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**Interviewee:** Everett Yowell

**Interviewer:** H. S. Tropp

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**TROPP:**

This is a discussion with Dr. Everett Yowell in his home in Dayton, Ohio on the 25th of September 1972. [Recorder off] We start with your own career and how you got into astronomy, because you mentioned your father was an astronomer. It might be worth going back--

**YOWELL:**

That's the, of course, very simple and obvious thing. My father was an astronomer and so I inherited and was exposed to his interest, and set out to be an astronomer from the time I ceased wanting to be a streetcar driver, something like that.

**TROPP:**

[Laugh] Where was he located?

**YOWELL:**

He was at the University of Cincinnati.

**TROPP:**

Most of his active career. He was four years with the U.S. Naval Observatory, and he taught mathematics for four years at the Naval Academy; back from 1901 until 1909 he was in the East. But, outside of that, the rest of his career was in Cincinnati. And so as a result I had the basic astronomer's training, which included computational orbits of asteroids and comets, and the need to get into computing. I had a major in mathematics at Cincinnati as my undergraduate preparation. A little bit of physics and a little bit of astronomy formally, and a lot of it informally. I went to Columbia in 1941 as graduate assistant in astronomy. And when I got there, of course, one of the interesting things was the Thomas J. Watson Astronomical Computing Bureau, which was housed along with the department on the top floor of the Pupin physics labs. And I had very little to do with that for about a year. Then, with the advent of the war, in 1942, we started doing more research projects there and I was sort of drafted as an operator during the summer of '42 and continued working there full-time until the war was over in '45, when I went back to my astronomical studies.

**TROPP:**

Was Schilt in charge of the--

**YOWELL:**

Lester Schilt was head of the department there full-time. Professor Schwarzschild was away in the Army for four years. I officially wrote my thesis under Schwarzschild. He was away the last year I was there, 1948, he'd already moved to Princeton.

**TROPP:**

And during that same wartime period, Wallace Eckert, you said, was at the Naval Observatory.

**YOWELL:**

He was at the Naval Observatory.

**TROPP:**

I know one of the things he was working on had to do with the calculation of tables so that the pilots who radioed in, or people radioing the location of submarine, could get an immediate fix and get to them. Were you working on that same problem?

**YOWELL:**

No. We had two major problems there. One of them was computing of bombing tables. Given the shape of a target, precision of the bomber, and precision of the bombing system, what was the probability of getting a hit? And, at least one bomb with a strain of n, or at least two, at least three, at least four, at least five, so they could plan the missions properly, know how many planes to send out, and the rest of it. And that was nothing but a long, involved series of integrations over the probability of distributions.

**TROPP:**

You really weren't getting into the area of operations research analysis.

**YOWELL:**

No, no operations research analysis. It was just long, arduous--we'd sit there and run those multipliers day in and day out, and sum them up on the tabulators for the integrations. And the second problem was the evaluation of the gun controls for the B-29, where the--

**TROPP:**

That's right, I heard about that problem. Yes.

**YOWELL:**

And that was a real messy one. Primarily because it wasn't really adapted to punched card use. We'd get in 200 frames, which were taken with cameras now where the guns were, and we'd have to go through, oh, it must have been four or five hundred steps of the computer including work-ups with tables and artillery mils, to the nearest tenth of an artillery mil, and inverse look-up for arc sines and arc cosines. We'd run 200 cards in a critical merge against a 5,000 card table, and it just wasn't an efficient punched card job but it was more efficient on punched cards than it was trying to do it by hand.

**TROPP:**

[Laugh] This is the project I think Ben Wood mentioned that ended up with a central control of all of the armament on the B-29.

**YOWELL:**

Yeah. And the real problem of evaluation was that the gunner at the sight was remote from the gun.

**TROPP:**

Right.

**YOWELL:**

So, you had that offset, which hadn't been done before. So those were the--

**TROPP:**

You also had different kinds of guns on the B-29.

**YOWELL:**

This I don't remember.

**TROPP:**

They weren't all the same. You had some 50 caliber machine guns. You had some--

**YOWELL:**

This could be, but the parameters were plugged in, and I never knew what they were. So,

these were the two big jobs that we did.

**TROPP:**

According to Professor Wood, that was a really effective change. It really made the B-29 much less vulnerable to attack from the outside by fighter aircraft. Now, he quoted some figures and I don't know where the data comes from.

**YOWELL:**

Well, he was in the group that would be evaluating the results. I was just grinding the numbers out and didn't know anything about it. The only anecdote I know really about the usage of those tables was one of old Professor Schwarzschild who was with the Army in Italy at the time in operations research. One of the jobs they gave him was to calculate the tonnage of bombs needed to close the Brenner Pass. And he wrote back if we could send him some information, which we, so we ground out a few special tables and sent them over to him. And he wrote his report. And, as I understand it, it came out with such a high tonnage that the command refused to accept it, suppressed the report temporarily until they closed the Pass. And then they looked back and found that he had underestimated by ten percent the amount of bombs required to close it.

**TROPP:**

[Laugh].

**YOWELL:**

And then I understand the report got published. At least this is the story that we heard around Columbia soon thereafter.

**TROPP:**

[Laughter].

**YOWELL:**

But, those were basic tables. We used them for all sorts of things. I know one of the men at the time was varying them a little bit to calculate bombing bridges which were extremely long and narrow targets. So we did some rather minor variations to get into what would happen if you were trying to hit a dam and, well, just minor variations that came out.

**TROPP:**

But, they were still primarily from a probabilistic point of view rather than from the standpoint of game theory.

**YOWELL:**

Always, we were always basically integrating a double probability distribution. Lillian Feinstein was in charge of the bureau during the time, all that time. She came in when it was first started and didn't leave until about 1945, when she got married.

**TROPP:**

You might get her married name on.

**YOWELL:**

Her married name is Mrs. Harold Houseman.

**TROPP:**

Right.

**YOWELL:**

And I was there. We had a couple of students who would come in and run the one shift. I'd take the midnight shift, Lillian would take the day shift, and a student to come in to run the evening shift. When we got the B-29 project, brought in six or eight more people. I can't remember.

**TROPP:**

I have a feeling that with that many people and the kind of equipment you were still doing a lot of hand calculations.

**YOWELL:**

We were doing a lot of hand calculation. And, except for Lillian and myself, the rest of them were strictly operator personnel. I got, it's about that time that I started really playing around with plug boards and really finding out what was going on inside the punched card machines. And I got a great deal of encouragement and guidance, really, from talking to Paul Herget a couple of times when I'd see him in Cincinnati, because he had joined the staff at the University of Cincinnati right after he got his degree at Berkeley. He was a Cincinnati man. And so I'd see him, knew him well, and see him every time I went home and he happened to be home, and he told me some of the things he was doing at the Naval Observatory. And then he got me interested in the insides of the machines. So it was sometime in '46 or '47, I think, that we first started doing some of the unusual things with punched card machines, when I found that I could do a lot better if I could round with four-tenths instead of a five-tenths factor, and induced the serviceman to change the timing on the round off cam for me.

**TROPP:**

As long as Watson didn't know you were doing it [laugh].

**YOWELL:**

As long as Watson didn't know we were doing it, everything was fine and dandy. But, once we got back to peacetime work in '46, we got into standard astronomical calculations, which, at that time, was primarily a photometric catalog of the northern hemisphere, which was being done at Columbia and Yale as a joint project. Yale would gather the observations. We'd measure them on the photometer at Columbia and then do the reductions on the punched card machines.

**TROPP:**

That photometer was one of Eckert's developments, wasn't it? Or was somebody else--

**YOWELL:**

Nope. I don't know who made the old photometer, but the one we used after the war was one that was designed by Schwarzschild more than anybody else, as far as I know. He was extremely instrumental in the design of it.

**TROPP:**

Well, you mentioned that you did your dissertation under him.

**YOWELL:**

Right.

**TROPP:**

In what area?

**YOWELL:**

Variable stars. It was officially "Period Variations in draconis' Stars."

**TROPP:**

All right [laugh].

**YOWELL:**

And the intriguing thing about it is--you may be aware of Cepheid variables of extremely constant periods. This class of stars, and there were only four at the time known, were short period, half day stars. But their period would change in the fifth or sixth place irregularly. And I was taking all the observations I could find in the literature, plus some I gathered myself in the summer of '46, reducing them on the punched card machines and trying to find out during what intervals of time the period was consistently constant where changes occurred. And the data just wasn't sufficient at that time. And I still don't know whether it is, to determine whether these changes were really short, over a short interval of time followed by long intervals of constancy, or whether they were relatively slow changes. And I'm still not sure whether, what type of change was occurring. Nor yet what the physical reason for it would be.

**TROPP:**

Do you know of anybody working in that area currently?

**YOWELL:**

I am not sure, because it's been so long that I no longer can follow the astronomical literature. There's simply too much I've missed in twenty-five years. But, Wolpshure [?], the present director of the astronomy department at Columbia, was interested in the same problem at the same time I was, when he was also a graduate student in Holland. So, he may still have done something on it since then. I'm not sure.

**TROPP:**

I guess one of the change--one of the reasons for asking you about your astronomical background, even though you've gotten away from that field recently, is because as a problem area I see it as one of the major impetus regions that helped bring about high speed computation, because of the needs.

**YOWELL:**

Well, you can't do anything these days in astronomy without a tremendous amount of calculation, whether it's the vast amounts of data that have to be examined in your atlas surveys, either photometric or positional, or whether it's your astrophysical studies, the interior of stars, or whether you are getting your cosmological problems. All of them are demanding tremendous amounts of computation.

**TROPP:**

I guess the reason I'm interested in the area is because so many of the early machines were funded on the basis of guessed needs.

**YOWELL:**

Yeah.

**TROPP:**

One of the clear needs was in the area of astronomy. Nobody at that point in time foresaw the massive handling of business data, for example, or other ways in which we use the computer so readily today. Nobody saw, for example--

**YOWELL:**

It was designed primarily as an engineering instrument, and in pure science astronomy was the obvious application at the time.

**TROPP:**

And the people--fortunately, many of the people who had decision making areas, who helped get some of the early machines funded, I think, were familiar with the astronomical needs as well as other basic scientific needs.

**YOWELL:**

Yeah.

**TROPP:**

I mean the weather problem, for example--that was one of Von Neumann's big areas of interest. The problem of turbulence, which is still an unsolved problem, [laugh] but which required handling and incredible amount of data very, very fast in order to get a fine enough mesh for some of these things. To do things quickly enough to handle the mass of data.

**YOWELL:**

I'd like to come back to the weather problem a little later when we get to the Institute.

**TROPP:**

Okay.

**YOWELL:**

Because we had one there.

**TROPP:**

Great, great.



**YOWELL:**

I think probably one of the other things that happened in that period was right after the war, was the establishment of the Thomas J. Watson Scientific Computing Bureau as a separate entity at Columbia, when Eckert left the Naval Observatory and came back as its director.

**TROPP:**

This was about '47, I think.

**YOWELL:**

Yeah. Now, there is one missing link in here, and that's when Herb Grosch was involved. Because Herb was there very early.

**TROPP:**

Right.

**YOWELL:**

Because I know right after the war he came in and I gave him and his wife lessons in running punched card machines before he went down to IBM to run their bureau. Incidentally, I might put in another--

**TROPP:**

Of course, Herb's in Washington and I can see him quite readily.

**YOWELL:**

Yeah. I'll put in another one that we taught at Columbia how to run punched card machines, and that was Stan Frankel, who went out to Los Alamos to set up their computing facility there early in the war. And they came into Columbia to get their first training on punched card machines.

**TROPP:**

I should write those two things down, because Stan Frankel also comes in later in terms of CRC when he's at Cal Tech.

**YOWELL:**

Yeah. Now I believe that I am right in that it was Stan Frankel, that it's the same Stan

Frankel, because I believe I remember meeting him again in Los Angeles. Now that's something I hadn't thought of for a long time.

**TROPP:**

Now he's somebody I hope to see in the near future, and I'd been thinking primarily of the Cal Tech machine which was modeled after the CADAC, the CRC 102 and that chain. And until you just mentioned it, I hadn't realized that I ought to push him back into the Columbia milieu through Los Alamos, and then--

**YOWELL:**

Yeah. He was there a very short time, trying to teach him how to work punched cards.

**TROPP:**

And Herb Grosch has not mentioned that early period, but I'll be sure and ask him about it.

**YOWELL:**

Well, again Herb got his start in data processing on a hand calculator, because again it's an astronomical problem. The three outer satellites of Jupiter have been extremely difficult to follow because the orbits are so strongly perturbed that there really isn't any one orbit. It is constantly being changed. And in the late thirties and early forties these three were being followed by three different men. Each man was following one. Herget at Cincinnati was following Jupiter 9; Sam Harrick at UCLA was following Jupiter 11; and Grosch, I think, was following Jupiter 12. So, I'd met Grosch--he was at Michigan at the time--I had met him at several of the local astronomical meetings and I'd known Herb before. But then he got out of astronomy and went into IBM. Of course, there's a little story about Grosch going into IBM.

**TROPP:**

Right [laugh]

**YOWELL:**

Grosch and his beard.

**TROPP:**

Right. Grosch and his beard. And Grosch being the only one who was fired and rehired some number of times by Watson.

**YOWELL:**

Mr. Grosch has always had a very great talent for making provocative statements, and I have heard him make two or three in my time that I thoroughly enjoyed.

**TROPP:**

He hasn't changed. [laugh].

**YOWELL:**

I'm sure he hasn't. He was in town here about a year ago, and I didn't hear about it until about two months later.

**TROPP:**

Well, I've seen Herb on a number of occasions, most recently at the Atlantic City meeting. But my predecessors on the project did a number of tapes with him that are currently being transcribed, and ultimately I'll have all the input from Herb. I knew about his background in astronomy. I didn't know about his connection with the Columbia Scientific Laboratory.

