Computer Oral History Collection, 1969-1973, 1977

Interviewee: Harry Huskey and Mrs. Huskey
Interviewer: Bobbi Mapstone
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MAPSTONE:

The date is April 19, 1973, This is Bobbi Mapstone and I'm talking to Professor Huskey and Mrs. Huskey at their home in Santa Cruz, California on April 19, 1973. Perhaps we could start with your joining the Institute of Numerical Analysis, what your position was, and discuss the charter of the Institute and how it influenced computing and people.

HUSKEY:

I think the Charter of the Institute was to foster work in computational mathematics, with the idea that with the new computing machines one could do things that were more interesting, more significant and that you weren't able to do before with desk calculators. There was a research program there, and over the years most of the famous men in analysis have spent time there. For example, Hartree was there for awhile, in fact, he was Director; Barkley Rosser was there; Mark Katz. I guess we have named some of them previously; Rutishauser was there from Switzerland; and from Oregon was there--he was one of the early people in the innovation of differential equations; there was a whole host of names. That indicates the primary function of the Institute.

In order to move that kind of research along and to make sure that it had some relationship to computers, the idea was to have a high-speed computer. Originally it was expected this would be purchased from some vendor, and the Bureau had money from the Air Force to do this sort of thing. However, there was a lot of delay in the development of computers. Eckert and Mauchly were having financial troubles and the Raytheon Corporation was not moving along as fast as people would like, so ultimately the Bureau made the decision to go ahead and build a machine itself.

MAPSTONE:

Who actually was the decision maker?

HUSKEY:

The Mathematics Division at the Bureau under John Curtis carried on all this activity, and…the Machine Development Laboratory, as it was called, under his direction.

When I came back from England I was expecting to spend about six weeks in Washington to become familiar with the Bureau, and then I was supposed to go out to Los Angeles. It ended up that I spent almost a year in Washington, and during that time we had the SEAC started while we were in Sam Alexander's division. When it was coming time to go out to Los Angeles, I found that the decision to go ahead and build this machine on the premises of the Institute of Numerical Analysis had been made by the Mathematics Division with the help of an Advisory Committee of people outside the Bureau. I was placed in charge of that project and went out and started work.

MAPSTONE:

There was quite a lot of difference between SEAC and SWAC.

MAPSTONE:

There was quite a lot of difference between SEAC and SWAC. Were these ideas that you instituted, or did they come from elsewhere?

HUSKEY:

SEAC was all the way through designed to be a serial delay line storage machine based on the development at the Moore School. When it came time to start the project in Los Angeles, we had been watching what had been going on at other places in the country and also in England, and the most promising alternative approach seemed to be the Williams tube storage that was developed in England. The advantage was that you could build a parallel computer at a much greater speed. So the idea was to build these two machines using these different techniques to see which would turn out to be better.

MAPSTONE:

Oh, I see. It really was to explore the field totally.

HUSKEY:

Yes, and to give breadth to the Bureau's activities.

MAPSTONE:

When was SWAC running?

HUSKEY:

It was dedicated in August of 1950.

MAPSTONE:

Was it used primarily by the people at the Institute?

HUSKEY:

By the people at the Institute and some of the local aircraft people. If they had government contracts from the Air Force they could use the computing facilities at the Institute, so it was used for some work of that sort. By 1952 or 1953 most aircraft companies had bought equipment from IBM, and they ended up having much bigger computing establishments than did the Institute.

MAPSTONE:

Yes. Looking back what do you think were the significant contributions made by SWAC and the Institute?

HUSKEY:

Well, I think those are really two quite different questions.

MAPSTONE:

Okay. Let's separate them.

HUSKEY:

As far as the Institute was concerned there was a lot of significant numerical research done with publications coming out of that, and there was a great deal of attention to methods that made it easy to program computers, because in those days we had a limited amount of memory and it became important to develop an algorithm that had as few steps as possible. Papers were published by various people there on methods of that sort.

As far as the computer end of it was concerned, I think it was an interesting design of a parallel computer that was different from some of the other activities. It was closer to what was being done at the Institute for Advanced Study, which was also a parallel computer, but different electronic techniques were used. However, in many respects they were similar.

HUSKEY:

Please enlarge on the differences and the similarities between SWAC and IAS.

MAPSTONE:

I think the significant thing there was that it was a unique design from the point of view of the command structure; it was a so-called 4-address computer and we developed interesting, space saving kinds of programs since memory was so limited. Some other work on programming languages got a start there and we were in some sense fore-fanners of things like FORTRAN, but they never developed far enough to really get to the publication stage. Can you identify any of these methods or give names of people involved? In what…If one had known how things were going to go, one would have put more effort into them at that time and gone further along, but we got interested in some other things and didn't work hard enough on some of these.

MRS. HUSKEY:

I think that one of the most significant things was just showing that it could be done, and especially by a smaller establishment without a great deal of money. At this time, the environment was so different from what it is now. I mean, not many of these machines had really been built or designed. One of the problems at the Bureau was that they didn't even know if one machines would work when the companies built them.

HUSKEY:

Reports from people like Von Neumann saying we had better develop better components before we start building machines didn't help very much.

MRS. HUSKEY:

It was the whole idea that they would go ahead and do this type of thing themselves which, I think, did sort of pave the way, and soon everybody was getting their own machines and the universities and so on were all building their own machines. It was significant in that it was sort of a forerunner, or something like that, on a small scale.

MAPSTONE:

For a little while it was the only large-scale, stored-machine program on the West Coast. BINAC had been delivered to Northrop but according to all accounts it was never fully operational.

HUSKEY:

Yes, I guess so. There was work on developing a computer at the RAND Corporation but I'm not sure just when that became operational. I don't think it was before 1952.

MAPSTONE:

Oh, no. It was quite a bit later.

HUSKEY:

Then I guess the next machines out here were the IBM 701s that were delivered about 1953.

MAPSTONE:

Yes.

MRS. HUSKEY:

Another thing that you partly mentioned is that they got a lot of famous mathematicians and so on thinking about machines. The Institute was really sort of a cross roads where people from America and Europe would come and spend a couple of months or a summer.

MAPSTONE:

In a way the machine was very persuasive by the fact that it was there and mathematicians could get their hands on it.

MRS. HUSKEY:

They could just go in and use it. I mean, it was right there and they didn't go through ...

HUSKEY:

Not much red tape.

MRS. HUSKEY:

Much red tape, that's right. They could just go in and use it themselves like the Lehmers. I'm sure they told you that they would stay there late at night and run the machine themselves. You wouldn't be doing it through your staff because there wasn't any staff; everybody just ran it for themselves. I think it got a lot of people, famous mathematicians and so on, interested in computers and thinking along these lines.

