out and sell the military on new things that could be done. Often they would go to great pains to evaluate these ideas and finally, when they decided that these ideas could be done, and that they were new and different, I'd sell the contract. Right away, when the money came in, they'd hire more people and run it through the conventional way. Several times I didn't know a contract had come in until a year later. I sold the contracts and, supposedly, they were to come to me; at least I thought they were. A lot of things I was supposed to be getting done, didn't get done although originally they were sold as if they were my work, which they were in a way. This created misunderstandings from time to time. For example, the Litton-20 that came out didn't resemble the DICO-20. I felt rather badly because I'd given so many presentations and told so many people how simple, light and cheap it was, and then it came out way, way off. T

There were several other computers done at that time. There was the advanced MOAT Computer, an advanced mortar trajectory device incorporating most of the radar. It never really got going. There was a large DDA that was designed and never built; Ralph short detailed it for me. He became a designer at Convair Covina. I did a one-channel, slow but highly serialized general purpose computer for evaluation and forwarding data. In a rather general sense, if you have a place that's collecting data from various sources and then you want to run that out on various wires—people were putting GP's to work, sort of combining them, looking up where it should go and then forwarding. I put all of that on an acoustic line, which is the smallest I'd gotten, yet it still wasn't one flip-flop. It was only three flip-flops, which was a little better than twelve.

Then I did a machine called Trim Ten, which was supposedly for the Navy. That wasn't build because they had already started building a general purpose computer. It was equivalent of the servo multipliers; one long and one short channel had about 40 servo multipliers; and that would have given some practice on how they work. Of course, they were about the same speed as the servo multiplier but more accurate.

MAPSTONE:

These were the machines you were designing while you were at Litton?

STEELE:

Well, they got underway with DICO and were on the boards or could have been done by 1954 when we went to Litton. During 1954-59 none of these things really got underway.

In 1962 I did a GP for Northrop which was farmed out to me in secret. The reason was that Freddie Stevens and the boys at Northrop were still so sore from the problems associated with the computer group clearing out. The people at Northrop who had given the computer project so much opposition, became the most decided anti-digital people going. North American then moved ahead in their area with the things Northrop pioneered and could have done. When they needed the ground support computer they came to me. I designed a 24 flip-flop machine including the printer.

MAPSTONE:

Was this the Tabula Rasa?

STEELE:

No, it was like that, but it was not as good. It didn't have any frills, it was straight binary with fixed points. There was just one equation to be run off and then put into QUAC. They wanted to get the constants to fill QUAC, which would then interpolate. Consequently, it didn't need floating point, it didn't need lots of things, so I left them out. I didn't out the microprogramming part into it.

Then I did an inventory machine. I got the specs from a great big folder some company put out that was selling what they called a fifteen GP machine. (GP stands for girl power). This wholesale company specialized in Christmas sales and they had an acute inventory problem. Girls opened the orders and it had all been handled by paper. The company wanted the girls to have keyboards and enter the orders right into the computer. Then the computer would tote up each day how many of each kind of item was out so the could reorder. Arthur D. Little designed them a 1400 flip-flop machine and they got so enthusiastic about it, they decided they'd sell the computer on the market.

MAPSTONE:

What was the company?

STEELE:

I can't remember, but it was Arthur D. Little based on their requirements. They took the job being handled by people and generalized it into a machine. I did a design that would do the same thing with the same number of girls, and that was 15 flip-flops. That was quite a coincidence since they called it the 15-Girl Power machine. That had keyboards and everything and was heavily serialized. It closely resembled what I did for Northrop, which was a straight GP, but the memory was organized serially.

MAPSTONE:

Was this machine built?

STEELE:

No, none of these were. They were a bunch of loop machines that could be done today.

MAPSTONE: The Northrop on wasn't built either?

STEELE:

Yes, it was. They raised the complexity sadly, because from development they just didn't want to bother and they threw in a lot of flip-flops. Even then, I heard through Litton later on, the Air Force was very enthusiastic, apparently. Northrop wouldn't even invite me to see it running after they built it.

MAPSTONE: You were so bootlegged at that point?