**YOWELL:**

Probably, both he and Frankel are liable to have forgotten it because it was a very short incident, and it was just a quick training session for them.

**TROPP:**

Mhm.

**YOWELL:**

It was as a result of many of these things I had gotten some sort of a reputation for being able to do things on machines by 1947. And I haven't the vaguest remembrance at the present time how I got involved with the Math Tables Project. I know! I do remember: It was Billy Lane, who was their punched card supervisor when the installation went in, in late '46 or early '47; they worked with us on the B-29 evaluation project, so he knew me there. And we kept in touch and he suggested that I come down and help him during the summer of '47, when they were just getting started. So I had about ten weeks with the Math Tables Project in downtown New York during the summer of '47.

**TROPP:**

They were doing primarily Bessel functions?

**YOWELL:**

They were doing tables of various sorts. And I think my big contribution that summer was developing a plug board for a standard 405 tabulator which would automatically take six differences, ten digit functions. And that was quite a job. And I knew we had our usual trouble with the service men. We'd say it isn't working right and he'd take one look at the plug board and say, "There's something wrong with your plug board." I finally got the standard answer of, "Look, I'll tear that plug board apart or rather I'll make another one. I'll duplicate just the function that's missing and show you from something that comes out of the manual." I had to only do it once, and show him that according to the manual wiring, the function is wrong as I had anticipated. Then after that he'd start coming over, "What have you got on that plug board?"

**TROPP:**

[laugh]

**YOWELL:**

He'd start getting very interested in what was going on, but always the first impulse was "you're doing something that isn't allowed and that's the reason you have problems." And, of course, that was many times, but the problem was that you had to know enough about the machine to recognize the things you couldn't do. And when you got too close timings, get rid of them. But, one of the main men who did a great deal of encouragement of the use of punched card machines at that time at the Math Tables Project was Milton Abramowitz, who, of course, has been dead about ten years now. He was one of the mathematicians who really saw what the punched cards would be able to do for us.

**TROPP:**

Did you have any contact during that short period with any of the individuals at the, what later became the Courant Institute, in terms of their interest in applied mathematics, numerical analysis?

**YOWELL:**

I don't think so. When I was with the Institute, Fritz John from the Courant Institute was at the Institute one time. I met him there, but I never had any real close contact with the Courant Institute.

**TROPP:**

Mhm.

**YOWELL:**

Then, when I finally got out of Columbia in 1948 with my doctorate, I found there were a

lot of people looking for somebody who could run punched card machines.

**TROPP:**

[laugh]

**YOWELL:**

Not very many people interested in finding astronomers.

**TROPP:**

[laugh]

**YOWELL:**

In fact, I had inquiries addressed to me if I was interested in joining about half the aircraft companies at the time who were just beginning to get punched card machines for engineering calculations. I ended up joining the Institute of Numerical Analysis in Los Angeles.

**TROPP:**

Well, they had really just been founded.

**YOWELL:**

They had just been founded. Their equipment was just coming in, and, of course, the thing, the final clincher as far as I was concerned is that they would give me one of their research positions and I could spend half of my time doing astronomical research. And when I got out there I got in contact with Sam Herrick at the astronomy department, who was also very much interested in what was going on at the Institute, since there was a chance that they'd do some of his minor work on Jupiter satellites, and astronomical or interplanetary navigation, which he was very much interested in those days, long before anybody else. And I did some observing at Mount Wilson on weekends during the next two or three years working for Dan Popper, who was professor of astronomy there. He was a spectroscopist who didn't have very much to do with computing, but at least it was an excuse to get out and be an astronomer once in a while.

**TROPP:**

When you came to the Institute, was Hartree the director?

**YOWELL:**

I think he was. I think he was there as the first director when I came. And at that time--

**TROPP:**

And what--I'm sorry.

**YOWELL:**

I was going to say at that time our equipment was a couple of 602 multipliers, a tabulator, a 416 tabulator, and that was about it.

**TROPP:**

Do you remember when the decision was made to build the SWAC?

**YOWELL:**

I do not remember when the decision was made to build the SWAC because that was made in Washington and I had very few contacts with Washington except every time I'd go East, I'd stop in to see people there.

**TROPP:**

Aha.

**YOWELL:**

But I do know that Harry Huskey showed up at the Institute about December 1948, and that was the start. And I know at that time one of the first things we did, Ida Rhodes came out from the Bureau of Standards, and she taught us a course in BINAC coding. And that was my first introduction to coding was the BINAC.

**TROPP:**

Let's see, in 1948, I guess BINAC was on its way to Northrop.

**YOWELL:**

It was on its way at the time. It had not arrived when we had that first course in programming. Then, as far as the Institute went, we kept upgrading the punched card installation at the same time we're developing SWAC.

**TROPP:**

Well, in terms of the development of SWAC, it's clear that this was a very different machine than SEAC.

**YOWELL:**

Yes.

**TROPP:**

A whole different world. And in terms of how SWAC was designed--architecturally and its philosophical design and what its capability was to be--what were the main ideas that went into it, as far as your needs at the Institute were?

**YOWELL:**

Well, it was always my understanding that SWAC was designed primarily to prove the practicability of the cathode ray tube memory. And while it was being built there was very little else considered as far as operating ease or programming went. I think that the programming system was a very useful one. The four address system worked very well. It gave us a lot of ease in programming. There were a few problems with the machine; not really problems, but I mean awkwardness's. Like the only way you could get all the, set to rest position after it had been turned off was to open the back doors and ground one of the flip-flops. There was no way of doing it from the console. Once that was done, you could get off and operate from the Flexowriter console without any difficulties. And, of course, it started out just with 256 words of storage, 36 bits. Later on they added a drum and later on they added a card reader for more rapid input of data. And that's all that had been added by the time I left the Institute. There was talk of putting a printer on, but I don't know whether that ever occurred or not. I just never kept up with it after that.

**TROPP:**

Well, you mentioned some of the early problems and I think it's worth retelling your story about Professor Lehmer and his Mersenne prime computation.

**YOWELL:**

I can't remember when Professor Lehmer came in as director; whether he was the second director or what. I think he may have been--no, I think he was a little later than that. But, in the early days, he was one of the first people was contacted if he had any problems for the machine, because, of course, we were designed to service government agencies and government contractors on the West Coast, as well as the mathematics community who was interested in developing new techniques for solving problems. And so Lehmer was one of the first persons that was contacted, and he wanted to take a crack at the machines and we sent him a programming manual, and about three months later, he sent down a program. He said, "I've written this program. It's for testing Mersenne numbers for prime ness, and here's a list of the ones I want to test. If it's prime, you'll get a type-out of all zeroes. If it's not prime, you'll get a type out of a non-zero number, and the number of bits is equal to the binary power. And put it in and run it for me, and send me the results." We wrote back and answered him and told him we would be most happy to, but didn't he want to come down for a code checking session to make sure the code was

running right. And the answer came back, "Well, I don't have to check it. The code is right." So we put it on the machine and ran it and he was right. The code was right.

**TROPP:**

[laugh].

**YOWELL:**

And I still do not know of any other program I have seen run on any computer that was right from the first.

**TROPP:**

[laugh].

**YOWELL:**

That's the only I've ever seen. That was one of the early problems we put on, and it worked very fine for the small programs. One hundred twenty-seven, 130--get through in two or three minutes, no problems. By the time we got up to the 1000 bit operations, they were running forty, fifty minutes and SWAC at that time just did not have a mean error free time that long. So we'd run them two or three times and we wouldn't take an answer until we got the same answer repeated two or three times. When we got up to 2037, well it was running two and one-half hours, we must have tried about ten times before we finally got two answers that were in precise agreement. And the bulk of the problem was the cathode ray tube storage, where when you'd reference one spot, you'd get a spill-over of electrons in the next one, and it took a major revision of the circuitry before that was cleaned up. But when it was cleaned up after the circuitry revision, SWAC settled down to a very solid machine. And, of course, the demonstration was made, cathode ray tube was an acceptable memory, and that's where the 701 decided to make their cathode ray tube memory.

**TROPP:**

Another area that's interesting in your Los Angeles period at the Institute, I guess, is the impact of the availability of this high speed computational equipment on the development of numerical analysis as a branch of mathematics; the sudden availability of being able to do large computations. Can you think of any particular impact in that field?

**YOWELL:**

Well, I was primarily familiar with what the research mathematicians at the Institute were doing and I'm not sure that they ever succeeded in coming up with any really practical, new computing technology. They explored an awful lot and I'm sure they showed a lot was not any better than it was, but there are three or four examples. I suspect that the one



that came closest to providing a good method was Hestenes' conjugate agreement method for finding eigen values and eigenvectors of real symmetric matrices.

**TROPP:**

Now who is this?

**YOWELL:**

Magnus Hestenes, who is a mathematician at UCLA.

**TROPP:**

Yes.

**YOWELL:**

And we used that very successfully on SWAC in flutter analysis of the tail surfaces of the F-105. I think it was the 105, but that came out to be about a 93rd order complex symmetric matrix and we needed to find the first fifteen or twenty items on these. And it worked as smooth as a whistle; we just ground them out in that thing. So that's one that I know worked very well and we used it extensively at the Institute. And I find it is still written up in the literature. There were others. We were working on variations and methods of solving linear programming I know. Theodore Motzkin was in there working on that.

**TROPP:**

Was Dantzig involved at that period in linear programming?

**YOWELL:**

Dantzig had already developed his simplex method and he was at Rand Corporation. He would come out and attend the seminars. We never got anything that was nearly as good as the simplex method. And I know we were doing work for Herman Kahn at times when he was at Rand Corporation investigating Monte Carlo procedures. And I remember one case I ran for him when he was looking for whether or not weighting Monte Carlo procedures would give faster convergence, and he ended up with the conclusion that if you knew what the answer was, you could design the weights to give you faster convergence.

**TROPP:**

[laugh].

**YOWELL:**

But that also didn't get too much for it. And we did a lot on partial differential equations, and I'm not sure that any of them ended up with better calculating schemes. We had a lot of interesting problems that came in from various places on the West Coast. I mentioned earlier I'd like to just say one about meteorology.

**TROPP:**

Right.

**YOWELL:**

The Meteorology Department at UCLA had a problem of trying to analyze the weather pattern. They were taking observations, I guess, at 900, 700, 500, 300, 100 millibars over a grid over the entire country and trying to apply a general equation to them. That was enough data we couldn't put it on SWAC, so it had to go on punched cards. And the remark that I would like to make was that with very simple equations at that time it was taking us three months on punched cards to reduce one day's data.

**TROPP:**

It's unbelievable; it's unbelievable.

**YOWELL:**

Which is proof of the pudding as to why you need big, fast computers if you're doing meteorological problems.

**TROPP:**

Well, that--I was going to say that particular system today would be done in a matter measured in an hour or minutes.

**YOWELL:**

Could be measured in minutes because it was a small system, basically. They tried nothing but the grid over the United States and it was widely spaced. But it just is such a massive amount of data that you just couldn't do it in anything less.

**TROPP:**

They weren't working on the turbulence problem at UCLA?

**YOWELL:**

I don't know what they were working on. I just don't remember. I know that the

Department of Agriculture gave us two problems we were never able to solve. Both of them because we couldn't find a way of expressing them. They were very simple problems that'd been solved practically. One of them: what's the optimum design for a pea freezer; for freezing peas? And nobody could solve it, because we'd start out, we'd know the temperature at which the first layer of ice crystals would form under the pea skin--that's thirty-two degrees. It's modified by whatever the chemical that is dissolved in the pea juice is. As soon as that first layer of crystals formed, then nobody could tell us what happened to the pea juice. You see, you couldn't tell what freezing point that would be. But on the other hand, cut and try--they got pea freezers and worked very well. The other was a similar one: What happens when you try to dehydrate potato strips? Here the problem is they curl, and nobody can predict why a straight cylinder of potato will curl when it dries. You've lost your geometric configuration and you don't know where to go. So its simple problems like this one we still can't solve, not because we don't know the mathematics. We kept advancing the punched card equipment. Went from 602s to 602As, from 602As we went to Card Programmed Calculator of 604s first.

**TROPP:**

604s, right.

**YOWELL:**

Then the Card Programmed Calculator, then the Model 2 Card Programmed Calculator. We had a 101 statistical machine there. Tightrow [?] was the one that got us started on a very good general purpose board for the Model 2 Card Programmed Calculator.

**TROPP:**

Who is this?

**YOWELL:**

Dan Tightrow now at the University of Michigan. Because he's the one that started us looking at continued fraction expansions for the arc tangent, the logarithm, some of these other functions that are very hard to handle with your--

**TROPP:**

They've been around for centuries.

**YOWELL:**

Been around for centuries, but Dan's the one who pointed them out to us. And we got a very good general purpose card that gave us all the arc functions, powers, roots, exponentials, logarithms, as a result of using the continued fraction expansions.

**TROPP:**

I'm trying to think of the gentleman who ended up writing a book not too long after that on continued fractions, who I think was in that environment.

**YOWELL:**

Wall?

**TROPP:**

No. I have a copy of it at home. I can't think of the name right now, but I think he came out of that same period.

**YOWELL:**

Yeah.

**TROPP:**

Olds? No, it's not Olds. I'll think of the name later.

**YOWELL:**

We had a large number of contacts, of course, with the aircraft companies at that time. I knew most of the guys that were running computing sections at that time--John Lowe with Douglas, Woodbury at Northrop.

**TROPP:**

I'm trying to locate a key name at Hughes in that time period. Who do you remember at Hughes?

**YOWELL:**

Hughes, I can tell you some people who went to Hughes when INA broke up in 194--1953. Frank Meek, M-e-e-k, Frank and Helen Meek--they originally joined the Bureau of Standards in Washington and moved out to the West Coast when INA sort of broke up in 1953 over funding problems. They went to Hughes. They're still in the Los Angeles area because I saw them when I was out there about two years ago.

**TROPP:**

You don't remember any of the people at Hughes who might have been using--

**YOWELL:**

I will one of these days, but at the moment I can't.

