

INTERVIEWEE: PAUL MORTON

INTERVIEWERS: Robina Mapstone and Henry Tropp

DATE: 12 October 1972

RM: Today is October the

PM: 12th.

RM: 12th.

HT: You'll have to speak up, too.

RM: And we are talking with Professor [Paul] Morton in his office at Berkeley in California. OK - take it.

RM&HT: [Laugh]

HT: Well, we -- as you were just doing, I think we ought to start with your undergrad- your undergraduate training at the University of Washington, and then talk about that period at MIT when you were getting your Masters Degree.

PM: I went to the University of Washington after several years of work as a lineman with the power company. And [I was] interested in power networks. And I finished in 1931, .. after working for the Puget Soun -- the Puget Sound Power and Light Company while I was going to school. Then I worked -- well, I had a couple of years out for illness, and then worked for

Ford, Bacon and Davis of New York, consulting engineers, on a court rate case which involved the system, the Puget Sound Power and Light Company system, as a whole. And my job was to inspect the transmission lines to get the depreciation which had occurred, the accrued depreciation, and report on that as evidence to be used by expert witnesses in this rate case.

Then in 1935 I went to the University of Washington as an instructor in engineering. I stayed two years and went to MIT in 1937. At MIT I was still working on power system .. analysis, in course work and so forth. And the second year, from '38 to '39, I worked on the network analyzer as, essentially, an expert consultant for power companies like the American Gas and Electric Company and the Brazil Electric Company, and so forth .. who brought in data for their systems and analyzed it on this public utility computer at MIT.

The network analyzer at MIT was a rather clumsy, .. low .. frequency, that is, 60-cycle machine, which had some advantages, but was

rapidly superseded by improved designs at Westinghouse and at General Electric, which were 400-cycle machines. .. These were model systems of power systems, of large-scale power systems, with 30 or more independent sources which led to complicated differential equations, which were essentially modeled and measurements [were] made to find the actual operation of a three-phase system from measurements on a single-phase model. At this time I also got interested in the differential analyzers.

In 1939 I came to the University of California and tried first to get a, a network analyzer installed here, which proved to be impossible in the school, because it seemed to be undesirably advantageous to the single utility, the Pacific Gas and Electric, which had a monopoly in this area. And so this engineering college was reluctant to invest in a device, or even to supply services, which seemed to be so monopolized by one industrial firm. So I rather changed my interest to digital machines, which I had seen at -- well, first, this

differential analyzer which was not a digital machine, but a Kelvin type integrator.

HT: By the time you were at MIT they had a fairly large machine, didn't they?

PM: They were just starting. Caldwell was just starting to build his - his large machine.

[knock]

The characteristics of those machines were somewhat as follows. The network analyzer, which is a model of power systems, made measurements essentially of transfer impedances. The basic problem was to find the swing curves for the machines, which was done by digital difference equations. So we had measurements of interconnections, but we had to make, by hand, discrete calculations step by step of the interesting and important features of the problems.

The differential analyzer had the, just the opposite characteristics. It did the integrating of the essential part of these usually second order differential equations with mostly constant coefficients, not always linear, however, and it did the integrating, but it was limited in the number of interconnections. So, you could

have only, for example, 6 or 8 stations and a limited number of transfer impedances. The network analyzer could have 40 or more stations and several hundred transfer impedances. So they were quite two poles.

RM: Yes.

PM: In any case, I got interested in both of those and here I tried to see what kind of computing equipment we could develop. And so, when I came here in 1939, [I was] still working essentially in power systems and beginning to move into electronics. In about '45, I believe it might have been the summer of '45, I went to Schenectady and spent three months with McGinnis in his laboratory. Also Concordia and Crary -- yeah, Crary, I believe it was.

HT: What's his full name?

PM: Frank N. McGinnis, I believe.

HT: Is he still alive?

