

COT MEETING

Los Angeles, California

December 1, 1971

Moderator: Mr. Mills

MODERATOR: We are at the Cockatoo Restaurant and it is December 1, 1971. We have Owen Mott, Erwin Greenwald, Charles Swift, Joel Swartz, Jack Strong, Wes Mellon, Joe Smith, Ellis Meyer, Oliver Smith, Bernstein, Jean Malin, Roger Mills, and This is a group of people gotten together to talk about the history of the old computers on the West Coast and some of them, I think, are foreign-born from the East Coast. You came from the East, didn't you, Charlie? Charlie: Yes. I think what we'll do is each talk and identify ourselves, when you got started in the industry and where, and what the first machine worked on was. Let's start down here.

BERNSTEIN: I started in 1954 with Rand on the Joniac and 701, in reverse order.

MEYER: I started on Douglas Aircraft Project at Rand on October 23, 1947, the same date the IBM machines arrived, and my first project was counting one million random digit

MILLS: I started at Northrup in 1951, January, during desk computing getting ready to go to Binac. I worked on Binac and my first project was to code up nims for demnnstration.

J. SMITH: I started at Purdue University speaking through a bad tooth which I lost in a scuba accident. I started at Purdue University in 1953 on a CPC and then went on to a Datatron which was my first contact with a

MELLON: I started at Harvard on March 1, 1947, and went on to Rand in 1948.

OLIVER SMITH: I started at Remington Rand on bad equipment in 1946, in Los Angeles.

JACK STRONG: I started in 1936 in Oklahoma City with a company
ON IBM equipment with lug wires as big
around as my arm.

JOEL SWIFT: I started, I guess, at Columbia University. I was a graduate student there in 1953 and went on to Rand in 1954. I worked on the Joniac and 701.

CHARLIE SWIFT: I started in 1951 on the Seac in Washington, D.C. and went on to the 1103 at Convair in 1954. My first project was to and walk in a 27 dimensional space.

ERWIN GREENWALD: I started in 1950 at Rand doing hand computing, 604, CPC, and what have you. The first big machine I used was the Univac I which was out in Philadelphia.

OWEN MOTT: My first exposure was as a key punch operator for the U.S. Army in Cherbourg, France in 1945.

QUESTION: Perhaps since Jack Strong--you went back to 1936....

STRONG: I didn't go back; I started.

QUESTION: There were computers in 1936 that you could recognize?

STRONG: Absolutely there were very intelligent computers then and some people. We had key punch machines and we had tabulators which you would put cards in and it added them and subtracted them.

QUESTION: When did you go to North American?

STRONG: In 1941.

QUESTION: And you were in the Finance Division then?

STRONG: Yes. Seven years later Owen Mott came to work for us.

QUESTION: Still punching cards?

STRONG: Still punching cards the last time I saw him.

QUESTION: I think we need names of people working with you in those days, I guess. Oliver, you had people that were working on the CPC-- the designers of CPC--at Northrup. Do you remember their names?

O. SMITH: Yes, Bill Woodbury and Greg ^{Tolson}~~Tolton~~ were the two the CPC guys who worked on ~~that~~; it wasn't called that at the time.at Northrup.

QUESTION: Was it the

C. SMITH: No, that was way later and the same two guys, by that time, were working for IBM who did the design. The last I knew of them, Woodbury was up in San Francisco working as a consultant for Ampex and Tolson ~~Tolton~~, some time before that, was still working for IBM.

QUESTION: What was that original CPC made out of, an old 603 or what?

C. SMITH: I can't remember but they had a very large trajectory problem by now to remove. They were ^{originally}~~first~~ going to do it on the differential analyzer and ended up with UCLA and got an estimate of I don't know how many years on the job for the required accuracy. They got hold somehow of the circuit diagrams of a ^{and summary punch}; they discovered that these two things could be linked out with a ⁶⁰³/multiplier and essentially pass numbers between each other by way of the summary punch. I think at that time they actually constructed a working model out of the thing there. My recollection is that they did not have IBM's permission. They then, I believe, decided that they wanted some additions which only IBM could provide. IBM wasn't about to go along with these nuts. And again, according to the story as I recall, Northrup called Tom Watson---one president

to another---and asked for this. They threw the stuff back East anyway and made the additional modifications. I think Woodbury and ^{Tolson}~~Tobin~~ had already made the thing run. They had made the additional modifications. Then IBM got the idea that there might be a few others who would like such a thing and started to do a CPC, adding in the process the bathtub memory. The icebox was not part of the original design. The rest is history.

For the record, Jerry Mendelson knows a lot of this story, too. He is currently at Xerox. Mendelson knows the Binac, too. I took my first course in programming from Jerry Mendelson; but he was handicapped by the fact that he didn't have a Binac.

QUESTION: What date was Jerry teaching a course in programming?

C. SMITH: I left Northrup at about the time Roger arrived, January 1951.

He started Northrup roughly in the middle '50's, about the same time I started at Rand.

That may be the first so-called course that was ever taught in this area.

They had SEAC courses at UCLA---not Seac but Swac....

That was a little later. They had courses at Aberdeen also by people like John Crowler, Fred Goren, and those people.

Do you have the original notes from that course?

I doubt it.

Q. About the Binac courses, was it programming or routine exercises?

ANSWER: Binac didn't have a submachine linkage; we had open sub machines and what you did was actually involve a code and direct line of what you wanted to do. Instead of code, you coded an absolute There was a set of codes sitting in there and you changed the numbers to match your program

as you went through 512 locations, all of them usable.

That reminds me, I worked on the Edvac---one of the few people that ever did. The Edvac didn't work much more than the BinaC.

Q. Owen was a Swac programmer, weren't you?

A. No.

Q. Do you mean, Owen, that you didn't work on the Swac?

A. No.

Q. You lied to me when you came looking for a job at North American in 1951. You told me that you were one of the principle Swac programmers.

A. No, I worked on the 604's and CPC's; I wired a number of CPC's. But I never worked on a Swac.

I had fifteen people at one time and 25% of them programmed Swac.

Fred Hoffman was an engineer; he worked on Swac for several months and finally gave up and moved back East to the Seac.

At the time I was working for him, we were working on the Seac and the Seac started acting up. He had big high priority, so he went up to Aberdeen and he got a crew doing the same set of equations both on the Eniac and the Edvac.

Herman Kahn took a crew back to Eniac and they found out that they needed so much 604 support to prepare the cards for it that I was flown back to do the 604 work to prepare for the Eniac.

We just about got it going on the Edvac and one of those draftees that was the engineer they had for the Eniac blew out half the one night and it was out for two months. So that was the end of my experience with the Edvac.

*I lost a whole
story at this point.*

We used the Seac about half time there for quite a long time.

QUESTION: Were those the same darn things that we wound up doing on the CPC and eventually on the Univac?

ANSWER:

QUESTION: That was the one where Keller kept insisting when results printed out that energy could, in fact, go negative. After two weeks of fighting this one, he went home and we that you had predicted. They couldn't use automatic time step changes because the equations were off. You had pointed that out before we left Philadelphia, and everybody forgot it.

QUESTION: Where are Talbott and Woodbury now? Still at IBM?

ANSWER: No. Talbott might be; his brother was in Albany for a long time. Woodbury was in San Francisco the last I heard. IBM did show its gratitude by giving them jobs at which they developed a little. Woodbury was not one to stay put; in fact, when Woodbury developed a problem with the CPC and wanted to think, he would get a Coke. He'd have 20 or 30 half-drunk Coke cups all over the CPC and everything in the room. He'd write himself a great big sign saying "Go to the center," "Go to the punch," go here, go there. There was a big 18x18 Masix---he invented it. He got the first step done and he didn't bother doing the rest of it. He just wanted to see if it could be done.

Another guy who was there then, who holds the record for being the longest hold-out for plug boards, is

Contrary to Jackson Granholm or Eric Rex didn't have much hand, in my recollection, to development of the CPC.

There was an open shop user who came in and availed himself of....

He was down in the machine room while it was all going on.

He was a great director; he used to be my boss.

Another name at Northrup at that time is Irv Reed who is a professor at SC now who was heavily involved in the That was off in a classified building and I never knew much about it. But if you're interested in the he is an available source. because he was at MIT for a long time. That was on the East Coast, too, / But he broke off and formed a company down in Miami Beach.

There were several break-offs; they broke off from Alwac, they broke off and formed CRC which became NCR. They broke off and Paul Steele went down and formed an outfit in La Jolla called Dico. A guy named Hugh Millets was his patent attorney--he came up through the electronics ranks and became a patent attorney for the thing. A Dico machine was supposed to have had-- according to instructions was supposed to have bit patterns and spacing of the bits to tell you what you want to do with the bits coming through. So if they were having a code to interpret that was the cycling of the bits coming through, it was supposed to work like your brain that tells you what you are going to do next. I don't know what ever happened to it.

I knew this customer engineer from IBM over there, he is now president of Data Processing; his name is Tyson.

I remember Tyson. Some IBM guy must have had a finger in the pie on that CPC; otherwise, they wouldn't have got those drawings out of the lock. The drawings were locked up inside the equipment and somehow they got to them. But you can steal/ almost anything from IBM if they're not looking. But Tyson was one

of those guys over there that had to play the "white shirt" game working on these greasy miserable machines. There was a guy besides Tyson that was at Northrup that was very heavily involved in maintaining the machines and you could get all the answers out of him. If I remember the guy's name--I thought it was White but I'm not certain.

When you talked about those spin-offs a few minutes ago, Roger-- the spin-offs were what?

Well, the guida punch was developed by Reed, Mendelson, and Steele, but Steele was actually the guy that did it. Then there was another guy that came out of UCLA in about 1950--one of the doctors out there. The guida punch was gone by 1952 probably--no it was gone by the beginning of 1951. Steele was gone; Reed was gone. Reed had gone before I left; I know that. When I got there the plain people of the --Mendelson and that bunch-- were still there. The spin-offs had started by the time I left.

Didn't ^{some} part of the guys go with North American and part become Modern

I think they went out and started their own company. CRC was sitting right across ^{on Broadway} from Northrup when it got started.

Al went with a different bunch.

The guys at finally came--Fairchild and that bunch finally came

He came directly from Northrup and went down to Manhattan Beach....

You're talking about Walker and Pierce? And they formed a computing outfit for anybody's computers--a service bunch. Walker and Pierce formed an outfit for computing services and used calculators to do their job. A bunch of Northrup people were moonlighting for them, and Walker and Pierce were the heads of the Binac program. They had about six Binac programmers and the idea at Northrup was that every group had a consultant; that consultant

would learn the machine and he would let everybody else do the job. Jess or Wright White formed a general purpose board that would solve all problems for all things on the CPC; that was his claim to fame. When you didn't use it when you worked for him, you were in trouble; I was in trouble.

I can't think of the history of all of the outfits that broke off of Northrup. Who started the Recomp bunch? Was that Baker?

No, he was involved in the LCP-30.

Who else? Hagen was in that bunch.

Do you remember some histories of maybe, if possible, 1953-54, people involved in the Datatron machine. There were a number of people floating around there--a lot of mathematicians like Paul Brock ~~Richard-Roth~~, Sybil Roth; Richard Blanch was there and Jack _____, but he didn't stay around.

The story goes that the machine was designed by some Norwegian while he was studying a year at Princeton and toward the end they were doing it by mail. The engineer they elected, I think, was Lloyd Karee. The design kept going back and forth by mail while they were building the prototype. After a year, this cat packed his bags and went back to Europe, I suppose Norway. His name was something like . It was a remarkable machine design.

Who are some of the people who might know about that?

Paul Brock who is now with either the University of Vermont or the University of New Hampshire would know all about it.

Robinson's there, too.

Right, he worked....

Which Robinson is that? Not Jackie?

Paul Brock is the easiest one to find.

I don't know where the other people are now. They are probably listed in the roster.

An early mathematician that had an awful lot to do with being able to use the punch card equipment efficiently was Cecil Hastings at Rand. His rational approximations that he developed; in order to get those out with accuracy, he had me wire 18-place floating point boards for the CPC. The board worked fine on everything except for one little problem-- I couldn't divide so I took a reciprocal and had that available to multiply afterwards. Everything worked fine until we tried to divide by one, but since the reciprocal of one is one and not a decimal and I could not

Did you have to go with reciprocal tables?