**TROPP:**

Because I think along with Northrop, they're one of the environments that helped generate a number of things, but I haven't been able to come up with any names as yet.

**YOWELL:**

Yeah. Oh, there's Randy Porter at Boeing in Seattle, in charge of their computing effort. Inyokern was another one that was very active. I guess that was Naval Ordnance Test Station. Northrop, North American, Lockheed. Was it Bill Bell at Lockheed that formed Telecomputing Corporation?

**TROPP:**

I think you're right.

**YOWELL:**

Yeah. Rand, of course, and the local ones.

**TROPP:**

So you stayed then with the Institute until it became part of UCLA in '53?

**YOWELL:**

UCLA, yeah. Yeah.

**TROPP:**

And so when you joined CRC, it had already been operating for a number of years. As you mentioned by then Floyd Steele was already gone.

**YOWELL:**

That's right. Not merely had already been operating for a number of years, but already the majority interest had been sold to NCR.

**TROPP:**

Oh, that had already occurred?

**YOWELL:**

That had already occurred and that was an important factor in my decision to join them, because I felt that the era that was coming in the data processing systems that small companies would have a great deal of trouble in competing with IBM and the other large manufacturers. And I felt that NCR had the financial resources and the experience to stay in the business. And they have to this date. They are getting so deeply involved that the only way they're going to get out of it now is if the company folds. They can't pull an RCA. The people over there at CRC when I joined them were Eckdahl, Sarkissian, Sprague, Dick Dabney was still president, Jack Warshauer was treasurer, I guess.

**TROPP:**

Yes, finance.

**YOWELL:**

But they lasted only about three months while I was there, because I joined them in October. And then in January NCR bought out the remaining minority stock and converted it to the Electronics Division. At which point, Dabney and Warshauer left.

**TROPP:**

Sprague left.

**YOWELL:**

Sprague left after that. He didn't leave at that time. He was around for another six months or so. But they brought an NCR man out--Bob, I think his name is here.

**TROPP:**

Yeah, it's on that list.

**YOWELL:**

Sprague left after that. He didn't leave at that time. He was around for another six months or so. But they brought an NCR man out--Bob, I think his name is here.

**TROPP:**

Yeah, it's on that list.

**YOWELL:**

It's on that list. Pearson, Bob Pearson. Bob was an old NCR accounting machine salesman and he knew something about selling, but he didn't know very much about

computers. And I would say that the real adjustment that was made between NCR and CRC in the next three years was the two companies learning to talk to each other. When I got to CRC, along with Arnie Hestenes, who had been with us at INA, we moved into the sales department. Arnie was, I guess, Applications Manager and I was Scientific Applications Specialist. And we started looking at the commitments which the CRC officers had made to various prospects and the like. And we suddenly found that in our opinion, they had probably fifteen years' work lined up for the staff we had at the present time. Because Sprague was a very promising young man; he'd promise anybody anything. And I don't think that Dick had a real good idea the amount of work that was necessary to try and demonstrate some of these things. So the end result is many of these prospects we just never got around to; we just didn't have the manpower to handle them. And those of us at CRC that were in the sales department, none of us had any sales experience, and we had a long, rough learning time. We started out with four salesmen when I got there--one in Washington, one in Chicago, one in New York and one in Los Angeles. And somehow or other they managed to sell about twenty-five 102A's. And the 102A is a nice little machine. There was one problem with it and that was maintenance. If you had a sharp maintenance man who would pick errors off as they occurred, you had no problems. But if you've get two or more errors simultaneously, then it was a dog to try and find them. And part of it was the result of miniaturizing the machine where components handled the functions.

**TROPP:**

Don and I were talking about this in terms of design how they took each of the flip-flops, if you will, and designed them so that could do a number of things.

**YOWELL:**

Yeah.

**TROPP:**

And when one wasn't being used for the logical function, it was being used to store something. If it was not being used to store something, it was being used for something else.

**YOWELL:**

The way the design was, you set out your logical equations and you put them up against your program counter. And on step 7 you saw you needed fifteen flip-flops, OK, you put fifteen flip-flops in the machine, because it's the maximum. Then whatever you weren't using in one step, you'd use it for whatever function you needed, so the net result is these things had tremendous different uses. And you got something that was failing in two or three places and you got caught up--you could be days trying to get that machine back up again. But if you had a good service man who'd keep it up to date, the thing would run and run and run. And we had one at Bakersfield Standard Oil, where they were doing

seismographic analysis that they normally would load their paper tape with seismographic data on, that, when they went home in the evening, and look at the answers when they came in the next morning. And, say, maybe half of the time it would still be running, they'd get good answers, the other half of the time they didn't. But, at least they were getting half the time, they were getting work --

**TROPP:**

But they weren't concerned with speed

**YOWELL:**

No.

**TROPP:**

in the problem, in an application like that?

**YOWELL:**

Sure, they were concerned with speed, but their idea of what speed was very different than it is today. That's one of the reasons they went to the computer because the need of getting some idea of where to drill was still very paramount, especially when you're bidding for leases. You want to get some idea of how much to pay for some--

**TROPP:**

In terms of the speed that you had to offer, that really wasn't a critical point of view I'd say, concerning the--

**YOWELL:**

No. It was so much faster than anything else at the time.

**TROPP:**

That's right. I think Dick mentioned that particular application. He also mentioned the fact that about this point in time, he had a whole bunch of machines built, but very few delivered, and maybe none delivered.

**YOWELL:**

That's right. I know it was close to a year after I was there, before they shipped the first 102A. They just couldn't get the thing to run reliably. And they were building them and checking them out and they just couldn't get them checked out.



**TROPP:**

Well, what was the 102D? Was that a digital version?

**YOWELL:**

That was a digital version of the 102A. It had a digital adder instead of a binary adder. It was still coded in binary, but it had a digital adder.

**TROPP:**

I want to look at some of these machines. You may be able to--

**YOWELL:**

Now, the other thing that was added to it was a magnetic tape drive. Now this was a basket type drive.

**TROPP:**

Now what do you mean by a basket type drive? I'm sorry.

**YOWELL:**

Well, you had basically two chambers in the front of the machine, bottom, which were very narrow; just the width of a plastic tape separated with, I guess it was some plastic, polystyrene or something for the two sides. And you'd take the tape off the reel and dump it down into the basket. Well, because it was just the width of it, the tape would lie flat in there, but lie coiled up with the coils lying on top of each other. Then you'd take one end, put it through the read/write head and dump it down into the other basket. Now the idea behind this at the time, of course, was that you had a very small mass to accelerate. Say it gets good start and stop times, not having to worry about the reels. The net result was, however, that even though the magnetic tape was very nice, it had a fantastically low transfer rate. I believe it was of the order of five kilocycles, so it was very low. But we did have magnetic tape units on many of the systems we sold and delivered. Now that was the big machine--the 102A or D series. I think that there were a total of about thirty, about twenty 102A's and roughly ten 102D's sold, and the next most common one was the 105, which was the digital differential analyzer, and there were about eight of those sold.

**TROPP:**

Is the 105 a modification of the 101?

**YOWELL:**

I'm not sure what the 101 is.

**TROPP:**

Well, the 101 was--

**YOWELL:**

If it was the original digital differential analyzer, the 105 was the commercial version. The same way the 102A was the commercial version of the 102.

**TROPP:**

And how about the 106?

**YOWELL:**

The 106 and the 107 were large, general purpose machines. Basically the same, which were developed for, one of the Navy installations in Washington and one of them for White Sands. And they were characterized by extremely large drums. I think they had 10,000 words of storage on the drum plus 1000 words of fast access on the drum, in which the drum channels were divided into tenths with the read and write heads each 36 degrees around the channel, so you could get into it with access about a tenth of the time. And there were two of them; each of them with two cabinets. Internal refrigeration.

**[End Tape 1, Side 1]**

**[Start Tape 1, Side 2]**

**TROPP:**

In terms of this room, how would you estimate the size of the 102A?

**YOWELL:**

The main frame itself was about the size of two standard file cabinets placed end to end, six feet high, two feet wide and sixteen feet long.

**TROPP:**

That's all?

**YOWELL:**

That's all. The console was just an ordinary desk with a Flexowriter on it, and that was it. The 106 or 7 was, had cabinets that were somewhat bigger, but I didn't see enough of

them to maintain a good impression except that they looked similar-sized.

**TROPP:**

And because all the cooling was built into it you weren't adding massively to the size of the machine by refrigeration or air conditioning?

**YOWELL:**

That's right. The 106 and 107 were, again, they were tough machines to get started and checked out, but once they did they ran beautifully. The 107 was at the Naval installation in Washington was a horrendous thing. I know after I'd moved back to Dayton, there came a time when Mr. Allyn, who was then chairman of the board at NCR, went into Washington and agreed to take the machine back and rework it. That took about a year, and I think he said it cost him a million and a half bucks to put that machine in, but once it got in after that, I think for the next four years they had better than 96 or 97 percent up time. And it ran until it just wore out.

**TROPP:**

What were the 102 series? What were they selling for?

**YOWELL:**

Around \$100,000.

**TROPP:**

Which was very cheap at that point in time when you figure that most of the machines that were built as a one-time only shot were running, I would guess, close to a million.

**YOWELL:**

Yeah, very much so.

**TROPP:**

Like the RAYDAC was a million dollar, I think, machine.

**YOWELL:**

Yeah. I think it was more than that.

**TROPP:**

It may have been more than that, but--

**YOWELL:**

That was the one at Mugu, right?

**TROPP:**

Yeah.

**YOWELL:**

Yeah, that's the one which when I went to see the room before they brought the machine in, they were showing us how carefully they had it all shielded to make sure that none of the radars of the base would interfere with the internal operation of the machine. They even had a copper screen in the middle of the glass window connected up with a complete shield, copper sheet through the door that made contact with the copper in the wall so it would be completely shielded. When they brought it in, they had nothing but problems. They couldn't shield the lead in cables.

**TROPP:**

[laugh] Well, you stayed then with NCR until very recently?

**YOWELL:**

Until 1969, the end of 1969 I left.

**TROPP:**

And so you were essentially with this whole evolution then of CRC through the NCR organizational development?

**YOWELL:**

Basically, up through the 304, 315, early stages of the present Century series.

**TROPP:**

The 304 is the first NCR machine?

**YOWELL:**

That's right.

**TROPP:**

And by then you were into core memory?

**YOWELL:**

It's a core memory, transistorized machine--I still believe that we were the first people to announce we were going to bring out a fully transistorized machine.

**TROPP:**

And this was at what time?

**YOWELL:**

1955, if I remember correctly. I think it's that, because that's the year I went to England for the Company, forming our relationship over there with--gee, I can't even remember the name of that firm now.

**TROPP:**

It wasn't Ferranti?

**YOWELL:**

No.

**TROPP:**

Lyons?

**YOWELL:**

No. It will come back to me. They were another one that could account and sort.

**TROPP:**

I didn't bring it with me. I had a manual of foreign developments that I left in my hotel room.

**YOWELL:**

It will come back to me--Elliott Brothers.

**TROPP:**

Oh, yes.

**YOWELL:**

Elliott, we formed an agreement over there with Elliott Brothers.

**TROPP:**

Backing off, before you went to England, during this whole period when you were at the Institute and later with CRC--we're talking about 1948, 1953, '55 roughly--what contacts did you have with computer developments in England? I guess one of the things I'm looking for is the flow of information between different areas. Of course, the East Coast of the United States is one center of activity. Another major center of activity, of course, is in England; Manchester, Cambridge, National Physical Laboratories and so on.

**YOWELL:**

We had primarily contacts through seminars at UCLA, at the Institute, when visiting people would come over from England.

**TROPP:**

Like Harry Huskey.

**YOWELL:**

Huskey had worked at Manchester.

**TROPP:**

Right. He had just come when he had come to UCLA, he had just come.

**YOWELL:**

Yeah. So, he had a lot of contacts. They'd all stop out to see how he was doing and so there would be a seminar and you'd go in and sometimes I'd go and sometimes I wouldn't. I can't remember really anything that went on.

**TROPP:**

I guess what I'm looking for is any impact they might have had on the development in your area or conversely that you know about.

**YOWELL:**

They had no impact on our area, which was the use of the machines at that time. Practically all the contacts that I remember were engineering contacts. And I just don't know enough about engineering to have any way of evaluating that; the importance of

those contacts. In fact, I think, looking back over what I know--[buzzer sounds] Excuse me. [Recorder off] Well, looking back over my particular career in data processing, I don't think that there has been very much that has come out of the European area in the way of methods of solving problems or coding techniques, except for a tremendous amount of theoretical work in the ALGOL area and other automatic programming areas, which has been contributed by the European groups. But when it comes to practical methods of solving problems and much of the programming language and coding systems for use in this country, most of it has gone the other way. Now machine design-wise, it may be a much more two way street; I just don't know. I know that Huskey is--there's a bit of nostalgia.

**TROPP:**

There was a lot of interchange in the early period. I mean like EDSAC is really the first realization of the EDVAC that was designed and conceptualized at the Moore School, or by Eckert-Mauchly and Von Neumann and that group. Another question that I wanted to ask you about--I just, I lost it for one second but it will come back to me. Oh, yes. The 701.

**YOWELL:**

Mhm.

**TROPP:**

which was in an evolutionary stage while you were still at the Institute

**YOWELL:**

Oh, yes, oh yes.

**TROPP:**

Well, what are your memories in terms of that development?

**YOWELL:**

I can remember when the IBM representatives came around to give us a two day briefing on what the 701 would be; to give us a chance to evaluate whether or not we wanted to place an order.

**TROPP:**

Excuse me. When you say IBM people, would Cuthbert Hurd have been one of them?

**YOWELL:**

No, Cuthbert was not associated at that time. I don't remember who--now wait a minute. I may want to take that back about Cuthbert Hurd.

**TROPP:**

Yeah, Hurd was associated, right.

