An Interview with

J. PRESPER ECKERT

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Abstract

Eckert, a co-inventor of the ENIAC, discusses its development at the University of Pennsylvania and the interaction of the personnel at the Moore School. He describes the difficulty in securing patent rights for the ENIAC and the problems posed by John G. Brainerd, the first director of the project, and by the circulation of John von Neumann's 1945 *First Draft of the Report on EDVAC*, which placed the ENIAC inventions in the public domain. Eckert claims that von Neumann had an interest in keeping these ideas from becoming patented, and deaccentuates von Neumann's role in the development of the EDVAC. Eckert also discusses the ethical questions that were raised by the corporate funding of the University of Pennsylvania's computer research, and relates the reasons leading to his and John Mauchly's resignation from the faculty.

J. PRESPER ECKERT INTERVIEW

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LOCATION: Sperry Univac (Blue Bell, PA)

STERN: First Dr. Eckert, I'd like to begin with the ENIAC period. What emerges from the literature is that Mauchly had the idea for an electronic digital computer and it was his concept which resulted in the ENIAC. Do you think that's a fair evaluation?

ECKERT: If you have to say that in a few words, yes.

STERN: We don't have to say that in a few words.

ECKERT: The idea that he had was (for example in the sense of the Patent Office) *not* an invention. He had an *idea* that he would like to take counters and electrical switches which are called gates and hook them together in some fashion to do the computing. He knew that he'd need to have some sort of counters. As it turned out later we didn't need the counters. We did it without counters in the second machine that we built. But, he thought that we had to have counters and we thought so in the beginning. He had, before I met him, attempted to build some counters. He had built these with gas tubes for a very good reason. The gas tubes consume much less power and have longer life and require simpler wiring--and would have been preferable way to go. Except at that time the best circuits that we had were gas tubes. And I tried to build some after I met him and we talked about this --only we could count at something like 500 counts a second. His experiment with gas tubes which was done even slower than 500 counts a second was done at Ursinus. He had not envisioned any scheme at that time for tying these elements together for programming. Any real conceptually important ideas for tying this together did not exist at that time. It was only a matter of faith that one could sequence these in some fashion. He did have one important idea at that time -- I don't know whether the idea emerged in his brain at the time that he was at Ursinus or whether it emerged during conversations that I had with him in the laboratory when-the curious thing was-I was the teacher and he was the student -- although he had a Ph.D. and I was simply studying for my masters at the time.

STERN: You're being too modest, in terms of your ability as an engineer. I think that you were far beyond the norm.

ECKERT: I was a laboratory instructor and he had come down to take courses in electrical engineering, even though he was a physicist. It turned out that this particular course for him, I think was a waste of time because he knew all the material and so he did the experiments in the laboratory that I was in charge of, and the rest of the time we spent having rap sessions.

STERN: He did have engineering experience from what I understand. He started as an undergraduate in engineering.

ECKERT: Well, not in the league that I'm thinking of. In my concept of engineering, he was a physicist. Now there's nothing wrong with being a physicist but I might tell you my definition of a physicist and an engineer. An physicist is one who's concerned with the truth. And an engineer is one who is concerned with getting the job done and not with the truth. An engineer could if necessary go out with a gun and hold up somebody on the street to get the job done--a good engineer would probably do it you see--a physicist would not. I'm making a slight exaggeration.

STERN: Part of my research is going to be to try to break down categories--so that definition is useful. That is, do people like Mauchly and von Neumann--the scientists--visualize a computer and its application in different terms? What your saying is yes, they certainly do.

ECKERT: Yes, however von Neumann would not have been able to build a counter in his laboratory to try anything out. We'll go into that later.

STERN: Nor would Mauchly at this stage.

ECKERT: Well, Mauchly had built a small counter before I met him -- with gas tubes.

STERN: But not electronically....

ECKERT: He probably found the circuit for it in a book -- a physics book. Because you have to realize that electronic counters were originated by physicists and not by electrical engineers. They were interested in counting pulses -- from Geiger-Muller for measuring such things as cosmic rays and radiation and that sort of thing. They used counter circuits and there was a history of people such as Eccles, Jordan, Wynn-Williams, and so on, who were physicists. The amount wasn't very great -- I made a library of research of all the work that we'd done in counters and electronic switching that was not classified material. That is to say, I had access to classified material because I was working with radars at the time.

STERN: Now are you talking about research relating to developments beyond the NCR thyratron and the RCA counters and those kind of things?

ECKERT: No, it preceded that. I made this library search in one day. It took me a second day because I found a Russian article and I had to find someone to translate the Russian article for me. The others were in German, French and English, which I could stumble through. One day I took myself and one other man by the name of Vincent Porter to help me to make a library research of all available literature on the subject of electronic counters...including a small book called *Electronic Counters*, written by somebody whose name I've forgotten. In any case they included biquanary counters; 2 x 5 counters and things like that were already being discussed. But in all these devices you put pulses in and indicated the number of pulses you put in and that was it. They didn't for example even subtract pulses for the most part. As far as I remember, it only adds. And there were binary ones and decimal ones. Some people seem to think that since we built some for the first ENIAC which was decimal, that we didn't learn about the binary system. I assure you I was familiar with the binary system when I was 14 years old.

STERN: So the decision to go to decimal was conscious decision, it wasn't a question of....

ECKERT: Yes, as a matter of fact, it isn't strictly a decimal machine. There were many sub-elements of the original ENIAC that are binary in nature and then could be converted to decimal before they came out of the machine. It was a conscious decision forced on us by the fact that the IBM card punch and printers and things that we were hooked to were decimal. It had nothing to do with anything but that.

STERN: What puzzles me on that statement is that from what I read of the original reports, it wasn't clear that you were going to use IBM equipment at the beginning anyway... that is, you might have developed your own I/O. Am I misreading it?

ECKERT: Yes, but before we froze the design we knew we were going to use it. But the only alternatives that we had were also decimal...and there was some teletype equipment and so on.

STERN: I see. But there was no serious thought to developing your own I/O at that time.

ECKERT: No. Well, John Mauchly had an idea of a display. We talked about him using a strobe lamp and flashing it at the right time behind some whirling cylinders -- something which an engineer did. But we didn't go with that. You know many of the things that we went with were things we could do quickly.

STERN: That is, because of war time considerations.

ECKERT: Most of these were a matter of expediency and not necessarily the soundest way to go. As a matter of fact our first decisions were not even to build a digital machine in the full sense of the word. Our first discussions from a practical point of view were that we have a differential analyzer--in the differential analyzer we had mechanical integrators. The mechanical integrators as they were original built were fairly inaccurate. They had errors of the order of a few tenths of a percent and perhaps even as much as 1 percent...depending upon how carefully you maintained them. As a practical matter, I think they could be tuned up a few tenths but they tended to run more like a fraction of a percent in error. We had a machine that finally had ten and later thirteen integrators. We had this

hooked up so that one of them feeds the other around the chain of the differential equation involved. You finally get errors in the final result of the order of 1 percent or worse. The ballistics work that we were doing required errors of something like 10 times better than that. So what was being done at the time was that things were being run on the analyzer and then they were being smoothed by hand calculations...being calculated by some interpretive schemes to try to improve the accuracy required now. You might say why in the world, if you are going to do that, don't you hand calculate the whole thing? Well, we did a lot on the whole thing and the most accurate stuff we did was all by hand calculations...or we did some of it on punch cards down at Aberdeen. But with some of the stuff, we were able to save time by running it on the analyzer and then doing some smoothing numerically on the desk calculators and coming out with a faster result than if we'd do it all by one method of hand calculations. We couldn't do it all at that time with the analyzer. It wasn't accurate enough.

Now, my first interest was as a practical matter. These discussions took place through this period in parallel but they were just back at the initial discussions. My practical interest was to improve the analyzer. And I wasn't officially connected with it. I just sort of drifted in and had ideas and the guys in the shop would put them together and finally the professor who was in charge of the differential analyzer--Dr. Weygandt--who had been my teacher so I was on intimate terms with him--kept getting me in more and more until finally I ended up doing more and he was doing less of it. Another fellow, Phil Cook(?) worked on it too. In fact, Phil Cook was officially assigned to work on it and I was only in there as a sort of volunteer in the beginning although as a result of the work that I did on that and several other things there was finally a memo written by the Dean that said, anybody wanting to talk to me about any of the projects in the school to go ahead and do it. They would regard me as an across-the-board consultant and that's really why they gave me the job. Because I was sticking my nose into everybody's business but apparently coming up with good enough ideas that everybody was happy about it and I was happy about it. It was my 24th birthday when we started the project. I was 22 or 23 at the time we are talking about it. I just graduated the year before. Well, let me first tell you what I did with the integrators. Some guys up at GE had an idea of following the wheel in the integrator around with photocells and polaroid(?) discs and so on which I won't go into detail on. In order to relieve the mechanical error effects on the driving of the wheel which would improve the accuracy--provided you also make the wheels accurate (put better bearings on them)--we looked at what they were doing and in fact Irven

Travis--another teacher I had--went to Burroughs and became a vice president there. Irv Travis was a consultant with GE on that project. They had taken the blueprints of the machine we had at the Moore School which was in turn a copy of the machine that Dr. Vannevar Bush had developed at MIT. They had taken those things and made further improvements on them. I looked at their improvements and found that broadly I liked the ideas that they had on the photocells but the way the motor drive circuits and the other circuits they used to carry it out didn't look so good to me. So we got some equipment from the Army--we had a connection with the Ordnance Department and we'd get catalogs of spare parts for field equipment and we found they had an excess of some devices called amplidynes or servomotors. We would be able to get some because they weren't using them up as fast as they had stocked them for repair purposes. Otherwise it was very difficult to get parts those days, unless there was a high priority. So we got these amplidynes--we got these parts from the Army and we modified them and put them in the analyzer instead of the mechanical amplifiers that were there. The mechanical amplifiers were what's called *string and band* amplifiers. They were an idea that Bush got from looking at great big band amplifiers that they used in steel mills prior to servomotor development, prior to good motor development. And we took the string and band devices out of the integrator and put in the electric motors, gears, and photocells, and a variety of other things. (They originally had students down there building pointers, putting arbitrary functions into the machine and we got rid of those and put in devices that followed curves drawn on pieces of paper with photocell motors.) By the time we got done, I had to put in a couple of hundred tubes into the otherwise mechanical machine. Not only a couple of hundred tubes but 13 amplidynes and 15 to 20 servomotors and a bunch of photocells and various other power supplies and contraptions. So the machine was no longer a mechanical machine. The original machine could have been driven with a steam engine, it was just mechanical, you just had to turn it with a couple of motors; actually there was a motor for each integrator--18 motors to be exact. But it could have been 13 steam engines just as well. So, by now it really was an electro-mechanical device, though still analog. The improvement however was pretty exciting--the speed was about 10 times and the accuracy about 10 times. The individual integrators were accurate to 100 to 1 percent--which is pretty accurate for an analog device. In fact to get beyond that, you had to air condition the room better. To go beyond that with analog devices, we were already grinding parts down to 50 millionths of an inch or so in integrator wheels. We decided it would be an error to get another factor of 10 to improve speed of accuracy, a Herculean error which we probably would still be at if we had chosen that road.