No, I did the division using the 605 that was in the CPC; it took about four cycles to divide 18 place--to take reciprocal of 18-place accuracy. Would you be interested in the wiring diagram for that board, by the way? I have that at home.

That would be very helpful to me; I'd like to have it.

The next most helpful thing that's ever been designed by anybody was designed by Owen one day. He came around and said, "Hey, I'm working on something; I don't know if it's going to be any good or not but I'll tell you about it." And it turned out to be that did decimal arithmetic to 364 places. Is that right, or 318. It doesn't matter, you know. And he's probably the only guy who ever computed pi to 364 places on a 71 or anything else, for that matter. It was very useful.

I talked to somebody just the other day. Somebody called me up and asked me how you do multiple....

Another thing that ought to be talked about, and I thought Patrick was going to be here, was the development of the system, the GM-North American monitor system. That was the first monitor system that I knew anything about.

_____ Sweeney and Lou Gadt did the....

Lou Gadt, by the way, is in Mexico and I do have his address.

Another thing we ought to do is get all these addresses of everybody you know now, and then I'll get one to Henry and one to the DCA meeting.

Who was the guy that used to come up and work on the Seac in the early days from Los Alamos. His name escapes me but.... This guy was a very good programmer but he also had an alcoholic problem and I think he dropped out of programming about six years later.

Well Alan had a marital problem about that length of time and he was doing a lot of drinking but I don't know if it's the same guy or not. He wasn't a brilliant programmer, however.

I guess one of the things that's just beginning to come out in the conversation is something that never shows up in the literature and that is what the atmosphere was like. There seems to have been a state of that vanished when you got out into the post '55 era. Maybe the best way to get at the atmosphere and what it was like is through the various anecdotes--- the kind we were talking about earlier.

One of the things that was rather prevalent in those days, I think, which you kind of reminded me of was that there was kind of a lot of ^{hardware} high-grade development going on in the East Coast but the West Coast was principally involved with problem solving. Those guys were in another world back East. All of us, at least, were involved in the development of software. We were within a ten-mile radius of the Rand Corporation with the largest concentration of internally-

program machines in the world; there was Northrup, Lockheed, Douglas, North American, Rand. Everybody and all of us had multiple installations and a lot of money and engineers who didn't know what the hell they were doing.

Back in Washington the Fiat was running....

I'm not talking about those machines; I'm talking about production machines.

Fiat solved a hell of a lot; it developed the first nuclear submarine.

I'm not talking about that; I'm talking about when we all had machines and we had a medium for communication with each other.

You couldn't even find out what kind of programs they were using. I learned a heck of a lot of programming by going in at night and watching ~~all-the~~ over peoples' shoulders and thinking, "What the hell is going on here?" I think they had tricks we never thought of and we had tricks they never thought of, but nobody talked.

I think the thing we learned in the local area Jack has referred to is that it was to our mutual benefit to give up our proprietary stuff in order to show our mode. Like how did we get into DCA, how did we get into Pact, what motivated share; that whole history. We knew each other, we talked to each other, and we found out that we could develop things cooperatively much better than others could do individually. I remember, at one point, there was a big question about proprietary types of things and we decided the hell with it.

In the 701 ^{era} ~~area~~, there was a lot of trading. Everybody agreed that it was...

Individual ~~management~~ corporation managements were not apprised of what we were doing. We did it as individuals.

On the 701 still, we talked about the way things were done but North American and Douglas were right across the street from each other, and I don't remember much cooperation between Douglas' bunch and North American's bunch.

There was a hell of a lot.

There was great comradery between us which later allowed us to have machine time and we needed it badly.

We weren't^{worried} about machine time, we were worried about getting problems solved.

An example at Rand, on the 701 we used the Los Alamos assembler, we used the Douglas assembler, we used dual from Los Alamos, we used the North American library. We used everything. We didn't do any utility programming to speak of on the 701.

When we got the 701 at North American, we were behind everybody. We really didn't know very much and Owen was one of the two programmers we had. Ed Lawless, Jack ~~Ruckshier~~^{Brookshier}, Owen, and Elliot North.

Where's Brookshier now?

I don't know. I remember calling Ray Goodpastor one day after we got the machine or slightly before we had gotten it, after looking at our manpower resource which was very small. Ray had been writing programs for quite a long period of time. He was at Lockheed. I told him that it would be kind of nice, as far as we were concerned, if we could get together and kind of discuss what he had and maybe see if there was some way that we could come to some agreement about our making use of some of his programs. He said, "Well, it sounds like a great idea." So he called me back a week or two later and he said, "I've been talking to my management and I think we can probably work something out. It would be along these lines: You could send two or three people over here to work on kind of a full-time basis for about six months and you could pay us about \$25 or 35 thousand. Whatever you can learn and take away with you, you can have."

Was Goodpasteur the same guy that went to Philco?

No, there were two Goodpasteurs.

The Goodpasteur you're talking about was Julian J., right? He was the first supervisor of the project Rand had. The other Goodpasteur was Julian's brother.

In line with the way we communicated, it might be worth talking about how ^{share} Schere got started. My boss, Dean Jacobs, came walking down the hall one day and said, "Hey, we're getting a 704 and you're going to be responsible for it." Because of the way we worked locally at that time, I called Jack and said, "Hey, what are you doing for the 704?" He said, "Nothing, what are you doing?" I said, "Nothing, let's get together." I called Frank Wagner with the same thing, and I called Lee Mayer with the same thing up at Lockheed. We all got together in Lee's office I think; there was Jack, Frank, myself, and Lee and somebody else from Lockheed but I can't remember his name. Then there was some kid that worked with Frank who had just made a tour around the country to try to find out what everybody was doing. We decided that that kid was ^{to become} ~~the~~ secretary for our group, and he would write to the 18 people or 16 people who had 704's on order. He had just finished touring around the country finding out what the hell people were doing to get ready for 704's. He wrote this letter inviting everybody to the meeting at Rand and that's when we decided "Hey, anybody can do this alone, but let's do it together." The way that we worked here at that time, everybody tried to get as much as they could out of everybody else---if you want to call that cooperation---that was the way things went.

Can you give us a date on that? I think it's important.

One of the reasons for insisting on a date is that Lockheed took the tapes system that they developed, Rand did the utility system,

and North American was doing the input-output, I think. Everybody contributed what the heck they were doing. There were about 14 square root machines that got submitted to the library because that's how we trained our programmers, and I had some special purpose things I was doing for the STP people in the way of card reading and what have you routines, and Owen was doing some different card readings.

North American and Rand were working on it but Fred had general charge of the Matrix because I know Chuck Rothway did a Matrix card read.

At one of the meetings---I don't know if it was the first one at Rand or the second one at Philadelphia---everybody got up and said what they were doing and when they would have it done so other people wouldn't have to replicate it.

I would like to ^{read this letter dated} ~~say that on~~ August 9, 1955, signed by Fletcher Jones, secretary pro tem. We worked for Frank Wagner at that time, and Frank and I elected him as secretary pro tem.

"In the past, one of the greatest problems involved in the utilization of new computing machinery has been the designing of machine methods. The programming within each installation of assembly routine, the utility routines, and various abstractions have been the cause of much loss of manpower and machine availability. The second problem exists in that communication and dissemination of information between companies has been retarded because of a lack of conformity and in It has long been the feeling in several of the prospective 704 installations that the responsibility of designing machine methods should be distributed evenly over the field of 704 users, avoiding all duplication of efforts where possible. It is felt that the standardization necessary to accomplish this would establish communication between the participating installations and thereby

promote a valuable interchange of data. Also, this cooperation would lessen considerably the initial programming burden of each company. On August 4, 1955, an informal meeting of contributing personnel was held in Burbank. Represented were the Rand Corporation, North American Aviation, California division of Lockheed Aircraft Corporation, and IBM. The purpose of this meeting was to investigate the practicality of the joint efforts by several 704 users to coordinate activities in the initial programming of machine methods. All present agreed that such cooperation was desirable, practical, and necessary for complete exploitation of computing machinery. It was also agreed that this coordination presupposed standardization between installations on pneumonic operation code and card form. It was unanimously decided that this group would urge participation in the project by all 704 users. The installations represented were committed to the acceptance of the decisions arrived at by majority vote of a council comprised of those wishing to participate. Due to the fact that many of the prospective 704 users anticipate delivery of their machines in the near future, it was decided that those wishing to participate in the project should meet soon. A date, August 22, was decided upon. The Rand Corporation of _____ will be host, and it is expected that the meeting will consume one week's time. It is most important that each participating installation have someone with the authority to make decisions and firm commitments. It is highly desirable that there be present someone/^{who}is familiar with and has opinions on the items on the agenda. Enclosed is a partial list of the subjects to be discussed. Suggestions for additions to the agenda are solicited, as are proposals for an organization name. We have tentatively called the project Share. The letters are not significant in a pneumonic sense, but the word describes very well the objectives of the

group. Monday, August 22, will be devoted to ^{the} establishment of operating policy for the committee, election of officers, and the final specifications of the week's agenda. It is anticipated that the business will have been finished by Tuesday morning and the working committees will begin taking up items on the agenda. Each representative on the working committee will not necessarily be required to program routines but must have the authority to have the routines programmed at his installation according to specifications from the working committee. It is strongly felt by all concerned that Share is a vital project, one which will benefit all participants far beyond their investment. You are heartily encouraged to contribute and profit.

Signed, Fletcher Jones, Secretary Pro Tem."

Who was it addressed to?

It was addressed to 17 organizations--Boeing, California Research Corporation, Curtis-Wright, General Electric, General Motors, Hughes Aircraft, IBM--Poughkeepsie, IBM--World Headquarters, Lockheed--Georgia, Lockheed--California, Lockheed--Missile, NSA, NorthAmerican, Rand Corporation, United Aircraft, University of California at Livermore, and University of California at Los Alamos. The agenda was as follows: (1) Pneumonic operation code; (2) Assembly programs; (3) Utility routines; (4) Diagnostic; (5) Continuance of Share activities after initial programming effort; (6) Systems of use of utility programs; (7) Register usage conventions. Parenthetical remark: The 704 will be discussed if this is desired by the participants.

This is a curiosity that I've had for years because if you'll look at the brochures put out by Univac on the start of the Univac users conference in about 1955, you'll find

This is an official document.

Well, I've always wondered what the ^{actual} ~~official~~ date was because I've had continuing debates with people over which of these two organizations really got off the ground first.

Preceding this we had a meeting which coincided with the meeting at AIA. We got together and the way the name Share came about was we met at a restaurant called Rich's on La Cienda---they're no longer there. There were about seven of us there and one of the guys named John Hughes from There is where we decided on the name Share and he is the guy who thought up the name Share.

Regarding that week of conference, what kind of records or documents are there in terms of what was decided on each of the agenda items? Are there any records?

Yes, there are minutes of the meeting. Any Share installation like Rand ought to have them.

There is a microfilm copy somewhere.

The AIA meeting was an interesting affair when they came out to explain the 704. Remember, they told about all the great things they could do with the index registers and gave us two ways of turning index registers off. Owen Mott sits back there with his pencil and paper and figures that two things that turn off the index registers happen to be two instructions that were published in the IBM brochure. These are things to do and then were dropped out because obviously these were not the ways it was done.

And the data given on the 704 was what would happen if you throw these bits into the code; see what happens.

And I think they had a list of 20 or 25

which happened to be the negative of accumulator.

Los Alamos did a very detailed and methodical study of all of the operating disc patterns that didn't put the machine into hang-ups. They published the work and it was put out at a very early Share meeting. I don't know where that is. But some of them did such delightful things as add 1 to the low end of the accumulator, put the two ends of the ~~accumulator~~

One of the things we decided upon--a very important thing in a letter dated the 29th of August 1955. It is addressed to Dr. Charles DeCarlo who was then director of Applied Science for IBM. It says, "The first meeting of Share convened on August 22 as outlined in the letter of August 9. To refresh your memory, this organization is devoted in part to ~~esta~~ the standardization of machine methods on the 704. While the items for standardization were being surveyed, the subject of print-wheel configuration arose. It was stated that during the 701-704 Symposium held in conjunction with the Western Computer Conference in March, IBM had arrived at a print-wheel configuration which was approved by all the prospective 704 users. It was thought that you were in agreement with the decision and would direct the print-wheel design to conform. It was found during investigation of the subject that the print-wheel configuration considered now by IBM to be standard differs from that decided upon in March."