STEELE:

Litton found out about it later and they re-advised Wright Field. I redesigned the Norden Bombsite on paper, talked the Norden Company into a small contract to digitalize the whole thing. The bombsite was a mixture of every kind of thing you could imagine and had broken down entirely. I redid it. In fact, it was, also, an advancement, since I did some intermediate things with the navigation we didn't usually do, so it was an interesting study. As a matter of fact, they only wanted me to do a piece of it that would help in the navigation first, and we actually did the whole thing. I finished it up one night, zeroxed what I could of all my work sheets, and then went back on the plane next day to go over it with them. There just wasn't time to write big reports on a project that large. I went back and spent 3 days telling them what it was about. Apparently they didn't get it, and decided it wasn't worth anything. They had all the commercial patent rights, so they filed two. They wanted their $7,000 back and didn't want the patent rights at all. This really clobbered them later, because once Litton got hold of all this, the story got out and apparently went all around the country of how they reacted so unfavorably. I remember at the time they were in financial troubles. They sold out to Gordon Kitte. Litton just loved this because they paid Norden their

$7,000 and cleaned up all the patents.

All these things were always getting ready to go, but there wasn't the time or the manpower to make a nice showing. Even if the project was hollow, people were used to magnificent demonstrations being made.

MAPSTONE:

In each occasion the pitfall seems to have been that the idea wasn't presented with the fanfare and trumpet.

STEELE:

Yes. And there really wasn't anybody who could evaluate ideas. Everybody took the opinion, "Gee, if it were this good, somebody would already be doing it." of something of this sort. It's one of these things where as soon as somebody got on the bandwagon, then everyone began to get excited and went great guns.

I'll never forget that trip to Norden. I'd been up all night working on the stuff, and when I went to their place I spent a total of three rather unpleasant, hectic days. Most of the time, I was arguing with them. I got lower and lower on sleep. On the third day, I developed double earaches from the winter weather and all. Finally I went to New York. When I got off the train, I was really feeling depressed, blue, sick and running a fever. Staggering with my suitcase, I get on the escalator going up. It's a long, slow escalator and gradually it dawned on me as I looked around that this was a line of prisoners; I suppose they were deserters. They had on civilian clothes, they were in a long line, and they were handcuffed or chained. I was to embarrassed to see exactly how. At the front and the rear there were armed guards and I barged into the middle of this line. All the way I thought, "Will they shoot me if I get out of the line? Will they think I'm escaping?" Finally, I thought, "This is too much, I'm going to walk away, let them shoot me." (laughter)

MAPSTONE:

They didn't?

STEELE:

I think they had noticed. That was funny.

MAPSTONE:

Were you living in New York at this time?

STEELE:

No, in La Jolla.

MAPSTONE:

Oh, you were commuting.

STEELE:

DICO was founded at Playa del Rey very briefly, and then we moved to La Jolla. It grew to a fair size. Quite a lot of people worked for free for awhile, and I trained them. We gradually got income for a payroll. Some of those people have become computer designers, some of them later became vice presidents of computer companies. I trained Jim Price, jr. Ralph Short became a designer for Convair, Bob Buzzard went back to MIT and became a Lincoln Project designer. I trained Don Nelson at STL. Not one of them had any technical background at all.

MAPSTONE: You really trained a lot of people and got them in the field.

STEELE:

Yes. Every time I start, I get untrained people who have enthusiasm. At least it's great fun, because they get very excited when they find out they really can. Of course their next attitude is, "Is this all?" At least all of those men have had careers out of it, and almost invariably have done technical things and remained in the field.

MAPSTONE:

In a way you've been a great teacher.

STEELE:

Well, not a great teacher.

MAPSTONE:

A teacher?

STEELE:

I had no other choice.

MAPSTONE:

Tell me about the patent. Though these various skirmishes, have you acquired patens in your own name on any of your technology?

STEELE:

Gradually the problems have cleared. The MADDIDA patent is going into public domain now; it's been out for 17 years. I have a whole bunch of improvements I could file and they have disclosures in them. This means I can get a couple of year protection.

MAPSTONE: That's recently.

STEELE:

Yes. It's possible, you see, to do a 4 flip-flop DDA with 80 integrators, which is four times the MADDIDA, but the 4 flip-flops also handle the graph plotting servos, they're the interface for it to boot, so it does quite a little more. By going to 7 flip-flops you can extend this to 300 integrators which would be very different in capacity. That would plan a whole flight of trajectory.

Automatic scaling is something I've been able to so since Northrop days. Everybody always wanted it, and I guess a lot of people have tried, and that ought to be included in the DDA which is a computing center type and more expensive than most individuals can afford. There are a bunch of improvements one could do in DDAs. I've got some disclosures in. With the people here, one could develop a second round of improvement patents on the DDA, and get a hold of them. Although if anybody finally wants to or not, I don't know. I have a lot of stuff in the 1960s on the automatic control of the Modal Computer.

MAPSTONE:

Do you have the patents?