STERN: Mauchly was not involved in that project at all. Am I correct?

ECKERT: That's correct. Although his present wife used to operate the machine in the evenings during part of that period. I used to ride out on the last train at night--about midnight--with her. What we first discussed was that instead of having an integrator, we could produce a variable ration(?) by controlling the counting of pulses. You could count pulses into a counter and by controlling the rate in which they're issued from one device and the rate in counting the numbers that are received into the device we can in fact integrate. So we then conceived of some electronic counters rigged up in such a way so that a shaft would come in and the signal would be changed. It would turn a disc with teeth on it or something, which would interrupt a beam of light and which would cause pulses to occur. We'd feed those into counters in such a way that they would receive a count and then on the output shaft of the integrator we would have another wheel with teeth on it which would interrupt a light beam and it would turn until the count on the output shaft was matched against the output counter.

STERN: Sounds like a kind of digital....

ECKERT: Yes, analog-to-digital and accrued(?) what we call tracing type of analog. We conceived of having counters to do the actual integration process.

STERN: In the transcript of the trial there was some mention of GE and Irv Travis serving as a consultant.

ECKERT: It was an analog device used in the photocell. I said we did get the idea of the photocells for this first modification-- all analog.

STERN: Now did GE specifically ask Travis if such a digital device were conceivable?

ECKERT: GE had nothing to do with digital.

STERN: I spoke to Travis last week and he told me that based on his consultantship with GE he was aware of the fact that you could go digital and that he just didn't think that it could be built in time.

ECKERT: That's not true. I remember precisely what he thought. Irv Travis considered a scheme using frequency modulated carriers which could be called digital in some far-fetched interpretation of the word digital. But the counter of an FM system is ordinarily considered another form of analog representation. He has chosen to let the words frequency modulation counter now mean "digital," in his Monday morning thinking about what he should have done and that's the real truth. The reports show that he wrote at that time--and I read it at that time--way back at the time I was working on the analyzer--that the frequencies that you have to go with to get the kind of accuracy that you even got mechanically were prohibitive...in terms of the equipment that could be built at that time. And so it wasn't that he did it for time; the scheme wasn't practical. And that can be verified by getting that report out. It should be in the archives but I don't know.

STERN: I do recall reading such a report.

ECKERT: Now, he was vaguely aware of something else digital. What he did do digitally was that he had some discussions with GE and with others on getting a whole lot of desk calculators by Frieden and Marchant and by putting solenoids on the coils that can push the buttons and put electrical connections on the wheels that read them out...and connecting that stuff together in some vague way. About as vague as Mauchly's when I originally met him. he talked to people that made machines like Friedens and found out how many operations they would stand. When operated with solenoids like that, the frequency of operation was so much more efficient than a person who, while they can do 10 a second for a few seconds, will then goof off for a minute and a half. The cycle operation was so stepped up by mechanical operation even though the instantaneous rates of hitting the buttons is the same that these machines would last for two months and then you could throw the whole piece of equipment out as worn out junk. And so he decided it was impractical and he would have to develop devices like this to do it. Well that is really the direction in which Aiken went. Because Aiken took already developed digital devices which were Clare Lake

counters and Clare Lake relays...and built the MARK I, and so this was already in process at Harvard at the time that Travis was speaking of.

STERN: You were not aware of this though?

ECKERT: Vaguely. I had a friend at Harvard who was a student of Aiken's and he told me. He was an aeronautic engineer and he wasn't into that particular area at that time--later he was one of the first engineers at Univac and designed the tape unit and several things.

STERN: May I ask who that was?

ECKERT: Frazier Welsh, he'd dead now. He was killed in a plane crash in a private plane. But, at any rate, he only told me what the size of the machine was and what the estimated cost would be and some vague ideas about what it would do. He was not fully sure when I talked to him that it was digital... for example so that's about all we knew about that.

STERN: How about the work at Bell Labs. Were you aware of that?

ECKERT: Yes, we went and looked at the work that Bell Labs had done. They had a complex multiplier--sometime earlier--I think about '37 or '38 but I'm not sure of the dates. They had actually hooked it up to remote terminals in another city through the telephone lines. If you do network problems and telephone transmission problems, there's a lot of multipliers of complex numbers and this machine was specialized to do that task. You've probably heard of that machine.

They then built some devices which were fire control interpolating devices for interpolating and smoothing firing tables. They used relays and those machines were built by Williams and George Stibitz. Really they were built by George Stibitz--Williams in my opinion, and I knew him pretty well, was the guy that was trying to collect a lot of

credit for stuff that Stibitz really did.

STERN: Stibitz was really a mathematician. Did he do the engineering, too?

ECKERT: Well, you see--he said he had to have some relays to do this, that and the other, and Williams had figured out where to run the actual wires--which was standard telephone practice. Technically, they are both inventors. Somehow I looked at some of those patents and they only had Williams' name on it. And in that sense, those patents are invalid because they were really joint. Patent laws are very hard to define in joint inventions. In my work, whenever in doubt you always put everyone on. But some of these guys at Bell Labs and RCA were pretty competitive with each other and ripped each other off in my opinion all the time.

STERN: I thought the companies had policies which said that the patents revert to the company as opposed to the individual inventor.

ECKERT: A patent must be taken out in the name of the inventor. All that can be given to a company or anyone else is a right to use it. This is called an assignment. But if a patent is taken out in any other person's name other than the person that actually did the work, it's invalid and becomes public property.

STERN: Well, but if the person takes it out, does not the employee at a place like Bell Labs sign an agreement which says any invention....

ECKERT: To assign it. But we're not talking about an assignment. We're talking about whose invention is it? You can assign a right--it's like a writer--if I wrote a book, you see, it's my book and I wrote it. Now I can assign the copyright to the publisher, but it's still my book.

STERN: Right. I think I understand. In other words, they did not get any royalties or any....

ECKERT: Yes, but they got something equivalent. They got status in the corporation which affects their salary and job promotions. If there are two guys working on something, they both should be joint inventors--and one guy can claim it all for himself if he is the other guy's boss and get away with it...this happened a lot. It happened on some very important inventions that have nothing to do with computers. For example, with the television, I'm reasonably certain about this because Carl Chambers was there at the time...(he was my teacher and Vice President of Engineering at Penn.). Carl was in the laboratory at the time when Zworykin supposedly invented the iconoscope which is a tube that takes pictures.

Now, I happened to work for Farnsworth in my spare time when I was in high school. When the thing finally went to a court battle, the court decided that 75% of modern electronic television was invented by Farnsworth with another 25% by RCA. Most people don't know that. The NBC and the RCA company had pretty well obscured the history of the real invention--of television--which really occurred about 8 blocks from where I lived. And I used to go and talk to the guy and I know the thing very well. In fact, for so many inventions that I know about, what the public believe is complete nonsense. Not just computers but every area. When I see my son studying history I think he's reading a lot of trash which isn't true... because everything I know the history of it is screwed up.

STERN: Yes, the way things come out is far different than the way it was. That's one of the things I'm interested in.

ECKERT: I can give you a classical example. Most people think Edison invented the incandescent lamp. There were guys like Star(?) that were manufacturing incandescent lamps in England 25 years before Edison started on that job. Now, what did he do? The guys that were making it 25 years ago were getting a 2 hour life out of the lamps, and Edison's lamp gave a 40 hour life. That was the invention. Also his lamp worked at a higher voltage, so he could parallel them. That was another important invention. The earlier lamps were not sufficient. The earlier lamps came out at a time when the generator hadn't been perfected. The earlier bulbs worked at low voltage--you had to put them in serial. And the generator happened to be timed just about the time that Edison came out with his light bulb so that there was synergism there.

STERN: Those elements then in your mind are part of invention.

ECKERT: I think synergism is.

STERN: How about things like applicability?

ECKERT: I think for the most part, necessity is the mother of invention. I think it's rare that inventions come along and then you try to find a use for it.

STERN: Well, I would say in some sense if we talk about the computers...it appears to me that the Eckert-Mauchly organization had to convince the rest of the world that there was a use for computers say around '46.

ECKERT: Yes, but the reason we got the financing to do the first project was because of necessity of getting out firing tables.

STERN: But I think it's less rare than you seem to indicate--that you need to convince people that there is a use for this thing.

ECKERT: I think what you have is what I call the molasses phenomenon: People just hate change of any sort about anything...politics, inventions, anything. Anybody that's going to get something new is not really well liked. Today I have just the same problems trying to get things done around here as I would if I walked off the street as an undergraduate student. It is no easier for me today than it would have been...maybe it's easier for me to get to talk to people, but it's no easier to sell them.

STERN: I don't know whether I should bring this up, but I would say in part that that's the way I look at this Atanasoff matter. Besides saying something about the technology involved, Atanasoff was unable to "sell" his

machine.

ECKERT: The Patent Department requires what is called diligence in order to get an invention. Are you familiar with this concept of diligence? It's important; you ought to look it up. There is a case where a guy built a machine of some sort--I think it was gasoline proof test or something like that. Anyway, he put this aside for awhile (a week or two) and then came back to it and in the meantime someone applied for a patent on that job. And he said well I had this going, and the Patent Department said, well you didn't pursue it, because you stopped working on it for a couple of weeks to do something else. (It was a short period. I can't guarantee it was two weeks but it wasn't years.) The Patent Office says that not only must you establish prior date to doing something, but you must also show that you diligently pursued that without any interruptions. Something most people don't know about.

STERN: How can Atanasoff argue diligence?

ECKERT: He got around this by saying there were more important things for him to do during the war. Of course, he got $50,000 or so to testify for Honeywell.

STERN: Okay. Getting back to the ENIAC...the ring counter....

ECKERT: They both built ring counters--it's a question of which used gas tubes and which used hard tubes.

STERN: I think it was the hard tubes. Now, regarding the specific counters that were used on the ENIAC....there was a period in which you did some investigation on counters and there was a period in which you requested information from NDRC on the counters that were being used for their fire control devices.

ECKERT: Actually the information requested was by Herman Goldstine. He got the information and gave it to me.

STERN: Now my understanding is that NDRC was not too happy about giving out this information.

ECKERT: They didn't have any choice. They were doing the work under contract to the United States Government. And we were working under contract of the United States Government. Under public law (I think 700) they have to give us information.

STERN: But some documents suggest that they were lax about it.

ECKERT: I have no evidence of that.

STERN: Some of the documents indicate that you had requested RCA for information on that counter--that is, your group--and RCA--would say "well we need an okay from NDRC" and NDRC wouldn't give their okay right away. It was that kind of hassling.

ECKERT: Well, that's a different matter. If the documents went from NCR to RCA, they are not the proper authority to give them to us. They must come from the Ballistics Research Laboratory.

STERN: No, I'm talking about the National Defense Research Council. The group that....

ECKERT: Oh, I thought you meant NCR the company. You understand that some of these counters were developed by National Cash Register.

STERN: Well, I know that the thyratron was invented by NCR. And there was a Lewis counter, and an RCA counter as well.

ECKERT: Yes.

STERN: Now, the RCA counter and the NCR counter was developed under government projects for the National Defense Research Council. And that was the major organization which had to give its approval before that information would be made known to the Moore School.