Did they change it to conform?

"Share will be interested in hearing from you on this matter at your earliest convenience."

Did they succeed in getting it changed?

I can't remember but I know there was a lot of discussion about it.

Jack, you were involved in ~~Sharet~~ DCA. How did it get going?

It started at a brawling dinner at the The first official thing that was called Share was convened 20 years ago, as a result of a letter that came out from North American. I had a copy of that letter after the last DCA meeting but I don't have a copy with me.

DCA finally gave up in 1950 what?

It was sometime between 1960 and 1964 because _____ was at
FTC and was program chairman at the time.

It was well before 1964, maybe 1962.

I heard a talk on at DCA while it was still going on, and that had to be after 1962.

In 1962 we were dying very slowly.

I went over to ACM to look at them from the DCA. Being used to the rowdies in DCA, I walked over the ACM and these little guys in their blue serge suits were standing around talking very quietly. I didn't fit in too well. I walked up and tried to get in a conversation and I didn't understand any of it. I went over their facility for days. Then I went back to DCA and when DCA demised, the ACM bunch moved in on us---the DCA bunch moved in on ACM, they came up through the organization. But at the time, I think they were rewiring 519's; I'm not sure what they were doing. Mostly they were a mechanical bunch.

I had the original letter inviting people to participate in the Pact project but somehow I've misplaced it. I can't remember when that occurred. It was well in advance of....

Pact was in 1955 or 1956....

I remember the Pact 1A program that was all right but they never could....

Does anybody have the preliminary manual that was released on the original 704 which does not look like the 704 as it finally appeared? There was some capability as a carryover from the 701.

I may have; I don't know. I have a bunch of 704 manuals.

There was an interim design that was released that was called 701A and I cannot find that piece of paper in my files but I didn't look very hard. There was a machine that was never realized between the 701 and the 704.

There was a machine called the 708 between the 704 and the 709.

There was a 703 that was never produced; that was interesting, too.

When you talk about machines that were described in a manual but never produced, what does this generally imply?

IBM chickened out.

I don't know what it implies but nobody ever wrote a real live program for that/^{machine}to my knowledge, but the functional description of the machine was on paper in printed form.

It was very possibly a prototype that just didn't make it.

It also implies that IBM had very unusual ways of doing marketing research.

If you'll remember what was going on with the 1101-1103 series; it was just at that time and I don't know have any direct notes on this but I have a sneaking suspicion that the 701A came out as a proposal and not as a machine design. I would guess that that's why that got in there because they were really exerting on that military work with the 1101.

Well, the 1101 was a hardly existing machine as early as 1952 or 1953.

That's why I say IBM was hurting because that didn't have that 701 follow-up to compete in hand and yet they were having to meet....

So ^{the} ~~there~~ really was no competitor for the 701--the 1101---it was a drum machine if I remember correctly primarily.

As far as I know, there was only one variation of the 1102 that was built, and the 1103 followed right behind it.

Charlie would know about the 1103; he worked on that.

As Erwin pointed out, IBM has unusual marketing techniques---701A. Marketing research!

I like techniques better.

John Carlson went to North American in about 1954.

I think Boeing made the heaviest use of the 1103 of anyone I know.

Did Univac use the 90-column cards then or did they have IBM cards?

At we had a punch and we used IBM cards.

Getting back to the philosophy of

I think it was because that was the first time, to my knowledge, that anyone had gotten together on some kind of a noncompetitive basis to try to do something. And we were all competitors, you know; there was Lockheed, Douglas, Rand, and North American.

There was also one advantage that we had in this area which shouldn't be overlooked and that was the position of Rand as a nonprofit organization which could get these competitors together until we had kind of a good environment that we could work in.

That's also why Rand tended to have a lot of other people who in the early days; that was explained to me. Since they were nonbiased, they could get other other people in other

The principle reason for DCA, I think, was the fact that we had Bian and we had ACM and they were both very much the tea-drinking types. They weren't much fun.

I think DCA was just kind of a, pardon the expression, Joe, long-haired group. They were people who wanted to sit around and talk about routines and so forth, and some of them didn't want to talk about serious things. I think that was the environment....

I think Blair Smith had an awful lot to do with that.

Joe McClellan, ^{Rand representative at IBM} /according to him, used to have to go around in a white shirt and tie and coat at IBM; when he went to Rand he had to take it off and put a sport shirt on because nobody would talk to him if he had a tie on. So he changed in the car. One of the rowdiest meetings that have ever been held before today was when the clown, Jack here, decided to buy everybody wine at the table. That was just a start and they got drunk, and ~~Joe~~ poor McClellan was trying to give a Pac I lecture. Armour had Elaine Bowman, trying to drink her under the table---get her fired by IBM so he could hire her. She drank the whole table under the table. I was trying to listen to what McClellan was saying; I was in the first row and could hardly hear him.

Don't forget Don Pendry.

After it was over, Don got up and said, "Next time, I think if you want to do some talking you should go to the bar." The program had been over by then. McClellan couldn't do anything because he was IBM rep to Rand, and he couldn't hurt their feelings.

Also, he was laughing too hard. McClellan was so drunk....

McClellan was not drunk.

The hell he wasn't.

You wouldn't know if anybody was drunk; you were too drunk yourself.

Your recollection of Pendry doesn't even agree with mine. I remember him getting up in the middle of ~~a~~ ^{the} meeting after a cork bounced off McClellan's head and saying that this rowdy-ism would have to stop if you wanted the

presentation to continue. I remember the great debate between Bosack and Wright over floating 6 points.

I remember the only time Beamer and Carlson and Rose ever agreed on anything, they had a panel talking about check-out. And the full panel agreed that your program is checked out if it matches the answer you get with a desk calculator whether the equations are wrong or not. And if you

They sincerely believed it.

I forget who was on that panel now. They sincerely believed that if you checked it out

I had the 701 routine and was supposed to check the X-15 at 750 miles; power hadn't been turned off yet and my program; it never would recover.

Wasn't Boeing out of this in many ways just because they were where they were?

The only reason they were in, I think, is because ^{of} Randy _____ and John Jordan. Those were the only people I ever knew up there, but I knew them quite well.

It seemed to be a center all by itself.

Well, we were sending them data from North American on row binary cards, and North American had a way of doing business---only the bosses ever went to meetings. I wanted to go to a meeting in San Francisco. I had to pay my way but they would only pay me for my time. However, I was on an extended work week and they couldn't pay me five hours' overtime, but they paid me the 40 hours. I get out to San Francisco and a guy walked in and says, "North American; what's this." He looked at these row binary cards and he had no idea what it was or what to do with it. I spent about three hours.

When I got back I explained to Frank that I was worth more to North American in San Francisco than I was to North American.

We agreed with you.

Jack, it just occurred to me that at one of the DCA talks someone from Rocketdyne , I can't remember who it was, got up and described what to me was, in retrospect, about the first application I ever heard of.

That was me.

Well, you were involved in it but this was the Santa Susana into Rocketdyne.

That was the guy who had the radio program, Martin Fine.

The guy at North American who developed the loader for Fortran-- what did they call it? Anyway this ^{guy} was doing things and never would write anything down.

He rode a bicycle and carried it in ~~the basket~~ his back pocket.

Wasn't he the secretary for the uncle group?

Oh no, he didn't write; I don't think he could.

On that communications thing, we had a ^{committee called the} data transmission study group which was the first effort to get the communications people and the users and designers of communications gear together so that they could understand what each others functions were. That was in 1957, I think. The communications link-up that we were talking about was the one North American had between our Rocketdyne division and our Los Angeles division with a dish-up on Hoot Mountain; it was tape-to-tape system.

Our computers were at Los Angeles; wasn't that the problem?

Well, we had computers at both places. The interesting thing about that was that the Rand Corporation had some guys staying down to develop

some specifications for such a system and he had a couple of tomes that thick that he had come up with which were instructions to the communications people. We decided we wanted one, so we called IBM---specifically, Callahan---had a little meeting with him and said, "Okay, what we want you to do is take this tape and make another one for us, just exactly like it, and send it over that mountain." We had a one-page letter of agreement with North American, Pacific Tel, and IBM. They went off and came back in a month and a half, and they did it. This was in 1955 or 1956.

The interesting part was that we let those people do it just the way they wanted to do it instead of telling them how to do it.

Jack, it had to be 1956 because the 704 went into Rockadyne in the spring of 1956.

We went back in 1956 to ^{do}/check-outs and IBM in New York---in fact, Rand, Lockheed, and North American all shared time and rotated around.

That was another interesting experience, because North American and Lockheed at that time were doing their check-outs using the operating system. I remember one night when Watson Jr. wanted in, which was an interesting story in itself, because we were back there at World Headquarters with plush rugs at midnight, and we all had our shoes off. The guard walked in, took one look at our feet, and drew all the drapes. At any rate, Watson came wandering in and he was completely nonplussed because that machine was going all the time. He had never seen check-outs done with machines going. People would be there playing console games. That may have had an impact on their deciding that operating systems were a way to go.

....because the stupid machine broke down. Irv took 13 tapes up there to get printed. Owen was operating the machine and the card in one hand; Irv brought down the stop, and we ran everything off on tape and the tape went down. I think it was the DeCarlo who came along and asked me how things were going. Well, the first thing was he made a mistake in asking that question and the second was asking me.

There was another interesting thing going on at the same time. Quite often, our time slot would be right after NFA, and NFA was very busily in the last 15 minutes of their time slot erasing all the tapes they had used because this was security information supposedly. Tom Steele, who was then at Rand and had previously been with NFA, swears up and down that they were using the tape to generate random numbers.

Another thing back in the 704 check-out days was that they made up all these things. They had a 720 and a 716 meter, 714 tape-to-card and a 720 tape-to-printer. We hadn't developed a card-to-tape yet, but we had developed a tape-to-printer. So we loaded the cards on a 250 card meter and put everything on tape. In the process, Paul Tanney had written the end-of-tape routine. The end-of-tape routine was to put it in the file and after it was in the file it would say, "Please take this tape off the printer; it's been finished." Then "I said take the tape off; it's been finished." Then "Take the damn tape off." There were about ten of these. And comes up and complains about the fact that we've got not to do that because they're running tapes off the other end looking for more things. Then when I asked the question, "How did you get to the second in the file?", they wouldn't answer me.

Back then IBM always had luncheons for all of their guests, and Mann was back there at that time working. He thanked us for having our shoes on. They had a West Coast guy back there with us, and that poor guy was told to make reservations at Delmonico's. And he made reservations at Delmonico's but it was down at the Battery and it should have been somewhere else but he didn't know there was two of them. Anyway he made it at the wrong place, and DeCarlo from IBM and all of them showed up at the wrong place where the rest of us were. I had gotten there early and couldn't find the luncheon. I asked them where it was and they said, "It's all arranged." And they walk up like the typical IBM executives and found out that there wasn't any luncheon arranged. Those guys crucified that poor guy, and he was from out of town--I don't know why he should have known it. But IBM was very upset and we had the luncheon the next day.

You talked about that check-out on the 704. To give some credit, the stuff that we checked out--I don't know about other people-- was greatly aided by the fact that Tom Steele had written the 704 simulator version on the 701. Most of what we checked out were timing problems.

That was the first real simulator that I know of.

We didn't use it on a print program.

Yes, it simulated tape and drum, I know.

It worked the card reader because I checked out card read programs, but I don't know about the printing. It was remarkable considering the size. That's the first machine simulator I came in contact with.

That's the most practical thing I remember Tom Steele doing.

North American wrapped up an old key punch, made me a carrying bag, and I carried it with me like a suitcase.

This is slightly off the subject, but does the Rand Corporation have any kind of in-out history that they have done over the years?

Well, Fred Gruenburger wrote a history of the Joniac which had a lot of that.

On and off, I don't know if they do this every year, they put out an unclassified report on what Rand has done this year, the accumulation of which could provide some history.

How far back do they go?

I don't know. I only remember two or three of them in almost 19 years there.

You indicated in an earlier conversation that Rand was at the center of so many of the things that were going on there and it seemed to be that so many things that were generated were either coming out of Rand or would have come into Rand from external organizations, as desired.

The best source of that kind of information is apt to be Paul Armour's files.

He wasn't interested so much in this kind of thing. Paul's greatest interest was in accumulating information; he was a sponge.