MAPSTONE:

I'm interested in the people who did some early work in programming languages even though they didn't come to fruition. Do you recall some of the names?

HUSKEY:

Well, a person who did some of this is now head of the Computer Science Department of UCLA, this is Malkenoff. He worked up a programming system called SWAPAC which was a starter on this sort of thing. I had been doing some experimental work but it wasn't pushed along far enough. The other significant effort out here on the West Coast had to do with the 701. There were really two developments; the Los Angeles aeronautical groups developed an operating system called PACT, and the other interesting work was done at Boeing on a system which, I think, was called BACAIC.

MAPSTONE:

I don't know about that.

HUSKEY:

It was a programmer language for the 701; an interpretive system to try to make it easier and also to produce good diagnostic information.

MAPSTONE:

Didn't they join in with the PACT group?

HUSKEY:

No. I think the PACT group grew up to where it was a significant contribution, and the Boeing sort of took it over and had both systems, I guess. I didn't know enough about it to be sure of my facts, but from the outside it looks like that is what went on.

MAPSTONE:

Excluding the mathematics, what are some of the other significant jobs that were done on SWAC?

HUSKEY:

You mean computational problems?

MAPSTONE:

Yes.

HUSKEY:

There was lots of computation going on. I have no way of knowing.

MRS. HUSKEY:

Well I'm sure the Lehmers told you about their numbers.

HUSKEY:

They had a real interesting time finding more Hircine primes and Robinson up here at Berkeley wrote the program to compute more primes. I guess the largest one known was 247, or was that a composite? I don't know enough math to remember these things. Anyway, at the time I think the largest known Mercene prime was near two hundred and forty some odd - if that is a prime. Let's hope. The whole question was, were they composite or not. Robinson in the Math Department at Berkeley wrote a program to compute larger Mercene primes, and he wrote this without ever coming down to see the computer. We just sent him the programming instructions, and in due course he sent back a program. We punched it up and it ran. Most people have a long debugging phase where they are trying to figure out what is wrong with the program, but his ran. We started working about 8 o'clock one evening, and it ground through the known Mercene primes so we were feeling pretty good about the fact that it agreed with those. It ran on and about 10:30 it found the next pair - there were two close together around 511-and about 1:00 a.m. it found the third one which is in the area of 1,000. That was a real exciting night. (laughter)

MRS. HUSKEY:

The Lehmers were there since they were the ones that were primarily interested and they stayed up most of the night. They were very excited about it.

I don't know what type of thing Forsyth did, do you?

HUSKEY:

Forsyth was interested in, I guess, square roots and linear equations and so on. There was just a lot of computing done and at this point there's no saying, what part of that was significant - I don't know. It was people exploring things and ideas.

MAPSTONE:

And that is what is significant.

HUSKEY:

There was a lot of publication but how much of that really depended upon the computer I couldn't say. Certainly the computer was an encouragement to the work in these various fields.

MAPSTONE:

How long were you Director?

HUSKEY:

I was never Director, I was Associate Director, and I suppose it must have been two years or so. We started in December 1948 and I was just in charge of the electronic computer part for awhile. We were away for a year's leave in Detroit and on my return I became Associate Director. We left there in September, 1954.

MRS. HUSKEY:

One of the interesting things that they did very early was to consider the possibility of translating languages. We had a German professor interested in it but we got some letters from people higher up in the Bureau that we shouldn't waste time on something that was so utterly impossible to ever be done on a computer. (laughter) It was a very serious letter, a reprimand, that we shouldn't be doing something like that.

HUSKEY:

When was Director he used some of his own discretionary funds to start the project. We got some interest going in it and an interesting paper came out of it called the <u>Mechanical Resolution of German Syntax</u>.

MAPSTONE:

Can you give me an approximate date?

MAPSTONE:

Who was the author?

HUSKEY:

A fellow named Oswald in the Language Department at UCLA.

MAPSTONE:

Would this have been an Institute publication?

HUSKEY:

Individuals published in various journals and this was in one of the language journals, I guess <u>Modern Languages</u> or something like that. I'm not sure which one it was.

MAPSTONE:

That must have been one of the very early, if not the earliest paper, on the subject.

MRS. HUSKEY:

It was at a time when most people didn't think there would ever be any possibility of doing anything like that with the computer.

MAPSTONE:

That is really exciting.

MRS. HUSKEY:

HUSKEY:

interested in the solution of differential equations

MRS. HUSKEY:

Randy called me from Amsterdam. He is a very famous Mathematician

HUSKEY:

MRS. HUSKEY:

It is a little bit difficult to convey the real excitement of this era because Curtis had this Navy Board of some kind, I forget what it was called, breathing down his neck for results and he was very afraid that maybe it wouldn't run.

HUSKEY:

That was the Advisory Committee for the Mathematics Division. Some of the people on it had been instrumental in transferring funds to the Bureau for these activities, so they were all anxious that the thing would work.

MRS. HUSKEY:

They were afraid that it wouldn't work, so the engineering staff would go to work at 5:00 o'clock in the morning and nobody worried about overtime or anything like that. They would meet out at the house and it really was a very exciting time.

MAPSTONE:

I understand that you were quite a task master and that you were there all the time yourself. (laughter)

MRS. HUSKEY:

It was a real good group. I mean they would come out to the house and they would sit around and talk you know about how they were going to get this thing done before anybody expected to get it done and so on.

MAPSTONE:

You were very involved with the building of it weren't you?

HUSKEY:

Oh, yes. Some people thought I had my finger in too much. (laughter)

MAPSTONE:

Harry Larson told me the story of how you and he set the machine on fire. (laughter) Do you remember that?

HUSKEY:

Oh, yes. He was soldering some part of the frame and got the installation ablaze.

MAPSTONE:

So now, just let me check some names on the key people as far as the building was concerned. Harry Larson, of course. Ragnor Thorensen is a name I have. What was his contribution?

HUSKEY:

He joined the project when it was halfway through and he worked some on the drum attachment. Then when I was in Detroit he took charge of the whole project.

MRS. HUSKEY:

But that was after it was completed.

HUSKEY:

That is after the main part of the computer was completed. If the computer was dedicated in August 1950, what kind of development work was still underway during 1952-53?

MAPSTONE:

Would you enlarge on the drum. Who built it? Problems? etc?

MRS. HUSKEY:

It was 1952-1953.

HUSKEY:

David Rutland worked on the control parts of the system, Ambrosio worked on the memory and Ed Lacey on the arithmetic part of the system. Those were really the key people. Of course, there were lots of other people. Bill Gunning was there from the RAND Corporation and he worked with various people, but he was primarily on the memory part of it. There were quite a number of other junior people involved, but those were the main ones.

