INTERVIEWEE: Derrick H. Lehmer

INTERVIEWER: Robina Mapstone

DATE: 18 April 1973

RM: The date is April the 18th, and this is Bobbi Mapstone and I'm talking to Professor Derrick Lehmer at University of California in Berkeley.

Maybe I thought what we could do is do this on a more biographical basis than the earlier tape and start with where you went to university and events that led to your becoming involved -- well, of course, in numerical analysis and how numerical analysis and computing came together. And let's sort of follow it through on a semi-biographical basis.

DL: I see. You suggest starting with my university work, which was done on this campus for the A.B. degree. I got a degree in physics in 1928. I then went to Chicago to work with Dickson on number theory and helped him with some computing problems we had there. And then a year later I went to Brown in Providence and got - two years from then I got my degree [Phone rings. Recorder off] Before I left Berkeley even, I had built an automatic sieve - you might say an electro-mechanical sieve, in 1927.

RM: What were the reasons or the ideas that led you to build this thing?

Princeton; I worked a year at the Institute for Advanced Study there. And then I took it across the country again to Berkeley, and it is still here. Well, that was the project I was working on pretty steadily until the war came. In 1941 I got into war work mostly having to do with analysis of bombing. And I built a machine there for automatic - you might say - bombing planning or strategy.

RM: Where is "there"?

DL: Oh, I built the machine right here on the campus. I was working here in Berkeley. Then that would take me up to about 1945, I guess, '46. Then the Ballistics Research Laboratory in Aberdeen wanted me to come to help work on the ENIAC which was just being completed. And so I joined the small committee attached to the - to Colonel Simon's office for making plans for computing - large scale computing using the ENIAC and the other machine that was under development - the EDVAC. I also worked on the Bush Differential Analyzer they had there - trying to improve its output facilities. But the main thing I was doing there was making plans and helping operate the ENIAC. I was put in charge of the ENIAC about January, I guess, of 1946.

RM: At this point it was mostly being used for ballistics; is that right?

DL: Oh, well. It's an old problem. Sifting used to be done on paper - I mean, one got a big piece of paper and a strip of paper that you applied successively to positions on the paper and you just sort of crossed out. What we call stenciling. You know what stenciling is in art.

RM: Mhm.

DL: Well, you make a stencil and you cover - finally you apply a lot of these to a piece of paper and you finally rub out nearly everything and what is left is the answer that you are looking for. The thing was to do this mechanically, of course. Electro-mechanically ... So I built this machine in Berkeley as an undergraduate. Then when I was at Brown I designed this photo-electric sieve which was a factor of a thousand faster than the other machine I had. So part of my postdoctoral work was to build this photo-electric sieve which I built myself partly and with the help of R.C. Burt Engineering in Pasadena who did the electronics on it. I did the mechanical part. And it all came together in 1932 here. It was next assembled at Pasadena. Then it was taken to Chicago. We were still doing problems on it. It was taken to the Chicago World's Fair, in 1932 it was. And then I took it to

DL: No, it never -- it wasn't, if you read the history of it. It was just -- the first problem that it ran on was a problem for the Atomic Energy Commission. They were worrying about Atom Bomb calculations. So the AEC stepped in and took it away from Ordnance. Ordnance could wait for their bombing tables - first you need a bomb, I guess. I found out later that the calculations that were being made turned out to be the wrong calculations. But they never told me anything about it. It was all super secret. Classified in some way. Anyway, that - after a year's work and some of my own work done on the ENIAC helping other scientists, I got fed up with the setup and came back to Berkeley to do some more work. And I tried to interest the University, especially the Physics Department in electronic computing. But they told me that all they needed was a slide rule.

RM: [Laughter]

DL: Most of the -- it just took a smart physicist to solve any problem. They didn't understand. But a few years later the word got aroung that electronic computing was a good idea and when they did get their equipment, of course, they were very careful not to share it with anybody else. All right.

My efforts to get something started here in Berkeley were more or less unavailing for two or three years and then finally Professor Morton and Professor Cunningham, who had also been with me at Aberdeen, in astronomy, and myself got together and drew up plans for a magnetic drum machine called the CALDIC that you are going to cover with Professor Morton. This was supported by the Navy and I guess Professor Morton worked with these people of the Navy for about two or three years.