If you want some human interest stories, maybe I should tell you about the first time I met Paul Armour which was the first day I went to work at Rand. As I was introduced through the office by Cecil Hastings, I came across this desk with this character sitting there working over racing forms. I thought, what a wonderful place this must be. And that was Paul Armour working on the racing forms. After I'd been there a few days, I found out that the racing forms were all a year old and he was trying to see if there was anything to the theory of prediction. By the way, Paul

kept on working over the racing forms after the project was closed for quite a while.

There were also you and Bruno Chipanelli and a few other people who used computers to enter some of these riddle contests.

They entered \$25,000 riddle contest on names of cities and they did it on computers at Rand, three of them.

Bruno Chipanelli and I think Mendelson was in on that and Bruno's friend Lindberg were in on that.

I remember doing that one time. I had all the machine time and all the expertise of everybody around me. Some little old lady won the damn thing.

I've heard about people who've tried to use computers in these newspaper riddle games with large sums of money. When you got up to the semi-finalists, the guys who made a business of wording contests....

As I recollect, Lindberg won a boat.

There were three prizes won at Rand; one was a boat, one was a swimming pool won by Milton who was living in an apartment.

What was the date of the first Rand proposal?

Each year they had a proposal before....

Do you remember these things?

Identify them.

Scientific computations for 1948, sponsored by the....

It seems to me that Gruenberg had published several articles on datamation that was either the result of some....

When Datamation came into being, it had a contract with the people at Rand for and that was one of their big fortes.

Are you trying to collect old manuals and things like that, too?

Yes, we're trying to collect old manuals and any early descriptive material because these are among the first kinds of things that tend to get lost. People move from job to job. These cartons of material are the first to when the moving van comes.

I have ten big boxes of those things and I'm trying to find a repository for them.

Will you explain to everybody how you'll be glad to send them mailing labels.

Right, as I collect names and addresses of people who have things they would like to send me and the cost may get prohibitive, what I can do is send out the Smithsonian's mailing labels with note "Official Business" and you can send them to us at no cost to you. Because it can get pretty expensive to ship these things across country.

I left all of my collection at Rand because at one time someone was collecting them all there.

Yes, I left a whole file full of stuff at Rand and I have no idea what ever happened to it.

I can site one experience: I left Rand in 1950 and returned in 1964. An awful lot of the files on, in particular, Pact, Share, and DCA were suddenly handed to me because nobody knew what to do with them. I handed them to somebody else along the line and I suspect they just disappeared over a time as has been suggested.

Chuck Baker might have the file of Pact documents.

I may have one Pact manuscript.

There were several papers in the ACM 55.

These are the ways that things usually get lost. People remember, for example, seeing Norbert Weiner's files, memos and various things, as of a given year with nobody seeing them anytime after that.

Computer cards have developed a great solution to the document storage problem. After you send away the stuff to storage, they burn down the warehouse.

I think Roger's first suggestion of individually talking about some of your own experiences and involvement was a good one. Roger, do you want to direct this?

Well, I thought we---Jean hasn't said anything up there.

Yes, she wasn't even identified on the tape because she was out of the room when we went around.

Right, we haven't heard her voice.

Do you want me to say something about the CIC computer; that's the first machine that I worked on?

What we started out with when you were out of the room was we all identified ourselves, said when we entered the industry and what we first worked on.

MALIN: I first ^{started} ~~worked~~ to work in the industry at CIC, and the first computer I worked on was the 102A.

What date was that?

Somewhere around 1952.

I think what we'll probably do is try and go through the machines you worked on and where, so if Henry has to come back and correlate something he'll know who he talked to about what and where they were. So I thought each one could give a history from CRC where you went and what you worked on at CRC, North American, and so forth.

MALIN: I worked at CRC; I worked on the 105 which was essentially analyzer and I worked on the 102A. They took the 107 and sent it to Washington; it never worked the whole time.... When I left, they were coming out with the 102B and then another computer after that. I went over to North American where I worked on the 701, then they came out with the 704. I worked in engineering and I went over and worked in Data Processing with Ed Law on the 704. Then we went to the 705 and then, ^{we worked on} ~~of-course~~, the 709. I went over to CRW and worked on the 1410, 7094 integrated systems, then transferred back to Data Processing. I ended up doing one program....

MOTT: The first machine I ever physically saw was the Ebiac on a tour in 1952 of Aberdeen. The Ordvac was just being put together and they were hanging on an 077 and having a terrible time. I remember walking in the room and being thrown out because the engineers were having a terrible time and they didn't want anyone else ^{looking on} ~~hanging-around~~. I was shown the Edvac which was completely dead and dark, and someone said it will never work again; I don't know for what reason. The first programming course I ever took was at the Department of Agriculture Graduate School; it was originally taught by Dr. Cannon and we started out learning the Seac.

This was Walter Cannon?

I don't even remember his first name. But about half way through the course, Dr. Cannon had to exit the course because there were terrible problems at UCLA with the Swac. That was the only explanation we were given. Ida Rose took the course over. I think it is interesting to note that Ida would turn her hearing aid off---I think I told some people this at lunch---when she would start to lecture. She has a stentorian voice and with the

hearing aid off, it went up about 40 db. We were sitting in the back row of the class, and it was still painful to listen to her. The one critical thing she said during the course that I remember---she switched us over, by the way, from the Seac which she thought was a terrible machine to the Univac I, which she apparently helped design and it was the only machine. Someone brought up the 701 in discussion in the class one day, and all she had to say to that was essentially, and I paraphrase, "Yech." Her comment to all us people who were trying to become programmers or thinking about becoming programmers was, "If you can't get all the bugs out in desk checking, you're never going to make a programmer because you can't go to a machine with a program with bugs in it." So there aren't any of us who made it, I don't think.

You have any of the notes or material from that original course?

Buried in the garage somewhere in a box there may be a collection of notes from that particular course; I think I may be able to lay my hands on them if I still really have them. But in six moves I may have lost most of them. In the interim I did a lot of desk calculating from 1952 to 1954, and at one point in time I got very upset by finding a desk calculator that did ordinary calculations. I said to my boss, "There has to be a better way." He found out that DRA had a 1101 service bureau in Arlington, so we went over and discussed the problem with them and what we were doing. It was certainly amenable for being programmed and run on a computer the cost. That was the first and last discussion we had with them. In 1955 I went to Rand and learned the 701. I guess the first job I had was working with Bill Orchard-Hays on the simplex method of programming. One of the

funniest stories to come out of that was when George Danzig decided that somehow his wonderful and glorious technique had been perverted by the people who had done the diet problem first using linear programming and came out with, I think it was a minimum cost diet to meet the minimum requirements, and essentially it was peanut butter and lard; that somehow offended him. He was going to do it better because there was a better functional that you could evaluate; he wanted to hold the calories constant and maximize the bulk instead of minimize it to meet the minimum daily requirements. We started off with---I can't remember how big the basis was---but we punched up the entire Department of Agriculture book on food content as the basic matrix and started to run programs. I ^{doubt} ~~thought~~ that I have any of those left that I could reinterpret because all of the absolutes were

But I remember the first feasible basis or the first optimal basis we got was a diet containing 17 gallons of ginger ale, guavas---I can't remember what all. Anyway, we decided that we had made a terrible error; we had maximized the bulk certainly---oh, it had a lot of vinegar in it too---we had maximized the bulk but we had failed to maximize the bulk properly so we went through and repunched the whole damn deck to take all the water out of everything---dry solids, okay?

Dry ginger ale?

The next useful diet we came out with had, I believe, 3 lb. of ketchup or something delightful. Ketchup has a lot of minerals and vitamins. So we decided to throw some of those things out completely---all of the spices eventually went out. The last diet I remember, which was the point at which everybody gave up, had 4 lb. of bouillon cubes, 3 olives, half a guava, swordfish---well, it was all very delightful. At that point, George decided he

might be willing to try that diet. I said, "George, you're out of your mind;" and he said, "No, I have a doctor's supervision." Then I guess I wandered through the 701, the Joniac, the 704 and did a variety of things mostly in systems, programming, languages, etc. I eventually got to NPC and worked on the 332 and 360.

What kind of background can you give us on Dausig's development of Simplex?

I was not at Rand when this was developed; I was working in the Pentagon when it was first operational and I know at the time there was a great deal of desire and worry. The project I was working on at the Pentagon was ancillary to the group trying to implement the Simplex method on the Univac. And we were supposed to be supplying part of the input during that time and the main role of the project we were on was essentially to straighten out the fiscal planning for the Army-- a major task in itself. But generating as sufficient input that someone could actually take the Simplex code and do some optimization for some small piece of the Army. And I guess the Korean War was going on and they wanted to optimize the effectiveness of some small group. One of the students happened to be a sargeant--he was drafted and was a sargeant; he was running this whole project and had two colonels, a major---it was delightful. He started out and he wanted to know---- We were supposed to look at the leave time and planning for/a platoon in the field with their weapons ^{putting} in a ~~and--the~~ cost effect fashion; we were supposed to supply them with all the necessary input so they could go ahead and do this. So we generated all this stuff, and the next thing we know, they came back and said, "Well, gee, nobody's going to be impressed

with a platoon---solving a problem for a platoon---because they're so damn many platoons, you'd have to replicate it and they're all special. Let's do something a little bit ~~gib~~ bigger; let's do a company." In the meantime, somebody was bartering for Univac I time and Univac had agreed to give several hours to check it out and ^{run a} ~~one~~ demonstration. But by the time we got through with the thing, I think we were trying to model using linear program/^{ming} the whole Third Army or something like that. The Matrix had gotten all out of hand and they finally got some version of linear program running, and we were all invited to go up to Philadelphia and watch this amazing demonstration. Since there were no travel funds, only some of the guys went up. The story I heard was that they were demonstrating for a 4-star general, and he walked into this huge complex of the delightful Univac I installation, just like in the pictures, and he was very impressed with the hardward. They had extra memory banks cooking just in case. They fired up this program, it started to grind, the tapes spun, and it ground, and the lights flashed and he said, "Okay, where's the answer?" And they said, "Oh, the answer doesn't come for 12 hours." At that point, he stomped out and our project was cancelled.

Bill Orchitate is the one who can give you a lot of information on the development of Simplex.

Where is Bill Orchitate now?

He's still got his own company; I don't know what he does back in Washington.

No, he's moved from Washington.

One of the funniest experiences---you ought to talk about it, Owen---was installing the Prototype 32K ^{core} ~~store~~ on the 704. WE got engineering Model 1.... We had the first fire on the main floor with the 704. This fire was in the basement and lasted for approximately ten seconds; we had been lucky rather than smart. The fire extinguishers were in the right place for the operator, who was Bob to get it out quickly. And that 10-second fire took them something two weeks to rewire the main frame and we had a big squabble with IBM over whether that main frame was ever going to work right again. I think we finally had that main frame replaced. I don't remember that funny core story, so you tell it.

Well, the 32K core didn't work too well and I walked into the machine room one morning and here is what used to be our 32K core box which was built in a box X with the core in the middle and all the drivers at the four sides; the whole damn thing was wrapped up in tape and corrugated paper with a chimney up at the top out of corrugated paper and over. It looked to me like they were going to ship it out in toto, and the problem they believed was that it was over heated. But here is this modern computer all wrapped up in corrugated cardboard and going.

That's not as bad as when we had to build a tent over ours when we had the rain storms.

There was the story about the neons on the Joniac.

Oh yeah, there was only one computer that we know of that was afraid to work in the dark---that was the Joniac. It turned out that I was mothering the machine for a while and periodically, like approximately every

two weeks or twice a month, I would get this trouble report from the gal who ran some of the payroll at night on the machine and she was always having checks on the output. It suddenly got worse and worse, and finally one night she couldn't do her run. We ran diagnostic tests the following morning and the damn thing ran absolutely 100% perfectly; not one machine check we used. Payroll data ran and the payroll program couldn't find anything wrong, so somebody said why not try to simulate what she does. In order to complete the simulation, since it was night, we would draw the drapes. So we drew the drapes and it still didn't fail. I guess a couple days passed and somebody asked the gal who was running on graveyard what else she did. Well, we went through this whole business of trying to drag out of her in great detail ~~everything~~ every step she went through. Finally she said when she left the room she turned the lights out. That was the only thing we hadn't done. We turned the lights out and drew the drapes; sure enough, that damn machine wouldn't go. The neons had gotten so old and creaking, they needed what little radiation was coming off the fluorescent light~~s~~ to keep the ionization level up. As a result, the machine was always run whether the lights were on or off, fluorescent lights were installed to shine forever onto the neons.