MAPSTONE:

What about for the programming aspects of it - Malkenoff?

HUSKEY:

Malkenoff was there as a graduate student and he got involved sort of late in the game. There was a MRS. Lipkis that did some work in programming and Everett Yowell who is now with National Cash back in Dayton, I guess.

HUSKEY:

There were a lot of people programming for particular problems and things of that sort, but in terms of general contributions these were the main ones.

MRS. HUSKEY:

I remember a number of girls.

HUSKEY:

Yes.

MAPSTONE:

Good, I'm glad to hear that. (laughter) The computer field was very low in women in those days.

How about problems with that Williams tube memory?

HUSKEY:

Well, it was a problem that is right. The trouble with Williams's tube was that it was really designed for oscilloscopes, and the kind of quality control that we needed wasn't necessary for that purpose. If you bought commercial tubes, some might be good and some might not. The trouble turned out to be little particles of carbon on the inside face of the tube which were probably from lint and things like that which deposit out with the phosphorus. When the tube is evacuated it is heated to drive the last of the gases out, and this heating converts these pieces of lint to carbon. If there is a little string there it conducts electricity and what you are trying to do is just charge a particular spot, either positively or less positively. If a piece of carbon is there it can disperse this charge and you are in trouble. We designed this thing and found out that tubes typically had three or four such flaws, so we designed a very stable power supply system so that we could adjust this pattern so that it wouldn't fall on any of those flaws. After we were almost all completed and had bought all the tubes for our system, we discovered that Dumont, who made these tubes for us, made them in a converted mattress factory. (laughter)

MAPSTONE:

That is lovely. So you had feathers in your tubes. What were some of your other major problems you had to overcome?

HUSKEY:

I think the memory was the most significant problem. With the arithmetic tubes we just used brute force techniques. There was nothing particularly surprising or any particular problems in designing them. We had problems putting it together but once we knew what the problem was, we could solve it because it didn't involved a different technology than what we had been doing for some time, at that time.

MRS. HUSKEY:

They were building most of the parts so they decided to build the cabinets up and down, rather narrow, so they could easily get to the components at the back. In order to shut out the dust and stuff they decided that they should have some doors. Were they really shower doors?

HUSKEY:

The shower door company made them. (laughter)

MRS. HUSKEY:

Curtis always talked about having the computer look like the shower.

HUSKEY:

This was in a wooden building, you know, and we were worried about it burning down, so they decided to install sprinklers in the building. They kept asking us if we wanted the sprinklers in the ceiling or inside the computer! (laughter)

MAPSTONE:

Well, it is not a bad idea, you know. If the computer hadn't of worked at least you would have had a brand new shower design. (laughter)

Many of the people I've talked to said that they never thought the SWAC would work. Then the darn thing worked for ten years or -

HUSKEY:

Yes. It ran for a long time.

MRS. HUSKEY:

Even longer then that, I think.

HUSKEY:

You need to check at UCLA sometime when you are down there. Mike Malkenoff would know.

MRS. HUSKEY:

Also there was the other fellow that kept it running all the time.

HUSKEY:

I'll think of his name in a while.

MRS. HUSKEY:

Fred - Fred.

HUSKEY:

Fred is his name. (laughter)

MRS. HUSKEY:

Hollander.

HUSKEY:

Hollander, that is right.

MRS. HUSKEY:

Have you met him yet?

MAPSTONE:

No.

MRS. HUSKEY:

He was in charge of the SWAC for many years after we left there, and he would know when it stopped running. I know we were surprised to find out at that time that it was still running, and this was many years after we had gone to Berkeley.

HUSKEY:

They actually moved it from the Institute down to the Engineering building where the computer area was, and they used it there for quite a long time.

MAPSTONE:

In part, it still runs, right? I'm referring to those R.E.S.I.S.T.O.R. kids at the FJCC.

HUSKEY:

Yes, they collected some parts together.

MAPSTONE:

You weren't there, were you?

MRS. HUSKEY:

No. No. I wasn't.

MAPSTONE:

We arrived at this little place where these young kids who call themselves the R.E.S.I.S.T.O.R.S. were set up. There was Harry Larson hanging on to a cathode ray tube as if he'd found his lost son. (laughter) These two were just like kids. Some Somewhere along the line you started to think about smaller computers.

HUSKEY:

Yes. The year that I was in Detroit was when I started plans for a small one.

MRS. HUSKEY:

It was the school year 1952-53, I believe.

HUSKEY:

I think that is right. I spent a year on leave at Wayne University in Detroit helping them set up their Computer Center. During that time I drew some plans for a small computer which was based on some of the work done by Turing much earlier.

MAPSTONE:

Was this Turing's work on Pilot Ace, or does it refer to something else?

HUSKEY:

In fact, when I was in England, I worked out a one-page design for a real simple computer. Do you have this? I never did any more to it until I took a year off in Detroit. Getting out of the rat race at UCLA gave me time to think about it, you see. I worked out the design and it was ultimately sold to Bendix who manufactured it as the G-15.

HUSKEY:

Do you have a date?

MAPSTONE:

Just to go back a little, did the work that you did in England with Turing affect the SWAC in any way?

HUSKEY:

No. SWAC used the Williams tube which was under development by F. C. Williams at Manchester during the year that I was in England. I went out and visited him a number of times, however, he was using it in a different way. He read information serially across the face of the tube, so logically his design was more like the Moore School design and the

SEAC design. However, once you think about using it that way, you can think about having thirty-six tubes all working in parallel so that you can go to any particular place; in other words you have random access. That was added to the design that we built at UCLA. Of course, that made a much faster computer, and we were able to do a complete addition with two numbers and put the answer back in the memory in sixty four micro-seconds. This was with the sixteen micro-second cycle memory. That was real fast stuff in those days, (laughter) although it's not very fast today.

MAPSTONE:

In the meantime, though, you were involved with a machine at Wayne.

HUSKEY:

I was involved in setting up the computer lab, in teaching an academic program, and in acquiring a computer from the Burroughs Corporation in Philadelphia. There wasn't any construction activity at all.

MRS. HUSKEY:

He was working with Dr. Jacobson who was in charge.

HUSKEY:

Joe Weisenbaum was there as a student, you see.

MAPSTONE:

That is right. Now, did you do some work on a programming language?

HUSKEY:

Well, I guess we talked about the things that I was doing at Los Angeles. I had more or less forgotten about that.

MRS. HUSKEY:

That came much later though didn't it?

MAPSTONE:

INTERCOM. Was that later?

HUSKEY:

INTERCOM really followed through a little later. It was a programming language for the Bendix system that was done in Berkeley after they had got me a computer up there.