Well, before that was finished we had oath trouble at the University by this time. I decided that I would not sign and so I walked out, so to speak. They didn't fire me, but I just refused to sign. And the job I walked into was Director of the Institute for Numerical Analysis and that was run by the Bureau of Standards on the UCLA campus.

RM: How is it that -- was this a California oath as opposed to a national thing?

DL: Oh, yes. No, it was a California oath.

RM: So it wasn't a security problem then, it was just --

DL: Oh, no, no. It was just something they threw at all the professors and, you know, they said, "We will decide what tenure means at the University." I decided I didn't

want to work under those conditions. So -- this was the last year of Truman's administration.

So at the Institute, the big job there was to get the SWAC -- or the ZEPHYR it was called originally -- the SWAC on the air and producing. And that is more or less what I did during those two years. Also, of course, administering and organizing research activities of a large number of mathematicians mostly who came to the Institute to learn about computing as it was done at high speed.

RM: When was the Institute formed?

DL: Oh, it was about four years before that. I guess it would be about 1945. You could look that up in the Bureau of Standards.

RM: Right. And your position was --

DL: Well, that was 1951, '52.

RM: And your role was --

DL: I was Director.

RM: You were Director.

DL: Yes.

RM: Okay. So SWAC was being built, under Harry Huskey?

DL: Harry Huskey and his crew were just finishing it up in 1951. Yeah. Well, it grew after -- it kept on growing, of course, for some years. This I took to be my primary

mission there - to help them get the thing set up and running, writing programs and helping with - actually with the engineering, too.

RM: Yeah. And also to spread the work - the gospel to the mathematicians that this was a tool they could use.

DL: Right. Yes. We invited a large number of very competent, applied mathematicians and pure mathematicians. And they would come and stay for six months, or a week or maybe a year. I had some permanent staff that were helping with what you might call major problems in computing. We had a big project on solving linear equations for instance. And a large number of people were working on differential systems. And some of them were working on combinational problems. It was a big and interesting group of people - when you get very talented people who had to learn to think a little differently.

RM: Who were some of the names that you recall?

DL: Oh, it's a matter of record, of course. Well, one name I can think of was Paul ~~Erds~~ Erdös, and Walsh from Harvard, Mark Kac from New York. I guess he was from Cornell. As I say, there was quite a list of them. I'm trying to think of one man's name I have forgotten. Van der Corput of Berkeley. Plenty of people who came --

ERDÖS - later pres. of the Hungarian acad. of Sci & a well known graph theory & combinatorics man. (misspelling)

some of them came to scoff actually, but most of them remained to pray.

RM: [Chuckle]

DL: We did a little scoffing on our side too. There was a lot of good feeling, however. I guess we would say good vibes existed in those days. It was a fairly free and easy government installation. Its distance from Washington was something of a benefit for us. It was also a drawback, because we weren't close to the Bureau of Standards physically. You couldn't walk across the corridor and talk with somebody about the problems. So it was operated that way until 1952. In about July then of '52, I had almost two years then and quit.

RM: And the Institute folded later, is that right?

DL: Yes. It started to fold about that time. At least Nixon and the other boys started throwing security at us. So it got to be an unpleasant place to work. A lot of people were discharged. Some of them went away in disgust. It was a rather unpleasant affair. Finally the Navy withdrew its support and the outfit, the physical plant and so on, was given to UCLA; there was some government support there. You would have to go to UCLA to get the details of that.

RM: Yeah, oh yeah.

DL: That was sort of run by the Math Department for a while. It was under Tompkins. Tompkins took over from me. And it struggled along for a while. I don't know whether there is any remnant of it left at all. Probably not.

RM: Now what was your relationship actually with the Bureau? Were you doing work for them, the Bureau of Standards?

DL: Yes. I was the "engineer in charge of the installation" there. Under the Bureau of Standards.

RM: But they, for instance, they would not send out work to swap --

DL: Oh, sure.

RM: Oh, they would. Although they had SEAC.