ECKERT: But there's no way they could have gotten out of doing it--they had to do it.

STERN: Sam Caldwell and Harold Hazen and people on this council were unhappy with the Moore School project. Is that correct?

ECKERT: Caldwell...you see there's more background to it...are you aware of the fact that the Dean of the Moore School, Harold Pender, used to be head of electrical engineering at MIT?

STERN: No. I never knew that.

ECKERT: They were sore that he left MIT to come down here (Moore School) and was setting up a pretty good organization. You see, it's a funny thing...the Moore School at that juncture was probably the best place I could have gone to college in the world for the reason that probably the best place for electrical engineering at the time was MIT. But it was big and you have the problems of any large institution...you get personal tension. We had the advantage at the Moore School of having the guy that was head of electrical engineering at MIT only a few years earlier and yet we were small--only 100 students total. The school had its own endowment and therefore was actually capable of operating with a great deal of independence from the rest of the university. For example, the university's regulations would not allow undergraduates to be given open book exams--for what reason I don't know...some idiotic reason. We did, in fact, give open book exams. It's the only way to teach electrical engineering. There's so much material in electrical engineering that if you were to attempt to memorize it all you would actually inhibit learning the stuff. Your time should be spent learning a vast variety of material and not spent memorizing a vast variety of material. Too much of medicine, for example, is memorizing and they're getting more and more away from it.

STERN: So Pender in some respects was responsible for the relationship between the Moore School and MIT from the very beginning; that is, with respect to the analyzers as well as the radar project would you say?

ECKERT: No, I wouldn't say on the radar project...it was Brainerd and Chambers. But in the beginning, yes. The relationships were through Pender.

STERN: So that part of this hostility resulted from the fact that Pender had left.

ECKERT: More than that. He got to build from the government two differential analyzers during the WPA--they weren't actually WPA--it was a technological equivalent of it. WPA, you'll remember, was Works Project Administration created by Franklin and Eleanor Roosevelt. It was a technical version of WPA for unemployed engineers and draftsmen. It wasn't called WPA; it was called some other letters...a name I can't remember which you could find out. But they got money from the government under this equivalent of the WPA grant. WPA was for digging ditches and so on. They built an analyzer. They built one at Aberdeen and one at the University of Pennsylvania with the understanding that what the government would get in return is the use of the one at the University of Pennsylvania at the time of a national emergency. Of course, the machine was built at a time when no emergency was anticipated, but this understanding was sensibly written into the original agreement.

STERN: It might have been anticipated: it was just not existing at the time. You're talking about just prior to the outbreak of war. I'm sure it was anticipated.

ECKERT: No, that machine was completed before we knew we were going to be at war, by several years. There wasn't a war hanging in the balance; it was just their tradition to do it that way.

STERN: So you would say, then, that the Caldwell-Hazen problem was more for this reason than the fact that you were going to work on something that would compete with their differential analyzer....

ECKERT: I think that added fuel to the fire. But the anger was already there, because the next improvement over the Bush analyzer was not built at MIT but was built at Penn and at Aberdeen. And then the second machine which had the electrical interconnections through telephone relays (I forget what they called it--the fancier differential analyzer) was built at MIT which Caldwell was interested in.

STERN: The Rockefeller analyzer.

ECKERT: Right. The original analyzer only had six integrators. The one they built at Penn had ten, and then during the war we added to it and got up to 13 or 14, I've forgotten the exact number.

STERN: I think it was 14.

ECKERT: I guess it was 14. But anyway, it was originally ten and it stretched out to 14 later when I was there. It was already 10 when I arrived there as a freshman in '41 and I don't remember the history earlier. But there was some feeling about that all along and Caldwell felt that WPA type of work should have been done at their place in the beginning.

STERN: Now, there was some talk about a joint venture...

ECKERT: Incidentally, Dean Pender's influence in all this has probably been underplayed. He was a funny joking sort of a guy. He had a company called the International Resistance Company which at that time had 2,000 employees; he owned over a third of it himself. He was a clever sort of guy in doing things. He's also a guy who had an IQ of nearly 200. He was a genius. He was a guy which at a very early age like 20 sent by a Dean and one of the very classical physicists down at Hopkins to Paris to demonstrate a classical experiment in which it was shown for the first time through an actual experiment that static electricity and magnetic electricity were the same.

STERN: It was Poincare who did not believe that...as I understand the story.

ECKERT: I don't remember that part of it. Poincare didn't believe in superposition and a few other things which he was very wrong about later and inhibited Fourier(?) from...Well, various things could have been developed a few years earlier.

STERN: In discussing this with Herman Goldstine and I think his book says the same thing...that it was Brainerd who convinced Pender to go with this ENIAC project. Pender gave his okay but he indicated that if it did not work, Brainerd's job was on the line. Is that your recollection of the situation?

ECKERT: It sounds like a complete bunch of nonsense. I'll tell you, Goldstine was a guy that every day I saw him when he came into work--had a story about somebody in the history of mathematics. A little funny story and there were stories where stories didn't exist. I can remember quite a lot of them--some of them I verified as true and some I verified were not true, by talking to people later who were involved...about other mathematicians. He's a guy who saw politics and personal stories and things like this around every nook and cranny. He was also a fantastic hero worshipper. Everybody that ever did anything who was well known was to him a hero. One suspects that it's because Goldstine wanted to bask in the reflected light, you see, so he was always telling these stories.

STERN: So that this story about Brainerd's role in getting the contract to be acceptable to the Moore School is incorrect as far as you can determine.

ECKERT: I would give most of the credit to Goldstine, who I liked very much during the whole period I worked with him and who later on I didn't like because he told lies. No other reason. Brainerd was in my opinion nothing but a paper pusher. And the way he had his job and got along as well as he did is that the Dean was not a guy who liked routine work. As the Dean, he should have gone over all these routine schedules for the classrooms, and this and that and the other, and all that kind of detail...day to day detail management of the School. He pushed as much as he could of that off on other people. And the guy he pushed most of that off on at the time I was a student there was Brainerd. And Brainerd was the grind who was always sitting in the office working on paper work and really spent

more of his time administrating. In class, Brainerd was a disaster. He would come in and lay on the blackboard for the whole period exactly what was in the book's chapter--all the equations which he memorized apparently...and to the point where as a graduate student, I caught typographical errors in the book going on the board. I'd say, "Dr. Brainerd, that can't be so." And he'd say, "Well, it's so and so." There was a book in which Brainerd was one of the coauthors--Goldstine and I found an error in the definition of a step function that made a certain complex relationship equation wrong in the book. We took it to him and said, "Look, you defined the step function and you got an error of 2 to 1 in the equation. And the step functions here has a unit step function and that goes in equation so and so." He said, "You don't understand. That's not the kind of step function I'm talking about. The kind of step function I used can be defined as having a value of only one half." We checked it again and Herman with a Ph.D. in math knew a lot more math than Brainerd did. And I did not have a Ph.D. in math, but I had the same course value that a Ph.D. in math had. No way was this true. I just finished taking a graduate course in theory and function of complex variable with Rademacher who was the outstanding world authority practically in that subject during that time. So no way was what Brainerd said true. Not only could we not convince him that he was wrong--and he knew he was wrong--but he didn't take any notes I noticed to get that corrected in the next revision of that book. I checked in that revised book to see if it was corrected and it wasn't. Furthermore, there were other things that happened with Brainerd that never came out--such as some students who came in to complain that their papers and some of their masters theses and material had been used in public papers which had been printed by Brainerd without credit. Stuff like this. And this was the kind of stuff that was making Herman and myself pretty angry. Goldstine may treat Brainerd as buddy buddy now, for other reasons, but he hated Brainerd at the time--much more than I did. I tolerated Brainerd. I couldn't afford to hate him. I was in his class. I was very young (only 24) at the time when we started this and all these guys frightened me anyway and I put up with it but I certainly didn't like him and I never have since then.

STERN: Now the way it appears at least from the Goldstine book and from the documents was that Brainerd's reputation with the Ballistic Research Lab and with the differential analyzer was important to...

ECKERT: His reputation was based on administrative handling of the contracts.

STERN: ...and that...

ECKERT: He was the front man administratively speaking.

STERN: ...so that to give him credit in an administrative sense you would say is appropriate?

ECKERT: Yes, he was the administrator. He handled the papers.

STERN: And he was doing an effective job in the sense of expediting...

ECKERT: Herman did a lot of that, too.

STERN: Herman did a lot of liaison as well. Getting material from other places...

ECKERT: Well, he got the papers and handed them to Brainerd. If they would not have gotten handed to Brainerd, Brainerd would never have known about our project. You know the famous story about Brainerd. He never could find the original write-up that Mauchly wrote and it had to be recovered from Mauchly's shorthand notes. Then later on--years later--Brainerd claimed he found it in the files of something, but at the time he couldn't find them. He was so insensitive to the value of this idea that he was unable to find the memo that was sent to him on the subject when it was brought up a second time by Goldstine who had been indoctrinated by Mauchly about the idea. That's how insensitive he was. So you cannot sensibly give credit to Brainerd for pushing the development because he was more of an obstacle than he was a help. Now...it was his job certainly to get money for the school to do research. This was his major task that he was assigned by Dr. Pender. When you say it had been Dr. Brainerd's neck if he made a mess of it...it would have been the Moore School's. It would have been Brainerd, Pender, Goldstine, and everybody's neck. Since Pender was the titular head of the thing--he would have gotten a black eye.

STERN: Now let's...

ECKERT: I think this project was a little different from the usual projects they took on. The usual projects they took on were research and development work. And in research and development, if it works it works; if it doesn't it doesn't. In this case they undertook the job of *building* a piece of hardware that would function. In that sense they would have gotten into more trouble. But it wasn't because it was Brainerd or Goldstine, or even because it was a computer. It was because they undertook to deliver a piece of goods rather than to deliver a piece of paper describing something that could be done.

STERN: Now in 1944 you requested the engineers at the Moore School to sign a document asking them whether they would want to claim any patent rights. Brainerd took great exception to your authority to do that.

ECKERT: I know.

STERN: Would you respond to that?

ECKERT: He was asked to do it by me. He failed to act. A date was arrived at which the patent lawyers said, "Look, we've got to know who to put on a patent." It is a factual matter of patent law that the patent must be taken out in the name of the inventors or inventor. Any deviation of this either by adding people that weren't inventors or by subtracting people who were inventors invalidates the patent.

Well, you know that von Neumann happened to intercept a paper from Mina Rees, which showed that Brainerd had written this whole thing up as if he'd done it and sent a paper out on it. And Goldstine found out about it through von Neumann and we raised the devil about it...and finally we wrote it as a joint paper. His paper was absolutely so screwed up and mixed up that Mauchly and I rewrote his paper. The paper was finally put out by Mauchly and myself. We really should have let him put his paper out and he would have made a complete ass of himself because it was factually 90% wrong because he didn't understand the machine and didn't know what the hell he was talking about.

STERN: If he did not understand the machine why would he do this? Was he looking for credit? Was that the purpose?

ECKERT: I just told you that prior to all this, back before I got to the Moore School, he had stolen ideas from graduate students and published them as his own.

STERN: Okay.