MAPSTONE:

That is right, I'm getting confused now.

While you were at Wayne you started to come up with the concept of what became the G-15. Can you be specific on some of the ideas that you had come up with in England and that you later used in the ultimate G-15?

HUSKEY:

The idea was keep it as simple as possible because vacuum tubes were expensive both in terms of initial construction because of all the components, and also in terms of maintenance because they were the one component in the system that tended to stop performing. The idea was to design a system that had a minimum number of such components in it. So the design was based on a magnetic drum but used in a mode of re-circulating information. The information was read off the drum at one point and recorded back on in another place, and since these signals were much stronger than the mercury delay lines you could do this with a lot fewer tubes than you could in the mercury delay system. It was a little slower because you were limited by the mechanical rotation speed possible on such a drum.

The real difference between the G-15 design and the kind of design developed in the SEAC or in the Moore School EDVAC, was that the logic was all dispersed through the memory. In SEAC and EDVAC you had a control unit with an arithmetic unit, and you brought stuff out of memory, processed it and put it back in memory. In the G-15 design, and this is true in some of the early work that Turing did on what was called Pilot Ace, for example, the logic was spread along in the memory so that you would have logical functions of and or between lines of memory. If you wanted to get on and of a couple of variables, you just looked at a certain place in memory rather than having to bring it out and do the process. This made it possible to

HUSKEY:

Do you have any material on your early work on the G-15? Drawings, proposals, descriptions, patents, etc? Also, do you have any G-15 photos? process long strings of numbers all with one command, which is another feature of the G-15 that was different from the other American designs. For example, you could take two vectors each with a hundred numbers, add them together with one command and put the answer back someplace else in memory. This is in contrast to what was possible with the EDVAC-type design, where the machine would bring each operand into the arithmetic unit operate on it, put it back, then step along and get the next one and so on. Here the sequencing was really tied in with the time sequence of a rotational drum.

MAPSTONE:

Were there any other differences?

HUSKEY:

That characterizes the main difference. Otherwise we used diodes and vacuum tubes in a more or less conventional way.

MAPSTONE:

Did you actually go to Bendix and work on the machine?

HUSKEY:

Not to the extent that I did with the SWAC, in terms of making the circuitry and using the oscilloscope to see why it didn't work and so on. But I participated pretty regularly in consultation periods where we discussed what to do about this and that and so on.

MAPSTONE:

Who were some of the key people on the Bendix?

HUSKEY:

I guess David Evans, who is now at Utah, was the main person. Maurice Horrel was Manager of the Computer Division of Bendix. He is president of a company back in Washington D.C. or Maryland.

MAPSTONE:

Do you happen to know the name?

HUSKEY:

(Inaudible) It is something like that.

MAPSTONE:

Do you have his address or could you get it later? Yes, please.

HUSKEY:

I can get it for you, yes. He was President or General Manager, whatever they called it, of the Computer Division of Bendix in Los Angeles. He placed David Evans in charge of the G-15 computer development project and he had a team of people working on it, of course. As you mentioned, it was some of that group of people that quit and went to Packard Bell to build the Packard Bell computer.

MAPSTONE:

That is right. I believe that Bob Beck probably was the logic designer.

HUSKEY:

Yes. He worked on this logical design and then went along with group that went to Packard Bell. Have you talked to him?

MAPSTONE:

Unfortunately, no.

HUSKEY:

The last I heard he was a farmer in Colorado somewhere.

MAPSTONE:

That is right. He also comes to L.A. but he is a little bit of a recluse and he doesn't want to be interviewed. (laughter)

Did Bendix go on building machines based on the G-15, and, therefore, did it influence their…

HUSKEY:

Well, not really. They built the G-15 and they worked on some related designs. They had already built a digital differential analyzer, and they worked on the idea of combining the design of these two, but they never really pushed it through to a marketable project. They got started on what was called the G-20 which was an entirely different sort of machine; it was much more sophisticated and had fancy addressing capability and so on. At this same time core technology was becoming better established and transistors came in so they went away entirely from magnetic drums and vacuum tubes.

MAPSTONE:

That was really the generation change. Someone said to me that they thought the G-15 was both a DDA and GP, but that is not true.

HUSKEY:

No. They had plans for a combination but none was actually made.

MRS. HUSKEY:

The G-15 was quite a successful machine, and when Bendix went out of business it kept on being rented for a number of years.

MAPSTONE:

The G-15 and the LGP-30 were really the two machines that put ...

HUSKEY:

Two competitors, right.

MAPSTONE:

... the small machine into the market place. Did you plan it? Was part of your aim to reach the small person market?

HUSKEY:

Yes. I was shooting for twenty five thousand dollars but Bendix sold it for fifty. I thought that was too much. In fact, when I worked out this design I went over to Librascope and talked to those people about it, as well as to Bendix and to some other people. The Librascope and Bendix offers were the most attractive from my point of view, and it ended up going with Bendix.

MAPSTONE:

Librascope hadn't yet picked up Frankel's design.

HUSKEY:

No they hadn't. I don't know whether they had talked to him or not, but they made me an offer.

MAPSTONE:

Was there much difference between the LGP-30 and the Bendix G-15?

HUSKEY:

There was quite a lot. The LGP-30 was more like the other American designs than the G-15. It had less of this dispersion of functions of the memory that I talked about..

HUSKEY:

Do you know when G-15 was complete? R.M.

MRS. HUSKEY:

We had a G-15 at out home in Berkeley and that is where Joe Wiezenbaum worked on the INTERCOM. Harry was to work out the software for him.

MAPSTONE:

This was the software package that actually went out with the Bendix G-15.

HUSKEY:

Yes.

MAPSTONE:

Would have this been an assembler type language?

HUSKEY:

No. It was a floating point interpreter and it allowed people to do scientific computation relatively easily.

You were asking me what different features the G-15 brought in; one of its interesting features was that it had pretty well developed buffered input-output, all in the hardware. You could initiate an input transfer or an output transfer and this would go on while you did computing in the rest of the computer. The control unit didn't have to handle every character that went out, you see, but it was on a level with the input-output part of the machine.

MAPSTONE:

That was one of the problems with the early machines?

HUSKEY:

That was a problem with the early machines, right.

MAPSTONE:

Joe Wiezenbaum and yourself were the co-authors of INTERCOM?

HUSKEY:

Actually there was some contribution by a couple of people in Texas who had bought a G-15 and put some development into it. Joe and I took their results and what we had and put together a package that Bendix marketed with the machines.

MAPSTONE:

Do you remember who the people were in Texas?

HUSKEY:

Mmmmm, some oil company.

MAPSTONE:

Some oil company. (laughter) That is a pretty good guess for Texas.

