An Interview with

IRVEN TRAVIS

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Irven Travis Interview
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Abstract

Travis gives his recollections of the ENIAC project at the University of Pennsylvania. He begins with his employment at RCA, where he was before moving to the University of Pennsylvania Moore School of Electrical Engineering in 1931. He describes his early work in analog computing: hearing about Vannevar Bush's differential analyzer at MIT and "borrowing" Bush's draftsman to build a differential analyzer at the Moore School for Aberdeen Proving Ground; direction of anti-aircraft fire control research at the Bureau of Ordnance; and membership as a Navy officer during World Word II on a National Defense Research Council task force on fire control at the Moore School. Travis then turns to his interest in digital computing, beginning with visits from John Mauchly at Ursinus College. He describes the ENIAC project, the technical and leadership abilities of chief engineer J. Presper Eckert, the working relations between Mauchly and Eckert, the disputes over patent rights, and their resignation from the university.

IRVEN TRAVIS INTERVIEW

DATE: 21 October 1977 INTERVIEWER: Dr. Nancy Stern

LOCATION: Paoli, PA

STERN: This is an interview with Dr. Irven Travis, 21 October 1977, at his home in Paoli, Pennsylvania. Dr. Travis, I know you were born in 1904 and I know you went to Drexel and to the University of Pennsylvania. But I know very little else about your background. I wonder if you can just spend a few minutes telling me about your background, pre-1939 and then we'll pick up from there.

TRAVIS: All right. I was educated in public schools in Ohio and in Philadelphia, and went to Drexel. When I graduated from Drexel, I worked for the telephone company for a year, and then I went to the Moore School as a graduate student and got my Master's Degree in 1928. From then on I had several consulting jobs and summer jobs. I worked for RCA for a while, and my original interest was communications. But early in 1930s -- about 1931, I guess, Nicholas Minorsky ? who was in charge of machinery courses at the Moore School, left, leaving a vacancy. And being a bright young man, I told Dean Pender, "You know, you need somebody to pick up the machinery work, but nobody's going to do it around here that I can see. I'd like to take a swing at it." So I became the Moore School's machinery expert for several years. And that began my interest in computers.

It was about 1931 that Bush and his associates developed the differential analyzer. About that time I was stuck with the solution of non-linear differential equations in connection with machinery research, and I needed a tool. An at that time, that was the obvious tool. So I visited Bush at MIT and borrowed his chief draftsman, and got all the information that he thought was right, and many times that he was sure we're wrong. And so we built a differential analyzer at the Moore School, sponsored by Aberdeen. You probably know this background?

STERN: Well, there were two built.

TRAVIS: There were two. One was built at the Moore School, in the Moore School shops. The other was built by contract - by Tab Person Company - he was a contractor for Aberdeen. so two identical machines were built from the same plans. And at that time, interestingly enough, there was a clause in the agreement, between Aberdeen and the Moore School that in the event of war, the Moore School machine would be made available to Aberdeen to supplement the capacity of the Aberdeen machine. And that's really how the whole computer project at the Moore School got started. They sent a team up to do computation on maxi-desk calculators, supplemented by the work on the differential analyzer. The whole thing was designed to produce firing tables for the Army Ordnance.

STERN: How did the Moore School first become involved with Aberdeen? How was that formal arrangement worked out?

TRAVIS: The Moore School sought, I guess it was PWA funds, in the early '30s, to support the development of the differential analyzer. And I was getting a leave of absence for a year to head up the project, a leave of absence from teaching. Professor Fawcett who was then one of the senior members of the Moore School staff, and a pretty good promoter, recognized the importance of getting one of the government agencies to support our application for funds. And I don't know who thought of the idea that the Aberdeen Proving Ground would be a good place to go for such support. He may have.

STERN: Well this was before the war?

TRAVIS: Oh yes, this was in the early '30s.

STERN: It is kind of surprising for a university to go to the government for support at that time, it would seem to me.

TRAVIS: Well, possibly. I do not recall whose idea this was. It might have been mine because I was in the Naval Reserve at that time, and interested in Naval Ordnance. But it might have been Fawcett's. I don't really remember whose idea it was.