ECKERT: That's the key thing to remember. This guy was police court reporter before he was a professor of electrical engineering. Did you know that? This guy grew up in a tough background, you see.

STERN: Okay. This is very enlightening; I didn't know that. What about Pender? How did Pender react to this whole patent problem?

ECKERT: Pender was a gentleman and a scholar and a very brilliant man. Brainerd wasn't brilliant at all. I can tell you other instances about how stupid this guy is. We had a building some distance from the Moore School (up the street--34th Street), we had a room in there and a bunch of people--mostly girls. They had a projector in which they could project pictures taken by a phototheodolite.(?) You know what a phototheodolite is? It's a machine that takes pictures of shells and they travel through the air for the purpose of finding out what the trajectories are. So they were taking pictures that were taken by the cameras and the phototheodolites and projecting them on a big movie screen and then making careful measurements on these things and converting them into numbers in order to get the input for our equations. And to check calculated data against experimental data. There's an experimental coefficient called the Ballistic Coefficient C in the ballistics equations. It cannot be arrived at theoretically, at least it could not at that time.

STERN: Is that the *drag* function?

ECKERT: No, it's the Ballistic Coefficient--it's exactly what I called it. It's usually symbolized by a Capital C. One of the things you had to do was to--arbitrarily--at least you had to do it at that stage. Maybe you can do it theoretically now but they could not at the state-of-the-art as of those days. What you had to do is arbitrarily adjust C which is a number normally near 1, but you didn't know if it was .98356 or what it really was until you made the calculated curve fit the experimental curve. And you did this by turning this value of C slightly until the experimental data and the theoretical data fit each other. Now, in order to do that you had to make measurements to the experimental data which could then be sent to the calculator and analyzer people to make this comparison and to readjust the value of C to get the proper answer in the firing table. The room in which they were dong this in order to be able to see the screen had to have all the windows drawn. There was no air conditioning and the girls doing this work were perspiring pretty badly because of the heat. Well, it was a bad deal for them; in fact, I remember going up there to get some numbers and they had taken just about everything off but their bra. This was before bikinis were popular. In any case, they complained to Brainerd and on one of the occasions a refrigerator had been put in the room for coke and he said, "Well, just leave the door of the refrigerator open to cool the room." Now anybody that ever heard of Lord Kelvin knows that a Kelvin heat pump pumps heat from the inside of a refrigerator to the outside, and simply allows the heat to come in again so you can pump it out adding the heat loss of the motor to the system. So if you have a refrigerator in the room and you open the door on it, it will heat the room more than if you were to keep it shut. Now that calls himself an electrical engineer. That story got all over the school at the time, by the way.

STERN: Once the project was approved in '43...what was Mauchly's role from that point on?

ECKERT: He was a consultant on the project which means he worked on it part time.

STERN: Officially. But how did he contribute to the project at that time?

ECKERT: Well, he taught, so he had to spend a fair number of hours giving classes and marking papers and he worked whatever other hours he could wedge in on the project.

STERN: In what sense? How did he work on the project?

ECKERT: Mostly by my talking to him and the other engineers like Arthur Burks, and other people in the project about what his ideas were. Now I can remember in the very beginning there was one very important thing that Mauchly did suggest. And suggested very early. It was not my idea. It was an idea that I instantly recognized as the key to this whole thing, and that is the concept of subroutines. I think Mauchly arrived with that idea from Ursinus. Whether he got it from Ursinus you'd have to ask him--or whether he had it after he got to the Moore School, I don't know. All I know is that I did not know of this idea and he pointed out that these problems we were doing were highly repetitious and that obviously you couldn't store the program by cables or switches. However, we were planning to do it. However, we planned to do it over and over again. But what you should do is store the program once and then call on it and use it over. And that furthermore, you needed some higher degree of flexibility in this respect. And we had (in the master program of the original ENIAC) the ability to have subroutines and subroutines of subroutines.

STERN: What comes across again in the literature is...that subroutines were a British innovation.

ECKERT: Absolute nonsense. It's involved in the very basic concepts of the original ENIAC. And the master programmer has the number of the outputs for rewriting the program. It allows you to do this. It's built into the hardware and this is before the British ever saw the machine.

STERN: So the concepts like the subroutine would be something Mauchly would have to discuss with the engineers in terms of the development of the master programmer? Even during...

ECKERT: I'm not sure what we called it--I'm talking about the concepts. I think we called them simply repeated routines or something like that rather than well, subroutines. The phraseology changed, but the fact was known that you could break a complicated routine into a number of pieces that are identical and then reuse it. That's what I'm

talking about. And nest them into each other. To the best of my knowledge that was Mauchly's idea. I don't know whether any of those ideas showed up earlier, because I was not familiar with Babbage's work.

STERN: Once the project was approved, I'm interested in the working relationship among the engineers with respect to Mauchly.

ECKERT: Well, the other engineers worked in the laboratory all the time with the exception of a few other responsibilities--Burks had some other responsibilities when he worked part time. Sharpless and some others had some other small teaching responsibilities and only worked part time--but the majority of the people worked full time, including myself.

STERN: When you say full time, did you work at all the details and then say, "Go do it"...or was it a group venture where everybody sat down and said, "well, this is what we're going to do...or what we should do."

ECKERT: We usually had weekly or biweekly meetings in which we discussed these things.
STERN: I'm interested in knowing whether this was a group effort or whether you and Mauchly developed the ideas and assigned various aspects to the engineers.

ECKERT: Once things got going, everybody pretty much knew what his bag is and you don't need these meetings. And the meetings become less frequent. The meetings become more for management questions like we've got to hire three more wirers, and we need more postage money...you know stuff like this. But in the beginning you had lots of technical meetings. And everybody was there. Many of these meetings were attended by Carl Chambers as a consultant. Many of these meetings were supposed to be attended by Brainerd; some of them weren't. More were actually attended by Carl because Brainerd's interests were really not technical. He never worked technically. Throughout his life it's been administrative with a technical demeanor.

STERN: So that if I understand Mauchly's role during this...

ECKERT: Later on, Carl's interest became entirely administrative. He became Vice president in charge of Engineering. But his interests were much more technical in those days. Carl was much better in mathematics and engineering than Brainerd was. Carl's father was a mathematician and was the head of the Actuary Society. When Carl was a young man, his father single-handedly prepared the actuarial exam and gave them to the American Actuarial Society. When Carl was in college, his father would give him the exam and if he passed it Carl would be able to work with his father that summer marking the exam...if he flunked, he would have to get a job elsewhere. I don't know whether you know it, but the actuarial exams are considered very tough.

STERN: It is correct to say then that Mauchly provided some of the ideas from the '43 to '45 period in which the ENIAC was developed and interacted with the engineers in that regard.

ECKERT: Yes, I can remember, for example, when we first started to build this. We had the counters and we figured how to build a counter mechanism, but we didn't have any way of controlling this in the programming system. And I remember Mauchly was sick with the flu at the point...when this became urgent. I said, "We've got to decide now what we have to do next in this program. We can't wait another week; we've got to decide now." And we couldn't have a meeting with Mauchly (he was sick) so I went up there and I remember being in his bedroom and telling him we've got to decide...I came back and told the other engineers that I was talking with John and John thinks this is the way to do it and I can't see any other way of doing it. I said, "I can see another way of doing it if we knew what this machine was going to look like." But you must realize that we were building this machine in a funny way in that the Army kept adding to the requirements of the machine as we went along. We had to build a programming system which could be expanded with the machine as we went along. So, the ENIAC is a peculiar machine and it's the only machine that we ever built that didn't have centralized programming. It had what I would call a distributed programming system--which I would have later called local control (which has later been called local control and many of the original patents are written for this principle of local control and nobody could use it much in later machines). Now the curious thing is that many of our machines that we now build have a number of micro-processors--for example, the small office machine actually has three processors. And many of our larger

machines have several processors in them. In a sense, we've localized the control to those areas even though those individual processors are centrally controlled devices--they are locally distributed. As one goes to higher speed machines, we are having to put more control functions locally and we may see that process going even farther in the future than it has today. But we did it for two reasons in the ENIAC. One of the reasons for the local control was that we didn't know what we were going to come up against next. We had to add control with each new thing that came up, but we couldn't built it in advance. We were afraid because the machine was physically large. Because, we were afraid of the long lengths of the leads, we only sent a single pulse to get something started; thus, the time lost would only be lost once...whereas if we had to go back to the central control unit and each tiny little individual gate decision...the time lost would have been largely due to the size of the machine. That is exactly the same reasons for the local control for the future. If the machine is any bigger the signals couldn't get there in time.

STERN: That's right, I did remember reading about that.

ECKERT: It takes a nanosecond for electricity to go in a wire about 6 inches.

STERN: You said that a physicist is looking for the "truth" and an engineer more or less isn't interested in that but in getting the thing to work.

ECKERT: The truth is getting it to work...of course, in real life things are broader. There are not physicists, there are not engineers, there are people that are of different percentages that you grade together.

STERN: There's no black and white... In your relationship with Mauchly, would this particular distinction you talk about be applicable?

ECKERT: Well, it's always been true that I was interested in the details of how you go about calculating the specific design pieces to make things work. And in fact, in that original digital machine, I took every engineer's work and checked every calculation of every resistor in the machine to make sure that it was done correctly. Normally, I wouldn't want to have to do that. But this was the first time for a machine with on the order of 100 times as many

tubes as anybody has ever built electronically. And if it was going to work, one had to be 100 times mo re careful.

STERN: What was the maximum...

ECKERT: 100 times the peak.

STERN: The peak was what...

ECKERT: Well, I'd say the biggest thing that I ever heard about anybody building was a counter that Wally Higginbottom(?) built at Los Alamos (I think that's his name)...he had a couple 100 tubes.

STERN: That's what I thought...a couple 100.

ECKERT: Now there was one other thing, and I still have at home in my living room an old organ designed about 1938 built by Hammond--the idea of the Hammond organ goes back before it was invented. That machine had about 144 tubes in the generator plus 15 more--about 160 tubes in it--and they built 1,000 of those machines starting around 1938 or '39 and they were called novachord. The novachord was the first tone modulating electronic instrument for chords. That was quite a roadmark in that it was the only commercial thing I ever knew that they put out (these 1,000 novachords put out by Hammond) with as many tubes in it. So me TV sets had 30 tubes.

STERN: You said you were mainly interested in getting things done. When you had a problem you would go to Mauchly. He also was interested in getting things done. So would you call him more of an engineer with respect to development?

ECKERT: What we were doing was an engineering job. Certainly when he proposed ideas for building a box to control this thing he was doing...I guess engineering. But he was also doing something basic that hadn't been done before when he proposed subroutines.

STERN: What else can you tell me of his contributions during this period?