So really you were a consultant to Bendix during this period. I have not been able to follow the Bendix developments very well. Can you refer me to someone on the West Coast who might be able to help me?

HUSKEY:

Well David Evans would be the best person but he is at the University of Utah. Max Palevsky in Los Angeles was the person who really made hay out of all this. He led this group that went to Packard Bell and so on, and you might get his story.

MAPSTONE:

I have that story; (laughter) It is a great success story.

MRS. HUSKEY:

It was rather sad that Bendix went out of business. It was an instance of a successful machine not being able to make it.

MAPSTONE:

What one starts to realize as we do these interviews is that it was not just the machines, it was also the management behind them.

MRS. HUSKEY:

Right. You have to have support.

HUSKEY:

What frightened off Bendix was the financial support that it required, and they decided they weren't big enough to stay with it.

MRS. HUSKEY:

The computer group wanted to keep on since they had no doubt that they would be able to make it. However, the higher-up management in Bendix decided that there was not quick enough a return on their money.

MAPSTONE:

That is right, this really was the state-of-the-industry. As these companies bought up computers, they had no understanding of the amount of money it would cost to support a computer.

MRS. HUSKEY:

Or how long it would take to get their money back.

MAPSTONE:

I suspect that this is the reason why so many of the companies fell.

MRS. HUSKEY:

Bendix had gone on and built a G-20 which was a larger computer and which was quite successful.

MAPSTONE:

When did Bendix fold? Was it after they built the G-20?

HUSKEY:

Yes, I think eight or ten of those then were out in the customers hands by the time they sold out.

MAPSTONE:

Did you have any later relations with Max Palevsky and his various companies?

HUSKEY:

No, not really later. I published the handbook on computing and he wrote the chapter on DDAs, but that was while he was still working for Bendix. After that we had an indirect relationship in that I was in charge of a project for Berkeley to develop a time-sharing system which started with an SDS-930 which we modified. SDS marketed this but the University didn't benefit from that particularly, unfortunately.

MAPSTONE:

Professor Lehmer was saying that Berkeley wasn't terribly forward thinking in the computer science area.

HUSKEY:

They should have cashed in on that development because Max made enough from that. (laughter)

MAPSTONE:

How about any relationship, or cross-relationship between the Packard Bell PB-250 and your machine, the G-15?

HUSKEY:

I really never looked at the logic very closely, but from what I've heard it seemed to have some similarities. Since the people who made it had all worked on the G-15, it is not surprising.

MAPSTONE:

That is really what it is, was like on the West Coast, wasn't it? The people moved and took the ideas with them.

HUSKEY:

That's right.

MAPSTONE:

After your involvement on Bendix, there were other machines that you were involved in - Electrodata for one.

HUSKEY:

The relationship with Electrodata took place while I was still at INA. I commuted over to Pasadena and lectured to their people about once a week, and that was pretty much all my involvement with that. They used some of the ideas, I suppose, in their computer, but I didn't do any designing for them or anything of that sort.

MAPSTONE:

I believe they used the B-box.

HUSKEY:

Yes, they had an index register. The term B-box really comes from Manchester.

MAPSTONE:

Yes, Now was Ernst Selmer working?

HUSKEY:

Yes. He was at Berkeley and worked on Paul Morton's project. Have you talked to Paul Morton?

MAPSTONE:

Yes.

HUSKEY:

Then he went and worked with the ElectroData people too. He worked on the design of the Datatron 205, I believe that is what they called it. It was a predecessor to the 200 or whatever.

MAPSTONE:

I've so many numbers in my head these days that I forget them, but it was the Datatron.

HUSKEY:

That is right.

MAPSTONE:

You were not involved?

HUSKEY:

No, except in this indirect, educational sort of way.

MAPSTONE:

Did you have involvement with any other West Coast machines, either indirectly or directly?

HUSKEY:

No direct involvement. I've had some consulting jobs that involved surveying things of that sort, and I guess that is the Floyd Steele incidence that you talked about. Convair asked me down to look at what they were doing and to advise them whether or not to buy in. So I visited Steele's activity in San Diego.

MAPSTONE:

He was building a DDA?

HUSKEY:

Yes.

MAPSTONE:

Was Convair interested in actually acquiring the device?

HUSKEY:

They were trying to decide whether they wanted to make DDAs or not.

MAPSTONE:

And what was your advice to them?

HUSKEY:

Oh, I wrote them a great, long (laughter) report on this is good and that is good and here you are taking a risk and so on.

HUSKEY:

Do you have a copy of this report? It is interesting. Floyd Steele is certainly a very bright fellow. He had a fairly tight operation in that he was short on funds, so he was trying to get enough stuff together to convince people he could really make it, and he needed a lot of resources to do this. The real question at that time was, what were you buying? Northrop had already developed the first MADDIDA and undoubtedly held basic patents on the thing. Whether any of these new things that Steele had going in his machine were significant or not was the question. I didn't have any experience with patents in those days so I knew even less what to say about that.

MAPSTONE:

What action, if any, did CONVAIR take in this regard?

MAPSTONE:

How about involvements with other machines in your work as a consultant" You must have had a good feel for some of the machines that were on the market?

HUSKEY:

Well, some of them. I guess not really a lot of involvement with respect to other machines except, you know, in the way that I created the project at Berkeley for example. I didn't have anything to do with anybody else's computer design. Of course, I'm still doing that kind of stuff now.

MRS. HUSKEY:

When we were in England with Wilkes you worked primarily on programming languages.

HUSKEY:

I worked in programming languages trying to develop something of interest. In fact, I worked on what you might call an early version of ALGOL - before it became called ALGOL - when it was still called an International Algebraic. That was done at the Naval Electronics Laboratory in San Diego. Then I started work on a system that ultimately came to be called NELIAC and it was used quite a bit for awhile.

MAPSTONE:

When was this?

HUSKEY:

That started about 1957 I suppose.

MRS. HUSKEY:

Maybe earlier.

HUSKEY:

The earlier work that I mentioned at UCLA. Are you referring to SWAPAC and Malkenoff's work? It was the beginning of this, so the idea had been worked on for some time. The actual effort at San Diego was about 1957, maybe 1958. ALGOL was published in 1958 so the work had preceded that.

MAPSTONE:

Did it influence ALGOL?

HUSKEY:

Oh, I don't know to what extent it did. A lot of the people who worked with ALGOL weren't in touch with this part of the work.

MAPSTONE:

Please explain the work you were doing?