PM: I believe so.- I am not sure. And Charles C. Concordia, who is a very perceptive individual. And Crary was the head of the department, only I can't think of his first name. Anyway, we were working there on a number of different kinds of problems, whatever came up in General Electric and on this differential analyzer.

Near that time I got wind of the work of Mauchly and Eckert of Pennsylvania.. And then during the war this was classified, but our Professor Cunningham here, and our Professor Lehmer here both worked at Aberdeen on firing tables, for missiles, for which the ENIAC was first built as a military tool. I saw that demonstrated rather early. And when I got back here, and finances permitted, I started to work on a small-scale digital computer with the conviction that large-scale machines would have limited usefulness. I never believed in the single national computing center which was the fad then; one large center at MIT or somewhere that would handle all the computing in the nation by telephone connections, and so forth. It seems funny now, but that was a serious concept.

HT: It was serious, I think, because nobody thought that you would need that much of it.

PM: That was one point. But the other thing was so obvious that in the first place, the programming, which was largely in binary machine language, was so difficult for the average user, that it was thought that programming specialists

would almost certainly be necessary. Second, the dollar efficiency per addition and multiplication of the large machine was clearly far superior to any other conceivable arrangement of smaller machines and so forth, that it was not thought to be profitable to work on the smaller machines.

My opposite conviction was based to considerable degree on pedagogical reasons. I thought that an institution which was teaching in this area would have to have demonstration equipment even if it was not very practical. And so I thought that every significant engineering school would need some kind of demonstration parts of digital machines, even if it did not try to set up a useful computing center in competition with the national large scale machines.

HT: When did you first start thinking about setting up a small-scale -- building a small scale digital machine?

PM: My first correspondence with ONR was, I think, in about '46. The first contract I had was in - I believe it began in June of '48, I believe I have that date down here. It started in March 1948 was the first contract, which was for, I

think, something like \$25,000 dollars for a study contract to make a tentative design. And after one year it was extended for a 3-year period, and then for another 3-year period. It finally terminated in 1955.

HT: Do you have copies of some of this correspondence and material that we could have for our archives?

PM: Some, yes. Or at least we could copy it. We'll look into that.

HT: Aha - I'm sorry, Bobbi.

RM: Oh, go ahead.

HT: Go on.

RM: No, I was just going to ask [about] the subject of binary versus decimal. And we'd been reading through your, this report coming here, but I was interested in the technique of putting decimal into the machine. How you did it, how the circuitry was designed and how the tubes worked. We were sort of visualizing in coming here how that might have been done.

PM: Well, we used a 10 digit plus sign in decimal digits on a 4-channel, called bit-parallel, character-serial design. That is to say, each number was represented by 10 decimal digits, each of which was represented by 4 binary bits. So the typical

register had four 10-bit shifting registers in which the number could be shifted one character at a time on 4 parallel channels. There was one additional flip-flop used for the sign, so our representation was the sign and magnitude one with the sign handled separately.

HT: So then it essentially took a minimum of 44 elements to represent the 10 digit numbers each of which had 4 binary bits.

PM: Right, yes. Of which the sign element only really used one of the 4 flip-flops for storage; the others were used only for shifting in and out for timing reasons. There were 44 flip-flops on each register. And I have pictures, I think, of those registers and what they looked like.

HT: What made you decide to go to a decimal machine?

PM: Largely the fact that I expected to have students and other comparative laymen rather than a professional staff to program it, and so forth, and it seemed to have an easier interface to people. That is the essence of it there. It did not seem unduly wasteful because our speed ambitions and so forth were not so pressing, and the capacity which seemed quite feasible of a 10,000-word memory seemed to be so far beyond most of the 256 or --

HT: Some of them got to 512, even the ones that said they had 1024 really didn't. Their effective memory turned out to be nearer 256.

PM: We had trouble with our 10,000 memory too, [laughter] but it did seem adequate for many problems since the instructions as well as the data were to be stored on the drum, and we didn't contemplate excessively high speeds, it seemed reasonable --

HT: In a sense, you are saying that instead of doing binary coding in the machine you were going to have the students be able to do decimal coding so they wouldn't have to go through the conversion.