STEELE:

I have disclosures. It costs too much to go ahead and file a patent unless somebody is interested. Disclosures are a great idea. You file for ten dollars, and it gives you time to promote to see if you can get somebody else to go ahead.

MAPSTONE:

It protects you through that two years. Then if you want your disclosure is covered and you can go ahead.

STEELE:

Yes. Of course it gives you time to see if you can get enough money. Patents are pretty expensive. Not only does the modal computer have its own set of patents for all the little peripheral pieces as well as the central device, every single machine which tied into it needs to be redone on a rather sweeping scale. Most industrial machines today took their form during 1890 to 1910 and haven't been really changed much since. All the original patents have expired and the people whom manufacture now do so on about third generation refinements. The changes that one would make in each industrial machine would be big enough to give you a fairly commanding patent on that machine itself, as well as on the computer which went with it.

MAPSTONE:

I know there is no written documentation from the Northrop days but do you have reports that you might have written or proposals of some of the ideas, for instance, DICO-20 or MADICO, from the later period?

STEELE:

I gave two papers, one to an aerospace conference that the Air Force set up. One of them was a brief summary of non-numerical computation but it didn't do it justice because I had to throw in all the math I could.

MAPSTONE:

When Litton bought DICO, did they pick up all your papers?

STEELE:

They kept DICO as a corporation, instead of dissolving it and merging into themselves, as they usually did. They kept it as a patent holding corporation and charged the Air Force royalties. I had learned, and this is completely without foundation, that at a fairly early date they had already picked up twelve million dollars in royalties from the military on DICO patents.

MAPSTONE:

You could be a rich man.

STEELE:

These were the only digital things in the country the military ever paid for, so it didn't have the rights on them, Litton very carefully kept those squared away and kept collecting the money.

MAPSTONE:

Was your name on any of these patents?

STEELE:

It’s on all of them. Only my patents were kept in DICO, as far as I know.

MAPSTONE:

Do you have listings of your patents?

STEELE:

No, I don’t. Maybe, D.J. has a few, I don’t think I’ve ever read them. As I told you, I have wondered about that MADDIDA patent because I got some weird looks.

MAPSTONE:

Can you think of anyone at Litton who I could contact who might help me get the files?

STEELE:

The guy who knows the whole early story is Seymour Scholnick. He was the one of two guys who discovered DICO. An ex-Hughes patent writer, Hugh Millis, was working for me.

MAPSTONE:

That’s the name I wanted to ask you about.

STEELE:

Yes, you should talk to Hugh. He was early in DICO. I hired him away from Hughes as soon as I could meet a payroll. Sy Scholnick was early and so was Seymour Rosenberg.

Seymour Rosenberg started as head of the patent department for Litton and worked up to V.P. For a long time he was with Mattel, the toy company. He and Seymour Scholnick were both hired away from Hughes.

It’s funny, Seymour Rosenberg had an interesting background in all of this, but I’m he doesn’t like to discuss it. I hired a patent man from RCA part time. I always had a dickens of a time training anyone to write patent circuitry but he just couldn’t catch on to this digital thing at all. He wanted to take a little time off and go up to Idaho. When he came back he had written the patent and it was brilliant; it wasn’t just good, it was terrific. I though, boy that mountain air is what did it. It turns out he had gotten in touch with Seymour Rosenberg at Hughes, who was probably the top digital patent writer in the country for years. Out of sheer interest, Seymour had gone over this stuff and written it up because he liked it. He then got very interested in the computer he was writing up, so he kept track of it more or less. They let Hugh Millis come to me without rancor, and Rosenberg kept in touch. It was through this connection with them. That Litton brought in. They know the story from the beginning and know the patent story cold. He was an officer at DICO for a long time.

MAPSTONE:
Where is Seymour Scholnick?

STEELE:

They're both around L.A.

MAPSTONE:

Are they with Litton?

STEELE:

No, neither is with Litton now.

MAPSTONE:

What happened with John Matlago? Did he stay with you for any length of time?

STEELE:

He stayed with CRC. I don't know if he stayed on with National Cash. It's always been quite a problem to write good computer patents. Like everyone else, patent attorneys vary in abilities enormously.

MAPSTONE:

There's a great story that Cuthbert Hurd tells about John von Neumann. He came to Hurd one day and said, "I need a patent attorney." Hurd called up some guy and said, "Take care of this mathematician I'm sending you." Later the patent guy called up Hurd and said, "What are you doing to me? This crazy guy brought me a patent and it's all equations. I can't read a word of it."