HUSKEY:

It is hard to say. In another sense perhaps. We spent the summer of 1959 in Amsterdam and the fall at Cambridge, and during the summer I was busy trying to get this stuff going on the computer they had made; it was called the X-1. Anyway, I was busy trying to get that made and Winegarten, who was running the computer part of it, had invited me to come and spend my summer there. Ultimately he became the main wheel in the new ALGOL 1969, so I can take the credit for getting him interested in it.

MAPSTONE:

Now this language - NELIAC,Is there a genealogical relationship between the UCLA language, your ALGOL work and NELIAC?

HUSKEY:

Yes.

MAPSTONE:

Was this for any specific machine?

HUSKEY:

No. It was a language really designed for the command and control kind of environment and not for scientific computing. For example, most NELIACs didn't have any provision for floating point packages. You would use it if you were interested in writing a program that would allow you to analyze information and make decisions in a military environment, such as the problems that the Navy was concerned with. Of course, they were in competition with the Air Force who was supporting SDC with their JOVIALs. If you go back and read <u>Datamation</u> you will find that it will say one is bad and the other one is good, and so on.

MAPSTONE:

There was an awful lot of room for everybody to try to bring it all together. Some efforts were made and ALGOL was one example, wasn't it?

HUSKEY:

Yes, the idea was to standardize it into a single language. In that respect now ALGOL was a real success.

MAPSTONE:

What is the conception story of ALGOL? From whence did it emerge?

HUSKEY:

I don't really know the very initial people that were involved. In this country Perlis, who is now at Yale, was one of the early people that pushed it. In Germany, Samuelson - somebody else ...

MRS. HUSKEY:

Walton?

HUSKEY:

I don't recall at this time? There was the German Computer Society then in this country Perlis got involved as an ACM kind of activity, and them committees were set up and there was a lot of communication and effort to arrive at a definition of a language that would be acceptable to both groups of people. In this respect, I think it has been very successful as a well-designed language from a logical point of view, a powerful language

in terms of being able to express certain kinds of algorithms but less successful as a practical computing language because it was complicated.

MAPSTONE:

Did you ever get into any of the more data-processing oriented languages?

HUSKEY:

No. Not much. I never did anything in COBAL for example.

MAPSTONE:

I want to go back and ask you about several East-West type of controversies in the computer field. One, of course, is the Boolean algebra design of logic as opposed to the block diagram of the East Coast. Do you have any strong feelings on the issue?

HUSKEY:

I guess I don't. I think it is whichever way a person learns to do it first. Out here on the West Coast it was probably taught at Berkeley.

Actually, Professor Morton's machine. The Caldic, was designed using the block diagram approach and a whole lot of people went off to the aircraft companies and kept the habit. I think there is a difference among individuals. Some people can visualize things more easily, and some people like the formalism. I think, what is best for one person may not be best for another. It is sort of interesting that there were so many people who all agreed on one way of doing it out here, and on a different way of doing it back East. Part of this might have to do with the kinds of circuitry used, or course. The SEAC was quite well thought of and that led to certain kinds of techniques you see. That may be a factor. I think much of the West Coast effort started at Northrop with the MADDIDA effort, and that may have been a factor. I don't know. I don't think it is a very significant question.

MAPSTONE:

It is just one of the East-West controversies.

HUSKEY:

If they hadn't had that to fuss about they would have had something else. (laughter)

MAPSTONE:

That is right. Then you've got floating point and fixed point. I guess that one still goes on.

HUSKEY:

Well, not so much in the choice between those two. But there is a lot of discussion of how you do floating point, particularly when you do round off. Typically IBM has not done it the way people think it should be done, for example.

MAPSTONE:

Which is an awful lot of the market, isn't it? (laughter)

HUSKEY:

That's right.

MAPSTONE:

Somewhere along in this time period, you went back to England.

HUSKEY:

1959.

MAPSTONE:

What did you do when you were there.

HUSKEY:

I spent the summer in Amsterdam working on the programming system, and I went to England and did the same thing at Cambridge and got Wilkes interested in working on getting the NELIAC language going in their computer.

MAPSTONE:

Wilkes up to that time hadn't been too involved?

HUSKEY:

I think he was not - he appeared to not have thought there was much in it at that point. (laughter) I don't know whether that is a fair statement or not. The Mathematics Center at Amsterdam is where we spent the summer.

MRS. HUSKEY:

Our acquaintance with a lot of these people seemed to stem back to the Institute. We met Winegarten there. Of course, we met Wilkes in England.

HUSKEY:

We first met Winegarten in Montreal (?)

MAPSTONE:

It was a small enough community that people could keep in touch.

HUSKEY:

Yes. You knew everybody that was working back in those days.

MAPSTONE:

You could influence each other and contact each other.

HUSKEY:

We all had so much difficulty we were embarrassed to talk to one another. (laughter)

MAPSTONE:

That makes me think about the kinds of organizations that were being set up for just that purpose, talking to each other. Were you involved in any of those?

HUSKEY:

Through the IEEE, or what was IRE at that time--Institute of Radio Engineers--we started a computer group in Los Angeles and this ultimately became a professional group. A group started about the same time back East, mostly around the Boston area, so that group and the Los Angeles group got together and started a professional group on computers. This outfit survived and is now known as the Computer Society and is part of IEEE.

MAPSTONE:

Who were the people involved in the beginning of the West Coast group?

HUSKEY:

People like Harry Larson and myself started the thing in Los Angeles.

MRS. HUSKEY:

Somebody else came and talked to you about it.

HUSKEY:

The people that ran the IRE section down there were really the people who started it and ...

MRS. HUSKEY:

They came around and asked if he would be interested in starting it.

HUSKEY:

They weren't computer types particularly, but they were in charge of the IRE Los Angeles section and they encouraged us and so we got going.

MAPSTONE:

Do you have a date?

MRS. HUSKEY:

They came to the Institute and wanted to know it Harry and someone would start a chapter for computers to fit in with their IRE section there.

HUSKEY:

So we did it. And it took off.

MAPSTONE:

Were there proceedings from the beginning?

HUSKEY:

We had meetings at Los Angeles for which there were proceedings and the joint computer conferences grew out of that thing. I don't recall when the first of those occurred, but it was three or four years later, I think.

MRS. HUSKEY:

First they were just sort of local chapter meetings for people in our area.

MAPSTONE:

There must have been a tremendous need for people to get together.

HUSKEY:

There were a lot of reasons for getting together and the first meetings held out at UCLA had thirty people attending. I'm not sure you would do much better now because people wouldn't care so much about going.

MAPSTONE:

Were you involved with any of the other organizations such as the DCA?

HUSKEY:

I had nothing to do with any of those. The ACM…

MAPSTONE:

Your comments on the value of ACM and the role it served in the computer world from 1948-60 would be appreciated?