PM: Well, yes. They would just use decimal numbers throughout.

HT: Right. And the thought of then doing decimal coding and then converting to binary did that seem prohibitive to you in terms of what you would have had to add to the machine?

PM: It would have simplified the machine if you had made it like you do now with binary throughout, but with the decimal I/O. Then you have a more complicated program or perhaps more hardware in

the input-output section in order to make the conversion. But the machine itself, the basic machine, is simpler; and if you are willing to put it in binary, as they did then -- there were, for example, desk calculators made in hexadecimal and octal systems for the convenience of those strange characters who wanted to work in binary numbers. All of those things were tested and many varieties from the excess 3, and binary and 2^{421} codes and so forth, all of those variations were explored.

HT: Did you attend any of the Moore School courses that were given during that latter half of the forties, like the one in '46?

PM: No. I attended most of the conferences of the ACM, IEEE and AIEE. I was involved in the Joint -- I mean the joint part of the Joint Computer Conferences, in making them joint in the first place.

We had a joint branch here, which was a branch of the IRE and the AIEE simultaneously, and also the ACM when it came along.

RM: Wasn't Harry Huskey very much involved with the early part of the joint whatever?

PM: Yes. He was a good friend of mine. And also Professor Lehmer, although he is a mathematician, was

also in this group. Both of them -- well, Lehmer, of course his father had been here and he himself was here before I was. Huskey came later, I guess when I was Chairman. He came to Berkeley as a professor after he left SWAC. I knew him before this and we met frequently. I attended most of the conferences but I never took a course or anything of that kind.

HT: As you were looking at this proposed machine conceptually, what were some of the early developments and people that influenced you the most?

PM: It was guided, you might say, conceptually, by Cunningham's work on solar mechanics, orbit calculations and so forth. He was the center for collection of data on new astronomical objects. People from all over the world would send him postcards and he would calculate the ephemeris, then he would ask one of the larger telescopes to focus on that area and the next night would get a picture of it. So that material came in. For various reasons I don't completely understand, the Julian date has been traditional which involves something like 6,000 numbers. So 10,000 seemed to be an adequate increment about that, so our memory

was fixed at 10,000, not necessarily as the requirement but as a convenient size which seemed to be within the bounds of physical possibilities, I mean, the number of bits you put per inch on the tape and the spacing of the heads that we made and so forth. A combination of practical elements and problems that were certain to be here in quantity once we got the machine running were significant in fixing the characteristics of the machine.

HT: In terms of some of the other machines that were at least conceptually around that time, there was the work that Von Neumann, Eckert and Mauchly had conceived in terms of EDVAC, the first stored program machine; the work going on at Princeton.

PM: Yea.

HT: Might back up even further. Were you familiar with Howard Aiken's work at Harvard --

PM: Yes, I knew about that.

HT: and George Stibitz's work at Bell Labs?

PM: I wasn't too familiar with George Stibitz. I knew him, not personally, but of his work and approximately what it consisted of. I did not

visit Bell Labs or see his machine during that time. I expect I heard him talk on it at different times. I don't remember precisely.

The Aiken machine I was less enamored of because it seemed to me too much brute force. For example, his magnetic drum was still using 10 bits to the inch and 10 tracks to the inch or 100 bits per square inch, when we were getting a 1000. It seemed a factor of ten in size. His drum had a 50 horse power motor; eight horizontal drums, four on each end, made a major installation for a relatively small memory. So I didn't think -- but I did give him a great deal of respect, and I still do, for the fact that he didn't make any compromises with reliability. He knew everything really worked, and it did work, even though it was brute force and everybody made fun of it.

It still was an operating machine which did a lot of work. Probably the wrong work.

RM: [Laughter]

PM: I don't know if anybody ever took his set of tables off the shelf.