ECKERT: Oh well, there was a guy by the name of Sharpless who, in one of the most critical times in the project, went on vacation without telling me. His boss said he was going to do it. He left a note in his mail box and said, "I'll be back in a week," or something and the big problem was that he was working on the central cycling unit. That first panel sent patterns of pulses around to all the other units and synchronized them together. We were really stuck. Without it we couldn't test anything else. So Mauchly and I personally (more or less) put down what else we were doing in the project and tool a soldering iron and made that thing work ourselves. So he did do things like that but that was not the general mode of his operation or mine. My general mode of operation was to go around and find out every morning what people were doing and what their problems were and make suggestions on how they might solve them. If the problems were too great, we'd call a meeting and get everybody's ideas. And then we had the everyday things of making sure we had enough wire, and parts. An awful lot of time was spent with Goldstine and we'd look at books and find out if we can find some couple thousand tubes that the Army didn't need. We used 10 different types of tubes in that machine. I could have done it with 4 or 5 but I couldn't get enough of certain types so I would find a tube that I could get and then I would just find a circuit that it seemed to fit best and probably got some gain. But it probably took more time because I had to design more circuits.

STERN: What about the mathematical aspects of the machine in terms of things like round out errors. I know that Mauchly was responsible for the particular method used with the machine, the Huan method I believe it was called. He claims he developed the method--what he says is he developed this method that was better than the Runge-Kutta method for round off.

ECKERT: Well, Runge-Kutta isn't a method of round off--Runge-Kutta is a method of integrating.

STERN: To produce fewer round off errors?

ECKERT: No, I believe that Runge-Kutta refers to a method of integration which requires fewer steps and consequently your round off error isn't as severe simply because there's less truncation taking place. If a problem can be done in 200 rather than 2,000 integrations than your round off error isn't as severe because you don't accumulate as much error from step to step.

STERN: But I think the Huan method of preferable in the ENIAC.

ECKERT: As I recall the Huan method had a worse round error and took more steps but was a simpler formula and therefore didn't tie up so much programming equipment to do it.

STERN: So that is the question of not only having the mathematical knowledge but the knowledge of the machine to be able to go in and figure out the best kind of method to use.

ECKERT: You might have several formulas for doing something. One formula might be better in that, if you were doing it by hand, it would take a fewer number of calculations. But the calculations involved might be such that you would run out of programming equipment on the original ENIAC before you could use that method. In which case you would look for a math method which took more steps, more iterations, but in which formulas were simpler...didn't take as much programming. We had to balance the two requirements. Programming steps were very expensive to come by. It took boxes and cables and things. Doing something a second time or ten times reiterating something over, was very cheap because we were a thousand times faster than our predecessors. And our predecessor was 10 times faster than a desk calculator which was ten times faster than man...so we were 100,000 times faster than a human being. And therefore doing something a lot of times was cheap. Fancier formulas were tough so we looked for methods which used simpler formulas and we went after them more ferociously. And I guess John was concerned with picking which of the methods of integration would be most suitable in terms of the restrictions on our machine.

STERN: So in terms of mathematical input, that's certainly a consideration.

ECKERT: He did this. I did not concern myself with this. He was not the only person who concerned himself with this. There was Herman, possibly Mrs. Goldstine, possibly John's wife.

STERN: How about Burks?

ECKERT: It's possible that Arthur looked at that problem. There was also some girl programmers who looked at things like that, too. I would suspect that Mauchly would be the first but I don't remember. I don't recall any particular discoveries being made. I recall that as being mostly a matter of looking at what people had developed and picking the thing most suitable for our use rather than inventing a new formula.

STERN: Mauchly said he worked on it, and he did come up with a formula that he thought was really suitable. And then later it was found that this was actually on the shelf.

ECKERT: Quite possibly.

STERN: Can we talk a little about von Neumann?

ECKERT: Yes.

STERN: According to the documents I have read, von Neumann came to the Moore School in September, 1944.

ECKERT: Well, to find out when he came, you'd have to check with the Legal Department. Because when Goldstine said he came, and when he actually came, we found from the clearance were different. By the way, Mauchly went out to some meeting on history and I think may have mentioned that in a speech out there if you have a copy.

STERN: When I spoke to him, he did not bring up that point.

ECKERT: I gave him the documents to take out there. I was supposed to speak, too, but I refused to go because we were supposed to be introduced by Nick Metropolis.

STERN: Oh, that's the Los Alamos Conference.

ECKERT: And the last minute they--Nick called me up and said that, "We changed things around and you and John are going to be introduced by Brainerd." And I said, "Like fun, I'm going to be introduced by Brainerd," and I said, "You can either rearrange your schedule or I don't come." So it was too late to change his schedule, and I said, "Well, it's not too late for me not to come." However, I wrote the speech and gave it to John to present with his own and in this speech this point is brought up. It is my understanding from one of our attorneys who was at the speech and worked on this case--and is familiar with this thing, that he made it clear.

STERN: Cleaver?

ECKERT: Yes, Cleaver would have that information.

STERN: When von Neumann came, did it lend any kind of prestige to the project?

ECKERT: Well, in the first place, I was not familiar with great mathematicians so I hadn't heard of him. Von Neumann didn't mean any more to me than Joe Apple or something. I know Goldstine was very impressed. Goldstine was pretty impressed with everybody--I didn't know what to make of that. In fact, that was a kind of joke--that Goldstine was impressed with everybody all the time. And, so it didn't mean very much. I got to know von Neumann and I thought he was very quick mentally in mathematics and things. He grasped what we were doing quite quickly. I didn't know he was going to go out and more or less claim it as his own. He not only did that, but he did it at the time when the material was classified, and I was not allowed to go out and make speeches about it. And he went out and made them anyway without clearance and got out of it because nobody wanted to come down with the Espionage

Act on a prestigious guy. If I had done it, they would have come down on me with a ton of bricks. So he just used his prestige to pull a Pentagon paper deal as far as I'm concerned.

STERN: This was called a draft report. Now is it possible that anyone within the Moore School....

ECKERT: It wasn't even a draft when he wrote it. He wrote these as letters to Goldstine, and when we asked what he was doing this for at the time, Goldstine said, "He's just trying to get these things clear in his own mind and he's done it by writing me letters so that we can write back if he hasn't understood it properly." That's the basis in which he wrote it.

STERN: So it was never made clear that this was supposed to be an informal report.

ECKERT: I told you what it was.

STERN: So it came as a complete surprise?

ECKERT: You know, we finally regarded von Neumann as a huckster of other people's ideas with Goldstine as his principle mission salesman. Now, if you don't believe this, talk to Julian Bigelow at the Institute for Advanced Study who holds a position that Einstein held during his life. He is a person of some reputation. I was going to work with von Neumann at the Institute part time and we were supposed to have a three-way deal with the Moore School, but von Neumann got into terrible fights with Dr. Pender, and I couldn't work.

STERN: Over what?

ECKERT: Von Neumann was stealing ideas and trying to pretend work done at the Moore School was work he had done.

STERN: Over matters like the *New York Times* leak and things like that...I assume you mean?

ECKERT: Yes.

STERN: Now, there's no records anywhere that the Moore School was ever to get involved with the IAS on a computer, and when I asked people like Brainerd and Travis, no one knows about it. Could you account for that?
ECKERT: I talked to Pender and we agreed to do it only if Brainerd was excluded from the operation, and that's why he doesn't want to remember it. It was supposed to be handled through somebody else like Warren.

STERN: Why Warren? Warren had no experience whatever with computers.

ECKERT: Because the guy was honest.

STERN: And that's why he was chosen as a supervisor of EDVAC?

ECKERT: Yes. Comparably honest. He was sort of a soft noodle as it turned out...an honest noodle that could bend too easily. Didn't do anything. But at least he was honest.

STERN: What could he have done with respect to von Neumann?

ECKERT: Well, what we were trying to do was kind of a 3-way deal with Princeton, and the Institute for Advanced Study. I think Tukey was at Princeton and von Neumann was at the Institute. And John and I were at Penn and we thought we could work a 3-way deal out. And we were going to work on the I/O equipment at the University of Penn, because we had more mechanical figuring facilities and we had a mechanical engineering school and we thought mechanical problems would be involved, and it was an appropriate place. We thought Princeton would be good to do the programming work because they had good people like Tukey and we thought that we would actually build the central processor at the Institute for Advanced Study. I was going to travel back and forth between the three

institutions because they're not too far apart, and be in charge of the three. That was to be my task. Von Neumann did two things which infuriated Dr. Pender. He tried to make a fourth partner into this: RCA Corporation, involving Zworykin. Well, Carl Chambers had worked in the laboratory at RCA when Zworykin supposedly invented the iconoscope and knew Zworykin couldn't be trusted, to be quite blunt. And there were two people that the Dean trusted in the Moore School--one was Carl and the other was Brainerd. When I say trusted, I should say "depended on" would be the better phrase than trusted. Carl thought this deal with RCA was bad. In other words, if you have three educational institutions and one commercial institution, you'd have nothing but a fight on your hands because the commercial institution would be ripping off the educational institutions all the time and there'd be all hell to raise about it.

STERN: How do you account for the fact that von Neumann would be interested in a deal with a commercial institution, but yet objected to yours and Mauchly's ideas about commercialization?

ECKERT: I don't think he was a sincere person.

STERN: That certainly would account for it.

ECKERT: Look, he sold all our ideas through the back door to IBM as a consultant for them.
Well, there were very different estimates as to how much money he made but some people claim that he made as much as half a million dollars by consulting with them. Julian Bigelow who was close to him said it was less than that, but that it was substantial.

STERN: There are reports at the Library of Congress which have the actual contracts with all the companies.

ECKERT: So he spoke with a forked tongue. He said one thing and did something else. He was not to be trusted. You know, I think there's a funny story...It was a kind of a joke. Von Neumann and myself, Mauchly and Goldstine and a bunch of other people, I can't remember, were going up to Princeton or someplace. On our way, we met at the

Pennsylvania Railroad Station in downtown Philadelphia. We had breakfast or something. No, I guess this was coming back because we had dinner out. We were coming back and the waitress finally brought a single check for everybody. That is one check instead of individual checks. And so they said, Von Neumann you're the well-known mathematician here, you resolve the check. It was a joke. So she handed him the check, and he looked at the check and I don't know what he had to eat--some fried oysters or something--anyway, he looked at it and said, "They have left my fried oysters off the check. This raises an ethical question. The ethical question, of course, is whether to divide the savings between the rest of you or take it all for myself."

STERN: Did he say that?

ECKERT: Now, he said this as a joke, of course. But in some of the later things that happened, I often wonder if it was a joke.

STERN: To get back to a question I wanted to ask you before...how do you account for Brainerd's particular aversion toward Mauchly from about '44 on?

ECKERT: Very, very simple. Brainerd was a greasy grind, you recall. I'm not sure if you're familiar with that expression. It was an expression in college for a guy that worked all the time. Not particularly attractive to the rest of the individuals in the place. Mauchly was just the opposite. He was always kidding and joking with people. And after class all his students would come in and talk to him. In their spare time, they liked to be around him. He was personable. He probably wouldn't get his test marks in on time. Brainerd would have to get after him. He probably wouldn't get reports and details the university required and Brainerd would have to get after him. From that point of view, he was a poor administrator. He was the exact antithesis--he was brilliant in his ideas where Brainerd was not so, really. Brainerd had to study everything like mad to get it. Mauchly used to look through a book and read through it quickly and get the idea immediately. Brainerd used to take ten times longer to get the same material. It's not hard to see how two people like that wouldn't like each other.

STERN: Except that initially, that was not the case. Brainerd claimed that he was responsible for getting Mauchly to the faculty of the Moore School to begin with.