DL: Yes. We got money from -- well, yes. They couldn't do some of the problems on SEAC, or SEAC was booked up, or crowded. We would get jobs from them. We would get jobs from the Navy and the Air Force, and so on. And sometimes, sometimes we ran into difficulty - and that was one of the reasons, one of the things they threw at us. For instance, the Naval Training Station at China Lake wanted us to do some computing. We were ready to do it. And somebody in Los Angeles that had a little punched card setup decided that kind of work

should be done by private enterprise, not by government. Why this was socialism, setting up a government machine to take bread out of the mouth of this little guy in Los Angeles. Well, Nixon and this other guy from Michigan, a senator from Michigan -- I can't remember his name now -- heard about that. And made a big fuss about it. And we were told we shouldn't work on anything that could be done by private enterprise.

RM: There wasn't very much private enterprise in the field.

DL: Well, as a matter of fact there was very little. We tried to explain to them that our machine was capable of doing things a thousand times faster than any kind of a punched card setup put together. We proved that we were doing work for Lockheed and North American and so on, the Air Force ... But they didn't understand the difference, of course. We actually at one time had the only computer going on the West Coast here.

RM: That's right. And the aircraft companies were basically under contract to the Air Force anyway, so --

DL: Yes. Oh, sure.

RM: there was a tremendous tie in that was -- I guess that comes under Nixon mania. [Laugh]

DL: Anyway, we learned not to love Nixon in those days.

RM: It's easy to do.

DL: Now we have a new Nixon, of course. You have to love him.

RM: It's hard to forget the old one.

DL: Yes. Well, forget that.

RM: Yes. So what happened was the people who were trained at the Bureau now moved off into various areas of the industry.

DL: Yes. There was a lot [of] aircraft work and a lot of design work that all needed lots of computing and they sort of absorbed or vanished into these low pressure areas produced by the demand for this kind of work. People were just building smaller computers by that time. Little ones, like the CALDIC. And nobody knew exactly what methods to use and so on, [they'd come] to people who -- our employees who had learned a lot of that sort of thing, were immediately snapped up by industry. So, in a sense, we infected the whole West Coast -- well, you might say the whole country. A lot of our people went back to the Middle West and the East Coast.

RM: Would you say in the long run that that turned out to be one of the major contributions of the Institute?

DL: Yes. I would say that, yes. We would have, of course, preferred to keep the thing alive because it was

producing nice work all the time, but circumstances didn't permit that. We never heard cases of people who left us that couldn't find much better jobs. In that way, I wasn't too disconsolate about the situation.

RM: Were there other effects that the Institute had over the years, in the long run, on the computing industry or computing and mathematics?

DL: Well, we helped not only with the reports and papers written on methods and so on, [Phone rings. Recorder off] Well, I guess the dissemination of information was the real reason and the -- you might say the engineering and the production of the very high speed parallel computer was what we accomplished at that time. And we, of course, we did the dissemination of information not only by publications and also by holding symposia and getting groups of people there temporarily to talk about one particular branch of the problem like, for instance, the Monte Carlo process and we had a, we had a three day symposium on that. We also tried to help the Math Department at UCLA to get interested and we succeeded more or less, mostly less, and then they took it over; they made the take-over a little easier.

RM: So they operated SWAC?

DL: Yes. So that's what went on.

RM: When did you get involved with the Math Tables and Other Aids to Computation?

DL: Oh, that was a long time ago; Volume I, Number 1, I guess.

RM: How did that come about?

DL: Well, Archibald was at Brown. He was very much interested in mathematical tables and had produced a wonderful library of such tables. I was there as a graduate student and he and I got very close on the subject. And he helped me and I helped him with problems involving tables, their location, use and so on. I could describe -- he would send me a table he couldn't decipher, no title on it or something, and I could figure out what it was. I was just a graduate student. But when the Academy -- well, I was on a committee on tables of the National Research Council and then Archibald approached the Council to publish a small magazine about tables, giving information about them and so on. And I agreed to help him run it. He edited it for quite a while and then he decided to quit. Machines were beginning to become more important than tables; and so I took over the editorship from him for about ten years. And I was still editing it when I was at the Institute for

Numerical Analysis. And I quit that, I think, in 1951. So, it's just, you might say -- it runs in the family. My father was an old table-maker himself. I made a few small ones. So that's how it happened. I'm glad the magazine is still alive and kicking. It's a nice source of information about computing.