RM&HT: [Laughter]

HT: All those volumes of Bessel Functions. [Laughter]

RM: I think he made a major contribution to engineering reliability, although he himself was not really an engineer.

HT: He had experience in power supplies, I think.

PM: Very likely. The work of Mauchly and Eckert at Pennsylvania was most significant. In particular the stored program modification made to the ENIAC while it was still, I think at Pennsylvania, but certainly after it went to Aberdeen. It was done, I think, at the suggestion, of Von Neumann, but largely through the encouragement that Lehmer gave -- he acted as an operating supervisor or something with the ENIAC after it got into operation at Pennsylvania and then, I think, he went with it to Aberdeen. I'm not sure of that.

HT: I've talked with him recently and I can't remember quite that sequence, but I think he did go to Aberdeen for a period.

PM: Cunningham had been at Aberdeen before the war era. He was quite knowledgeable on the use of IBM accounting machines for computing purposes, much more so than the IBM people were. In fact, on numerous occasions I remember IBM salesmen bringing customers

here to get Cunningham to show them how to use IBM machines for their purposes.

HT: Did he talk much about the contacts with Wallace Eckert who was also using IBM equipment for astronomical calculations?

PM: I don't know, but astronomers in general are a pretty close fraternity, so I'd be very surprised if they were not aware. They even leaned toward conferences rather than publications. So they were very likely to know.

HT: Getting back to the CALDIC, I think there are a number of interesting topics that I would like to hear you speak on. One is this pedagogical motivation; the idea of a teaching oriented machine both from the design aspects, and its applications and usages. I guess the other is the impact of that in terms of the people that worked here, the design and use of the machine, and how they affected later developments.

PM: It is rather intricate and I didn't really follow even all the traces that the CALDIC went through through after it left my hands, after it was built. There were, of course, other aspects of its construction as it was here which were for that

purpose. For example, all the panels were exposed on a walk-up board so everything was on the front. Every significant point was within reach of an oscillograph on the face of the machine all exposed. This made it somewhat subject to noise and so forth, so that the engineer's versions of it were condensed and compacted and put in a case. The only real copy of it was made by Consolidated in Pasadena, and they sold a number but I don't know just how many. Shell Development Company bought one of those which was installed at Emeryville. It was the only one I really ever saw. I visited Consolidated, but only rarely.

RM: Is Consolidated still an extant company?

PM: No. ... I don't know just what its corporate history was, but it didn't remain that name very long.

RM: Do you know anybody one might be able to contact for that phase of the development? I've not found anyone yet.

PM: No. I don't know. I think I could look back in my letter file about that time for names for you.

RM: Okay, good. I'll get back to you on that.

HT: Back on the design aspects of the machine. You

conceived this as one where students would be involved in the design and building.

PM: Yes, in fact we never had any professional help at all; it was all done by students, with the exception of the machine work. There was some rather fine and precise machine work done by our electronics and mechanical shops, at that time mostly the mechanical shops.

The building of the magnetic heads was a very important part of that. These were done with number 479 molybdenum permalloy which we got from, I think it was from Indiana Steel. It was made in one mil thickness for magnetic repeaters using telephone and other high frequency transformers. We got them to wind us cores, which were about four inches long of oval shape, which consisted of, I believe, 400 turns of this one mil material. So counting the oxide coating on the surface which insulated one layer from the next, they were only about half-an-inch thick with 400 layers of material. Then we sliced this up and machined out a hole in the center of each core and cut, with the little saws which are used for slitting fountain pen heads, if you can imagine that. They have a

little saw that cuts the two parts of the fountain pen apart. These, as I recall, were only six mils thick. And so we cut the air gap and our heads with those little saws, and we machined them out, and then divided the sets of laminations into stacks of fifty and mounted 200-turn coils on them. Then potted the whole thing in resin for mechanical protection and used these as our recording heads. That's before ferrites were invented so there were no other, no types of recording heads available that even approached these.