MEYER: I started on the Douglas project at Rand in 1947, as I said, counting a million random digits. They had developed a machine at Rand which they connected up to a summary punch, and it punched a million digits before I had been ^{hired in} ~~there~~. ^{found} They ~~felt~~ the machine was biased to even numbers over odds, so they rewired the machine and punched a million more, 50 punches per card for 20,000 cards. My first job was to sit and count the

number of 0's out of a card, key punch that, then the number of 1's and number of 2's until I finished the card and found the serials missing. When I finally finished a million random digits, they had started their tab department at that point with Goodpasteur as the leader. Don Maddon was machine operator on day shift; they had a swing shift, and they decided they needed a graveyard shift. So they came to me---I was working part time, and suggested that they didn't really need a part-time person to run a desk calculator which I had never run and offered me the chance to train into data processing on the graveyard shift. The individual they offered the chance to train into data processing on the swing shift was Paul Armour. So Paul Armour and I were the first two trainees at Rand. All the others had had previous tab experience. There are quite a few famous names, I think, that came out of the early Rand people; I mentioned Bill Orchard-Hays, Bob Deamer, and quite a few others if I wanted to think about it. As it ran through the 602 days---before the 602A, the old 602---we would wire them out and IBM would ship us some new ones; we'd wire them out, and finally they came up with the 602A. But by that time, the 604 was so close behind that we never really used the 602A. Through the CPC era, I was a math student at UCLA but I got scared by something called line intervals and never did graduate. Then I went into the commercial side of data processing and still punched cards. Finally I came into North American and Ed Law had just started the first/^{business}programming group. The 701 was there and a second one had just come in about that time and something new--the 727 tape guides were added instead of the old early 726 that had the metal leaders where you had to Scotch tape the tape off the metal leader. So, I learned at that point, Rand had been

so thoroughly honest in what they were trying to do at least, when you wrote a procedure you followed that procedure and carefully ^{followed} ~~thought~~ it out. So I did much data processing there at North American. I programmed a financial inventory, and they had a 9-digit number that they were going to convert to 11 but they assured us that we could go ahead and program for 9 digits. So I wrote these programs for the 701 with the 9-digit part number. We started converting the master files from punch cards to magnetic tape. At that point, they decided they were going to convert the 9-digit part number to 11; of course, we had programmed for 9 digits only so that the program wouldn't work. They said, "Well, that's all right; we're going to get the 704 shortly so why don't you just rewrite the program for the 704." Then later I worked on the 705 at North American. I left North American in 1958 and went to SPC and worked there on the 709 and ~~790~~ 7090, data systems design; I did a little work on a 1401 for the Veterans Administration on a medical project. I haven't written a program other than on-line to a time check computer, the 232 and 360---haven't written a program honestly for 10-12 years at least. Now I'm at Computer Sciences.

Did North American have a 702?

No.

Ed Law did a cost statement program on the 701 that was really a miracle. It took one girl approximately two weeks to do what he would do in about six minutes on the 701. It was really remarkable and I think it's one that has paid off many, many times over.

....worried about production and checkouts. Jack Strong says you have to have more production than check outs as an ultimatum.

By the way, this rebellion against white shirts I think started at Rand in about 1949. They had a loud Hawaiian shirt there and every Wednesday you had to wear a Hawaiian shirt or you were ostracized at Rand.

Cliff Shaw would never wear one and when asked why, he said "I'm a nonconformist."

J. SMITH: In 1950 I went to Purdue University because I discovered they already got a computer. A battery of tests had been given to me after I got out of the Service and indicated that I was suitable for cybernetics. I didn't even know what it meant at the time. This was in 1948 and I thought about it for two years and decided to find a computer. In 1952 I went to work for the _____ on tabulating equipment, and in 1952 Al Fellows came there to teach. In 1953 we finally got a computer after a year with the CPC and in late 1953 or early 1954 the Datatron came in. As I recall, the most interesting thing that went on, besides servicing all the people at the University that wanted to use the ^{including ourselves} darn thing/~~themselves~~, some fellows came back from the East Coast one day late in 1954 with a bunch of material written by some kook named John Backus at IBM. Backus had been doing a lot of talking to _____ and I guess some of the Englishmen; he read the stuff and said, "By golly, it's probably a pretty good idea; we ought to compile it." He looked at the Datatron and someone observed that there was no way to get alphabetic input into it. So we got some postgraduate students in physics to design a nice piece of equipment that would take punch cards into tape. We went ahead and built a compiler. We got it working and called it IT for internal translators. Then I went to Carnegie Tech following Perliss¹ who was my professor and found the 650 which I guess was really the first heavily used

machine. This was in 1956 or 1957 and everybody was using it. We replicated our feat there. I recall early in 1957 had a conference with almost everyone who had written a compiler. I remember Charlie Baker dashing in from the West Coast with a listing of Pact 1A under his arm---his shirt was dirty from them---he slammed it down on the table and said, "Here we are; this is from our new compiler." All of us looked at it and said, " It reads up to down instead of left to right; it's no good."

That shows how much we all knew in those days.

This was at Carnegie Tech and we moved to a 650 then which became a pretty heavily used machine.

When did the 650 come out?

In 1956.

I thought it came out after the 704.

It was announced in 1953.

It first came out in December 1955, if I remember correctly.

I recall that we found a mistake in the divide instructions.

We've found that on every machine.

Because it said "after" instead of "before"; if you divided by the smallest number in the machine, it just stayed there forever. There were a lot of people who are at and ACM at Purdue then---John Handel

Tom Sheehan was there who is now at Harvard and he built a company of

It was a pretty active group at Purdue and later at Carnegie Tech. Then in 1958 I went down to work for a little outfit called Technical Operations

They
in Washington with Tom Sheehan. We were building model
for the Air Force. We decided we'd build an operating system for them,
if we could build such a thing. We played around with the 704, 709 and CL-1.
The most vivid memory I have of that period is that the 709 came into the
Pentagon, and the IBM guy forgot that it had a 3-phaser and blew the whole
machine when they connected it. They pulled it out and I think things
happened because a new one came in very shortly. Then shortly thereafter
the big Pentagon fire struck while one of our people was waiting to run.
We looked across the river and saw the smoke rising out of the Pentagon;
everyone instantly thought it was the computer and sure as hell it was.
One of our people did show some derring-do there; he rescued some tapes
from the room there they were all burning. Then I moved to the West Coast
in 1960, spent six months at FTC and then came to Rand where I did a little
programming for 4044, there was a 90 going on; then we got the PDP-6 in and
Erwin and I and Tim O'Brien pulled jobs. That was the last program I ever
wrote, 3 or 4 years ago.

I think you're the first one that I know of today that

Yes, you should really talk to Cliff Shaw about that.

Have you talked to Jim Backus or do you know who he is?

He's the guy who invented Fortran.

Roy Nutt might have a file on Fortran.

Yes, he's the guy who was on the Fortran committee and didn't
he write the....assembler?

Yes, he wrote SAP and he did, I guess, the IO for Fortran I.

SAP was what, System Assembly Program?

Symbolic Assembly Program.

The only thing he didn't do was document it, and we had to go back and write all the documentation on it.

He was also a microsecond / ^{chaser} who did strange things with instructions in order to make switches and things like that. Any change you made to the assembler had side effects that were quite unpredictable.

I think you also have to relate, Jack, to the fact that that was the order of the day. Everybody was a bit chaser; everybody was trying to squeeze one more instruction out of the

You know what? I can do that in five instructions.

But he's the world's richest bit chaser today.

In fact, he, Patrick, and Jones founded CFC; he was the brains behind CFC, Patrick was a salesman and Jones was a salesman and Patrick lost. Patrick had talent; I didn't say anything about Jones.

You'll have to ask Patrick about the CFC.

Just to fill in the flip side of the coin, you may get something from Grace Hoffer and her crew at Rand; to get the other side of that picture, you've got to talk to Tolly Holt---he's been around a long time too. •

Tolly Holt?

Anatole Holt, head of Data Research in Boston.

He's part-time at Harvard, isn't he?

8 He may be. In fact there are a lot of people floating around there---George Meeley!s.at Harvard now.

George Meeley? In dungarees?

I doubt it.

I don't.

The last time I saw him he had on a suit and tie.

George Meeley? When I went over to Rand, I heard about the great Meeley and I walked over and met this guy in a sport shirt, which didn't bother me---I had one on---and the dirtiest dungarees I'd ever seen before they had become popular with the new generation; this was in 1956.

In mentioning Meeley's name, I think he was the guy that had the greatest single influence on the 709 operating system coming into being.

MENAGE: As I mentioned, I started in ~~the~~ programming for the Mark I at Harvard in 1947 while going to school. I went to Rand then and was there during the early days. At Rand I was able to get time on most of the machines around the country and found a few programs that would help us. I worked on the Joniac, Seac, Ordvac....

Possibly, although the Mark system came out of Harvard; they were building the Mark II while I was there and then the III and IV.

On the FTC you worked on electrical and not electronics, right?

Right.

As a relay bunch?

On the early history, there are a lot of interesting stories about and IBM people....

Tell us one.

One I recall was very anxious to establish some kind of reliability for Mark I. If you'll remember, Mark I was a big relay type 10-point rotary / ^{equipped} type of calculator which had a sequencing device

that was card wide with

that shook the whole building. As soon as the shaking stopped, we knew the machine ~~machine~~ was down and would come rushing out to the IBM engineers to find out how long it was going to take to get going again.

So one of the most effective things we

was a long tape

and it had blank, blank, 9; 9 was the instruction to step ahead and read the next instruction and the blank-bland was do nothing. So the engineers would slap on this tape

Usually it wasn't the sequencing device; they'd be in the background trying to figure out what went wrong.

I think you had gotten yourself up to the Lincoln Lab. Were you at the Lincoln Lab....

I was there from 1955 to 1959 and came back to the West Coast and was with a division that was involved in a thing that split off from Rand and became STC. .

Is it true that they had a bunch of people who learned 7 or 8 instructions on the ^{Sage} ~~same~~ computer and nothing else because they were worried about getting it down to 5 instructions instead of 6. That's the reputation that STC had in those days, and if a guy walked out and applied for a job and said STC, they said fine and he went out the other door. I guess it was North American and Northrup that had this attitude on Eniac programmers. Emile Waters had the attitude that anybody who came out of the Sage system was so pigeon-holed on little bitty things that they had no idea what a machine was doing.

People like Erwin Brooks, huh?

You can look at it in different ways, but I believe that if you look at the techniques and technology that went into Sage you may have been able to guess the things that were going on outside. Lately we've been working on a different problem. There were the time-sharing concepts in the early days, concepts of commn data pool---that was years coming into use on the outside.

They weren't the developers, they were the coders that were hired and trained and put out.

STC, because of that requirement, also did binary training and its programmers were way ahead. ~~For~~ We were picking up the loose ends ten

years later.

Yes, but that's putting 200 or 300 people on the same program.

STC's training program, I think, became a saleable item, didn't it?
Aren't they still selling?

Not very many.

I would like to ^{tell you} know, if Hank hasn't already told you, a little about how the History Project started. Along about 1953 or 1954, when I was at the Pentagon, and I were talking one day and he said, "Why on earth can't we take advantage of having some of the people around that really helped put the computer thing together?" "Can't we get the total story assembled in an authoritative way?" So I said, "Oh, there must be all kinds of organizations around that would be willing to undertake that." So between us, he and I started a kind of a soft-shoe dance sort of thing around each coat. We finally got to the point where we were in the hands of some people in the National Science Foundation who'd published something similar for the Society of activities in another field. A couple guys then We took the stuff and gave it to them. They took one look and went "Aagh." So nothing ever happened on that. I worked on some of my friends to see if they could dig some money up. was there at the time and he was very much interested in it but he couldn't justify He had other projects to support like Project and a few things of that sort. So it kind of flatly sank for a year or so and then all of a sudden I was chairman of the Finance Committee at and we began to see opportunities to put a little money away for the project. They kept badgering the Committee and the board to come up with projects that the money could be spent on. Nobody could come up with much of an idea so I

said, "I've got one."