STEELE:
It really is quite a task. I was always interested in the fact that these steady-flow, continuous state, non-numerical things must be fairly close to the way animal mechanisms work, and endless flow of selections without generating numbers. The original non-numerical was worked up to be equally spaced ones and zeroes in time so it was going on with a clocked memory. The neurons aren't 'clocked', they run at their own rate. I took the time once to see if I could design a circuit that would do it probably the way the neuron did. I've always had a hunch, one might call it an axiom, that if you have two mechanisms that do the same thing, they are alike in principal even though they may vary vastly in detail. The principles of propulsive action are the same, regardless of the way you go about it. Therefore, if you know what it is the animal mechanism does, what it achieves, but

you don't know how it does it, take a circuit that you do know how it works, design it to do the same things, and then compare. I got started on that as a hobby and worked up a method of computing by impulses spaced in time much like variable block, and oscillator.

It turned out that you can do all the same things with this that you can do, let's say, with the DDA or the triades of the geometric process. This gave a very simple model of a digital electron. If you look upon the electron as being a thing which is constantly fixed, a thing that flashed into existence in time, in fact, you assume that time is divided up into successive intervals of the present. If you think of the electron as moving backward and forward through the present rapidly, at least the electro-magnetic part of this where you actually sense it, you find that each time that it appears in a sense you can think of it as creating an impulse, and you can actually compute with them and do all the things that you can do with the DDA with virtually no mechanism at all. If you set it up so that your electron is actually operating this multi-level present, and in a sense is clocking its way across space and its motions, you get a circular thing, you get wave action, you get all this stuff with it and it will behave according to the requirements of relativity, but for a different reason.

That was all worked out in about 1951. I tried a couple of circuits but there wasn't any vacuum tube circuit that was really good. Since then the uni-junction transistor turns out to be exactly like one of these. I haven't gotten back to fool with it yet.

MAPSTONE:

Is this something that you might start getting into with your new lab?

STEELE:

No, it's not that practical, although one could do analog circuits with it; get the equivalent of analog multiplier. If you actually took sets of these and did the logical designs for known animal functions, I think you could learn about the nervous system on that basis: regardless of how it's mechanized it does the same thing if it has the same inherent characteristics. This could be a method of medical analysis. It also gives a new model for digital physics which has a lot of theoretical interest. You could build a model but you really don't need to if you can do it on paper. It has a high theoretical importance to two different directions, and it might be practical. I always wanted to get rolling one way or another but it never struck anybody as being worth pursuing, so I always have to do it as a hobby.

Those are the main things I can think of that got going at that period. Very few of them ever got out. You might say that the lower level of the DDA keeps popping up again, because people know what it is, but all the other things that turn into the Modal Computer just haven't been accepted yet. These various things all became available between 1950 and 1954-55.

MAPSTONE:

Apart from you book, *The Crisis We Face*, have you published anything else?

STEELE:

No. I'm working on three mathematical theory papers which I will send off in the next few weeks. They're not computer papers. I've had such lousy luck with computer papers, I kind of gave up. In 1957 I was all set to get a paper off to some conference. It was about moving behavioral complexity of an automatic system into a serial memory. For a long time I referred to the use of memory to replace other mechanisms as memory mechanisms. I sent back a synopsis of the paper I wanted to give and I'll be darned, they turned it down. The guy didn't read it carefully, I guess, and thought it was on ordinary memory circuitry or something. This was supposed to be a logic conference, and because he didn't read it very carefully, he thought I was in the wrong conference. By the time that was straightened out, it was too late to get going.

MAPSTONE:

How about some of the people? Over the years you have probably met most of the people who've been important and shaped some of the computing directions. Have they influenced you in any way? If so, who would they be, and how? For instance, Mauchly came out here, didn't he?

STEELE:

Yes.

MAPSTONE:

Did anything happen for you when he was here?

STEELE:

No. I think at one conference von Neumann gave a talk, Weiner gave a talk, and Shannon gave a talk in the very early days. I think I learned most from listening to von Neumann because he had the courage to make it very straightforward and simple. He used familiar analogies and explained the computer without any mathematics at all. The analogies he used were useful and thoughtful. Actually I learned more from Galileo and Aristotle, but I learned a good deal from von Neumann. I learned something from von Karman one time in just one day. I took a course under him and during a two-day period I learned a great lesson. What you really learn from these people is how you go about thinking, rather than the details of what you think. In other words, this is what you today learn from Galileo better than you learn at any university: how you go about thinking about a problem. This is why Aristotle is so good, even though some of his results aren't so hot. He shows you how to sit down with nothing to go on at all, and begin to think accurately, rationally, precisely, neatly and with a slight sense of humor. I learned a lot just from listening to that paper by von Neumann because he was so lucid, so easy going. It was the fact that he appeared to go to some pains to look at another way of looking at things.