RM: So, in this sense you sort of pushed the technology.

PM: Oh, yes. We sold, we made four for the SWAC machine, and also for Illinois, copies of our drum after we got it running, which we shipped them. The one at UCLA was used for some time; I don't know how much use they made of the one at Illinois. But they used it as a back-up memory for the SWAC down there, which had only 256 words of its own. So they re-organized it, so they got -- let's see, what did we have? 50 channels -- we had 200 words per channel, 10,000 made 50 channels times 4 meant 200 heads. We had 200 tracks on the drum which was 24" long, 8" in diameter and rotated at 3600 rpms, so

it had a 16 millisecond cycle time. In that time, ... 200 10-bit words passed the reading heads. The reading heads were staggered in four stacks around the drum; the distance between adjacent tracks on the drum was a tenth of an inch, the track itself was a twentieth of an inch, so that half the space was used for recording and the other half was protection space between.

HT: The distance between the material and the drum face was what?

PM: The drum itself was aluminum sprayed with a ferric oxide coating made by Minnesota Mining. I think that's right. We sprayed it on ourselves, rather clumsy hand work, we polished it with a wood buffer, and then mounted the heads at a spacing of one mil or less from the surface of the drum.

HT: That was pretty close for that period of time.

PM: Yes it was, right. It was all mechanical; there was no floating heads. That was a later development. One of our students, Al Hoagland --

HT: I'm sorry, H-o-a-g-l-a-n-d?

PM: Right. He is a recent past president, if he's not president now, of the IEEE Computer Group and he works for IBM. He went from here to IBM and

developed the magnetic RAMAC; you know the Random Access memory system. That was his work, done with a group, of course. He wrote his thesis with me on magnetic recording ... and he followed that considerably farther. But he was one on the scene with this spray gun work.

RM: [Laugh]

HT: Conceptually, in terms of the overall architecture of the machine, this was your conception?

PM: Well, yes. It -- very largely Cunningham's suggestions. I consulted him almost constantly on what was the most convenient way to do things. And he made -- his calculations, of course, were multiple precision, many times. That is, he had 40 significant digits for which he used Monroe desk calculators. So, many of our thoughts were for the kind of things you could do if you wished to make multiple precision calculations. That was easily done and easily checked with Monroe calculators if you had it in decimal form. All these things influenced the design which was really quite simple. Our storage was only on the drum; we didn't have any fast high-speed, small memory or anything. Only on the drum. The arithmetic, however, was not done

on the drum, but it was done in three registers, two of them had twenty digit double length, and the third one only ten. But these were vacuum tube shifting registers,

HT: Yes, we've read about them in the --

PM: 220 tubes in each one and used gates which some years later were patented by IBM.

HT&RM: [Laughter]

HT: From the standpoint of the design of the machine and building of the various components, what role did students play in this?

PM: We had a group of students from the very beginning. In fact the first proposal names a group of students who were working with me at the time. They were all M.S. students, which meant they could spend as much as approximately half of their Master's Degree year on what was called individual study projects. And each one undertook some particular aspect of the design. One would be working on the adder, the other one on the shifting register, another one working on, for example, these different materials and arrangements of drum coatings and so forth. We started by taking magnetic recording tape, just the kind that is used for tape recorders.

HT: You did have that at that time?

RM: Oh, yes. We wound that in a spiral around a small test drum, the first drum we built, which demonstrated to us that even with a coating that just consisted of tape wrapped around a spiral and stuck with Scotch tape at the ends could record as many as a hundred bits to the inch with no problem. ... So that's what led us to feel that we could make a great reduction in physical size of the memory part. The size of the drum, as you would expect, was fixed by the available aluminum tubing. We could get 8" diameter aluminum tubing which then our machine shop could turn into a very precise cylindrical drum, put heads on it, and mount the motor on the end. We made them vertical so there would not be the sagging problems of a fairly long drum and so forth.