HUSKEY:

I was organized in the East, in the Washington area, and that happened about 1958, I guess, because when I came back from England it was already underway. I joined it then but I didn't have anything to do with the founding of it. Laizer was President and I was Vice President.

MAPSTONE:

When were you President?

HUSKEY:

1960 to 1962, I believe. I was Vice President for two years and President for two years. It was about 1960.

MAPSTONE:

On the last tape did Hank ask you whether you knew Von Neumann well? Did you get into that?

HUSKEY:

I don't know just what he asked. I met Von Neumann numerous times and talked to him but I didn't know him really well.

MAPSTONE:

One aspect of our project is to draw a picture of Von Neumann through other people. Do you have any anecdotal experiences that involve yourself and Von Neumann?

HUSKEY:

Not really anecdotal experiences. I guess my position in regard to this particular question is pretty well spelled out in my review of Goldstine's book to be published in <u>Science</u> fairly shortly. I think he gets a little bit too much credit for being <u>the</u> guy, and I guess I can quote from my review that "If he wouldn't have been there, somebody else would have done it. That's not to say that he was exceedingly quick and brilliant. He left the rest of us sitting like spectators at a race when he started this thing." I think that sort of characterizes my feeling about him.

MAPSTONE:

Good. I'll look forward to reading your review. Why don't I turn off this tape a minute.

Why don't we follow your careers in a somewhat chronological fashion. When you came back from England where were you at this point?

HUSKEY:

The first time.

MAPSTONE:

No, the second time.

HUSKEY:

In 1959 we were at Berkeley. We were at Berkeley from 1954 until 1967 when we moved down here.

MAPSTONE:

Were you taking sabbaticals to these other places.

HUSKEY:

That is right, and when we came back from Cambridge we just came back to Berkeley.

MAPSTONE:

What was happening then?

HUSKEY:

We were just teaching about this sort of thing. Not a great deal of developmental work was going on at the University at that time. There were people doing projects and things of that sort, but on a small scale. I think probably Lehmer was doing as much as anybody with his number sieve, but there was no formal push.

MRS. HUSKEY:

Was his machine running then?

HUSKEY:

His machine had been discontinued in 1960. The 7090 or 7094, I'm not quite sure which came after that. In 1962 when I became temporarily the Director of the Computer Center because they fired the previous one, I was involved immediately in the campus computing problems. One of the things we were interested in was time-sharing and there was a substantial effort to try to get some kind of a thing going in this direction as a service to the Berkeley campus. In that position, I managed to get funds from ARPA to start a time-sharing development. This occurred in 1962.

I got David Evans to come to Berkeley and join the staff and he carried on all this activity when I went to India for the first year. As far as the time sharing was concerned, when I came back from India we pretty much had the SDS 940 developed and had it running with one of their terminals in the Electrical Engineering Department. It was about this time that SDS decided that it was a good time to manufacture this version of their computer. They went ahead and sold them and used the software that the people at Berkeley had developed,' which was a good thing from their point of view.

MAPSTONE:

When you started to think about time sharing, had preliminary work been done in the field, or was this a brand new idea?

HUSKEY:

Not a whole lot of work. The whole problem was one that everybody recognized, namely the current method of batch process systems. To make the machine efficient you had a long line of problems waiting to be done, and when they were done you had a long line of

work waiting to be printed because that was a different device. You would keep all this efficient by making sure that there were plenty of jobs waiting to be done. However, from the point of the individual that means poor service, because between the time he submits a job it has got to go through this first line and get computed, through a second line to be printed and so on, and then he's got to come back and get it. So the best you can do is two, three or four hours turn-around time, which means that the user has lost continuity in the work and the project and so on. Everybody knew what the difficulty was. The question was, how could you do something about it?

It was about this time that people at MIT and at Berkeley had begun to think in terms of using very low-priced terminals, like teletypes, and then letting each person do his thing and have the computer go around from one to the other doing a little bit of computing for each one in sequence. Of course, there was no real reason why the hardware couldn't have done it five years earlier if somebody had worked out that kind of a design. It was really that people just got to the point where they couldn't stand the problems of the batch processing turn around. Also, it was the first time that we had really adequate computing power. Prior to that there was difficulty having big machines and making them work reliably. They would go down quite often, error free time was quite short, and so it was along about this time that we were able to build hardware that would run for a long period of time without failure. That made it more reasonable to think about this other kind of service, you see. I think for various reasons of that sort that the time was right to develop time sharing. We worked on it at Berkeley and they worked on it at places like MIT.

MAPSTONE:

What you are saying is that no one institution gets the total credit for the breakthrough?

HUSKEY:

I think not.

MAPSTONE:

It was going on simultaneously.

HUSKEY:

I think that is true. Of course, all of these people talked to one another, you know, For example, one of the people on the project at Berkeley is Peter Deutch who's father is a professor at MIT, so everybody knows what everybody else is doing.

MAPSTONE:

Did MIT pursue it with different machines?

HUSKEY:

Yes, a much bigger machine. The prototype work was done on IBM equipment and we used the SDS, or course.

MAPSTONE:

Then IBM geared up to get into time sharing?

HUSKEY:

No. Ultimately they made a machine called the IBM-67 which was supposed to do this. They had a few problems but it finally worked. Monterey has one down there.

MAPSTONE:

What do you have here?

HUSKEY:

Just a 360-40 with no time sharing on it. We have a separate computer for the time sharing made by Digital Equipment Company, which is a much more economical approach to the thing.

MAPSTONE:

Did you or other people publish on the time sharing work?

HUSKEY:

There were quite a number of papers published at Joint Computer Conferences, but I didn't publish anything directly.

Some of the people quit Berkeley and set up their own company which later folded.

MAPSTONE:

Oh, tell me about this. I don't know of it.

HUSKEY:

We came to Santa Cruz in 1967 and pretty much at this same time, a group of people who had worked on the system at Berkeley set up their own private company in Berkeley and proposed to build a new machine. Well, they had financial…or Hawaii and a couple of

the people went with it. The other people went down to NASA Aymes (?sp) over here, where they worked on the ILLIAC machine that the University of Illinois had trouble getting build. People move around in this industry.

MAPSTONE:

What was the name of the company?

MRS. HUSKEY:

I think it was called the Berkeley Computer Company.

HUSKEY:

I think that is right.

MAPSTONE:

How about names of some of the key people.

HUSKEY:

The key people were Melvin Purdle who is now at NASA Aymes, (?sp) and there is Lichtenburger who is at the University of Hawaii, and there were a couple of people in software--Bartley Lamson who is down at SDS in the Palo Alto area someplace. I guess that is ...

MRS. HUSKEY:

Was Peter Deutch involved in it?

HUSKEY:

I don't think Deutch ever went with them.