**YOWELL:**

Because I remember meeting Hurd while I was still an IBM user, so that's before I left. And I think it was probably at the last user seminar I went to at Endicott that I met Hurd, which was the CPC seminar. But we didn't see him at that time; it was a couple of young boys on the staff that came around to present the system. And they gave us about a month or so to analyze and decide whether we wanted to place an order. And our feeling was that it was a real nice machine but just too rich for our blood, particularly when this was getting very close to the time when the Bureau of Standards was folding the Institute for financial reasons. But I know that IBM had told us at that time that they were planning to make sixteen machines and that was all. And that on the order day they would contact everyone of whom they felt were prospects and if you wanted one, you could order one and if we didn't, OK. But they were only going to make sixteen, because that was all the market they could see.

**TROPP:**

And they ended up making eighteen.

**YOWELL:**

Maybe they did.

**TROPP:**

[Laugh] But I think you're right about the original number.

**YOWELL:**

Sixteen sticks in my mind.

**TROPP:**

Yeah. And everyone thought they were going to be number one on delivery. [Laugh]

**YOWELL:**

Yeah, yeah. But by the time it came out, of course, I'd gone to CRC and we were much



more interested in the 650 as a competitor with the 102A than we were with the 701 which we wrote off as a large engineering machine for aircraft companies, Los Alamos, and Livermore, and Bureau of Standards.

**TROPP:**

Which turned out to be true except that it generated the 704.

**YOWELL:**

Yeah. And it also opened IBM's eyes to the size of the business and eventually the 702, too. The 702 went into the 705, and that the 360's and from there we go; the 709's. And I think that's the first machine that IBM had ever brought out while I was with the Bureau that we turned down. But, of course, we had SWAC.

**TROPP:**

Right. And SWAC ran for a long time.

**YOWELL:**

It ran for a long time. Incidentally, one guy that was with SWAC from the last two or three years I was there and for years after I was with UCLA is Fred Hollander out in Los Angeles.

**TROPP:**

Is that H-O-double L-A-N--

**YOWELL:**

A-N-D-E-R, yeah. I think he's still at UCLA with SWAC. At least I know he's still out in California because I still get a Christmas card from him every year. He's another old astronomer. But we were primarily interested in the development of smaller machines. I think probably the thing that really put computing on its way commercially was the 650. No question about it. They just had so many of those. They started developing common languages for it. People got used to it. They found out what they could do with electronics. It was the machine that really turned out to be the opening wedge into the business of data processing. Because I really don't think that any of the 102 machines went to any place but engineering. We tried.

**TROPP:**

In the early period there didn't seem to be much interest or foresight in terms of other applications.

**YOWELL:**

In the early period the peripherals weren't developed to make it possible. I remember submitting a proposal to United Aircraft to Hartford for a production control job in which we were taking the paper tape that came off a high speed paper tape punch, cutting it up into pieces and feeding sixteen Flexowriters in order to get the reports out. And that is a boondoggle if there ever was one. I'm not surprised that nobody would buy it, but there wasn't printers at that time. I think the first high speed printer that NCR made was one that we put on the 107. Until you got the printers, until you got good card entry devices, you're just stymied as far as business data processing went. Even cards haven't proved the answer.

**TROPP:**

I was thinking though of some of the early interest, and one of the very first was the Lyons' Tea Company in England.

**YOWELL:**

Yeah.

**TROPP:**

And I guess, one of the insurance companies in the U.S., but other than that there are just primarily isolated interests.

**YOWELL:**

Yeah.

**TROPP:**

Although in this same period, the Barber-Colman Company had projected a large market if they could ever build a small, inexpensive desk size electronic computer for business purposes.

**YOWELL:**

Right. People were predicting a market was there, there's no doubt about it. And the interesting thing to me is that there was a huge swing as far as NCR went between the 102's and the 304's. The 102's all went to scientific and engineering applications. The 304's all went to business data processing applications, and we ended up moving 30 304's.

**TROPP:**

Now that's an interesting shift, and you indicated you felt that the first two years after NCR bought out the minority interest were primarily spent in the two groups learning to communicate with each other.

**YOWELL:**

Yeah, yeah.

**TROPP:**

Now how do you see this change then occurring to where NCR saw the electronic computer as an adjunct or a useful device towards business applications?

**YOWELL:**

Well, I don't think that NCR had to change in that respect. They had already seen that it was useful and they were actively looking for it. That's why they bought CRC, and they had had projects in electronic computers going on here at Dayton prior to that.

**TROPP:**

I was looking through a book of Stanley Allyn's, you know, My Half Century with NCR, and he mentioned that they were ripe when CRC became available

**YOWELL:**

Yeah.

**TROPP:**

In terms of their need for talent and experience. And I knew that there had been some work here before that but he doesn't mention that.

**YOWELL:**

Yep. Well, Carl Wrench and Joe Desch, both of whom are still here at NCR, had been working in electronics even before World War I--not World War I, World War II. And they have some patents going back to--decade ring counters and other things like this--that go back to the late thirties or early forties, I don't know exactly when. But, this sort of work was going on, and NCR had a project which they called NEAM, National Electronic Accounting Machine, which had a small engineering group here in Dayton. But with the success of CRC with demonstrated equipment, they basically killed that project, moved all the electronics work to Hawthorne, and switched those engineers to peripheral development here in Dayton. So the 304 and later machines, the basic split was Hawthorne developed the electronics and Dayton developed the mechanics, and that was the way it was envisioned. Now, of course, Dayton is getting out of it and there was

the agreement with CDC, and going into the consortium which hopefully gives you double the market once you get your prices down, but NCR basically is out of that now. But in the meantime they've built up a fairly good electronics staff here in Dayton. Take a look at these new terminals that are coming out--\$280 a retail terminal, \$270 financial terminal, the rest. They're really small computers. All of them electronic. All of them Dayton-designed, but the big computer operations is still Hawthorne. Now the 304, as I was saying that I thought it was 195-, I said '55 but it was '56, I believe, when we made the decision to go to transistors, because that's the year I went to England. When I came back, the big decision was being made whether we were going to come out with the last tube machine or the first transistor machine.

**TROPP:**

So in '57 they were at the water shed.

**YOWELL:**

In '56, no, October of '56 they were at the water shed; and they made the decision, "we'll take a year and transistorize." It took more than a year because it was late '59 before we got out with the machine and delivered it. November '59 we delivered the first one to the Marine Corps at Camp Pendleton. But the 304 was a real nice programmer's machine. And it turned out to be a real solid machine. That's the one GE built for us.

**TROPP:**

Mmm.

**YOWELL:**

Again, Mr. Allyn was real wary about the possible financial impact on the company if we'd run into a failure such as Underwood had with the ELECOM.

**TROPP:**

I'm not familiar with that, and you might take a few minutes and talk about it.

**YOWELL:**

Well, Underwood had designed a real nice machine, an ELECOM. And they had sold quite a few, including Standard Oil of New Jersey, when they came to a place where they realized they just did not have the financial strength to commit to the inventory they would need to build and resell these machines. So they decided that the only thing they could do, with the rest of their business suffering as it was, was to drop it. And Mr. Allyn was fairly close to them, as most of these executives of the business equipment industry are to one another. He said, "I would never want to have to do what the chairman of Underwood had to do--to go in to the chairman of Standard Oil and tell him I had to

break the contract." And he was real wary of the possible financial impact on NCR of a major failure. He was uncertain about CRC'S production capability after the experiences with the 107 and some of the 102 problems of getting the stuff out of the doors. And the net result was that we made an agreement with General Electric that they would manufacture according to our engineering designs. And this was a joint two way deal involving the ERMA system, which GE had gotten from the Bank of America, which our engineers said "they must have bought the business. They couldn't possibly do it at the price that was quoted." We'd been on it, too. But we ended up selling them sorters for ERMA and high speed printers for ERMA, and they turned around and then did the manufacturing of the 304's for us in Phoenix, which is where Dick Sprague gets the GE

**TROPP:**

Right.

**YOWELL:**

thing on his chart. And I have some questions as to how much NCR design from the engineering actually influenced the GE computer group, because I don't think--I think it stemmed much more from ERMA which came out of the Stanford electronics group.

**TROPP:**

I've been trying for a number of months to talk to Al Ziff,

**YOWELL:**

Mhm.

**TROPP:**

who was involved in that, and our schedules just don't mesh.

**YOWELL:**

Yeah.

**TROPP:**

But at this point in time all I know is a little bit about ERMA and I'm not competent to even make any--

**YOWELL:**

Well, ERMA, of course, was designed by--Bank of America paid Stanford to do the job. But I think that GE was far more influenced by that than they were by any of the NCR

design technology.

**TROPP:**

Who would you suggest I talk to in GE in terms of that?

**YOWELL:**

The engineer in charge was Bob Wooley, and I don't know where you can find Bob, these days. Last time I heard of him he was with their light military division in Johnson City, New York.

**TROPP:**

How do spell his last name?

**YOWELL:**

W-o-o-l-e-y.

**TROPP:**

But he's still with GE?

**YOWELL:**

Yeah. I'm sure he's still with GE. He was one of the GE career engineers.

**TROPP:**

The question is the influence on GE computers out of ERMA or NCR.

**YOWELL:**

Well, they did a real good job for us, because the 304's were real solid machines. And I was reading another one of these articles in this anniversary issue here of ACM Communications, I guess it was Jean Sammet's article on programming.

**TROPP:**

Mmm. On the programming languages.

**YOWELL:**

In which they mentioned that the first big programming conference the NCR 304 was presented as a machine with an attempt to develop a computer which would make

automatic coding unnecessary. Her final comment, "Although the machine was apparently a success, it did not eliminate higher level languages."

**TROPP:**

[Laugh]

**YOWELL:**

We had COBOL for the 304. We had our own compiler for it, we also ran COBOL on the 304. We never got into FORTRAN or any scientific languages with the 304, because all those machines went basically into commercial data processing applications. Three Marine Corps machines were all personnel operations. The Yards and Docks machine was budget control. The Naval Yard machine was production control. We had three department stores. We had three banks. We had a couple of insurance companies. We had American Greeting Cards in Cleveland in distribution operation, so that they were all commercial applications. And there was just no need for any sort of scientific operation there, except for one machine that the University of Dayton had. And for a couple of 304's which were placed inside the General Electric Company.

**TROPP:**

And they were doing other than business

**YOWELL:**

Yeah.

**TROPP:**

applied work? Now the University of Dayton was using their machine how, do you remember?

**YOWELL:**

Primarily to support their research division. Their research division was very active in government contract work, especially out of Wright Field and other places.

**TROPP:**

Do you have any idea what kind of language they were using?

**YOWELL:**

Well, they were using COBOL. They were using a certain--we made certain variations on the 102, on the 304, which we called the 304B, which broke the program counter

down to the place where you could get into it and do a little bit of microprogramming on some of the instructions to put in floating point and a few other mathematical instructions. But basically the machine was used practically, completely for--

**TROPP:**

You said you were in the sales division, so when you were involved in visiting financial institutions and other business groups and ascertaining their needs and interest in this kind of thing before the 304 actually became operative, I would...

**YOWELL:**

That is correct.

**TROPP:**

what was the atmosphere in which you were being received? What were you finding as you talked to groups that we normally consider fairly conservative, as compared to say the aircraft industry?

**YOWELL:**

I was finding at that time a great deal of interest in data processing. A real question on their part as to whether or not they could afford it with the price of equipment the way it was, because the 304 system was a million dollar system again. By the time you put eight tape units on, a tape banner, a high speed printer and a high speed card reader and a tape reader and the rest of the stuff, it pushed the million bucks real fast. Department stores are probably a classic example of a conservative operation. We sold three department stores. All of them for accounts receivable, because, once again, here was a huge job that they just weren't getting done. And as the comptroller at Macy's said, "Once that sales check is signed, nothing happens to the accounts receivable except shrinkage. If I get them on the computer fast and kill the shrinkage, I can pay for it." That's a place where they have 50 and 60 billing clerks, where you had replaceable personnel, and this is one reason we could sell them in the big department stores. And everyone we talked to was receptive; it was really a question of cost and economic return. We looked at Dayton's in Minneapolis. We thought we'd found a real good area in inventory control where they had 60 clerks in the various style departments keeping little black books, as they called them, which has got a complete record, by style, by color of each of the pieces of clothing--men's, women's, children's sportswear and the like. We found that these clerks were all assistant buyers and they had other duties. And you take their clerical jobs away from them, you still couldn't eliminate the job.

**TROPP:**

I see.



**YOWELL:**

So you just couldn't move into that area at the time.

**TROPP:**

Except from the standpoint of almost an operations research approach.

**YOWELL:**

That's right. You couldn't move into accounts receivable because the clerks there had nothing to do but run the billing machines.

**TROPP:**

Yeah, but in the sense of inventory control, if you could minimize the amount of inventory on hand

**YOWELL:**

Oh yes.

**TROPP:**

through automating the procedure.

**YOWELL:**

That's what is being done now, but that requires more data than we could collect in those days.

**TROPP:**

I notice now we have a realization of what Allyn was talking about in the fifties in terms of the department store the minute they punch the sale, it goes into the system,

**YOWELL:**

Right.

**TROPP:**

but this is just now happening,

**YOWELL:**

Just coming out now with the 280 terminals.

**TROPP:**

and I guess the question I was going to ask is that just happening because (a) the technology is there or (b) the cost is finally down?

**YOWELL:**

Well, isn't that saying the same thing?

**TROPP:**

Well, yeah. Really the same thing because you could have--

**YOWELL:**

We tried to do it at NCR back in the fifties when we brought out the Salestronic registers, which we'd punch out on paper tape all of the information, but it took a keyboard pass to get the information. In fact we actually made tag readers--a reading e...ers [?] and a kimble pin [?] punch tag. Put the tag in and you capture all the merchandising information, but the paper tape punch was too slow and the equipment was too expensive for what you could get out of merchandising.

**TROPP:**

I see.