HT: -Also easy maintenance with the vertical position.

PM: And the heads were exposed so we could see them.

HT: Everything essentially that could be exposed was, so the students could literally walk around the machine and see how each of the elements contributed.

PM: Right. They could see anything. Even the drum was enclosed only with lucite panels so you could look

in and see the heads.

RM: In one of the reports you said that you really did not plan to push the state-of-the-art at all. However, in retrospect, what would you say were some of the major contributions this machine made?

PM: I don't think it made all that difference in the technology, except rather conceptually. I thought that the idea of a minimal machine, that is something that could be used and be produced in quantity and widely distributed, even though we did not actually make copies of it, the idea was approached. The great increase of possibilities of the size of storage memories for relatively fast storage, it was slow compared with electronic means, but fast compared with any other punched tape or card handling or anything of that sort, was attractive. Something was done for small problems by having them in decimal form, because it simplified the I/O and made the speed effectively somewhat greater than it would be if it all had to be changed to binary in and out. This also made it possible, although still difficult, to check it by using the oscillograph anywhere in the machine.

However, the crucial weakness of our machine

was that servicing was difficult. That is to say the joints were soldered, there weren't any plug-in cards, and even though the tubes themselves plugged in, it's difficult to find a failing tube out of a collection of two thousand when the average life of a thousand hours meant that a tube burned out every half hour. We got much better service than that, but still it made it difficult to service.

HT: Did you do research here on tube life?

PM: No.

HT: How about the problems and the effective use of the machine once it was built? I guess I should get the date of its building and operation.

PM: Our contract terminated in 1955. We dropped it for various reasons, partly because we felt that we shouldn't pursue this farther because of its poor characteristics from the service standpoint. By this time, Consolidated, among others, offered machines which were more easily serviced. They were better engineered, in the sense of better packaged, smaller, more conveniently mounted, and so forth. Much easier to service. We really never used it in any great volume.

We had a computer laboratory which was started here in about 1953 which consisted originally of an IBM 602A calculator and peripheral punched card machines. The problems which we did use it on were mainly taken from things that were going through that computer laboratory, but there weren't many. Nearly always the problems were just, you might say, demonstration problems to show we could do certain things.

What we did use the machine for, and this was its major contribution as far as I am concerned, was repeated tests of logic circuit designs. Every student who started, or at least many students, took new designs for an adder, for example, and he would design, and draw, and build and test his adder. Then we would install it in the machine. As you probably know, although it doesn't seem as crucial as it did then, it is very difficult to test the circuit to see if it fails only one time out of a million. There are very few schemes that will do it other than a counter. You just let it run all day long, and if it misses even once you have to know it. You need a test bench to test logic circuits and the only test bench we had,

because we didn't have even counters and so forth, and so the only kind of test bench we had was the working machine. And so we would install things on it and just let them run for a while and see if they failed and count the number of failures or whatever. So that was really the major use, just as a laboratory test bench.

HT: Obviously a real teaching device, then.

PM: Right. Not teaching numerical analysis, but teaching logic and circuit design and so forth.

HT: Was the machine operative, am I wrong, about 1951?

PM: Parts of it were operative even before that. ... I would hate to put a date when it was all operative; I don't know if it was ever operative all at the same time.

RM: [Laughter]

PM: We could do any kind of addition and subtraction, and program multiplications and divisions from about 1951 and on till about 1955 or '56. I don't even remember exactly when we tore it down, but I guess it was at the same time we got the Bendix, which came after Huskey joined the faculty. It was the prototype of his Bendix design which was widely sold. The Bendix Company gave him essentially the

first ... I think we paid something like \$45,000 for that prototype machine of the Bendix. Then we had a mechanism for teaching applications rather than design.

RM: Did you, during this period ... I'm sure you did, your lectures on computing used this machine as a tool.

PM: Oh, yes, that was a laboratory --

RM: Do you have any notes and outlines of courses?