RM: It's a marvelous source of historical information.

DL: Yes, yes.

RM: It contains some of the really fine articles.

DL: Right. The first two or three volumes -- you can see things beginning to develop.

RM: Yes.

DL: I use it for my lectures once in a while. To get straight just what happened and how.

RM: Yes. That is really good documentation.

DL: Right.

RM: After you left the Institute, what did you do then?

DL: I came back to Berkeley. They decided to call off the oath problem - the Supreme Court had decided against the University. So I came back into the world of teaching and I've been here more or less ever since.

RM: Well, by this time you were obviously a gospel of computing and how did you --

DL: Yes. You would think great things would happen in Berkeley, wouldn't you?

RM: Yes. [Laugh]

DL: Not at all. No, we had troubles. The President Sproul thought he was a very good friend of Thomas Watson and if he didn't say anything and if I didn't say anything and nobody said anything, maybe Watson would give us a machine. Well, it didn't come across. Watson gave it to UCLA instead. Mostly because they had an Institute down there that already could build up a kind of a group of people in the Los Angeles area.

RM: I hadn't realized that the machine was actually given.

DL: Yes.

RM: No money transaction.

DL: Right. As near as IBM can do such a thing.

RM: Well, that must have been --

DL: That would be a good place -- they set up what was called a Western Regional Laboratories for Computing -- that isn't quite right. Western Computing Group or something like that. It was supposed to process computing from all the universities on the West Coast from Seattle down to Matzatlan, I guess. And they - the result was that they helped UCLA. But almost all the other universities were too far away and there wasn't much -- Berkeley never really got any assistance. Although you could probably find instances where some computing was

done for a person who went down there once or twice. But it turned out to be really a Westwood Village operation. It sat right up on the campus there. In their own building; they still have it down there.

RM: So how did Berkeley go about taking care of computing needs and --

DL: Very badly. We had a committee and no money. Our great trouble was the group on the hill here -- the Radiation Laboratory. They had lots of machines, but they wouldn't help us at all. When we -- they would encourage us to buy a machine or rent a machine and say, "We can give you our overflow of work." So that seemed like an awfully good idea to the university, but when it came to paying for that -- and they paid us a miserably low rate, it didn't meet the rentals at all. It was a good deal for the Atomic Energy Commission, they thought it was great. Livermore was in on it too. They got computing on the machines we rented from IBM. The 704, I think, the 7094, they got it for about thirty dollars an hour. They were charging the faculty about two, three, four hundred dollars an hour. That was the way they did it. And then, of course, the administration didn't want to antagonize the use of the AEC, because they had millions of dollars in contracts. Well, the professors

who wanted to get a lot of computing done who weren't on government projects were frozen out this way. The Physics Department boycotted the Computing Center entirely because they could go up on the hill and get it for nothing. So we expected the Physics Department with all the computing they were doing with fusion and fission and so on, would be big supporters for the Computing Center. They would have money and so on and so forth. But they didn't spend a dime. They didn't see that there was anything wrong with that. They just said, "Well, that is the University's business." So anyway, we had bad years at Berkeley. And it is not all that good now. The whole computing center's part of an economic crisis all the time.

RM: So, a computing center of some type was set up at Berkeley -- when? Approximately.

DL: Well, I don't know how far you want to start back. You can say we starting building the CALDIC and that really never turned out to be a University machine. The faculty didn't really use it. I used it once or twice. It was mostly for training engineers.

RM: Yeah.

DL: But we got a 701 computer, a cast-off from the Livermore

Laboratories eventually, in 19-- I don't know -- 1952, '3 or something, you will have to look that up. And then we kept renting from IBM till a few years ago. We bought the 7094 and then couldn't sell it, and then we switched to the 6400s in the end. We have. But anyway, the thing has been sort of hand to mouth. There was really not all that support for the computing center that you find in most universities.