**YOWELL:**

Now with the 280 terminal where you've got it all electronified and put it directly on magnetic tape cheaply with just a pass of the wand over the code colored bars they can record all of this information, they get the clerks' time down to the place where it becomes practical; they get the recording time down to a place where you don't need to have large numbers of them in order to be able to record all the information during the course of the sales day; and the technology and the cost have caught up to the place where it now becomes feasible to gather all the information and really go into some of the problems which aren't solved yet--what you do with all the information and how you end up getting the best style operations. It's easy enough to control inventory and big ticket appliances, and it's easy enough to control inventory in your standard, run-of-the-mill sheets and pillow cases and other standard products. When you get into style merchandizing and women's clothing, sportswear, and men's clothing where you've got sizes, colors, 650 different sizes in women's shoes, not to mention differences in heel height in the same style. There is just no way in the world yet of really figuring out what inventory you need. Still by guess and by gosh.

**TROPP:**

You still can't replace the guy who's been there for twenty years.

**YOWELL:**

You still can't replace the guy who's been there for twenty years. We don't know how he thinks.

**TROPP:**

Because that's the difference between, you know, the scientific application--it's very easy to walk up to the scientist in the early period of the machines and say, "How do you solve the problem stage by stage?"

**YOWELL:**

Yeah.

**TROPP:**

And build a machine that reproduces this, which is essentially what you did. But now you're dealing with something very, very different.

**YOWELL:**

At the present time I'm concentrating on food retail. And here the problem of inventory control and inventory selection is very different, because your items are standardized. In the first place you consider one can or box that's got the same label on it is identical with any other one on the shelf unless it's got an obvious dent or break in it. In the second place you don't worry too much about brands. Everybody's got their own particular preference of brand merchandise, once you've sampled and established it, but there are so many items in which brands are unimportant to the consumer.

**TROPP:**

Mhm.

**YOWELL:**

So it's a much easier problem to tackle than the style merchandise clothing problem.

**TROPP:**

But it's still a big model.

**YOWELL:**

It's still a big model. Yeah. Let me take care of ...

[Recorder off]

**TROPP:**

You were saying about ElectroData.

**YOWELL:**

ElectroData machine, yeah. That was one of the last projects we did at INS. We specified the acceptance test on the original ElectroData machine for the Jet Propulsion Laboratory, which was the first buyer.

**TROPP:**

That's an interesting topic all by itself, because this was one of the purposes of the Bureau of Standards, one of the roles it was going to play. And the decision by Curtiss and others, that if they were going to set standards for machines, they'd better build some, which they did. And looking at that whole period, that pre-1955 period, the whole concepts of standards seem so nebulous when you look at the different machines. And guess it's worth talking about how you established a framework within which to establish a set of standards for a machine.

**YOWELL:**

Well, we probably took a very simplified approach to it. We said, "OK, the engineering specifications are here. This is what has been agreed to, we have two things to establish--One: does the machine do what it's specified to do? And this is a simple matter of testing all commands, especially worrying about zeroes of functions and other little things like this. And, secondly, to get some sort of an idea of the test and reliability. By putting large problems through, we see how long it could run between failures. And this was the essence of the tests we specified, that you put in specified values, you should come up with specified answers. And make sure that you put in enough problems to test every command; to test for critical values; for divisions by zeroes; for critical values of tangents and other functions that might be in there and then just specify that it should run for a fairly long period of time. We faced the same problem when we came to accepting the 304 from General Electric. We set out with a specified acceptance test which, again, tested every command in the machine to see if it operated properly. And a set of test problems which were to run without failure for a specified percentage of the time before the machine was acceptable. And these then were basically tests that the engineering was right.

**TROPP:**

You were in a sense creating standards from the engineering data of each machine.

**YOWELL:**

That's right. And even the question of reliability was one that was specified in the original contracts--it was a question of demonstrating that the engineering was such that it made the reliability standard of what had been previously specified. Now we ran into some real intriguing things when--I don't remember about the ElectroData test because the Institute had pretty well dissolved by the time the machine was delivered to JPL. But I do remember that we had some real problems with the 304 acceptance test. One of them was with the high speed card reader, and that is the best high speed card reader that has been made yet. It's a shame that it's not being made anymore, because that was 2,000 cards a minute and that thing really moved cards. When we first got that thing, I helped run the tests here in Dayton before we shipped it out to Phoenix to be integrated into the system. We ran new cards through with no holes in them. We punched all sorts of test patterns in them. I called some of my friends around town at Frigidaire, at Dayton Power and Light, and we scrounged 10,000 old, used cards they were going to throw away so we could test used cards and beat-up cards as well. Everything worked perfectly; until we got down to Phoenix. The first programs we started to put through there, we couldn't get them to read. And what we found was we were getting card slippage. It was clocked to read all 80 columns in a specified length of time and the cards just weren't going through at the proper rate. Somewhere or other, it was slipping as it went through the feed rolls. And I know we brought in high speed movie cameras, we brought in everything. We were sitting there--the engineers and myself--just looking at these high speed photographs of the way the tail end of the card was wiggling as it would go through. There were all sorts of conjectures as to what was happening until one little service man out in the area said, "Heck, you aren't getting enough friction off these polished chrome rollers. I can fix it for you." So he wrapped a couple of rubber bands around the rollers. We never had any trouble after that.

**TROPP:**

[Laugh]

**YOWELL:**

Called Dayton and within two days they had neoprene coated rollers down and that cured the problem forever.

**TROPP:**

Did you ever figure out why it worked here so well before you did that?

**YOWELL:**

Yes. We used every card--every card we tried had been an IBM made card. The cards were punched with a test program which was written by a group in Hawthorne. Hawthorne's purchasing agent had bought Globe cards because they were cheaper and the Globe cards had a different coefficient of friction than the IBM cards and that was it. We tested all other cards we could get, including some from, I guess, it was GE had bought part of IBM's card making business under Mapaqua Company or something like this. We tested those and those didn't work either with the polished rollers. With neoprene rollers, we could put any card through there we wanted and everything worked fine.

**TROPP:**

That's wild. [Laugh]

**YOWELL:**

Isn't that something? Who would have thought that using anything but an IBM card, the surface of the card would have that much difference, but it did. Well, those were the sort of things that you just can't foresee in designing an acceptance test. We were lucky in finding that one out.

**TROPP:**

Of course, the question of reliability is kind of an open-ended one in that period;

**YOWELL:**

It was.

**TROPP:**

because what standard do you use? I mean, when you say a machine had to run for a specified length of time, some problems, like the oil company doing seismographic testing can stand a certain amount of down-time.

**YOWELL:**

Yeah.

**TROPP:**

There are some operations that can stand near zero down time.

**YOWELL:**

Right.

**TROPP:**

You have, you know, different ends of the spectrum.

**YOWELL:**

And the intriguing thing to me is that at about that time--the late fifties, the early sixties--with the advent of the transistorized computer, the reliability ceased to be a major worry for the user. I don't think we had any question about reliability on the 315 when it came out, but on practically every sale we made on the 304 a customer wanted some sort of a specification of the reliability of the machine. And I'm sure that that occurred with other manufacturers as well; probably not with IBM, with their reputation. For the other smaller manufacturers, I'm sure this question also came up, but it was not merely the advent of the transistor but also the fact that they were getting enough machines in the field. And they were proving themselves out.

**TROPP:**

Yeah, the level of confidence.

**YOWELL:**

The level of confidence generally rose to the point where this is no longer--

**TROPP:**

Back to the ElectroData and the Institute's role, or the Bureau of Standards role, then, in establishing reliability standards, were they in a sense going to JPL and saying, "what can you stand then we'll see if the machine does it." Or were you operating under another basis?

**YOWELL:**

It's my recollection that JPL came to us and they said, "This is what we've bought. This is the machine. This is the specs and this is the reliability. Can you write us a program that will demonstrate that they lived up to their side of the contract?"

**TROPP:**

I see.

**YOWELL:**

And I still don't know if anybody really has gotten very far with standardization.

**TROPP:**

That's still one of the big fighting grounds, or battle grounds of industry.

**YOWELL:**

Yeah.

**TROPP:**

But at this point in time there wasn't anything you could grab onto.

**YOWELL:**

No.

**TROPP:**

The early group, I guess, in SHARE during the early years were able to establish standards in certain areas, but that soon vanished. By the early sixties that was gone too.

**YOWELL:**

Yeah. And now about the only standardization you've got is the ones that have come through common input languages in your punch card, or your optical read, some of your optical read but not all by any means; magnetic characters and the standardization of the higher languages--COBOL, FORTRAN, and PL/I; although nobody but IBM still uses ... PL/I, I don't think. But your basic standard as far as even computer word size goes, has never been resolved.

**TROPP:**

Let me turn this tape over and let's talk about some of the people involved that you know about.

**[End of Tape 1, Side 2]**

**TROPP:**

As you see individuals on there that you can recall, you might indicate their role, what they did, any anecdotes about them and where--if you know where they are now, I'd like to know.

**YOWELL:**

Well, I don't even remember Eric Ackerlund. Floyd Steele was a legend when I got there.



**TROPP:**

He'd already left.

**YOWELL:**

He'd already gone. Don Eckdahl, of course, was Vice-President of Manufacturing when I joined CRC, and that's a job he has held, basically, his responsibility from then till this day, except now he's Vice-President for Manufacturing at NCR. And my impression of Don has always been that he has just been a completely dedicated engineer and there is very little that I have to--I have had very little contact with him.

**TROPP:**

Well--

**YOWELL:**

Now, Sarkissian, I guess, was Vice-President of Engineering when I first got there; and, again, I didn't have too much contact with him; I recognized him and he recognized me, and that was about all. Reed, I don't know. Wieselmann, I don't know. Isborn's name is familiar, but I don't know him. Bernie Wilson I have a vague recollection of, but that's all. John Mc [?], of course, I think is still with us as patent attorney.

**TROPP:**

Right. And I think I can locate him, I think he's in the Los Angeles or San Diego Area.

**YOWELL:**

I think he should be in the San Diego still as patent attorney for the West Coast operation. Al Wolf is another name to me. Jack Donan is a face as well as a name, but I recall very little about him; he was, I believe, another engineer. Bill Collison's name is not familiar. Will Dobbins, I know was there; he was one of the 102A engineers. He was a very silent man and I can remember him trying to get the first 102A checked out, just standing there by the machine with his oscilloscope thinking about it, month in and month out, it seemed. He could never quite get it out. Weaver, I don't know. Rawlins, I don't know. Ohlinger, was probably a Northrop man, I believe, I've heard his name as head of the Northrop group.

**TROPP:**

Yes.

**YOWELL:**

And, of course, Jack Northrop was the Northrop. The boss man. Glenn Hagen I met two or three times, mostly when he was with Bendix and then with ALWAC, and then with--

**TROPP:**

Do you have any idea where he or Charlie Williams are at the moment?

**YOWELL:**

No, I haven't. I haven't the least. Bill Woodbury and Greg Toben were running the punched card group that, as far as I knew at the time, they were the punched card men at Northrop. They came out to the Institute for meetings, and we'd meet under an IBM-sponsored users group at the West Coast at that time. And, it seems to me, that they contributed about half of the CPC concept.

**TROPP:**

Maybe all of it. [Laugh].

**YOWELL:**

Oh, I don't think so. They contributed the multiplier portion. The expanded multiplier. The expanded tabulator portion came from Inyokern.

**TROPP:**

That may be.

**YOWELL:**

And the two went together to the CPC machine, to the Card Programmed Calculator. But the two groups were asking for very different things when somebody said, "why don't we put them together"? And who finally said, "let's put them together," I don't know. It may very well have been Woodbury or Toben;

**TROPP:**

Right.

**YOWELL:**

but the original request to IBM came from different places. I can't tell you who got the idea of putting them together.

**TROPP:**

Well, I can check that with them.

**YOWELL:**

Yeah. Bill Speer is just a name. Al Doig may still be in the--

**TROPP:**

Al Doig is here.

**YOWELL:**

He's here?

**TROPP:**

I think he's here.

**YOWELL:**

He's still at NCR, I'm pretty sure. Dan Daugherty I knew very well, he was, when I first met him he was a 107 engineer. and he went into Washington when the 107 was there for quite a while and then came back to Hawthorne about the time I moved to Dayton, but soon after he moved in to be Administrative Assistant to the Vice President for Engineering, Bob Collard, as his chief electronics right hand man and coordinator between Hawthorne and Dayton on electronics, and coordinator with GE on the ERMA and 304 projects and Dan and I spent a lot of time in Hawthorne writing acceptance tests in 1959 on the first system.

**TROPP:**

Mhm.

**YOWELL:**

And he was a real good solid engineer.

**TROPP:**

Well, where is he now?

**YOWELL:**

Last I heard he was with Talley Corporation in Seattle. Roger Sisson was a service man, I believe, when I first met him. Then, of course, he joined with Dick Canning, was Canning's assistant, and then he was with Auerbach for a while. I'm not sure where he is

at the moment.

**TROPP:**

He may still be with Auerbach.

**YOWELL:**

He may be. Incidentally, Dick Canning is another Cincinnati boy. He was one of three people--I was the second--who once took a course in orbit calculations from my father at the University of Cincinnati.

**TROPP:**

[Laugh].

**YOWELL:**

How he got to be the chief exponent of data processing in department stores I don't know. Vern Walker was pretty much a nonentity to me. Stan Frankel: I remember Stan primarily for the week we spent together at Columbia before he went out to Los Alamos.

**TROPP:**

You were going to, I don't know whether we got this on tape or not. I'm trying to remember if we talked about that early.

**YOWELL:**

Yes, we did,

**TROPP:**

OK.

**YOWELL:**

we did. He and his wife came in to learn how to wire punched card machines

**TROPP:**

That's right. You were talking about--

**YOWELL:**

and they went out to Los Alamos; they couldn't tell me where they were going or

anything else about it.

**TROPP:**

Mhm.