PM: I don't know about that, I'll have to look. Well, I have one. For example, catalogue descriptions, this is somewhat later, the first one, I believe, was in the 1952, was in the 1952 catalogue and our courses were 151, starting here. This one and this one and this one, and the corresponding graduate courses are 251 and 2. Those are course descriptions.

HT: I'm going to read these into the tape so they're there.

RM: Why don't you do it.

PM: You can just take it in the office and get it photostated. You can get a copy of this page, 151, and this is a double page, this page here.

HT: And the cover so we have it. [Laugh]

RM: Can you recall some of the people who maybe I should

be talking to who were your students and have gone on and been influential?

PM: Oh, yes. Al Hoagland is one. David Brown who went from here to Lincoln Labs.

HT: I know David Brown. He is in the Bay area at the moment, I think.

PM: I think he's at Stanford Research Institute.

HT: That's right.

PM: Torben Meisling who went there.

HT: So Meisling was teaching one of the courses in that, 151, I think.

PM: Yes. Both of those men were very helpful. Brown especially came here with experience at Lincoln Lab and was a real help in the whole development. He was responsible for setting up the system of drawings, ... drawing files and so forth. He and Torben were, I think, more ... sophisticated in logic or logical algorithms and so forth than I was. Torben I think was especially --

HT: How do you spell that first name?

PM: T-o-r-b-e-n.

HT: Do you know where he is now?

PM: I think he is with Hewlett-Packard. He went to Hewlett-Packard in Germany and was responsible for

a laboratory which was, I believe, in Stuttgart, or somewhere. They set up a branch and they built instruments, not digital particularly. And some of these were built in Germany and sold here. Also they served as an [inaudible]. You should find him, though, in the IEEE Directory and so forth. We won't take time. I'm sure it won't be hard to find. I can find it if you can't. I haven't seen him for quite a long time.

HT: Do you have your course notes from that period?

PM: I don't suppose I kept them.

HT: We might run down some students who did. When did you teach your very first course here at Berkeley?

PM: In this subject? Well, you see the scheme was, and still is, to have what's called individual study courses, and each M.S. or Ph.D. candidate has as a large part of his work this individual project. And those students register under course number EE CS 199, which is just an individual study number. So as early as 1948, before I actually started to build the machine, [copies brought] I had a considerable class. Any time a professor has as many as three or four research students meeting with him, then they meet as a group and have weekly discussions

or something of that sort. So that's the type of classes that started it in about 1947. These group seminars for students interested, and they would have other students who were not actually working with us, but thought they might be interested.

HT: I think we'd very much like to have any correspondence, copies, not the original, but copies of any correspondence relating to the machine, and your arrangement with ONR, and anything that you see that would shed light.

PM: I have quite a little of that kind of thing; some pictures. ...

HT: Pictures we would also like to copy. We can make copies of these and return them to you.

PM: We have pictures, for example, of these magnetic heads, how they were constructed and mounted.

RM: I'll be coming back up into the area and maybe on my next trip we can spend a little bit more time together.

PM: Sure. I hope I won't be so crowded. I really feel rather badly about that.

HT: There are a couple of broad questions one of which I'll throw out quickly since we're running short of time. One is the West Coast environment and

how you saw your work here fitting into what was going on on the West Coast which seemed to have a center of gravity around Los Angeles.

PM: Well, actually I think we were quite significantly responsible for the installation of the IBM Laboratory in San Jose, because it just turns out that students educated in California didn't like to go East. So the market for engineers with any kind of training in this field was very competitive. Students like Louis Stevens, for example, went to IBM; John Haanstra, who came to be a vice president of IBM, he was one of my students in the same group. And Orwell wrote their thesis ... Meisling didn't go to IBM, but there was quite an alumni group down there. There was also a group of them at North American and at Hughes. It was the same kind of thing. And some of those led to quite large enterprises. The RAMAC magnetic memory development at San Jose was a major item in IBM sales for some time, and its successes been yet. I remember very well the development of the floating head down there and some of the mechanical designs we tested in the course of our development.