RM: Yeah, that's true. You mentioned that you did a couple of jobs on CALDIC. What -- can you talk about them?

DL: Well, one of them was a -- just to test it out -- an old unsolved problem. You may have heard about it; you may not. You start with an integer, a nice small integer like 89 and then you reverse the decimal digits on it. Then you add the two numbers together. And then you reverse the digits on the sum and then you add that, and then repeat that. You are looking for the number, a number which reads backwards and forwards the same so that when you reverse them nothing happens. So the question there was -- there were some sequences that ran a long time by hand and the CALDIC being a decimal machine, this turned out to be a nice problem for it. The paneling was open. Professor Morton could show you pictures of it. So you could go around

and sort of wire it a little differently than the standard way. So this thing about reversing the digits could be done by wiring very nicely. So I ran that for a few hours one time trying to look at some problems. How long it would go. The thing is I did some trial divisor type problems one time and that is really about all.

It was, as I say, used mostly for training people to design computers in general. I guess it was perhaps the first really operated drum machine that was built. People went in for that type of machine very heavily in the 19-- late 1940's and fifties because the, the big machines, they were having trouble for quite a while before they got all the reliability built into them or all the manufactured components turned out to be sufficiently reliable. So we went into less elaborate, less fast circuitry. There was a big IBM 650 program, you remember. Everybody had a 650 in those days, and if you will look at the circuits on that they are exactly what we had in the CALDIC. This is all right because the Navy sponsored it and it was okay to steal ideas from the Navy.

RM: [Laugh]

DL: Then the little machines gave way to the big machines

and then the big machines gave way to the little machines.

RM: The curve keeps going.

DL: Right.

RM: You told me earlier about the Computer Science Department; and when did Computer Science get into the campus?

DL: You mean formally as a department?

RM: Formally as a course, let's say.

DL: Oh, well I introduced automatic computing courses way back in 1946 I guess, before we had -- we had punch card machines which the kids could use -- some kids could use. We had 602A, and so on. There was a kind of computing center. Professor Morton can give you the dates on that. He was in charge for quite a while. So this was a course and it was also a service to the campus. It was very minor in terms of high speed computing. It was mostly punched card operating machines. It also had troubles keeping alive. This is an unfriendly campus for computing somehow. I don't know just why.

RM: It seems to be.

DL: We have had Harry Huskey here. He finally got disgusted and went to the Santa Cruz Campus where the climate was very much better down there.

RM: Yes. I'm seeing him tomorrow.

DL: Good. Good. Tell him when you see him that I want him to come up and give a couple of lectures for my course. And I'll try to get in touch with him if he doesn't get in touch with me. I hope he is -- you say you know he is in town? He is usually over in India or Afghanistan or some place.

RM: Yes, I know. By some great stroke of fortune, I caught him.

DL: Good.

RM: And he has promised not to leave at least for twenty-four hours.

Tell me about your course. How about sort of giving the rough outline of what you feel are the significant developments and the kinds of things that you are teaching in your history course.

DL: Oh, the history course, I see. That is the idea of Professor Graham, who is Chairman of the Department. I'm retired, you know, and he --

RM: No, I didn't know.

DL: and he called me back from retirement to teach this course. Well, it's a -- the title of the course is The Dawn of the Computer Age, or something like that. Not my title, but that is Martin Graham's title. And

it's about how things developed: why the time was ripe and when it happened; and something about the technology of the time and also the need and the consequences of the satisfaction of the need; how people got to thinking about computing in entirely different terms when you could multiply a hundred thousand times for one cent and that sort of thing. And it runs from, let me see, 3,000 B.C. based around a clay tablet from Sumeria and, it doesn't dwell much on that, but it goes quickly to the 1943, '44 area, where the ENIAC is. I'm going to dwell on the ENIAC for a couple of weeks anyway until you get enough detail so you can see where the various ideas came from for the later machines. And then it goes on to about 19-- well, maybe 1956 or something like that. Then the second part of the course will be a set of topics which are traced from their origin up to the present time. For instance, let's say, quadrature, numerical quadrature, how quadrature formulas were developed and how high speed computing influenced them. Systems of numerical equations and the problem from the very beginning to Gauss and to the methods we have today. So it is what we call a topical history. You keep going back to the beginning of some subject and then tracing it through.