So we started all over again and it took about a year and a half. Paul Armour and Pete Gilchrist finally got their nerve up and called the president who had taken over as executive director. They began looking around and, sure enough, it didn't take them very long to establish that the Smithsonian Institution had the finest reputation for this kind of history work of anyplace in the United States. So we got hold of the director of Museum of History and Technology. This guy had done a lot of work himself on ^{desk} ~~his~~ calculators, cash registers, and this sort of thing back to the 19th century. It was a hobby sort of thing. So the next thing we knew, we had an agreement with the Smithsonian. The Smithsonian would supply a principle investigator on his own time who would be made available; he would need funds for travel and things of that sort. The big argument at meetings was what the priorities were and we decided then that we would retain as the priority the capturing of statements from people as compared with simply swirling away a lot of documents

I was visiting in the Pentagon about three years ago on one of my many monthly trips to the Pentagon, and I had occasion to meet the historian of the Department of Defense....

Rudy Winnaker?

Right, Rudy Winnaker. I was in his home one evening, as a matter of fact, and I never heard a man speak of a name with so much violence in my life as he did of a fellow named Murtaugh. And he was most highly upset by the fact that that individual was trying to collect little pieces of hardware instead of the type of information you're getting.

Well, what happened was that the agreement with the Smithsonian was that there would be an advisory committee, and were assigned to this committee. The Smithsonian put up two of their directors, and I insisted that Rudy Winnaker who was the fifth person to be agreed upon by the two of them because I knew that sooner or later an awful lot of the material would have to go into some place being classified and probably^{not} with any longer degrees than them. Hank and Rudy are now in the process of getting a lot of that stuff declassified. The guy in the first slot was the Curator of Mathematics, and he got the job of principle investigator. The whole thing went on for about a year and a half until one day at an advisory committee meeting we discovered something that the Smithsonian should have told us but hadn't found out themselves. Rudy was extremely capable of collecting these items and had no basis whatsoever for communicating anything out of it; it all went in. Hank is still in the process of extracting some of the notes he took

When did you work for IBM? I've always wondered about that.

About 1957 or 1958.

As Walter said, the oral interviews is our prime objective but I discovered in the short time I've been there that it is important to collect these documents, not only for the reason that I mentioned but also simply because the project has been publicized in the last few months and we've begun to get people coming into the office to work on projects of their own; for example, on Eniac there's an article that's going to appear this month in a national business magazine and the author of that article spent a number of full working days going through the documents that fortunately had been collected and left in our office. We had all the original press announcements and fairly good collection of material for that project. One of the projects we hope to assist is the one that Don Hughes, Bob Greenburger, and a fellow in England are working on in terms of a source book on the history of computation. Again we're trying to document the kinds of material we talked about here which would be invaluable in reproducing those first occurrences of various things in that particular area. Another young man, ^{who is} Thompson, ~~was~~ a consultant to the

computer terminology. He wants some of the early glossaries and is interested in finding out when in various reports a first appears and how it's used, which may be very different from its current usage. We're helping on that project. So, while I've decided oral history is our prime objective and it is very important to get these documents--locate them, identify them---because we never know what kinds of demands the scholars of today or tomorrow are going to make upon these collections.

The funding---the amount of money on an accounting basis---that is put into this by the Smithsonian in terms of salaries, equipment, secretarial, and things of that sort roughly \$100,000 over the last four years. We're not sufficiently confident that we have the confidence and momentum built up but we're going---we're pretty aggressive---on fund-raising activities starting about the middle of this month. IFM is standing an offer of \$15,000 a year for five years. We hope that other people will come along and ^{help}/make these projects possible. I never asked my associates how many "others" had to be there before I started picking up their annual checks. But what I'm driving for is about \$55,000 a year

So if you know any benefactors who want to come along, let me know about them.

I think the prime reason for the external funding is to get the

Unless we set out some kind of productivity in that area, there's going to be the typical museum thing that never

Well, we're not entirely fund-raising because several of us

Before we go on, can you give us an address where we could send anything?

Museum of History and Technology, Room 4212, Smithsonian Institution, Washington, D.C. 20560.

While we're on this, I'll make an appeal for something else. Our journals in the field of computer science are as close to 0 as they can be. When I try to trace down articles and I want to read it, it takes sometimes 3 to 5 weeks to get a particular journal out of the Library of Congress. So when you're cleaning out your attic, if you have back issues of journals that you would like to contribute to our project, where I can put them into the Museum Library and have them available for my own research and for people who come ~~into~~ use our facilities, I would be delighted.

Now, define what journals are.

Is Datamation a journal?

Yes. I really want journals in the early years.

I've got a complete collection---there's a few missing that apparently were stolen---but I think it's up to date from the first issue with the title "Datamation." I think it was called something else before that. I think it was something Engineering and Datamation.

What year did it go to Datamation, do you know?

It was 1957 or thereabouts.

There ~~were~~ only a few issues that had another title.

Mine started in 1961.

Well, I'll define journals in the sense that those of you/^{who}are active in the field subscribe to certain journals they had material that you felt was important.

I've read a couple articles in house organs of various organizations that are invaluable, and those are the kinds of things ~~that~~ are never show up in professional journals.

If you can get a complete set of STC magazines, they won award after award for publication.

If the word journalism is as broad as it used to be....

~~If~~ Every time I meet someone connected with one of the journals, I raise the question that so far nothing is forthcoming, and he'll say, "We'll look into it and see what we can do."

Once again, a comment about STC. Their library had cut back during the problems of the last few years. They had a very thorough library with all of the early ACM material.

I don't think they did anything that had any value the way they decimated the library funding.

I think the collections of are still intact.

O. SMITH: As I sort of flipped back and forth in mathematic computing, my contact has been spasmodic and altogether different from that most of you people had. It goes back a long ways: I was an undergraduate at MIT in the days when Busch was the Mechanical Differential Analyzer and the CE Department was doing a fantastic job at analyzing and equation solving. Krout was developing the Krout method for solving numerical equations. I did a bachelor's thesis in 1940 on numerical analysis, and I worked on early projects. Then I went into the Army and all I had to do with a computer there was a few large fire control analyzing meters. I came out of the Army in 1946 and someplace along the line I had come in contact with, I think, Becker's book, Punch Card Methods and Scientific Computing. So when I started job hunting, I knew what I wanted to do. I started out here in San Francisco, and ⁱⁿ San Francisco the only

prospect I could find was the American Totalizer Company. Down here I tried IEM, and the receptionist had no idea what I was talking about, wouldn't let me talk to anybody that would have any ideas, and suggested that I take the operating course. Then I wandered over to Remington-Rand and it just so happened that Remington-Rand had the accounting contract-- and accounting machines---with Douglas for years. Now Douglas was talking about getting in a bunch of IEM equipment for scientific computing.

Remington-Rand was in a panic; they thought that would be a foot in the door. So they---the local office---wanted to know if there was any possibility that the Remington-Rand equipment was being used for scientific computing. So I tried for six months to make those things work. I know I made it work for a linear equation solver but

and it was just about impossible. I thought years later I would not have the gall to accept such a ^{such a} position in that stage of my experience. That was a temporary job and I then moved on to Northrup still looking for working computing. But this time I went shopping around first---there were about three places that were doing this then. I wound up at Northrup in their ^{Aerodynamics} ~~Mathematics~~ Department. The guy at the next desk was Bill Woodbury, who somebody said went off and drove a cab for a while. He came back and became one of the ^{two} ~~three~~ people who were instrumental in developing the CPC.

Was Branch Blough in that outfit?

No, he was around but....

I mean in your Aerodynamics bunch?

No, he was in Vibrations about 50 feet down the way, but he was there.

Another thing about that in another vein: The first job I did wound up in a very large computing job to be done on the IBM tab equipment, so we hired a couple consultants to do the job. One of them was

Nelson

He and Stan Frankel were in punch card computing at Los Alamos during the war. Nelson knows computing from the very beginning. I told about the CPC development, as much as I recall. I left Northrup to go back to school. Then again I started out to go into computing and got diverted by an offer at another university. I did spend a summer at the Institute for Numerical Analysis in one of their very early summer programs, the first or second summer program they had out there. As I said, I took a course in computer design. I was clear away from computing for a number of years. I came back and went to work for the Corporation, again not computing. I got fed up with being a systems engineer after a year or so and joined the Computing Center with the 1103 and then on through the whole series---1103A, etc., etc.

Well, I've got an 1103 story. The 1103 memory and they were having trouble with unexplained memory losses. Finally, somehow they ~~finally~~ tracked it down. The VIP's came and somebody would guide them around, and they would say "In view of the Just walking across the carpet they would pick up enough to start it.

We also had a roof leak in the next building; we had a new computer and a new building and the only leak in the roof was directly over the corridor.

You mentioned your first ^{master's thesis} ~~masterpiece~~ in numerical analysis;
what did you---did you ever have contact with

That was Senator Hitchcock who was one of the first people to

Is Hitchcock still alive?

No.

My master's thesis, although they didn't have the words then,
was on the matrix power method with the matrix to solve problems
with the number of available.

Did the phrase "numerical analysis" even exist?

I don't think so. Scarborough was the only text I had that

JACK STRONG: As I stated earlier, I had my first job in in
what one might usually call computing in 1936 at the University of Oklahoma.
We had a keypunch, sorter, and a tabulator---all IBM equipment. The tabu-
lator had a board that had three pieces of catgut that went over some
rollers that had

It added and subtracted and listed something like 40 columns. The most
significant thing that happened to me while I had this job was that the
sorter was by a window and it was customary to take the cards like this
and do a cavalier flip. One day I flipped them all out the window.

After a couple of years, I got a job at North American in Dallas---that was 1941---in the Tab Department there. We had what I guess was really one of the first IBM computers and a machine called the 601 which multiplied, added, and subtracted but didn't divide---a fairly significant shortcoming. So we divided by using reciprocal tapes. But the thing that made it a computer was that someone had the brilliant idea of putting a couple relays in it and giving it a time control. But that was a fairly significant improvement to this machine and truly gave us a computer. After spending a stint at North American in Dallas, I went into the Marine Corps and to North American up here in Los Angeles in their so-called Computing Department which consisted of three people, and I was one of them.

Who were the other two?

John Van Ness and Charlie Davis. And we had one of these 601's on which we did engineering computations; we used the printing machines, sorters, and so forth. But we actually did analysis and

The most significant thing that happened to me then was this crazy guy that we had named Charlie Davis. He loved matrices and so did another guy that you'll remember, Paul Fish, who was the engineer. In those days, matrix algebra was in vogue for solving engineering problems such as stress analysis and he came up with a set of equations, 167 or some such thing, as I recall, which I was trying to do on a machine that wouldn't divide. I worked on that for I don't know how many months, not ever knowing that I didn't make a mistake on the first data that was perpetuated through. Finally, after row after row after row of cards the project was not a worthwhile project. Sometime about there

the 602 came along and what we affectionately called Porkchop. We had the multiplexing devices that actually did/^{perform}the arithmetic functions.

Then we got the 604---I've forgotten when Owen came in....

July 1953.

That was prior to getting the 701, wasn't it?

Yes, in September we got the 701.

Owen was our first honest-to-goodness programmer and I guess I just found out today that he was lying; he said he wasn't. We had a guy named Jack Brookshier who was our CPC board wirer and programmer---he couldn't get to work on time. I don't know where Jack is now.

Jack went to Magruder from there because I met him at Magruder in 1953 before I came to North American.

Yes. We had Elliot Knorr who could talk to people. And Ed Goff, who wrote our first assembly to the 701 computer which, incidentally, was a very good assembly---it lasted the life of the machine. And Owen Mott, and I think that was our original group of programming people.

Don Veheim and Erwin Martin came at the same time and there was another guy, Bob Caiper.

Caiper was an operator with Royal---Royal Waymus, the guy that you tried to teach programming, he didn't make it; you tried to make an operator out of him, he didn't make it. He ran the 519 and reproduced cards; for 30 minutes he was reproducing cards and Ed Law came along and said, "How're you doing?" "Everything's fine; it kept stopping with the red light. I found out as soon as you

We got the 701 installed and had a beautiful layout---carpets and air conditioning which nobody else in the plant had except Fletch Candleburger....

And the visitor's room; remember that?