**YOWELL:**

And I think I met him on the West Coast when he was at Cal Tech. I'm not sure of that. Bill Saylor I don't know. Axel Wenner-Gren, of course, is a big name. I never met the gentleman. Eckert I never met, Mauchly I met at Lee's [?]. Dick Baker, I don't know. Cuthbert Hurd I've seen quite a few times.

**TROPP:**

You might want to talk about Cuthbert Hurd, in terms of the early entry of IBM into the field and the role that Hurd played as being their, oh, chief of scientific development, I guess.

**TROPP:**

Well, IBM's entry really goes back to right after the war. When Grosch first came in, they decided to set up the IBM Scientific Computing Bureau at the --on the Columbia Campus, although a different location than the Astronomical Bureau. And he didn't last very long before Eckert came in. And whether he was a holding operation or what, or whether he just didn't fit in with IBM, I'll never know. Eckert then came in to take over and Eckert, of course, was a great choice as far as the technician goes, but he just was not the type of man that IBM needed as a front man. All my experience with Eckert was that he was a relatively quiet, self-effacing gentleman who was primarily interested in getting his work done. And he was tremendously respected at IBM. But my feeling always was that Cuthbert was brought in because he had the appearance, he had the platform personality and he could, was happy representing IBM on a platform before groups and influencing people in that fashion. When I first met him, I guess, at a couple of the symposiums that IBM held for their CPC users at Endicott, I can tell you the years [reaching for volumes]: 1950, 1951.

**TROPP:**

I have copies of those now.

**YOWELL:**

Yeah.

**TROPP:**

I've also got copies of all the original materials on the first 701 users meetings that were held.

**YOWELL:**

That, I don't know anything about. But Herb did a real good job of organizing the scientific representatives who came out primarily for that Card Programmed Calculator and the IBM 701. And once IBM really made the decision they were going to go into electronics and pulled all their salesmen in, and gave them training in the 650, I think that that is the place when the position of an Eckert or a Hurd goes out the window in the IBM sales operation. Same way at NCR. All of us specialists couldn't survive the final decision of the company that all their old salesmen had to be trained in data processing. When that was made, and that was only three years ago, that was the end of the line for the specialists at NCR. Al Auerbach: I don't know Al at all. Bob Shaw: Bob Shaw was the chief engineer on the ELECOM.

**TROPP:**

Do you have any idea where he is?

**YOWELL:**

Nope. Ike Auerbach: Of course, everybody knows Ike. Evelyn Barrison [?] I don't know. Sam Lubkin I met once, I guess. Jacobs: no. Jim Bradburn: I don't know. Phil Fogg: I don't know. Everett Yowell: I can tell you where he is.

**TROPP:**

[Laugh].

**YOWELL:**

Mark Shiowitz is dead. There's one little anecdote about Mark that I think is highly interesting. ALWAC? I'm not sure. When he left NCR, he joined one of the small companies on the West Coast, and one reason he joined them was that he wanted to live abroad for a while. He could speak a little German and a little French, and they were going to set him up as their foreign representative at Brussels. And he took off and headed for Brussels and, somewhere along about Denver on the railroad train, he got to looking at his tickets and saw he had only a one-way ticket. So he figured if all they could buy him was a one-way ticket there was probably something wrong. So he got off the train, wired his resignation, and came back and set up Mark Shiowitz and Associates.

**TROPP:**

[laughter]

**YOWELL:**

Arnie Hestenes: I was very close to Arnie. He was our chief applied mathematician at the Institute for Numerical Analysis.

**TROPP:**

You mentioned the one--

**YOWELL:**

Magnus was his brother.

**TROPP:**

problem--

**YOWELL:**

Magnus was his brother and a mathematician at UCLA. He's the one that developed the constant grading method for a real symmetric matrix--for any symmetric matrix, as a matter of fact. His brother Arnold was the applied mathematician at the Institute. He was at the Institute, but Magnus was at UCLA. And he went down to CRC when the Institute broke up. Came in to Dayton with us from 1955, left the fall of 1956, and went to General Motors. And he is now on the General Motors staff in Detroit, an operations research group there, whatever they call it. I think he's more staff group in charge of data processing than anything else. But he's been there since 1956. He was a very competent applied mathematician. Charles Lindbergh I never met. I never met John von Neumann. Turnbull, I never heard of. Fuller, I never heard of. Stanley C. Allyn. I knew Stanley C. Allyn extremely well. I guess as close as nearly anybody in the lower levels at NCR, because after we moved in, NCR integrated CRC and NCR sales departments in 1956 and moved us all into--'56? No. '55--moved us all from Hawthorne in here to Dayton. Two-thirds of the group came in and another third stayed on at Hawthorne. And when Hestenes left in '56, it was about the time when I was named Manager of Sales, for electronics, of Electronic Sales, which was a holding position, again, I held it for four years until they brought one of their accounting machine men in. But during that time Mr. Allyn would frequently turn to me for advice and guidance on what was going on in data processing. It was during that period they sent me over to England to talk to Elliott Brothers and see whether or not it looked like it was a good deal to sign up with Elliott for ten years as our manufacturing agent and sales agent in the British Commonwealth. I remember only too well the time we had Mr. Allyn and Mr. Ullman, the present chairman of the board, financial officers, and the rest. And they were trying to price out the first 304 system. We had about three days meetings in the Board Room, to which the engineers presented the system and went through all sorts of cost estimates and how much had gone into this development and what it was going to cost us for the programming and how much commission we ought to set, and all the rest of the figures.

We presented four or five plans, and Mr. Allyn sat back and listened to them for about three days. And about four o'clock on the third day he got up and said "we'll price it here," walked out of the room, and that was it. [Chuckle]. That was it. And I think that he only made one mistake with that machine. He was too influenced by the possibility of failure. And we never pushed it the way we should have.

**TROPP:**

You say about twenty were built?

**YOWELL:**

We sold twenty-four and GE sold seven within GE, I believe it was.

**TROPP:**

Mhm. So roughly thirty, thirty-one.

**YOWELL:**

Thirty machines were built and used. Some of them are still in use today. Eh, I went with Mr. Allyn once to a meeting of financial analysts in New York and, I'll never forget, I was there with all my figures to give Mr. Allyn some technical information he might need. And someone asked him for an extrapolation what the size of the market would be; and I got so flustered I couldn't make the extrapolation mentally. He would kid me about that for years there.

**TROPP:**

[Laugh].

**YOWELL:**

Mr. Allyn was really to my mind a great individual. He was king of the company and grew up in the cash register era. He started as Comptroller of the company when the company had no accounting machines, no adding machines, strictly a cash register company; and that was Mr. Patterson's original concept: "Put all your eggs in one basket, and watch it carefully."

**TROPP:**

Mhm.

**YOWELL:**

But Allyn had been through the accounting machine operation. And when I joined the



company, NCR was selling two thirds of the accounting machines in the United States; Burroughs the other third. He had been through the adding machine operation when we acquired the Allen Wales Company, and we never did do very good in the adding machine business. It's just been too cut-throat a business, with imports, everybody in the business. We never did do too well, there. He made the decision we had to go into electronics, and he stuck by it. His only problem was he was afraid of the potential of a major failure.

**TROPP:**

How did that affect company policy? The fear.

**YOWELL:**

I'll tell you how it affected it. Just never were given the resources to grow as far as sales, installations, services, or anything else. He said, "I want you to buy twenty machines. Once you sell those, I'll let you buy twenty more." But always we couldn't take a step forward until we proved we could take a minor step. The other thing was that at that time NCR was bringing out, for the bank business, the Post-Tronics, which was a magnetically coded ledger card which activated, carried pass balance, other information, and activated the posting machines. The company was deeply interested in selling these, as a net result, the three guys we had traveling the bank tour were presenting the 304, but their orders were "present it in such a way that you're going to sell Post-Tronics and not computers." The result of that is we ended up with not one single major bank in New York City using the NCR electronic product.

**TROPP:**

Do you feel that if the approach had been kind of open ended in terms of resources and attitudes that you might have sold ten times as many 304s? That the market was potentially there for that large a number of machines?

**YOWELL:**

I'll say five times.

**TROPP:**

Five times.

**YOWELL:**

A uni-million dollar machine. I don't think the market was there for ten times at that time. I don't think, even if we'd had the resources, I don't think we could have built fast enough

**TROPP:**

Mhm.

**YOWELL:**

to sell and install 200 of them before the 360 cut us off at the pass. I think we could have gotten a hundred easy.

**TROPP:**

Mhm.

**YOWELL:**

Now the thing I don't know, of course, and the decision I always say I am glad I never had to make is where does the money come? The hundred million bucks at forty percent mark-up, the sixty million dollars you put in the inventory, and the other thirty million dollars you plowed into people, and training and software and installation, and the rest of the capital you plow in before the money comes back. And that, of course, was the big problem. But, of course, the company's raised a half a billion dollars or more in the past several years in inventory, so it can be done. But, once again, could it have been done in '62, what could be done in '68?

**TROPP:**

Mhm.

**YOWELL:**

And that's the one that I'm glad I never had to face.

**TROPP:**

Bob Ullman?

**YOWELL:**

Is another, as far as I'm concerned, another very great executive, as far as NCR is concerned. He was, again, a man who started out--he's younger than Allyn, of course, so that he started out when accounting machines were really taking over a good portion of the NCR business. He worked his way up to the executive department, and he has always, he has consistently backed a sound, solid expansion into electronics, because both Allyn--Ullman and Allyn recognized very early that electronics was going to replace mechanics, was the technology of the future, and that if the company didn't switch it was going to die.

**TROPP:**

Mhm. Now, Ullman is currently chairman?

**YOWELL:**

Currently Chairman of the Board. Currently Chairman of the Board.

**TROPP:**

Do you see any advantage in terms of the, this early period, in talking to him? Do you see any insight, in terms of his time?

**YOWELL:**

The only thing I think he might contribute is a different viewpoint on the purchase of CRC.

**TROPP:**

Different [from] what Allyn mentions in his book? The realization that this was the way to go and here they were already buying talent—

**YOWELL:**

Yes, Allyn barely mentions it, although I'm sure he has much more

**TROPP:**

**YOWELL:**

detail, records and references.

**TROPP:**

Right. Allyn has a paragraph on it in his book.

**YOWELL:**

Yeah, yeah. Of course, when you come down to it, they purchased CRC in 1953 was when the first purchase was made. They integrated the sales department in 1955. Of the people that were at CRC, the engineering group, there are still many of the senior engineers at the branch in Bernard[in]o who were with CRC at the time. Of the small sales group, there's still quite a few people left with NCR, who were with that group at Hawthorne in 1955. Many of them are no longer in data processing. They were

accounting machine salesmen, and have since moved up, they sold electronics for a while, have since moved up to management positions in other places. But a lot of that talent that they bought has staid. Seventeen years is a long time.

**TROPP:**

Is Allyn still alive?

**YOWELL:**

No, Mr. Allyn died about four years ago.

**TROPP:**

His book was published in '67, and I thought on an off-chance--

**YOWELL:**

He died three or four years ago. Bob Pearson I vaguely remember. He was a great big, bluff salesman. I think he's with Bank of America now. I think he's with the Bank Americard Divison.

**TROPP:**

That would put him in San Francisco?

**YOWELL:**

Yeah, in San Francisco.

**TROPP:**

I think that's where Dick said he was.

**YOWELL:**

Chuck Keenway: I worked for Chuck for many years. Personally, I just don't think Chuck's got the capabilities that go with the positions that he's held. He was Vice-President for Engineering, he was an accounting machine salesman. He went into it as an administrative job, and he was vice-president all the time during which we worked with CRC and the development of the 304 up to 315. Then he came in, is now sales manager, and my opinion is that he's just waiting to serve out his time.

**TROPP:**

[Laugh].

**YOWELL:**

I don't think that he's ever had the capabilities that he should have had in those positions. Herman Kahn, I knew Herman back in INA days. Did quite a bit of work for him when he was at RAND Corporation.

**TROPP:**

What kinds of problems were you doing for RAND that you can talk about?

**YOWELL:**

I guess that they were primarily Monte Carlo investigations.

**TROPP:**

Simulation?

**YOWELL:**

I think that they were--as I recall the situation, most of their real processing operations that they did, they did on their own equipment.

**TROPP:**

JOHNNIAC, which they had at that time.

**YOWELL:**

Yes. And then the Card Programmed Calculators, they ran a lot there. They had a good, big tab installation and, of course, the JOHNNIAC, then the 701s, and the rest of the stuff. And I have the feeling that what they did was their experimental and research type calculations they'd farm out to us as overflow work and even though many of us were cleared for secret Air Force clearance and q-clearance with AEC and the rest, they kept very close check on security and they didn't let anything out that was really secure information.

**TROPP:**

That brings me into an area here in Dayton that I've only heard people allude to, and that's the war time period, and possible involvement here in the building of machines for crypto analytic work. Have you run into any of that?

**YOWELL:**

Never heard of it.

**TROPP:**

I know there was at least one electronic cryptanalytic device that was built in England in 1943. I don't know anything about it. It's still classified. And a number of individuals have said, "Well, if there was anything similar going on in this country, one of the places it might have been happening was at Dayton."

**YOWELL:**

Well, of course, I don't know anything about that 'cause I came here in 1955.

**TROPP:**

Right. But people haven't talked about it in terms of

**YOWELL:**

No.

**TROPP:**

their wartime involvement.

**YOWELL:**

No.

**TROPP:**

OK.

**YOWELL:**

I know they had a big contract with AC sparkplugs for some sort of control system, whether it was a gun sight or what, I don't know. But there was a lot of that. I understand they manufactured rifles here, too.

**TROPP:**

This is another area that

**YOWELL:**

This is hearsay.

**TROPP:**

I'm trying to chase some--

**YOWELL:**

Yeah.

**TROPP:**

links, some conceptual links more of an engineering nature

**YOWELL:**

Yeah.

**TROPP:**

than anything else.