MAPSTONE:

Who really they set themselves up in direct competition to SDS?

HUSKEY:

Yes. Except they didn't last.

MAPSTONE:

They didn't have a Max Palevsky at the helm.

MRS. HUSKEY:

Weren't they trying to build a bigger machine too?

HUSKEY:

I think they took on too big a machine for the resources they had. Technically, they were able to do it, but they just didn't have the financial backing of people who were willing to give them a long enough time before they asked for return on their investment.

MAPSTONE:

You mentioned some work where you went to other countries and helped them set up in the computer area.

HUSKEY:

Yes. My first experience was in India

MAPSTONE:

Date please?

HUSKEY:

There was a nine university consortium for setting up a technical MIT or Cal Tech. type school in India and it was set up in Quampor, (sp?) which is sort of in North Central India on the Ganges River. We arrived over there eight days ahead of the computer and we proceeded to set up. That was an interesting time. The building wasn't ready yet' it was monsoon season and it rained quite a bit. We shipped the computer over in a DC-7 and the plane got flagged down in Saudi Arabia for reasons we never did find out. When it arrived in Delhi we didn't have a manifest for the cargo and the Customs Officer was upset. Ultimately he said, "Go down and unload it and tell me what you find." The plane couldn't land at the commercial airport because it wasn't big enough, so we had to land on a nearby military airport. There are tax gates on the highway in India which tax every truck that goes through, so we decided we didn't want to pay tax because that would be hard to justify on vouchers back in the U.S. AIDS. We borrowed some military trucks, loaded the computer and went steaming down the highway. The old man at the gate looked aghast as we went by.

We had great fun making that machine work. It turned out that we had forgotten to deliver a converter to convert the fifty cycle power to sixty cycle power. There we were with all the pieces of this machine and no power to run the central processor.

MAPSTONE:

I love those stories. They really bring the computer down to size, especially for people who are afraid of them. After all you can't plug it in the wall, it is no use. (laughter)

MRS. HUSKEY:

You left out the best part of the story. When the truck got to the IIT building, they couldn't get close enough to it because the man that was driving this big truck didn't know how to back it up. Harry wanted to get in and back it out, but he wouldn't let him because he thought that he wouldn't know how to do it either. So they couldn't get close to the building. But in India there is a lot of manpower and eventually they got ...

HUSKEY:

... about thirty people to push the truck. (laughter)

MRS. HUSKEY:

... and they carried the computer the rest of the way. The Indian Director of IIT even lent a hand to help carry it in.

MAPSTONE:

Lovely. No union problems there. (laughter)

MRS. HUSKEY:

He said he saw Harry take a hold of it so he thought he had to too.

MAPSTONE:

What was the computer?

HUSKEY:

This was an IBM-1620.

MAPSTONE:

Was the center set up for just the University of was it government as well?

HUSKEY:

Primarily the University, but not only did they give training for the University people, they also gave short term courses for people all over India, particularly in the government. In fact, the last time we were there, a couple of years ago, they were giving the twenty-fifth of such courses that we had started.

MAPSTONE:

This would be programming courses?

HUSKEY:

Yes. Primarily in FORTRAN.

MAPSTONE:

How about training computer operators and technicians?

HUSKEY:

They did all that too.

MRS. HUSKEY:

It has been very successful, one of the outstanding ones in India, I guess.

HUSKEY:

Yes. That has been a very good operation.

MRS. HUSKEY:

We went back two years ago and spent three months there and it was a pleasure to see something running well and to feel like you are not needed any more.

MAPSTONE:

Oh yes, isn't that nice. It's like having children and then having them grow up. So now they are autonomous in the sense that they are trained and self-sufficient. Are they getting into things like writing their own languages for their machines?

HUSKEY:

No. They haven't done much in that direction yet, but they are designing their own computers and beginning to construct them. I suspect they will get around to messing with languages sooner or later.

MRS. HUSKEY:

The head of it has written several books.

HUSKEY:

Yes. He has written books and published in the area.

MAPSTONE:

What is his name?

Two years ago we spent six months in Delhi helping the University of Delhi set up its computer center.

MAPSTONE:

Which American agency is something like this done through?

HUSKEY:

The Quampor activity was with U.S.AID a foreign aid program, the Delhi activity was with the Ford Foundation, and a few weeks ago I was in Burma and that is a UNESCO supported contract with the University. We spent two months in Burma this summer working on it.

MAPSTONE:

People weren't kidding when they said to me, "You mean Harry Huskey is in Santa Cruz! What is he doing there?" (laughter) I said, "Well, I caught him between plane flights."

MRS. HUSKEY:

Yes. Burma got their first computer in March and so he went over to help them do the acceptance tests. Now the University of California-Santa Cruz has signed a four-year contract with UNESCO to supply technical aid to this computing center to get Burma started in their computer industry.

MAPSTONE:

That is really exciting isn't it. Have you worked with any other countries?

MRS. HUSKEY:

We spent a summer in Santiago, Chile, but that had already been started.

HUSKEY:

I've been on a National Academy of Sciences Panel that has been advising Brazil on their computer science program. I've lectured in Lagos, Nigeria.

MAPSTONE:

This makes me think about how Computer Science has evolved as something that is taught on the campus and finally became its own Department. Were you involved in setting up structured courses or did it just evolve?

HUSKEY:

I've been involved in this all along. At Berkeley, for example, we set up a sequence of courses in the Engineering Department. I was jointly in Electrical Engineering and Mathematics so that is how Numerical Analysis courses and Mathematics had to get a more integrated activity going. I was still struggling with that when the opportunity came to come down here and start a whole brand new thing from scratch. I helped set it up here and I didn't have any competition.

MRS. HUSKEY:

It's a different department here, whereas at Berkeley the Computer Science Department and the Engineering Department are divided. The interesting thing about the Computer Center at Burma is that they had a new building for it, and since the religion is mostly Buddhism and is a Buddhist temple, you have to take off your shoes and socks before you enter a. Before entering the Computer Center you also have to take off your shoes because they have a very beautiful floor, but, of course, they let you wear house slippers or things like that, or you can go barefooted it you like.

HUSKEY:

Oh, I like that. IBM would get freaked out by that one, wouldn't they. The Director of the Computing Center barefoot. (laughter)

MRS. HUSKEY:

Yes. In fact, they posed for a picture with the Director of Education from the government, Harry and the Director of the Computer Center. At the last minute they told the Director, "Oh, you must run and get your shoes so all the foreigners will know that you have shoes." (laughter) So someone had to run out and get his sandals for him to put on.

MAPSTONE:

Oh, that is really nice. I think we have just gotten to the point where the transcriber wants me to cut off.

[End of Interview]