Oh, yeah, the ready room. And we had operated the machine for about a couple of months and found that it was time to hold a demonstration for ~~the~~ Fletch Candleburger who was the president of North American at that time, some generals and admirals, and whoever we had in the room. We very carefully practiced our demo and everything was going well except that Candleburger was standing over in front of the tape machine and he called me over and said, "I don't think this thing's working exactly right." And sure enough there was this tape streaming down to the floor. Then the next thing that happened to us was in our new building the rains came, and the leaks weren't concentrated over one unit; they were concentrated over the whole damn ceiling. We really had to bring in canvas to the machine room and set up a tent with holes cut in the canvas where it hit and put barrels under it. We emptied out the barrels about every hour.

CRW had to do the same thing, by the way. STR did exactly the same thing, only they were wiser; they had plastic sheets they put over and drained it all to one spot.

Anyway, the rains persisted and we decided the best thing to do was to get this stuff on the outside. So we had the tentmaker make a cover for the whole damn building for the outside, because the people who fixed roofs said they couldn't fix it when it was raining and then when it stopped raining they said they couldn't fix it because they couldn't find it.

So we went through the whole gamut of machines during this period of time. We divisionalized into Rockadyne Division and so forth. We installed I don't know how many machines---15 or 20 or 30 or 40 in that period of time. We also installed at North American the first commercially developed microwave system which we described earlier today, I believe. I left North American in 1962 and joined what Roger has referred to affectionately/^{several times}as

You had another job; you were with CEIR.

Oh, that's right, I was involved with the infamous ^{convinced} Program for about six months and finally ~~came-back~~---probably on the day that the annual report came out and had those ^{machines} spread all over the ^{---that that was really} cover/~~which was~~ bad news. Anyway, we cancelled those. I left and went with Fletcher. I think ^{Owen}~~you~~ went with Fletcher just before I did.

Yes, I left in 1961.

And we went through the 1107 and the implementation of software from there. Fletcher and CFC were on the way to success.

^{Fletcher started} Jack got ~~such-a-start~~; he was the guy that got/^{him}into a supervisory job at Columbus where he formed CFC out of.

He's also the one that made him the secretary of Share.

That was Frank; Frank did that.

But I think Jack suggested it first.

That's just about the story of my experience with machines.

SWARTZ: I got...my first contact with these things was at T. J. ^{in New York} Watson Laboratories/when I was at Columbia University in 1953---the 602A, CPC, that type of thing. I still remember a guy by the name of Eric Hankum one day spending a couple of hours talking about the coming 650's and it was if there was nevergoing to be anything---that was the end of the road. And

it sounded that way at that time. That was 1953 and I didn't realize there was a big gap that was mentioned earlier. I went over to Rand the next year where I did work on the Joniac and I read last year---I didn't realize that what I had done on the Joniac was to develop the first symbolic assembler; I didn't realize that's what I was doing.

The thing I remember about Joel was his coming to work at Rand--- I think he was back at Columbia and we understood that he was to come in June or July when school was finished. Sometime late in June, Madden got a frantic phone call from Joel who said, "Since I've been here, I've gotten married. Does that change anything?"

When I was a member of Pact I, I coded with another fellow by the name of Gus Hempstead, ~~who~~ the loop ~~the~~ generation part of that system, and the only thing I remember about Gus is that I had a lot of people fall asleep during lectures in a group, and Gus ~~was~~ the only guy I could remember. I could be standing in an office with him and me alone, talking about a problem, and he would fall asleep.

Don't forget to mention his songwriting. He wrote a song.

You'd never picture it, but he was an excellent musician. By looking at him or talking to him, you'd never know it. Okay, let's see. So then I went out to Sage where I did work on this Lincoln Utility System which was my major job. I think, as was stated before, thst was for a time a very advanced system. It did things that many systems failed to achieve and it had certain aspects to it that other systems still don't have. It was trying to do a job for a whole bunch of different programs which all had to work together.

The interesting part about that system was a major one---it was rather regimented there since they had hundreds of people who were former street car conductors, undertakers, and everything else who had been recruited and trained, trying to produce programs. So it was regimented and they made everybody use what was called a Lincoln ~~checklist~~ checker; that was one part of that system which was not very strong. It was very good in problems but it had one weakness and that was that it executed---it was a simulation of the machine on the machine, in effect, a real interpretive system. It executed one instruction at a time which it ^{went to} ~~wanted~~ the drum to get. It would take literally many minutes to do the equivalent of a micro-second of work. We had some pretty bright guys there who were doing the operational programming, one of which was Irwin Brooks; I don't know how many of you know him---a very bright guy. He was carrying on the operational programs and had only seen this program work under the checker which makes the computer look like a hand calculator. But he was doing a problem at one point which required the writing of tapes and the timing was such that he couldn't run without the checker and he said, "Is that possible?" And he was actually trained there---a very bright fellow. I said, "Yes. How do you think they checked out the checker?" and we went through that for a while. He thought maybe they built another checker. He finally said, "All right; I'll run without the checker." So he went down to the computer and he comes back and says, "It wouldn't run." I said, "What do you mean?" He said, "I put it in and nothing happened." So I said, "Try it again." So he went down and tried again and says, "Nothing happened." I said, "Are you sure?" It turned out that it had run; it took microseconds

and he'd never seen this before. He'd only seen it take minutes. So other than that kind of thing which was eventually scrapped, it was a very good system. I think it that were described in literature at that time, which it probably wasn't and still isn't, it would be of value. Then I went on to the 709, I guess. The 704 was in there for something. I worked on the first few Jovial compilers. The first Jovial compiler was on the 709 and it had some "firsts" to it; I think it was the first---one of the first anyway---that was trying to do something other than mathematical problems. It was also probably one of the first that was coded in itself---coded in Jovial. The first compiler was a 7-pass compiler, the last pass of which on the 709 took four hours to compile itself when we first developed it. It used to take all night to recompile the compiler. But then we found some problems and cut that down to perhaps several hours to compile the compiler. Of course, when the 709 came in and the 7094, it was a fast project compiler---it took an hour to compile itself. There was that sort of thing, and then I went on to 232 which was originally supposed to be the new Sage machine to replace the Q7 but became primarily a research computer at STC. And we developed one of the first large time-sharing systems--- that was the machine that was the unwanted baby we left on your doorstep, but it turned out to be very valuable for a lot of things. We did that under the direction of Lidlager who was a very intelligent and very emotional man. He had had a lot of experience with computing but had never heard of a punch card as far as I know. One time somebody mentioned doing something with a punch card, and he almost literally threw him out of the room and threatened to cancel the contract. He only knew about in the computer so he was behind the time-sharing work in those days. Since then I've been up with the 360 and currently coding on the 1108.

Who would some of the people be that would be the ones to collect information---to get information like this?

I know some of the people from the West would certainly know some of the people who were originally there.

(Names were mentioned here which I could not hear.)

Jim Burrows who was technical manager and who is still around at Miter would know.

Speaking of throwing people out, Jim Burrows---one night we were in trying to do a very ---throw a big show that Sage has, as you know a little late. We were madly working at the computer one night trying to get it to work. Jim Burrows walkedⁱⁿ and threw me out of the room because I was drinking a Coke. No Cokes were allowed in the Q7 room.

Strong had that rule in the 701 and the 704.

After you spilled a few of them.

I didn't spill any Cokes.

I gave Roger an article to read that was sent to me on the scrapping of the Q7. Apparently there are still 2 or 3 in existence, but most of them have been reduced to scrap according to this article.

The 232 was scrapped recently; I got a piece of that somewhere.

ERWIN GREENWALD: I got into computing in a kind of strang way. I was doing some graduate work at UCLA in 1950, ran out of the GI Bill and needed to go to work. So I went over to the Bureau of Occupations there and they sent me down to a strange little place in Santa Monica called the Rand Corporation which was then located in an old Santa Monica outlook building on the corner of Fourth and Broadway. They took me down to a ramshackle basement where Paul Armour interviewed me and took me through a maze of

strange boxes, explaining what they all did. He asked me if I had any questions on the way out. I said that I didn't understand enough to ask any questions and I guess that's why he hired me. At any rate, I did a lot of work then on Monroe calculators, some on the 604. I remember when I was doing my training in the tab room, I think we processed a million card files of medical data by reproducing them and a lot of other weird things, doing some statistical accounts. That was ^{Myer who worked on that} ~~my~~ work; he would know more about it. But these cards had circular holes in them and couldn't quite understand where they had come from. I have never had any really formal courses in programming, so I kind of learned by the seat of my pants, except for one brief flurry when Rand was building the Joniac. We had weekly seminars conducted by Cecil Hastings, Bob Bosack, Don Madden, and I think Wes did some of them. I remember Cecil Hastings taught us how to count in the octal number base by ~~building~~ drawing little rabbits on pieces of paper that explained what the octal number system was---three rabbits was a set. I found that piece of paper on one of my recent moves. At one of these sessions, and this was before the Joniac was even on the air, if my recollection is correct, Don Madden or Wes suggested that there was no reason why we couldn't feed equations into the computer and have it generate its own program. That's way back in 1950. One of these two guys had some insight because we all thought they were crazy. An interesting experience on the CPC--- I remember one run that I had that took eight hours and all we were interested in were the final results which Herb McGrath ran in the machine room, and the final results were unfortunately printed on the paten because the machine ran out of paper. The first job I actually did on a big computer was on the

Univac I back in---which was housed in Philadelphia, was the --the computer we used was going to the Census Bureau. We were doing a job for Livermore and they had another machine on the floor that wasn't quite ready. Our security was interesting in those days; the name Livermore was either classified or the fact that Rand was engaged in work that was classified, so the leader of this project, I think it was Hal Brogue, used to leave little messages on my desk, sort of like "The guys from "kidney-less" are going to be in here tomorrow." Our time slot on the Univac followed that of the Census Bureau---this was in 1951 or 1952, I guess---which was doing the 1950 census at that time. We'd walk in the morning and they'd be walking out shaking their heads. Finally at 7:00 one morning, we walked in and they came up to stop us because they were just completing a 12-hour run which had to run error-free. That was my first lesson in checkpointing because they had absolutely no checkpoints; they had to have a perfect run of 12 hours or all tapes

I guess I wrote the first production jobs for the Joniac which itself was interesting. We wanted two things out of it---we wanted a bread-and-butter job to run on the machine and we also wanted a job that ~~was~~ would verify that the machine itself was not goofing off. The so-called assembler I had was an octal loader that ~~was~~ Wes Mellon wrote, so we coded everything in absolute octal---or I coded everything in absolute octal, I guess. One of the things in order to check the hardware, we wanted to make sure that if the divide check light came on it was a hardware failure, as an example, so that prior to any division we checked that the numerator was less than the denominator throughout the code; all adds were checked to make sure that

they wouldn't overflow before any adding, or what have you. So that any machine indication that something was wrong would indicate a machine error. I also learned something interesting about /^{tooth} complement machines; it turns out that ~~when~~ⁱⁿ a tooth complement machine, 2/-78 when truncated becomes 2/-39 instead of 0 and so our equations went unstable. I had a big argument with Wes at the time over whether this was appropriate or not. So we fixed that one. That machine had the world's first high-speed printer on it. That machine had one of the first core memories on it---it was ^{Inter}National Telemeter that built it, was it? In fact, if I remember correctly, Bill Gunning wrote the specs for it and then went---had to go to work for them and meet those specs. The memory was good but ~~in-the~~^{Inter}National Telemeter, because there was a delivery penalty clause in the contract, didn't come out too well.

It also had the only select

I guess the two main functions I had for a long time was being responsible for the work of the Rand Circuits Division and also being what we at that time called the machine mother for the 701 and 704. I went to work at---worked with Owen and ^{Jules}~~Jewel~~, and people like Tom Steele, Roy Rigsby on the Pact project. I think one of the important things, from my point of view, was the people that I worked for, the people I worked with, and the people who worked for me in terms of how I learned about computing. When I first started, I had people like Bob Bosack and Don Madden, and Wes Malon, and what have you, showing me the way. I think Wes was the one that in these

Joniac sessions showed us how to do subroutine linkage which was a strange kind of thing at that time---I couldn't understand how that worked. I worked again on Owen, Tom Steele, and a bunch of other people who I can't recall right now on the 709 System Committee which was attempting to specify the language and systems for the 709. To my mind, other than the operating system, one of the big things that occurred there was that we formalized the idea of symbolic debugging which keeps bouncing back and forth in the way we do things; I believe it's a rather important concept. I went to STC in 1960 and got involved with the 709 there, the 1604, the 160A. I went back to Rand in 1964 and worked on the System that Joe talked about. There was another machine, the PDP-6^{on} which we found a divide that didn't work right, which was kind of interesting. They had a symbolic debugger which was the first one I ever saw, and Joe, Ed Bryan, and I could all work simultaneously. For two days one three-line had me befuddled and finally I said to Joe, "Shouldn't this division produce this result?" He said, "Yeah." I said, "But, it's not." He said, "There's a bug in the debugger." So went to the console and verified it. We had a teletype setup back to Maynard We got on it and said, "Try the following and see what happens." The answer came back, "Our machine must be wrong, too." Subsequently I did the software for the Rand videographic system which was based on the IBM 1800. Then around 1969, Joe, Ed Bryan, and a guy named Dave Farber who was going to XDS thought that it would be a good idea if I went there. Even though this is being recorded, I don't think it was. That's about it.