**YOWELL:**

Well, Herman Kahn is, I guess, he's still the same. Man can talk faster than anybody I ever heard. Is just brimful of ideas. Intense character, Chester Stone I never heard of. Bill Hewlett and Dave Packard are names to me.

**TROPP:**

Yeah.

**YOWELL:**

I never met them. Eric Weiss: not quite sure whether I remember a face that I associate with that name or not. I think maybe I do; but that's all. Everett Allison: oh yes. I knew Everett well. We worked together for three years. First at Hawthorne and then here in Dayton. And he was a real character. He was an uninhibited exhibitionist, as well as a very sharp young man. He never finished college. We'd go to New York, he'd take his bicycle with him, into our hotel, because it was easier to ride the bicycle around New York than to try to walk or to take the--

**TROPP:**

Is he the one who rode his bike to Washington from New York? Or was that somebody--could have been.

**YOWELL:**

Could have been. I know he was very interested in carillons; and he worked one summer as an apprentice, for the fun of it, using a carillon somewhere. I think he's also the one who walked into the Defense Department with dirty tennis shoes on. He was a real character, but he was a bright young guy. He came up with a lot of real good ideas as far as use of the computer went.

**TROPP:**

What was he involved in during the years you--

**YOWELL:**

Commercial applications.

**TROPP:**

This was during the 102, 105, '6--

**YOWELL:**

During the 102 days. During the 102 days. Dave--ok, I don't remember his name. Looks like Dick could-- Dave--Dave--I can see him yet. Dave got himself fired for counterfeiting one of the magnetic cards over from the locksmith's, security locks on the building after hours.

**TROPP:**

[Chuckle].

**YOWELL:**

Held it up to the light and figured out what the mask was, and made himself one, so he could get in any time he wanted to. We had a bunch of ingenious people. I'll remember his name in a minute.

**TROPP:**

Dick still couldn't about ten days ago.

**YOWELL:**

Yeah. Mel Posin, he was our salesman in New York when I first joined the company. He was an ordinary unprepossessing salesman. He joined somebody else, and I don't know where he is. Al Wilson represented us on the West Coast. Meggy Milligan, the last



I heard she was--had taken over Canning-Sisson's publication on--was it the review magazine? I thought she bought that from Canning, when he got out of it. Bill McClain I don't know. K--it starts with K--Dave.

**TROPP:**

Are there any stories or anecdotes you remember about the early period, either on your Math Tables Project or INA or the early days of your Hawthorne period? [pause]. Most of Dick's stories and anecdotes relate, I think, to a period when you were at the Institute.

**YOWELL:**

Yeah; and I have a feeling that I don't have too many anecdotes like that. I think that part of it is because Arnie Hestenes and I took a very different viewpoint of what constituted getting publicity and what constituted doing a decent applications survey than the engineers did. Anything to get some publicity was great by them.

**TROPP:**

How about that chess story that Dick recalled,

**YOWELL:**

Yeah.

**TROPP:**

that was a riot.

**YOWELL:**

I remember it was about that time, when I joined them, that they had the famous baseball episode.

**TROPP:**

I hadn't heard about the baseball episode.

**YOWELL:**

Oh yes.

**TROPP:**

That's a new one.

**YOWELL:**

They got some high-speed cameras out and photographed a pitcher, throwing pitches from both the side and the front, and the back, three angles, and they had some baseball executive-- some good baseball men say which ones were curves and which ones weren't, and then they took them into the 102 and measured up very carefully the coordinates on the film and tried to see how closely they could be represented by a simple parabola and their conclusion was that there wasn't any such thing as a curve, that either the plane was inclined in such a way that it looked like this when really it was simply a parabola thrown in a plane inclined to the vertical, or else some of the other effects on the ball moving at the place where the vertex of the parabola came and they concluded that there was no such thing as a real break. And, of course, I think you'll find a great deal of argument today from practically everybody in the country that's watched baseball on television, because every once in a while you'll see a curved ball really veer that you swear can't be due to merely a straight line path.

**TROPP:**

All you'd really have to do is stand up there and have one of these things thrown at you and then break onto your wrist [laugh].

**YOWELL:**

Yeah. So what I suspect is what they really came into was a problem in instrumentation in gathering your data, their photographs just weren't sufficiently accurate, or measured sufficiently accurate. And that is probably the way out operation we had. Kimball. Dave Kimball.

**TROPP:**

Dave Kimball, ok.

**YOWELL:**

Yeah. The other things that we did. Well, of course, you always did things like prime numbers, and tried to make Nim playing games and these other things that demonstrated computers in those days. I know we had SWAC playing music.

**TROPP:**

Somebody, I think it's Harry Huskey, told me about a machine he's trying to chase down, I think it's Westinghouse, supposedly, and this is all in the realm of vague memory, had a machine back in the thirties playing Nim and at the same time attempted to patent the binary number system. Now does that?

**YOWELL:**

That brings no memories.

**TROPP:**

Rings no bell at all. Because I just talked to Harry on the phone the other day and he says he's still trying to chase that down.

**YOWELL:**

Yeah.

**TROPP:**

I've decided I'll go do a patent search one of these days to see if there is anything on that, because that would be a very early application of the binary system when the few machines that were around were primarily decimal, except for the stuff that George Stibitz was doing at Bell Labs.

**YOWELL:**

I'm sure there are a tremendous number of anecdotes about people at the Institute. Once again, why I didn't have too much contact with the mathematicians which is where a lot of this stuff--

**TROPP:**

Well, from the standpoint of this development that I am interested in, whom would you suggest from the Institute period, outside of Professor Lehmer, being some of the key people I ought to talk to?

**YOWELL:**

Of course, John Curtiss you've talked to, I'm sure.

**TROPP:**

Yes, I've talked to John.

**YOWELL:**

I think Thompson's dead now--Tompkins.

**TROPP:**

Tompkins, yes.

**YOWELL:**

He's gone. Hartree is gone.

**TROPP:**

Mina Rees doesn't remember very much about that period.

**YOWELL:**

What about Milne at Oregon? Is he still alive?

**TROPP:**

I don't know.

**YOWELL:**

He was a fairly old man when he was at the Institute.

**TROPP:**

I honestly don't know. I've been out of that area for two or three years and I've lost contact.

**YOWELL:**

Magnus Hestenes is one at UCLA, for sure. Because he was closely involved in the Institute all the time that I was there.

**TROPP:**

OK. Let me write these down. Magnus Hestenes. That's H-E-S-T-E-N-E-S.

**YOWELL:**

Right.

**TROPP:**

Now at UCLA. Yeah. Milne, if he's alive, would be where? At Oregon or Oregon State?

**YOWELL:**

Oregon

**TROPP:**

At Corvallis or Eugene?

**YOWELL:**

Corvallis. The other guys on the staff--of course, George Forsythe was extremely influential.

**TROPP:**

Yes, unfortunately George just passed on.

**YOWELL:**

Yeah. Forman Acton was there for a while. Last I heard he was at Princeton.

**TROPP:**

There's been an interview done with him.

**YOWELL:**

Yeah. Gertrude Blanch. I don't know where Gertrude is. She was here at Wright Field until she retired but I suspect she moved back East when she retired.

**TROPP:**

Is her field mathematics?

**YOWELL:**

Yeah.

**TROPP:**

She might be listed in the--

**YOWELL:**

Yeah. She was with the Math Tables Project and then with INA. [pause]. She and Ida Rhodes.

**TROPP:**

Ida Rhodes is the one living in Washington.

**YOWELL:**

If you know where Ida is, you can find out where Gertrude is.

**TROPP:**

OK. Well, I have Ida's phone number. I haven't had a chance to talk to her.

**YOWELL:**

They were very close. The other mathematicians who came in would come and go. I know we had--Ted Motzkin was there and Fritz John was there; but they were there for a summer or so.

**TROPP:**

Presumably it's an interesting concept. The idea of establishing an institute like that as a milieu for a small group with a rotating kind of director and funds to bring people in for short periods of time

**YOWELL:**

Yea.

**TROPP:**

which was, at that period, really quite an innovation. We sort of take it for granted now as part of our intellectual milieu in the academic world.

**YOWELL:**

Mark Kac was there for two summers.

**TROPP:**

Right. I've talked to Mark Kac. ET: And Dave Saxon from the physics department at UCLA was there quite a bit. And Robert Feynman was in for one summer.

**TROPP:**

The physicist at Cal Tech?

**YOWELL:**

Yeah. Eh--oh, Cornelius

**TROPP:**

The reason I am asking you

**YOWELL:**

Lanczos was there for quite a while.

**TROPP:**

Lanczos? One of the reasons I'm asking you is that I'm interested in another area of research and that's one that I broached earlier, and that's the development of numerical analysis and the impact that high speed computers had on that development.

**YOWELL:**

I'll tell you one thing that Lanczos taught me that I've kept in mind and used every once in a while ever since has been the effectiveness of Tchebychev polynomials.

**TROPP:**

What's his first name?

**YOWELL:**

Cornelius. Wolfgang Wasow? That's another one who was there for a couple of years.

**TROPP:**

W-S-O-W? EY :W-A-S-O-W.

**TROPP:**

Well, it won't be hard, I think, talking to people like Lehmer

**YOWELL:**

Yeah.

**TROPP:**

and others at the Bureau that I can make contact with in getting, maybe rosters of the people who were around. But that will be an important input for this other area--

**YOWELL:**

Yeah.

**TROPP:**

as well as the ones who might have had some knowledge of the West Coast development. Let me throw another fairly broad question at you. And it's one that people tell me is still around, but I'm not in the industry, and so I just take everybody's word for it. But I, as I look back at the period, the Northrop CRC period, we have, in a sense, an almost independent West Coast development.

**YOWELL:**

Yeah.

**TROPP:**

You also have a beginning of a feeling--whether it was real or not--a feeling of the individuals involved of an East-West Coast kind of clash, division. Whether it's the west coast group feeling left out, put upon, not given proper credit for doing--whatever the reason, many individuals on the West Coast felt that they got left out when credit got passed out. It's also been indicated there's still kind of an East-West clash because of the Eastern environment and the East being the center of so many things. Do you have any thoughts or reactions to this kind of clash between the two coasts of the US?

**YOWELL:**

Well, I think that the clash was primarily engineering.

**TROPP:**

There were clearly two philosophies in that early period.

**YOWELL:**

There were clearly two philosophies. I never knew an East Coast engineer. I know that the West Coast prided themselves on what they'd done.

**TROPP:**

Mhm.

**YOWELL:**



And I know that they considered it to be better than what had been done in other places in the country.

**TROPP:**

Well, in terms of what eventually happened, they were right.

**YOWELL:**

Yeah. As far as we were concerned, in the applications and usage of the machine, during the early days, and I would say up to the time when Grace Murray Hopper started doing automatic programming, we felt that we were doing all the real good work in engineering and programming on the West Coast, we didn't have anything to learn from anybody else. In fact, when I first heard that Grace Murray Hopper was going to come up with automatic coding languages I said that was impossible. Which was not, obviously, the case.

**TROPP:**

[Laugh].

**YOWELL:**

There's been such tremendous advantages. But I have a feeling that with all of the aircraft and test ranges, except for the Banana River in the military and the Atomic Energy installations on the West Coast, that they were ahead in engineering calculations in the late forties and early fifties, and I have a feeling that the big switch to the East Coast, with the growth of commercial data processing, which led to the much bigger installations here and to many more people here--and if you get more people you should get more ideas out of them.

**TROPP:**

Do you still feel like individuals were in one environment or the other, feel a kind of coastal clash?

**YOWELL:**

Well, all I can say is that I feel more comfortable out on the West Coast, at a West Coast meeting rather than an East Coast, probably because I know so many more people when I go out there. In fact, I don't go to hardly any meetings any more. Waste of time. You can go to meetings, or you can do work.

**TROPP:**

Right. Meetings are nice places to talk to people, though.

**YOWELL:**

Yea. Last time I saw Saul Rosen was at a meeting.

**TROPP:**

I'll be seeing Saul tomorrow night.

**YOWELL:**

Give him my regards.

**TROPP:**

He said to pass his on to you when I told him I was coming here, he said to be sure to see you. I'll be there because Maurice Wilkes is going to be visiting him. Saul is interested in this area, as you know, and he has written a number of articles on the early history.

**YOWELL:**

He was at UCLA and close with those of us at the Institute, in the early fifties.

**TROPP:**

But you had originally known Saul at Columbia

**YOWELL:**

No, I had known him at UCLA.

**TROPP:**

You first met him at UCLA?

**YOWELL:**

Yeah, when he came out to UCLA. He and I were one of the standard group of bachelors who went to dinner together every night for a couple of years; but he was after me, as far as New York goes.

**TROPP:**

So Saul is one then that I can talk to about this west coast environment because he was there during the same period that you were there.

**YOWELL:**

Yes. He came in about three or four years later than I did, but he knew quite a mob of people at UCLA and he'd come to all our seminars at the Institute so he would also know some of the people and be able to evaluate better than I can some of the mathematical work that was being done.

**TROPP:**

Talking to Gerry Estrin there not long ago and I guess he would have come to UCLA much, much later.

**YOWELL:**

I would think so.

**TROPP:**

Because he came out of Illinois. And then he went to Israel; and built the WEITZAC, which was a copy of the ILLIAC, so I guess it would have been later that he came to UCLA; he wouldn't have been there during that INA period. You mentioned the funding problem that caused the dissolution of INA as a West Coast branch of the National Bureau of Standards. But essentially it was integrated into the UCLA program.

**YOWELL:**

Yeah.

**TROPP:**

Do you have any memory as to what caused that particular crisis?

**YOWELL:**

I understand it was political. We operated completely on transferred [buzz] funds ([referring to buzz] oh, catch that one)--we operated completely on transferred funds, so that, somewhere or other, somebody in Congress came up with the fact that the Bureau was running a fifty million dollar a year operation on seven million dollars of appropriated funds, the other forty-three million was transferred.