ECKERT: That was probably true. It was very hard to find decent people. All the good ones were drafted, so that anyone who looked good you were happy to hire.

STERN: The personality thing comes out afterward, I guess.

ECKERT: Of course, you don't realize that these differences are in people. There's no way you can learn that in a short time. If you're going to run a business, to make a profit you would not hire Mauchly, you would hire Brainerd. You understand.

STERN: How do you account for the fact that von Neumann would be interested in a deal with a commercial institution, but yet objected to yours and Mauchly's ideas about commercialization?

ECKERT: I don't think he was a sincere person.

STERN: That certainly would account for it.

ECKERT: Look, he sold all our ideas through the back door to IBM as a consultant for them. Well, there were very different estimates as to how much money he made but some people claim that he made as much as half a million dollars by consulting with them. Julian Bigelow who was close to him said it was less than that, but that it was substantial.

STERN: No, I don't understand that. Now that surprises me that you would say that.

ECKERT: Because business for the most part is not being brilliant. It's a matter of taking routine matters and taking

care of problems.

STERN: From an administrative point of view, I understand what you say, but Mauchly seemed to me to have...

ECKERT: It depends on what you mean by administrator. An administrator is supposed to have two characteris tics: one is leadership and the other is the ability to get routine work done properly and efficiently.

STERN: Well, you would say that Brainerd had leadership.

ECKERT: No. But if I were starting a business, you need many more of the Brainerds than you do of the leadership guy.

STERN: But you need a leadership guy.

ECKERT: Yes, sure. You'd like to find somebody that had both of these properties in one guy, but that's very hard to do.

STERN: Also, in terms of that statement about business, it seems to me Mauchly more than anybody else through this whole thing had the ability...

ECKERT: You see, Mauchly inspired people. Brainerd discouraged them, that's another way to put it. Well, I told you about those girls sweating it out in this room, and instead of saying well, "I'll get an air conditioner," he'd tell them to leave the door of their refrigerator open. He didn't even make a try at solving their problem. It's a trivial matter, but life is made up of a whole concentration of trivial matters. Certainly a computer is nothing but a huge concentration of trivial matters. It differs from a lot of other inventions in that respect.

STERN: Also, to start a business, well, even before starting a business, it seems to me Mauchly's ability was as an

innovator, if I can separate innovation and invention. And that is a particular thing you do need as a businessman.

ECKERT: It's easy to get the business started, but once you've got it started, that's a different matter...look, I can take most of the innovative businesses that I know of and show you that the innovators didn't last a great length of time. They aren't there anymore. The man who invented xerography had to huckster all over the place for 10 years before he got anybody to buy. And the only reason they bought was they were going down the drain, because Eastman Kodak was putting Haloid out of business and this was the last thing that they were going to try. The last grasp at a straw that they were pulling down for the last time and it happened to be the greatest straw in the world for them.

STERN: Aside from the problem of inertia, that is, where companies don't want change, what else accounts for the fact that innovators don't make it?

ECKERT: Innovators keep destroying good plans, good business plans.

STERN: Can you elaborate on that?

ECKERT: Sure, suppose I come in tomorrow and say we got together back in the lab there a new printer which can be built for 3/4 of our present printers...It doesn't use the mechanical hammer, but it uses a laser instead. This is a true story I'm telling you. It's a fight I'm having right now. And so on and so on. But you'll have to redesign all the interface circuits on all your computers. We've got a stock of $4 million worth of printer parts that will be jeopardized. The field engineer has to be taught a whole new bag of tricks--they've never seen the laser before. They have no experience at all in maintaining copy type material, copy type technology. I want you people to consider putting this printer in our line. "Hold on!" Everybody says from every department in the company. Whether they train the servicemen or inventory the parts or what, I'm just making a mess for everyone. Screwing the works up for everyone involved. In spite of that, we have managed to have innovations put through. It takes long tedious hours.

STERN: When I use the word innovator, I also include the idea of being able to take a particular technological advance and recognize where it would be most useful.

ECKERT: I wrote this report not too long ago...It flies in the face of the way IBM is doing something. Our people are now visibly copying the IBM machines, and management would have none of it. I've got about ten memos now, I've got to write answers to them, about which every kind of nit-picking complaint about what's in this thing is written. I have a drawer over there which is full of the answers to all of those things that I wrote before I wrote this memo, knowing that all these things would come back. Actually, I'll wait a little before I'll answer them, because I'll wait til they start to have a little trouble with the IBM parts and then they'll be more interested. IBM has a machine which is called the 38/50--it's a spool of tape. This is a proposal for a machine which has a double spool, something like a cassette. It takes the place of a single spool that IBM has and they're put on wheels. It goes on wheels instead of in a regular frame the way IBM has it. It's a series of wheels and it's a totally different way. Millions of dollars to develop. Totally different way of approaching things. Now, when I wrote this I knew that my chance of having this accepted with this company is 1 or 2 percent.

STERN: Speaking of IBM, IBM's involvement with your work was just with respect to supplying some I/O equipment to go with the ENIAC, as far as I know. They sent engineers over. Were they interested? Did they send the engineers over to get information...

ECKERT: Number one, we wouldn't let them in to see anything that we were doing, because they'd steal it. Number two, all they did was take stock machines and rearrange the wiring on them. A trivial modification. In one case they screwed the wiring up so badly that the machine we wanted was much closer to the original machine than what they sent us. We had to get the wiring diagram and wire most of it back into the original machine to make it work. That was done by Harry Huskey. Number three, you know they scheduled this machine and then when it was supposed to be delivered they wouldn't deliver it. And Goldstine had to threaten to send troops up to take it out of the factory to get it. Under public law, I think it was Public Law 700 or something like that. Did you know about it?

STERN: No.

ECKERT: After they had the machine all done and they knew we were ready to hook it on, and we're panting you know and dripping at the mouth for this machine, they said, well, they didn't want to send us this machine unless we would agree to giving them a royalty free license on what we were doing and so on and so on. And we didn't have to do it. There were times they'd deal with us the way they would deal with their business competitors and other business people and in this case they were dealing with the government under emergency laws under Section 700.

STERN: Big companies like IBM, but IBM specifically, never got involved with computers until they were fully developed.

ECKERT: Well, they built the MARK I. Howard Aiken's dead, so I guess you haven't interviewed him.

STERN: No, I read some of his interviews, though.

ECKERT: Aiken hated IBM in the end, and it was a terrible feeling there. The wouldn't even speak to them for years. Then he finally ironed it out. He was a kind of a forceful guy--a beautiful speaker. You'd go and listen to his speech and everything was clear-cut and perfect. You'd come out and say, boy that was a beautiful speech, I really understand it. Three days later, you'd realize that everything the guy said was screwed up, and that that was his distorted view of life that you were getting. But he was so good that it took days to realize that you'd been conned. He came out and said that if you built six UNIVAC's like the ones proposed for UNIVAC I, it would fulfill all the computer requirements in the United States and there wouldn't be need for any more than six. He apologized for saying that later. It turned out that he's one of the nicest guys and went around to all our people who wanted to install machines to recommend them. We didn't pay them or anything. He told them they should buy our machines, they were good. It seems he did a turnaround. That was very good. In the beginning, he was very hostile.

STERN: My understanding is that Aiken made his statement about six computers with respect to the NRC Report to

the Bureau of Standards. The Bureau of Standards asked the National Research Council for a report. Aiken was on that committee and Aiken said this to...

ECKERT: He said this in a public speech.

STERN: Oh, in addition, I didn't know that...Where? Do you remember?

ECKERT: That's hearsay. I didn't hear the speech. Somebody told me that they heard it in a speech, now maybe I've got that wrong.

STERN: But that's my recollection of it. Edward Cannon's testimony in the trial said that he got this information in the NRC Report. But what I found interesting...

ECKERT: That may be true, but he was also making speeches.

STERN: But he also said he thought that instead of making large computers money should be spent to build desk-size types of electronic computers. I thought that interesting in light of recent developments.

ECKERT: So did I, incidentally, and we did. We built a desk-size machine that we could have put out for $5,000. Studies were run to sell to Metropolitan Life. But we just couldn't get it cheap enough, and simple enough at that time. IT had to await the invention of the planar process by Noyce. Do you know who invented the process that really allowed all these integrated circuits to happen? Noyce at Intel. Not many people have a very clear concept of that, either. I happen to know because one of my best friends happened to be the patent attorney who filed. The integrated circuits, of course, have 100 inventions that are put together but the guy who really pulled it together more than anybody else and should get more of the credit is Noyce. He's president of INTEL.

STERN: Yes, I know about Intel, I didn't know the man's name.

ECKERT: Gordon Moore and Noyce left. You see he was vice president in charge of Fairchild. And he left Fairchild. I was at Fairchild the day he left. He left to start Intel. That was a number of years ago. And Intel in MOS technology I think is ahead of everybody else. I know they're ahead of everybody else because they've gotten some secret things that they told us that we're working on with them. But they haven't told anybody else.

STERN: In terms of the discussion we've been having about innovators, what I meant was that IBM did not get involved with electronic computers until late.

ECKERT: They built some kind of a big bluff over in--what is it? 500 something? 5000? I think they built some big monstrosity over there that I don't think ever worked right.

STERN: The SSEC, is that the one you're talking about?

ECKERT: They built it there, right at Madison Avenue.

STERN: But no effort was made to commercially produce them.

ECKERT: I guess the first thing they came out with was 601. Then the 604.

STERN: Which was late, considering. It's my judgment that lots of these large companies do operate by inertia and they are very resistant to change. Yet it's the innovators who operate in a small company who have the idea. But it's almost as if the deck is stacked and it can't work. That is, you can't make a go of it against these big companies.

ECKERT: That's right. Well, first, what do you mean by make it work? We have like 7% of the business which turns out to be--well, over a billion dollars.

STERN: When you say "we" you're talking about Sperry Univac. I'm talking about Eckert-Mauchly way back, you know.

ECKERT: Well, we were little, compared to IBM. We were all long. Even then Rand didn't have that much money. But it took $300 million before we could turn around and make a profit. Now, a lot of that was putting in idiot people that I had to work for as presidents. And the problem is that you couldn't be the president someplace like UNIVAC and also be a person pushing the technology at the same time. It's quite a large area to cover. And the people they kept getting in were former salesmen from IBM. If they had gotten a good one, all right. If they didn't, it could get bad. And we went through a series of bad presidents, which probably cost us. But it still would have taken 100 million dollars to bounce us back even more these days, of course. And there are many people of course who put more than that into it and didn't succeed in establishing a business.

STERN: Back to von Neumann. You called him a huckster who took other people's ideas. Most of the work he did...