Of all of these Rand people, nobody has mentioned what they really do over at Rand.

OWEN MOTT: As I said before, I started in this business as a keypunch operator in 1945 for the Army. There was personnel accounting that I was involved in, and I figure that I punched about a million cards literally. After that I went to the University of California.~~in 1949~~ I graduated from the University of California in 1949 and the only job I could find---I had a degree in Mathematical Statistics---the only job I could find was as a tabulator operator for So I worked as a tabulator operator for a year and a half, then I came back to the University of California and did graduate work in mathematics under D. H. Lamar and did free-lance computing. I was using some of the machines they have in the Engineering Department doing free-lance work for their engineers and professors.

What machine was it?

602A, 405, tabulator. One of the first 602A boards I wired, I couldn't close the door on ~~because~~ the 602; that was a series of so I had to redo it and do a simpler series. When I was in the 405, I was doing some work on ocean wave research. I was using the 405 to do other correlations. The Caldac was just starting to work because there were things I could compute faster than the Caldac. Then like Erwin my GI Bill ran out and Lamar offered me a chance to go down to INA, Institute for Numerical Analysis. My boss there was Emory Yell; he goes way back---he goes back I don't know how many years. Of course, Lamar practically invented the fancy tricks they use in punch card equipment. Dan Sacrow and John Postley were there; the three of us were working there in the essentially EAM room where the 604's were. One of my first jobs was doing a job for Tampa Research; it

was expansion of the problem. Apparently he had expanded around the wrong point. One of the first things I did was get on a setup where I would always get 0's, nothing but 0's; everything was canceled. So I sat down and analyzed it a little bit and discovered that it was not only around the wrong point but it was in the wrong order. If I had proceeded, I would have lost 90 places of accuracy. So we solved that and got it in the right order. I computed on that for a couple months and ended up with a set of tables---I think about 65 values. But out of 65 values there were a total of 70 good digits. I also wired some boards for the CPC's. At one point on the CPC, I made the CPC act like a select program calculator; it didn't have very much storage capacity but the Joniac didn't have very much storage capacity either, so I wasn't really that far away. It had a large capacity board, 32 digits 602A and 405. It took a while. You'd take the deck of cards and put it in the 602A in the morning and not have to worry about it until evening. We did work in support of the SWAC. The SWAC got around a collator, and they were using a collator for input to the SWAC but bringing in binary cards which really gave us a anyway. The only problem was how do you get the cards punched up before bringing them in. So for a while, everybody was punching binaries. Finally we got a system where you punched an octal, you kicked it into a 602A, and had the 602 rigged so that instead of punching out 5's, you on the 602A you had automatic switching and the 5 figure that went to the 5 punch could go to the 6, 7, 8, 9, 4, 3, 2, 1, 0, 11 or 12 punch. So you'd punch out 12 cards and take these 12 cards over to a 521, begin punching them together, and you had yourself a binary card.

The had 0's and 1's, and the Binac had an 0-10 wired

up so that when you got to 7 you got three 1's at a time because this was only 0's and 1's too.

At that point, they closed down INA and Everett Yell suggested that I go over to North American; he thought that was the most alive place for me to go. At North American I came in during the summer of 1953, about three or four months before the 701.

Why did you go to Finance instead of to Wagner's bunch?

Because that's where the real action was. So I started doing some of the work getting ready for the 701. We had gotten involved in the 70 assembly program for the 701, six fields, sequential symbolics. Ray Ferguson also wrote an assembly that checked out in two days. We concentrated on developing a discipline---I would say discipline at North American. I think it payed off when you compare our operation with a lot of others. We found that the real problem with the 701 was the printer. The tapes didn't work very well but the drums worked. But the real thing that defaced everything you did was the printing. We were very interested in going to tapes but using variable printers, so about a year before the 704 came in we took our system which really was not an operating system but a rigid system of running, we took our card read program and our print program and replaced those with tape write machines. We had ourselves a tape operating system.

What did you use card-to-tape for? Did you use the machine to put card to tape? You didn't have a card-to-tape and tape-to-card.

Yes we did.

Not on the 701.

Yes we did, about six months before the 701 went out.

The 714?

Yes, the 714 in and the 720 out. We used the same utility programs inside nobody else changed anything.

But you had to change the tapes on the 701 to do that.

I don't think we ever did have a 701.

Yes, we did.

What tape did you use. 727 was on a 714, right?

Yes, we got 727 tape.

726's were replaced very late in the cycle.

Anyway, we converted that one really to the first operating system on the 701. It was basically an experimental program.

You were largely responsible for that too, Roger, because we were trying to get you out of the Fifty miles of console, right?

Oh, you couldn't touch the console. You walked in and you couldn't even tell the clown who was on the console that you wanted. For example, in the speed core error was interpretive, you'd get a stop. They had a clown named Ramus sitting there, and I said, "Where would I stop? Can you find location 22?" "Oh, yeah." He started playing like on a console piano. When he got through, I reached into the drawer, pulled out the instructions, looked them over, and said, "I think this is how we do it." Five minutes later, I looked down; by that time he had cleared the machine off.

I was also involved with the Pact project at that time. The Pact project, I guess, started in 1950. We got together, as I said before,

with a group of Southern California people. We were aware that several people had done interpretive programs which accepted equations. We decided that that wasn't really the problem in computing, you know, doing equations---the easiest part of coding; the hard part of coding is bookkeeping and control. Pact was designed to solve that problem, but I guess we missed the boat because ^o tabular formats were going out. Actually it produced better code---slightly better code than Fortran did. /^{The first}Fortran was the best optimizer Fortran that IBM produced up to the last Fortran H.

Don't forget Tom Steele.

Yes, that's Tom Steele's again.

I was just up to the point where I was telling them when you came out from General Motors and proposed that North American and General Motors get together to....

So we put together the 704 operating system which was the three-phase operating system, and we and General Motors both used it successfully. It also went to Thunder. After the 709, I was involved in the Share operating system---the system for the 709. This began with a multi-phase system. I was involved with an assembler which was out of step with its time. The assembler involved a symbolic assembler and a reassembler. The reassembler concept might be much more feasible nowadays; we're talking about time sharing where reassembly might be much more viable product than in former days. The SQUOZ deck was a symbolic deck, actually involved a simple table, we really SQUOZ the hell out of the deck. But there were problems; if you dropped a card or lost a card, the machine skipped a beat, you'd lose your place and start all over. It goes in SQUOZ and comes out hash. I was on the 709 and the 7090. Then I left North American and went to Computer Sciences.

At Computer Sciences I worked on the operating system for the Univac III, I worked through the operating system for the Univac 1107. The 1107 operating system was primarily designed by Joel Erdwin. The operating system is still being used today on the 1108 which it was not designed for at all.

What did you have to do with Dyna_____, ^{MOBL}~~Mobil~~?

When was that, 1963?

1962.

Did you talk about ^{MOBL}~~Mobil~~?

No, I didn't talk about MOBL; that was in the last years at North American. That stands for Oriented Business Language. With the accessory to which would be the assembler we spoke about earlier, turned out to be one of the first slow processes. But the program degenerated real fast. Together with the operating system they ran in, they really ran fast. A number of programs were put on. I worked on a time-sharing system for IBM. I am still with Computer Sciences and am working on a time-sharing system for Computer Sciences. I got involved with a conversational PL-1 which was abandoned, Thank God; cobal, Fortran, etc.

Jack, I meant to ask you, did you have some involvement with formation of Codacil?

I was co-chairman of the committee.

Sometime that might be worth talking about.

When was that?

Cobal must have started in 1961, roughly.

No, it was earlier than that--1958 or 1959.

It came after Pact.

Well, it came during Pact.

I still think Pact was a better language than cobal.

We had a meeting with quite a few manufacturers and there was unanimous decision that Pact was the language that should be abandoned and that we should adopt cobal. All the machine people went back to their respective offices the following Monday, and it was not announced.

Actually it was something that was latched onto very early and looked at as the answer to problem solving, particularly in this area with groups around the Rand Corporation. While I was at I talked to Joe and Kelly Gottlieb about some of the early developments of and later their work with the Bromley which they bought from England. Kelly Gottlieb mentioned that when he got the their biggest projects were doing computations and ~~calculations~~ ~~compilations~~ for the River atomic energy storage in Canada.

Was there any tie-in with the work going on here and that going on in Canada, or was everybody working independently of each other?

I don't think we knew they existed.

Not that there wasn't any tie-in, because there was a lot of communication between North American and United Aircraft at that time.

There was a great deal of parochialism on the West Coast. We knew everything which turned out not to be true, as witness Fortran versus Pact.

In fact, we didn't write assemblers; we didn't realize for a long time that we were writing translators and that the assembly concept is still a mess.

I keep thinking of so many things that popped up. In 1954, the was around and just about that time were popping up at Rand.

Well, there was always a set of people in any area that "I can do it better." at Rand was done in the light of a lot of other things that had already been done. One guy decided he liked it better and tried it.

Most people identified more with industry than they did with university.

For an awful lot of years while I was at Rand, we felt that our function in the numerical analysis department was a service function. We provided bona fide service to people who needed computations done, and it's only recently that Rand decided it wanted also to do some computer science research.

Tragic blunder.

Well, the people they had may have been a tragic blunder.

Did any of you who worked on the IBM Joniac-Univac machines, ever work on the foreign machines?

Mort did a lot of work on the Triple 2000.

Did you have anything to do with the Triple 2000?

No.

Who did the compiler conversion for the 2000?

Cal Jackson.

I was doing a lot of interactive IO with the data display and it got to be more and more of a program. One day I went to compile; it got to about the second half and I had to shut the system down; it just wiped it out. He said, "Oh, the system moved somehow." It was a 50-60 minute compile---a large program. So we ran it again and got exactly the same results, exactly the same problem. We called the CE and said, "Fix your machine." He was a very frank guy; he was software oriented. The CE looked at it and said, "Gee, I don't think it's the machine; diagnostic. Why don't we ^{everything we can.}" He assured himself that it wasn't the hardware. He got the next job and the next morning he came in and said, "I found your problem; there was a bug in jovial." It turns out that I had just/^{enough} direct code. The storage area permitted for the direct code/^{code} on the back end of the drum which held the remnant portion of the system, but there was nothing to test whether you had run off the end of the drum until after it got to band 3 and the next logical address was 0, and I overwrote the system. I had a choice at that point ~~of~~ of either trying to put some of the direct code back ~~in--the~~ into jovial or take all the jovial and put in back into direct, because I wasn't going to assemble any more of that version.

The 101A---we were thinking of getting that at Rand one time and I was asked to put on a floating point interpreter which I cast in paper. I don't know why we didn't get it; maybe we thought the CPC could do the same job or we could put an interpreter on the Joniac I, which is the only drum computer I ever used and attempting minimum latency coding is a

Did anybody work on Radac? McGoo?

Tutack may have.

Tutack and McGoo?

Yeah.

The other step was the 2000 series. There's another guy who was very heavily involved in the 2000; the redhead.

Warren Meyer.

Yeah, and who was the guy that headed up the machine room for a long time on the 2000? He was a programmer who became a machine room manager.

Another unusual thing about that Raydac machine is that it was enclosed in lead.

Also the machine; it operated on B, fill in the C, and jump the D.

There was also the CEC machine that had this funny little fast memory that was rotating like the Uniac memory rotator.

Yeah, they became Electric Data and they had a computer out there that attempted to---it was a drum computer again---they attempted to speed it up by having a circulating fast memory of about 16 words.