I learned a lot from General Seville about how to go about thinking. Of course, I read Shannon's paper on relay logic, which was very nice work, and I profited greatly from Bush's work on mechanical differential analyzers. Bush's work especially, and Hartree's secondarily. Bush's works are classics in clarity and utility. Now we have processors of all kinds, but there is no background and it really takes a long time to learn how to couple and use them. It took Bush five years to work out all the rules of generating functions for this particular work. Until you've gotten to the position where you can't have a Bush to draw on, you don't really appreciate him. I never got to meet him and I always hoped I would. I talked to Caldwell one time. Somehow things like this MADDIDA business always circulated around in curious fashion. Every once in awhile I'd run into some guy, like Caldwell, and it turns out he'd be sore as hell at me.

MAPSTONE: W e didn't put the statement on tape that MADDIDA does not refer to Ida Rose.

STEELE:

All right. In fact, I didn't hear that for a number of years. Someone told me ten years after it happened that the story had gone all over the East and I could hardly believe it.

MAPSTONE:

Ida Rose, we hereby claim that the term MADDIDA is nothing to do with you.

STEELE:

I had never heard of her at the time that I began to go East and talk with computer people.

MAPSTONE:

Did you have arguments with computer people, apart from your Army-Navy guys, about your approach on Boolean logic and whittling down the computer? For instance, did you ever meet Jay Forrester, who built the biggest computer in the world, practically, and talk to him?

STEELE:

Yes, I did and he gave me a rough time. I finally got mad. After MADDIDA appeared, I tried to get the Air Force to go one; then the ground staff which fell apart at CRC; then the bomber. I talked to everyone in the Pentagon about a digitalized autopilot that went to J. B. Rea, and that was a fiasco. I talked to these people many, many times, and the next thing I was constantly battling was

everyone would say, and Jay Forrester seemed to take the lead, all I was doing was selling warmed over DDA. What I was trying to do was sell all methods around it. He proposed that it didn't really matter if you got the computer simple, because your peripheral equipment was going to be so deadly that if you cut the computer down to nothing, you'd hardly be ahead. This also wasn't true because at the time I was bringing all these pieces into the net so there was no interface at all. I'd gone to great pains to try and bring all these units into play. I was always battling this, and the only way I was going to prove anything, even if I built a small computer that people noticed, I would have to tie it into things that needed a big contract. I argued long and loud with Forrester one day and I got mad, which I shouldn't have. Then Caldwell got mad at me. Fortunately I didn't get mad at him. He apologized later. That's the thing that made it doubly difficult. Later on hersuddenly apologized and said he knew I didn't mean it and became more friendly. But I never have found out what it was that I was supposed to have said. At any rate, I'd always hoped I could talk to Bush.

MAPSTONE:

He is still alive.

STEELE:

He is?

MAPSTONE:

Oh yes. Did you read his most recent book?

STEELE:

No.

MAPSTONE:

Pieces of the Action?

STEELE:

He is still writing books?

MAPSTONE:

Pieces of the Action came out in 1969 or 1970. He's still alive so maybe your opportunity will come. Did you ever meet Aiken?

STEELE:

Yes, our first meeting was very bad, the second very good. He came up sort of an entourage at the Rutgers conference when I was running a problem on the MADDIDA. Apparently he had hopes himself of getting computers smaller and simpler, and somebody flagged him down and brought him over. Probably, he was arguing with them, I don't know. Anyway, they were trying to surprise him. Instead of stopping to talk or ask anything he said, “It isn’t what I had in mind at all, not at all.” And he just went walking off. He never inquired what it was.

MAPSTONE:

That was the first time.

STEELE:

Yes. Several years later when I was at DICO, he came through town and called on me and we had a marvelous time. He showed me some work he’d done on the trinary system, and I showed him what I was doing. I was very pleased and it meant a lot to me. I think he’s a crusty old coot with a heart of gold. At any rate, I thoroughly appreciated that. Since then he’s retired.

Apparently he had the same general struggle; if you’re a computer designer and you have a lot of guys working for you, you have a dickens of a time to keep the things from dwindling because it’s so easy to eke out difficulties by just throwing in some extra pieces. It’s so hard to stop and do it right. Apparently he’d had this trouble too, and that cheered me up.

END OF INTERVIEW