ECKERT: Bigelow may not use as polite terms as I did. You will get the same story from Julian Bigelow. What he did to Julian was kind of interesting. We had a deal with the University of Pennsylvania...the patents go to the individual inventors, except for the rights to the Moore School, institutions in government, and other universities, where we saw reasons for them having to pay. The same deal was to be made at the Institute for Advanced Study. I was to be in charge of the project there as well as the one at Penn. Then when I didn't go there because of the fight with Dr. Pender, Goldstine, I remember, called and gave me a long tirade over the phone that I should be working for our own company rather than the University. It was the worst thing in the world anyone could ever do and he would never work for a company. After the incident, von Neumann followed through on the proposal that I had originally made--that all the employees that he hired be given this same right there. That we had at Penn. Originally we called ENIAC the "MANIAC" when it didn't work right. And later they borrowed that name for some other actual machine. But if you worked for von Neumann on the MANIAC, then if you invented something it belonged to you. Well, on some relatively short notice, like it might have been a week, or a month, or something, a short time before the

deadlines hit, von Neumann went down and published all that stuff. All the reports of the engineers went to the Library of Congress which put a bar on any patents being obtained by any of his employees. And when they complained about it to him, he just said, "Well, that's tough; that's the way I think; that stuff should be in the public domain." Now there is a perfectly obvious reason for this. He was consulting with people like IBM. If the things weren't patented that would be a problem for IBM. The idea was he was selling ideas to other people...if it wasn't covered by patents, he would have been selling something they couldn't use. They would have come back and said, now, "what kind of a consultant are you, coming up with new ideas which are already patented by others and we can't use them." But if these ideas could come under the public domain, then he could go around and sell them to people. That was his game. Now this can be verified by you by going up and talking with Dr. Bigelow, who will tell you perhaps in different words and he certainly has a better memory for the circumstances. He can tell you more precisely what was involved because he went through it and I didn't. This is what happened to him and to other people. It happened to one of my classmates, Willis Ware, at the Rand Corporation. He was there and I had tried to get Willis to work for me and I didn't know that he went there even though I tried to get him there. There were a number of people that I was encouraging to go there.

STERN: Why did Burks leave? Do you know?

ECKERT: Leave what?

STERN: He started to work in the Institute by March of '46 and by September he was back at the University of Michigan.

ECKERT: One never felt that Burks' heart was in engineering, or computing. One felt that theoretical philosophy--which is where he started out--was his bag. Burks was a very bright and clever guy. Burks and Mauchly got along somewhat all right but there was some kind of jealousy between their wives and even between them. There was some personal problems. The problem spilled over onto me only indirectly. It was really aimed at Mauchly. There's a lot of people who considered that Mauchly was a loafer, which is one way of putting it. Because

Mauchly was sitting around talking to people and projecting ideas and not projecting the Christian ethic--the Protestant ethic--that hard work and this, that and the other thing are what makes the world go around, you see. Mauchly does not project that ethic. He projects a different ethic.

STERN: I've spoken to him and he is not somewhat cynical about the whole thing. It's hard for me to know what he was really like then, because I think his cynicism masks a lot.

ECKERT: He was woefully in debt, because he couldn't get any money to do anything. If you're in science, everything you want to do is blocked. Here's another guy with a pretty high IQ who finds the world pretty difficult to live in.

STERN: you seem to be able to...It's a different thing; you can cope with it...

ECKERT: I know his background. John's father was also a theoretician. He was the head of the Department of Terrestrial Magnetism at Carnegie. We had a different background. My father was the guy whose mother died when he was 15; he went to high school for one day and he got his high school education by mail order. He got his college and law degree by going to night school. He started in business with $50. One time in his life he was a millionaire. That was back in the days when a million dollars was a lot of money. And he was known by the rest of the family as "Johnny Rusher." He fits the definition of an engineer although he was not an engineer. But he was a builder--be built 100 buildings. I have a totally different background. My father was a businessman; he had a car agency and different things.

STERN: I knew he was a businessman; I didn't know about his background. That makes for a different...

ECKERT: That makes him self-made. A real Horatio Alger tradition. He fits the "American Dream" story very much. For inventors in my life, you have to go back to my great grandparents; they were Scotch when they came to the U.S. The boardwalk--that machine that twisted taffy--my great-grandfather invented that.

STERN: Really? I never knew that; that's very interesting.

ECKERT: He also made a lot of money; he was a Scotchman.

STERN: That's an interesting story; I'll have to look that up.

ECKERT: I think what happened to von Neumann was that he got scooped by somebody...He became cynical and I think probably in a very honest way, he decided he's not going to let any grass grow under his feet and that he'll get ahead before anybody else gets ahead of him the next time around. I think that's what motivated him by the time we met him. Otherwise it's hard to explain because of his background being a middle class background with no hardships or anything. No other way to explain why he behaved the way he did. Most people only behave the way von Neumann did because they are insecure for some reason. Von Neumann had no realistic reason to be insecure. And so there had to be some other reason for it. I don't believe that everything can be explained by Freudian reasoning but I think to a certain extent that you can explain people's behavior by their background.

STERN: The obituaries on von Neumann written by some mathematician also indicates to me, from looking through his record, that mostly everything that was written by him from the war time on was coauthored. Even his mathematical work...he would come up with important ideas but...

ECKERT: I think he was a damn fast guy for figuring out what the other guy was doing and explaining it better.
STERN: Not only explaining it better, but associating his name with an idea conceived by some mathematician 5 years out of school makes quite a difference to that mathematician. So that what happens is that these mathematicians who worked in such a relationship with von Neumann praised him for this association.

ECKERT: They've got a vested interest in praising him.

STERN: But the engineers look at the same phenomena in an entirely different way. I find it very interesting. you know that it's not part of the engineering norm to do what von Neumann did but it is part of the mathematical norm to do that. So that it's simply a different way of looking at the same thing.

ECKERT: Well, yes. The engineers cooperate but not in the same way. You see what von Neumann did is simply take the ideas that I had, some of which were written up and some of a couple pages of typing, written even before I met him. Let me tell you some other little anecdotes about von Neumann. One time von Neumann had insisted that you couldn't do what we call minimum latency programming. Because I had proposed that we do that and he proposed that we couldn't do it--it actually influenced UNIVAC development quite a bit. I was going to put 20-30 words of memory in the tanks to make the tanks longer which would double the memory in the machine and he talked me into having less memory in the machine because he said he didn't think anybody could do minimum latency programming. Later on, Dr. Grace Hopper worked on an algorithm which proved that von Neumann's impeccable taste is not very impeccable. Another big argument ensued when the LARC was built. This argument was between von Neumann and Edward Teller, in which Teller wanted a 20,000 word memory or bigger in the LARC. They were big words in the LARC and von Neumann said that there's no sense in ever building a machine with more than a 10,000 word internal memory...that all the rest could be on tape or in external form and that it's just a waste of money to put more than 10,000 words of memory in a big computer. The first story is kind of a small scale thing, because minimum latency coding with drums and tanks went out of existence anyway and that story's a kind of a dead horse. But on the second story he was so far off base that it isn't even funny. I never believed his predictions and neither did Teller. Teller took the same position I did. Of course, I could be accused of being biased because I was selling machines but Teller took the position you should use all the money your dollars could get your hands on.

STERN: Did von Neumann make any contributions to the Stored Program concept at all?

ECKERT: He brought up questions that we evaluated but he didn't answer them. He brought up the question of whether we should have one, two, or three addresses in our orders and we did have a great deal of discussion about that, but he never came up with any clear-cut way of answering it except that he thought three addresses were a little

easier to think about. He did question the--well, here again it's not clear--he came in with some ideas on circuits curiously enough at one time which influenced my thinking on BINAC and UNIVAC. In the earlier machines I had always used signals to set clock pulses and then I released the pulse from a flip-flop in the clock. I released the signal from a flip-flop of a clock pulse in a gate layer which means I always did a reshaping with a gate from the flip-flop. And he pointed out one day that you could do reshaping purely by sampling without necessarily having a flip-flop and do it in other ways. In a modified form, I did that in the UNIVAC's and the BINAC to influence my circuit thinking and since then modern circuitry is still influenced by that thing. But I found out that he got that idea from Wally Higginbottom and so I don't know to what extent he thought of it but at least he understood the problem. He talked to Higginbottom and brought it to me. So he had that influence on my thinking which was helpful. I found him a great sounding board to talk to because he understood what you were saying to him, and he could sound ideas out but I didn't really find him terribly inventive. Certainly very knowledgeable about mathematics and things but not very inventive. You heard of the big screw up we had on the round off problems with Rademacher where he solved the wrong problem the first time around?

STERN: Yes, Mauchly told me about that.

ECKERT: Rademacher was a great guy. I really liked Rademacher. In every third sentence he would say, "clearly" and his voice would break.

STERN: "Clearly" is a favorite word with mathematicians, anyway.

ECKERT: This is the tough one that you're not going to understand. And he said, "Clearly."

STERN: Or "obviously." When Travis returned in '46 and insisted that you sign that patent agreement, did he recognize that he was going to lose you?

ECKERT: Yes, we told him right off. Didn't Mauchly tell you that story? Well, he said, "You've got to sign this

you see." And so we said, "We're not going to sign anything." And he said, "Well, we'll talk about it later." So like a month goes by and he said, "You've got to sign this." And we said, "Look, really, we're not going to sign it, so if you want us to resign, we'll resign." "Well, I'll give you another month to think about it." "Well, there's no point in giving us another month to think about it," I said, "we're not going to sign it." "Well, I'll give you another month." And in another month, he comes back and says, "This time you've really got to sign it and I can't give you anymore time." "We don't need anymore time, do you want us to leave today?" "No, well, I'll give you another week." So he gives us another week and then another day and then another hour. He really went down to all these subdivisions. He was trying to set up a tough situation but he didn't really want it to happen and so then finally in the end he called his own bluff. Now I have a letter from Pender in the safety deposit box which says I resigned; but I was fired, by Irv Travis.

STERN: There's a letter on record--I'm not sure if you signed it--I'm certain that there's a letter from Mauchly saying that "we cannot accept these circumstances therefore I resign."

ECKERT: No, I'm talking about a letter from Dean Pender.

STERN: Firing you?

ECKERT: No, accepting my resignation. But it's a bunch of hooey to show to somebody else if I ever wanted to show it. Which I never had any occasion to. But I was in fact fired. It was covered up with a polite letter accepting my resignation. But I didn't resign.

STERN: Why would Pender go along with this?

ECKERT: Well, I was much younger than Travis; also Mauchly I think was not happily looked upon by the people because of his work habits. And Travis was incidentally the best teacher in the college and was a valuable person for Pender to have back on the teaching staff. He had a good reputation for the administrative work he had done in

the Navy and I think he realized that Travis was a better administrator than Brainerd and I think he thought of Travis as the next Dean in place of himself. He didn't seem to realize certain things about Travis --there were instabilities in Travis' life and relationship to his wife.

STERN: His wife now is his second wife I take it?

ECKERT: That was going to happen, he was running around with other people which I don't think the Dean knew about. He was in danger of becoming an alcoholic and other things I don't think the Dean knew about. But any rate, I think the Dean not knowing these things, felt that he had a great future. Later, he got a pretty good job at Burroughs.

STERN: Still with Pender's background and Pender's appreciation for commercial interest...

ECKERT: Well, he'd given Travis the idea of organizing the research program and this was one of Travis' schemes for doing it and I guess he was also a good enough administrator to realize you can't hire somebody and give them a job and tell them how to do it. I can see that there are two sides to this. I didn't particularly fault Pender for standing behind the man put in the job, if he trusted him. The guy might be wrong in which case he would fall. As it turns out he was wrong in my opinion and fell with him. I was in the process of trying to get a million dollar contract to do computer research with GE in which GE only wanted in return the rights to use the computer to do turban analysis and nothing else. They were willing to give all the rights back to everybody else.