**TROPP:**

[Chuckle].

**YOWELL:**

As a net result, somebody started saying, "no, you've got to cut down on these transferred funds, we have to have control of it." And the net result was that we lost the Air Force backing.

**TROPP:**

I guess I don't know what you mean by a "transferred fund."

**YOWELL:**

Well, what we got, the money we got, was funds appropriated to the Air Force for their work. And the Air Force then turned around and gave it to the Bureau of Standards

**TROPP:**

I see.

**YOWELL:**

to serve as their contractors, to do work for them. And they maintained a liaison office on the West Coast to keep a track on it, put his approval on which problems we took. But Congress, somebody in Congress was saying, "look, we gave that money to the Air Force, not the Bureau of Standards. And what you're doing is, you're building up the Bureau of Standards with money that Congress meant to go to the Air Force. So you've got to stop." That's the way I understood it. Of course, it was all tied in, part of it, to, going back to, the big battle of the ADX-2 battery additive.

**TROPP:**

Oh yeah. Right.

**YOWELL:**

Ed Condon was Director,

**TROPP:**

Right.

**YOWELL:**

and all that hassle.

**TROPP:**

Yeah.

**YOWELL:**

That was part of it. And some time at that time, the Bureau, somebody in Congress was criticizing the Bureau for not keeping their windows washed. And there was all sorts of picayune things that came up. The net result is, the Office of Naval Research managed to keep the funding, to keep the theoretical part going. But the Air Force, which had been supplying the bulk of the funding for the computational center, was cut off. We couldn't get those funds anymore, and that was the end of the computational center on the West Coast, except for what UCLA took over, and then they took it over only for their internal uses within the university community, whereas we had been servicing the entire West Coast.

**TROPP:**

Well, how close were you to Condon?

**YOWELL:**

I think I saw him twice.

**TROPP:**

Oh. OK, then you can't add much to what I already have on him.

**YOWELL:**

Nope.

**TROPP:**

Are there any other individuals that--well, I want to talk a little bit about Wally Eckert. You described him in terms of this

**YOWELL:**

Yeah.

**TROPP:**

quiet involvement in his own problems. As an astronomer, how would you look at him, in your period in astronomy? How would you view his work?

**YOWELL:**

Well, of course, my period in astronomy was one of the periods in the great advances in

astrophysics. And it was before the space age made the problems of celestial mechanics, again, of paramount interest. The net result was, in my time as a graduate student, and even before, we tended to look upon the celestial mechanics men, which included Brouwer at Yale, and Eckert, and Herget, Herrick at UCLA, we tended to look upon these guys as the passé guys; same way as when you started to do the men who do positional astronomy today in a field that's important that it gets done and people keep up with these things, but it's really not contributing too much fundamentally new along the frontiers of research. And suddenly we find we've got to send men to the moon, and the whole problem of celestial mechanics takes on a very different point of view. But he was an excellent mechanics man.

**TROPP:**

In terms of your earlier work, what would you rank as some of his major accomplishments?

**YOWELL:**

Well, I would say that, probably, to me, his major accomplishment was proving out Brown's lunar theory. And probably his second most one was mechanizing the Nautical Almanac Office. As far as his research in celestial mechanics goes, I'm just in no position to evaluate.

**TROPP:**

I see. Who would you suggest, Schilt, I guess, would be one.

**YOWELL:**

Herget.

**TROPP:**

Herget, at Cincinnati. Yeah. Very good.

**YOWELL:**

Very definitely. Paul worked with him for many a year, and Paul knows his field very well. He's another celestial mechanics man. Schilt is not a celestial mechanics man. Schilt is a statistical astronomer. Schilt is getting pretty old by this time.

**TROPP:**

Yeah, but he's apparently still active.

**YOWELL:**

Yeah.

**TROPP:**

It may have been something about the environment at Columbia that kept so many people around for so long [laugh].

**YOWELL:**

Yeah. I was going to add this one little story about Franz Alt. I remember very well one time when he came out to visit on the West Coast, and Arnie and I were talking to him about how you come up with reasonable estimates for the cost of a job. He said he had his own rule: He took a ruler and he measured the equation, and he charged \$5000 dollars per inch. HST;[Laugh]. Was that printed or hand written? BOTH: [Laugh].

**TROPP:**

That's a rule he's never mentioned.

**YOWELL:**

For some reason, I thought that might have slipped his mind.

**TROPP:**

Any contact with Dick Clippinger, who was involved in one of the CRC machines?

**YOWELL:**

I met him once, that's all. There're a lot of these people I met, that's all. I can't really say much about them. Where'd he go after that? He was connected with one of the big ones.

**TROPP:**

He's with Honeywell now.

**YOWELL:**

Honeywell, now. I think he was somewhere else when I met him. Don't know. These people bounced around so much in those days, it's hard to keep up.

**TROPP:**

He came out of the ENIAC group.

**YOWELL:**

Was he at Raytheon? Was that the way he got down here? Maybe--I think maybe that's it.

**TROPP:**

I think so.

**YOWELL:**

I think maybe it was at Raytheon that I ran into him, before Raytheon and Honeywell got together, and then Raytheon pulled out, leaving Honeywell with the operation. Of course, the other thing about the business that, I think, might be intriguing, is, I had the feeling for a long time computer that anybody with a sick computer business tried to sell it to NCR.

**TROPP:**

[laugh]. Besides CRC, who did?

**YOWELL:**

Underwood, Raytheon. Those are two that I helped evaluate.

**TROPP:**

What were the net results in each case?

**YOWELL:**

The net result, as far as Underwood goes, is that Mr. Allyn decided he was willing to make an offer for the Underwood business, but all he wanted was the typewriter business, he'd kill the rest of it. And so, his offer wasn't nearly as good as the Olivetti offer, and Olivetti took over. And as far as Raytheon, we never got very far, because Honeywell decided that they'd rather take it over themselves. So as the other half partner, they bought it out. We kept hearing about CDC trying to sell out to NCR, and every time anybody would come out and was sick we'd hear about they were going to try to sell it to NCR.

**TROPP:**

That's what happened after they bought CRC, they got a reputation [laugh].

**YOWELL:**



Yeah.

**TROPP:**

Well, in looking at the CRC position, when their major financial backer was killed and, you know, at that point in time I'm surprised that they were able to find anybody to help them out. All their prospects looked bright. The amount of money required at that point in time, with the immediate prospects, just made it very questionable.

**YOWELL:**

Oh, I still think it was probably a very good deal for NCR.

**TROPP:**

Well, as it turned out in terms of, if nothing else, the talent and the experience.

**YOWELL:**

That's right.

**TROPP:**

They were really buying a time factor that they couldn't get any other way.

**YOWELL:**

That's right. And the other factors that, to my mind at least, slowed NCR's growth in the electronics business, consigned them to a poor fourth or whatever they are in the business, are ones that would have influenced them under any condition, for they would have been ranked five years later. And five years later just isn't good enough. So I'd switch the problems that had Mr. Allyn so worried, namely the ability to manufacture electronic equipment. If he were alive today, I think, he'd be going through hell with the problems they've got manufacturing mechanical equipment today. The quality of NCR equipment has deteriorated along with the quality of everything else in the last twenty years. You just don't buy stuff that's made the way it was before; whether it's an appliance or a cash register or anything else. The quality just isn't there. You go out, you look around, you see cash registers which were made sixty, seventy years ago, still in use. The one problem, of course, is they get more complex; the other problem is that they keep trying to keep the cost down. As the cost comes down, some way or other you take out reliability.

**TROPP:**

One other thing that I've neglected: You left NCR three years ago.

**YOWELL:**

Yeah.

**TROPP:**

and you're doing what now?

**YOWELL:**

Right now, I'm management consultant, primarily working with food chains and problems of applying data processing management science techniques to inventory and selection problems, merchandise line selection problems. I have a feeling that this industry is getting the way the banks were the year before ERMA. They're ready and they're looking for something.

**TROPP:**

Do you see this primarily as one of education, or one in which you're going to have to design or come up with a new kind of systems analysis?

**YOWELL:**

You have to come up with new kinds of systems analysis, as far as I am concerned.

**TROPP:**

To the companies that are already convinced of the needs.

**YOWELL:**

Yeah

**TROPP:**

And the question is to solve the problem

**YOWELL:**

Yeah.

**TROPP:**

that exists, has been around, apparently, for a while.

**YOWELL:**

It's been around for a while. The problem is real simple: You've got space on the shelf for 5000 items. You've got 150,000 to choose from. Which do you pick? That's problem number one. Problem number two: Every store has to get about seventeen percent on their merchandise. They've got to pay their people, they've got to pay their rent, they have to make a little profit, and they make precious little; they've got to get the seventeen percent. And they're all trying to convince you they sell cheaper than the next guy. How do they do it? They sell the 100 items that everybody knows cheaply, and they raise the price on the other 500, or rather, 4900.

**TROPP:**

It seems to me that there are other basic questions, and I'm not an expert in the field, but, you know, as a consumer, it seems to me that when you say there's space shelf for x number of items, it seems to me that one of the first questions is "what is an optimal space availability, say, for a grocery store"? More and more, you walk into a grocery store, you find them branching into larger and larger variety, from the standpoint of optimization, of space utilization and turnover and the realization of the seventeen percent and everything else that goes into it, is there an analysis of the optimal space and the optimal variety of categories that you service?

**YOWELL:**

I think that there is. But I think that there is not any unique one. It depends on the strategies you're trying to adopt.

**TROPP:**

Right, yeah, yeah. It's like any game problem.

**YOWELL:**

Yeah. And basically, there's a half a dozen different merchandising strategies for a food store. One, we've got a small chain here in town that prides itself on having a wider selection than anybody else in town. You go for a wide selection and if you go for a wide selection you pull people in because you have got things that other people don't have. That's one type of clientele and its one strategy that will be successful in, amongst a group of strategies, serving a community. The second strategy is consistent low price. And you get this in two forms. You either get it in the form of the low priced standard store which carries a full, but not expandable item price and normal services. Or you get it in the real discount store. The K-Mart food department which they go for a very limited line of only the fastest moving items. And they sell those real cheap.

**TROPP:**

So that their inventory at any one point is very low.

**YOWELL:**

And their inventory is very low. And you get to the standard store, and a third of their items will turn five times a year. Because you've got to put a minimum amount of the case up on the shelf.

**TROPP:**

Mhm.

**YOWELL:**

It costs you too much to try to put half a case up 'cause your labor eats up the difference. So you've got to put a case up. If you have twenty items in a case, which is about the average, anywhere from 2 items per case up to 250, but the average is around 20. You put it up, you move one a week, and you turn two cases a year.

**TROPP:**

You know, there's another model that has vanished, in this country, and that's the model where, instead of the large store you have a series of stores, each of which is a specialty item store,

**YOWELL:**

That's right.

**TROPP:**

each of which then deals only in one large area of commodity.

**YOWELL:**

That's right.

**TROPP:**

And this is a very different kind of strategy than the one you describe.

**YOWELL:**

There are a few of those. You'll find a few in any community. Produce is probably the most frequent.

**TROPP:**

Produce and meat market. Meat market, I guess, is vanishing.

**YOWELL:**

Well, I don't know. What you find is a meat market that carries a certain number of dry groceries as a service to their meat customers. We go to a place, basically a meat market, for all our needs. And he must do sixty percent of his total dollar volume in meats. Similarly, there are specialty shops. I guess the snack shop that's associated with party stores. Your convenience stores are a completely different type of strategy. A narrow, limited line of the most frequently used commodities--

**TROPP:**

Very quick turnover.

**YOWELL:**

Quick turnover. High markup, long hours and a low volume. See, they'll operate on a, they operate normally on about thirty-five percent markup, instead of a seventeen percent markup, but your average sale is going to run fifty cents on the dollar, instead of fifteen dollars it does at the grocery store.

**TROPP:**

Mhm.

**YOWELL:**

And they go open for long hours, it's generally "mom and pop" type operation and I think that there is opening up a wide area for a very high mark-up, very low frequency business,

**TROPP:**

Mhm.

**YOWELL:**

if somebody can only get a big enough area to serve.

**TROPP:**

Well, in the Washington area, the "mom and pop" stores are the Seven-Eleven chain,

**YOWELL:**

Yea.

**TROPP:**

which exists three blocks apart.

**YOWELL:**

I think they've got some real big gourmet shops in the Washington area, too.

**TROPP:**

Just one that I know of and one of the department stores has just opened up, which is another, pretty much of the same kind of thing.

**YOWELL:**

Yeah. So you're beginning to get away from the standard, from a standard

**TROPP:**

I guess, what I was, I guess--

**YOWELL:**

But the candy stores are gone,

**TROPP:**

Right.

**YOWELL:**

practically completely.

**TROPP:**

Now you indicate that there are a lot of models then.

**YOWELL:**

Yes.

**TROPP:**

So you don't have one solution you have a variety of solutions.

**YOWELL:**

That's right.

**TROPP:**

The Safeway solution, super market solution, is very different from the 'Mom and Pop' solution, the specialty store solution, discount store.

**YOWELL:**

What I also believe, is that in an area like this, the difference in a Liberal--which is our local chain--supermarket, in the southern suburbs like this, which is a high economic area, in the west end, which is your ghetto area, in the east end, which is your blue collar area, that the basic model remains the same as the Liberal, although the details of implementation vary.

**TROPP:**

Mhm.

**YOWELL:**

But the model remains the same, because the strategy remains the same for the chain. The strategy for the Liberal, which is a standard supermarket, varies from the strategy of the Imperial Food Towns, which is an everyday cut-rate business, which differs from Miami Valley markets, which is a wide variety, somewhat higher-priced, operation.

**TROPP:**

Sounds like a fascinating area to be in at this time.

**YOWELL:**

It's a long way from astronomy, but it is very definitely a data processing problem.

**TROPP:**

Mhm. I'll turn this off and thank you very, very much.

**[End of Interview]**