The students write papers of this type also. They select some kind of topic. My idea is mostly that it would help them do bibliographic research that they don't get to do in their other courses. We teach out of textbooks too much. Textbooks are the last place where you are going to find new ideas.

RM: That's right.

DL: In fact, it is only the old ideas that are in textbooks. And learning how to locate material in the place where the material really lives and has its being, namely the periodical literature, is something worthwhile. Not necessarily computing -- most any subject. A lot of the people around here know a machine, the computing machine is a place where you leave the deck and then there is a place where you pick up the paper. That's what a computing machine is.

RM: Yes.

DL: And they are fighting this machine, trying to get it to respond to their demands, finally succeeding; that's what a machine is to them. They really don't have any -- I guess the way we say it today: they don't have a sense of identity with the machine. We used to have, when we had "hands on" policies, you know.

RM: Yes. What are some of the major cause and effect things you discuss --

DL: In the course?

RM: In the course.

DL: Oh. Well, for instance, cosmic rays. That caused people interested in ionization to build flip-flop circuits. These developed into counters. And these through Mauchly's idea developed into sensor counters which did adding, multiplying. Finally, the cosmic rays were forgotten or at least someone else worried about them. Something much more important than cosmic rays, at any rate: the Computer Age started. So that's a, that's a typical example of cause and effect.

And a much more simple-minded one would be horology, the study and building of clocks - started in the, with the Renaissance and that gave Babbage his idea of how to build - Pascal for that matter - Pascal, Leibniz and Babbage -- there was clock-work mechanism that they were dealing with. Completely divorced from any idea of how to run the thing. They didn't have a really good power supply in those days.

RM: [Laugh]

DL: Apparently Babbage's was going to use steam to drive his machine. [Laugh] Unfortunately, they weren't finished. Those are two examples of the sort of thing we're talking about.

Then there is the, well there is the more theoretical, the more mathematical, or the software side of it, too. Even more intricate, too. The growth of machine language, for instance, that's a course itself.

RM: It sure is. I'm covering it in talking to some of the people who were involved in some of the early languages. But it is a very difficult path to follow because it is not an obvious path.

DL: No, it certainly isn't. Well, it's based also on the technology of the darn things.

RM: Right. And, of course, the needs of the time.

DL: Right. Well, we were happy to have a wiring diagram. On the ENIAC that was our language.

RM: Yes.

DL: As things become more and more automated, of course, it began to separate from the machines to some extent; helping it communicate, but it also is a barrier between the operator, between the user and the machine.

RM: Do you get into the more recent causes such as need for defense work and --

DL: Yes, of course, I try to bring that in. You see that was the reason for the ENIAC, of course -- we needed bombing tables. And they were struggling to put them out on a Bush Differential Analyzer and not making a

go of it really. And there was that demand. I don't think Mauchly thought about it, considered bombing other people as a particularly good thing to do; but there was a place where there would be enough support to produce what he had in mind. He wasn't interested in military applications essentially. In fact his company got interested in horse racing, too, and John still writes me about number theory. He was doing number theory with me on the ENIAC way back, 1946.

RM: Were you checking the daily racing results?

DL: Pardon me?

RM: You were checking the daily racing results then?

DL: No. I wasn't. But his company, the [Eckert-Mauchly] Corporation was interested in building equipment for race track operators. Pari-mutuel betting thing where you have to get an instant odds from the various horses and so on.

RM: That is right. That's a whole field of computation into real-time feedback information that hasn't been gone into very much.

DL: Right, right. They were well along with some ideas there and plans and then somebody was going - the race track dealer interested got into an airplane wreck, near Aberdeen actually, and killed the people who

were trying to negotiate with Mauchly and Eckert.

RM: I'll be darned.

DL: And the other people in the company who inherited the company were less interested, so the whole thing went off. However, they did do some work.

RM: Just ran out of tape.

DL: We're done.

END OF INTERVIEW