STERN: Well, didn't that matter to anybody? And here is a man who knows all about contracts and not only in terms of commercial...

ECKERT: By that time, some of the fuddy duddy people were in control. The head of the University at that time was an English professor. He started worrying about whether it was really ethical for the University to accept money from GE and this, that and the other--so I went over to the medical school and I talked to them and they had one argument there that is, generally speaking--they said we accept money all the time from drug companies to do research and

once they had an argument over this. Keeping it proprietary was, they felt, keeping the public from getting enough use of it at reasonable rates. They developed one argument and they said the administration had been hung up on it ever since. In spite of that, the hospital is the most successful part of the University of Pennsylvania--always was and still is. I was just too young and didn't have enough prestige to put that over and Pender should have solved that problem. What it amounts to is that Travis saw them building up a big research operation...so, Travis really avoided all this. Anyway, we had just turned down GE and then Travis came in six months later and took things that were poorer than that. So it didn't make much sense. The conclusion I came to from being at the University of Penn. is the universities just aren't responsible in a business sense if my experience at the University of Pennsylvania were to be typical of the universities. It's just relatively an irresponsible organization, in the business sense. The Medical School was not run this way. But the Medical School had a bunch of smart guys running it who knew how to field what they were really doing from the management of the University. The management of the University didn't realize what was happening and that was the secret of their success. I happen to have a very good friend who was one of the people who did that--at the time he'd tell me all the things they did which was of real value to the management of the University. But the management didn't know what they were doing in the Medical School.

STERN: Interesting. It really is! What about government contracts? Did the University have any objections to accepting postwar government contracts at that time?

ECKERT: No, we were talking about accepting contracts from people like GE.

STERN: But here it was clear that the building of computers for government work would certainly be a lucrative thing for the University to do.

ECKERT: Well, these people were acting in very ivory towers. Don't forget von Neumann and Goldstine were saying you shouldn't have anything to do with companies because companies got ideas and suppressed them and didn't promote them. The only way real developments occurred during the war was by doing them at the universities.

STERN: But von Neumann was an academic. These people are engineers. There is some distinction even if the engineers are academic also.

ECKERT: Yes, except that Pender of course was an engineer and started a successful company. But he also had a doctor's degree from a very good academic background and worked with very good academic people at Hopkins. So he was really split right down the middle. He balanced right in the middle and he felt some way sometimes and some way another--you couldn't predict it.

STERN: That's revealing.

ECKERT: He was a rare combination. He had a great sense of humor.

STERN: It's interesting. Everybody has positive things to say about Pender. Everybody involved. It's an interesting thing. He must have been quite a person...You know, people say he was complex

ECKERT: I can say slightly negative things. Pender had a great idea once that it was simpler to represent integral equations and various types of differential equations by matrix notation and solve them using matrix notation, than it was by integral notations. So he gave me a final exam in some graduate course or something in which I was supposed to do the whole exam by matrix notation. I told him it was outlandish. He said it was a better way to do it. I said I would keep accurate accounts of how long it would take me to do this. And I would also do it the conventional way and keep track of that time. It was the type of exam which you take home and do it...It took me 48 hours to do it his way and 2 hours to do it the conventional way and he still wasn't convinced.

STERN: I think Reid Warren told me the story where you fell asleep in one of Pender's or Chambers' classes.

ECKERT: Who? I did?

STERN: It was a funny story. Yes, he was saying that the professor involved was really amused. I think it was Chambers.

ECKERT: I rarely fell asleep in anybody's class.

STERN: You knew that you really didn't need the information.

ECKERT: I know that in Chambers' class I got the top mark in the class so there wasn't any problem.

STERN: Warren did tell me that. He was not suggesting that there was a problem.

ECKERT: It's possible that if I knew what they were saying I would get bored.

STERN: That was exactly the implications. Let's talk about Travis. He was an administrator.

ECKERT: He was also a fine undergraduate teacher. The best undergraduate teacher I had.

STERN: I simply cannot understand why he would let you go. Why he would go that far. Certainly the Moore School's whole computer project declined from that point on.

ECKERT: He was a totally different guy from Mauchly. He was a more responsible sort of guy than Mauchly. I always felt that these things might have had a little more to do with Mauchly but I didn't know the reason to tell you the truth.

STERN: But you felt that you and Mauchly were a team.

ECKERT: Well, we got together and did this thing and I don't think either of us would have done it by ourselves.

STERN: From my records you were offered a position at the Institute for Advanced Study and Mauchly was not.

ECKERT: That could have very well been. I don't remember that.

STERN: Mauchly said that he was offered a job at IBM and it was a chance for the two of you to split.

ECKERT: I think that's true but so was I offered a job at IBM. Incidentally, the person who was against taking a job at IBM wasn't me--I was in favor of it--Mauchly was against it. We were both offered a job by Thomas Watson, Sr., at the same time and I can remember a discussion I had with my first wife (who is dead now)--she sided with Mauchly and thought I should not go and I thought I should. I didn't think we'd have enough money for this development and as it turned out I was right.

STERN: The point is when you make a decision like not going to IBM...Was it a decision that the two of you made collectively? Or you each made your own individual decision?

ECKERT: We discussed it together and we held the patents jointly. It would have been a hard break--for patent reasons. But my first wife didn't think I'd be happy there. So I can't tell you whether I was more influenced by Mauchly or by her. I was probably influenced by them both. I also think that it was probably a mistake and it wasn't a mistake. It's healthier for the United States to have a UNIVAC. You know Honeywell was probably a third contender in the field. Incidentally, the computer business as a whole is in very good shape. UNIVAC has a bigger back log then ever. The 1180 machine is entirely limited by our ability to make it, not our ability to sell it.

STERN: UNIVAC seems to be doing very well recently.

ECKERT: It's funny--if you go out to ask people about UNIVAC they don't have many good things to say about it. Yet we've been selling them pretty good. I saw a study in Japan that was taken and we were about 6th on the list or something...of course, they had all the Japanese devices in there first.

STERN: How long ago was that? Recently?

ECKERT: Yes. Well, we did turn out a machine called the 1110 which had all kinds of screw ups. Just among us we had 40,000 changes in wiring.

STERN: That must have kept somebody busy for a while. I thought the idea that one of the reasons you didn't go with IBM was the decision to stay in Philadelphia...that was important to you and your wife.

ECKERT: People have told me that; they have probably concluded that simply because my parents and grandparents back a couple of generations were born in Philadelphia...so they figured I must be stuck on the place. What they should also realize is that my father traveled a great deal and was out of the country four months out of the year. I have traveled a lot, which may not be a lot today but when I was 12 years old just before airplanes, I traveled 25,000 miles by my 12th birthday...which by cars, boats and trains is a lot of distance in those days. I felt at home anywhere; I'd been in Paris months at a time--it wouldn't have meant anything to me to live in some other place. This is just something that somebody has inferred from the fact that my family lived here for a couple of generations.

STERN: Had you and Mauchly talked about forming a company before the March '46 date?

ECKERT: You mean before we were asked to leave the University? No. Maybe in a far off nebulous future sort of idea that "someday" but not with any sort of time scale.

STERN: So it's a kind of thing where it just evolved out of the events.

ECKERT: Well, we asked, "What do we do now?" We either went to work for IBM or we could have done that.
STERN: I often wonder about the decision not to go with IBM. I think Mauchly always had a feeling against IBM...well a lot of people did who were inventors.

ECKERT: I certainly was influenced by him and by my wife. My own feelings about doing it were much less stronger than his...I can tell you that. I don't think Mauchly recognized the difficulties in doing it ourselves. I think I had a better sense of that.

STERN: It's interesting that IBM was interested in hiring you but then did not do any development work itself.

ECKERT: Well, they thought they were doing work. They had W. J. Eckert out there, and they had some guys from MIT who had been designers. I can't think of their names now...it's been too long.

STERN: Not the people working with Forrester?

ECKERT: No, before Forrester ever got into the act--I just can't remember the names. But they had some good circuit men there that they thought were going to handle it. And they had some guys who were already there that have done some sort of work like John Wheeler, Dickenson, who were very mediocre, and they had a guy by the name of Bryce who built a few things--be built the Mark sensing machine.

STERN: He worked on the MARK I, I think.

ECKERT: Yes, and he did some work--the original work on the Mark sensing machine--but I think they thought that these guys were on top of it. They weren't of course. I think this judged their competence in their people in electronics. They did have competent people in mechanical process...not as competent there either though because we tested some Lake relays with the telephone company and they went 100 to 1 different reliabilities...the Lake relay was about half of the expense to make so it had some virtue; but even so, something that lasts 1/100 as long and costs 1/2 as much to build is not a great bargain. Sort of like a modern car.

STERN: But then again the telephone company had an edge with respect to relays with all their work I would assume.

ECKERT: Well, they've been doing it longer. They had better engineers, too. Thornton Fry who came to work at our place for three years before he retired was Associate Director of Bell Labs. Fry was a fine mathematician. Really the first fine mathematician in the United States I think was Steinmetz. Steinmetz really got an awful lot of theoretical things going at GE in the early days and really turned their work from being haphazard to being scientific in their approach. But, he did not get into communications work and things like that. Well, that's not entirely true, he built dynamic loudspeakers and he built four different loudspeakers and found that the dynamic was the best of the four and he did this way before Peter Jensen(?) started his business. But in any case, Fry was the man that did the same thing at bell Labs on the communications theory. But he worked out all of the probability theories which tells how many lines needed between exchange to handle a certain number of subscribers. And he was a first class mathematician. Fry was a sort of a very practical guy and he was a very systematic guy and he really made Bell Labs as sophisticated in the applied mathematics sense as they were and he had lots of good people that he hired like, Claude Shannon--who was a young man at the time. he was very good friends with people like--well, he knew von Neumann well, he knew Norbert Wiener much better. Well, I used to meet all these people that were looking way up to Norbert and then I talked to Thornton. "Norbert, he was a nice boy but he was kind of odd at times." You get a totally different look at this guy through Fry's eyes.

STERN: I recall some difficulty between Fry and Stibitz. Did he hire Stibitz?

ECKERT: Thornton Fry was concerned in the days that we're talking about with a group of applied mathematicians at Bell Labs and later on with general administration in the laboratory he worked with. He was associate director with Kelly who was the director, and people like that. I've never heard of any difficulty at Bell Labs with anybody with Fry. I hear he had a lot of trouble with Bob McDonald, who operates very secretly.

STERN: What about secrecy with respect to computer development although. It seems to me a big problem from '46 on even before but particularly from that point.

ECKERT: You mean it isn't secret or it is secret?

STERN: Well, the government wanted to keep it open and yet everybody wanted to build their own machines privately and separately and this seemed to created some problems.

ECKERT: Well, you have a lot of people from universities getting into the big league, as far as industry goes, for the first time in their lives, who were seeing the world the way they thought it ought to be and not the way it really was. Why don't we go to lunch?

STERN: Fine. Thank you very much for the interview.

END OF INTERVIEW