RM: Did you know Ray Johnson?

PM: Oh, yes.

RM: I believe he had quite a lot to do with that development, too. How about some of the people who went to Hughes and North American? Can you give me some names?

[Unknown voice: Dick Tanaka.]

PM: Let's see. Dan Curtis. Tanaka went to Cal Comp later. He was a little bit later, I think maybe '53 or so, I don't remember just when he got his degree. Actually, he didn't get his Ph.D. here, partly for personal reasons. Let's see, Cameron was one -- oh, I'm sorry, Cameron Forrest, was his last name.

[Unknown voice: Wanlass.]

PM: Yeah, Chris Wanlass, although his later work was not so much with computers. He and a couple of brothers got into a company and they build regulated power supplies and things of that sort.

RM: Do you know where Cameron Forrest is?

PM: No, I don't. I haven't seen him in a long time. I don't keep as close track of students as I --

RM: Another name I have is Art Zukin.

PM: He was a very ingenious walnut-raising farmer.

[Laugh] Came in and I think he went to Hughes

Aircraft with that group.

RM: I believe he is still at Hughes. Would he be a good man to talk to, I wonder?

PM: Yes. For the time he was here he was certainly head over heels in it. He worked night and day, but he wasn't here for very long; a couple of years or so.

RM: And then was active in the Hughes developments.

PM: Yes. Actually the group, about six of them as I recall, went to -- Cameron B. Forrest, Senior Scientist, Litton Industries, Data Systems Division.

HT: We are getting very close to 12 o'clock, and I want to throw one last question at you.

PM: Sure.

HT: And that's a kind of retrospective, philosophical position. As you look back on that period from today, there seem to have been a number of major developments that started when you were starting your thinking and your work here. The CPC was one direction, the large-scale machine that you mentioned earlier, the small-scale machine for local problems that you ended up with here. Do you have any comments from a philosophical point of view, as you look back on the various directions, as you saw the

conflicts that were developing?

PM: Well, no. My major commitment at that time was in opposition to the general feeling that there would be a few, very large computer centers. That was the basis of the Bureau of Standards, that the Western and the Eastern centers would just about cover the country, together with MIT and so forth. The development that I thought most promising, and, of course, was the most interested in because my students were involved in it, was the memory development, the idea of getting very large disk storage like the RAMAC thing. To me, that was a wonderful accomplishment mostly done at IBM, or at least the part that I was familiar with.

The idea of the mini-computer, which I think Bendix got into -- well, I won't take credit for it, but because of competition with Consolidated and so forth. The design that Huskey took -- you see, Huskey came to the SWAC with his own design which he had made in England, and that was a real contribution. It was always one of the very fastest machines, I think. Although it wasn't copied as much, I think it really was as innovative and in many ways as provocative a machine as Von Neumann's.

Von Neumann was a very intuitive mathematician who gave lots of ideas, but as far as the designs, many of his designs were not well engineered and it took people a long time to fix them. But Huskey's design for the SWAC, and his later design for the Bendix G-15, which was quite successful, and his design for the G-20 which was not as widely accepted, I think also were significant.

HT: He still has a G-15 in his garage, he said.

PM: Well, we had a G-15 until last spring.

RM&PM: [Laughter]

RM: Did you?

PM: We didn't use it -- well, we used it for one special purpose. I think it was quite a perceptive design, and it aimed at the goal that I thought was important, namely, that it be widely disseminated so people would see them, use them, get their hands on them, and think what to do with them, and so forth. And those were the things that I thought were the most successful of the ideas that I was familiar with. Not all mine by any means. But it seemed to me the right idea and I pushed them when I could.

HT: I see it is 12 o'clock and we should let you meet with your students. Thank you very, very much.

END OF INTERVIEW

SEE

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