STERN: As a matter of fact, one of the questions I was going to ask you about later in '46 when you come back to the Moore School, how did you pinpoint just what needed to be done to get government funding and how a policy was to be established at the Moore School. It seems to me that the Moore School, even before the war, was aware of the way ne can go about getting government funding, more so than many other universities.

TRAVIS: Yes. Well, I think the two people who were most concerned with that were Professor Fawcett and I. But just what the relative impact was -- I don't remember. It's been a long time. That's 45 years ago we're talking about.

STERN: Right. That was the early '30s.

TRAVIS: Of course, when I came back -- the reason that I was pretty well versed on this interface between educational institutions and government agencies and industry was because I was on the other side of the fence there for four years. I was in the Bureau of Ordnance in Washington. I was contracting officer impacting mostly on organizations like the Bell Labs, Eastman Kodak, RCA, MIT, the Moore School. That's a representative group, but there must have been a good 25 organizations with whom we had contracts. When I say "we" -- I mean the United States Navy.

STERN: And what specifically did you do during that time?

TRAVIS: I was in charge of Anti-Aircraft Fire Control Research. Namely, analog data processing devices for a Naval Gun Lane -- particularly against aircraft.

STERN: Now, it was my impression that NDRC was also involved in funding research in that area.

TRAVIS: This is correct.

STERN: What was your relationship to them? What was the Navy's relationship to them?

TRAVIS: There was casual liaison. NDRC was primarily interested in long-term fundamental research. Some of it became shorter term, as the first couple of years of the war wore on. The Navy's interest was always more immediate. We called it research, but it was actually product development. The research that was done -- real research that was done was back-up to support something that we wanted to develop but didn't quite know how to go about. But it was always end-objective oriented, because after all, we were out there fighting a war and losing ships. And the urgency was always in applications: "Get it out there."

Indeed, when I first came in contact with the work being done at the Moore School, Chris Byer wrote to me sometimes and we discussed it, but I was quite lukewarm about the ENIAC project for one very simple reason: it isn't going to help win this war. And of course, nobody debated that, really. But, I didn't have too much interest in it. We were losing ships. And I was much more interested in fighting the present war, and let's do research when the war is over.

STERN: It seems to me he must have been a lot of duplication of effort, even if NDRC was working on long-term research; if it was in the same area.

TRAVIS: Oh, there was a tremendous amount of duplication of effort. I haven't researched it enough to know, but I would guess that in any war there is tremendous duplication of effort. There is a frantic requirement to get the job done. Research and development was done aboard ship in the Pacific that duplicated NDRC work. The guys out there getting shot at aren't going to wait 'til they hear from NDRC. They're going to do what they can for themselves. So, there was duplication of effort all along the line. The guys on the firing front did whatever they could to help themselves, and I guess the Bureau of Ordnance where I was, and the Bureau of Ships, and other places in the Navy, were the next line of defense because we were in communication with these fellows, and we were doing work next closest to immediate need. And behind that were projects at educational institutions and projects sponsored by NDRC which were more in the nature of back-up. I'm not deprecating these efforts. They were very important. And

many of them got into the fighting, too. I mean, the VT fuse for example, is a beautiful example of a crash project that was fundamental research when it began, but got into the war pretty fast. Burrow and his associates at Section T were responsible for that work. And the work was centered at Johns Hopkins University at my physics laboratory, as you probably know. But that was an outstanding piece of work. Then unfortunately they got into the Fire Control Director business after that, which they didn't know anything about --

STERN: "They" being --

TRAVIS: Section T of NDRC. And Johns Hopkins - my physics laboratory. With their great prestige, which they well earned in the VT fuse -- they got tremendous support and, in my opinion, spent a lot of money that needn't have been spent. Here is an example of duplication of effort. Just one that I happen to have been close enough to know about it. I'm sure there are many other examples. And I don't suppose one should decry this seemingly wasteful philosophy. When the urgency is so great -- when you figure the cost in manpower and dollars of carrying on three or four parallel efforts, as compared to the losses if one of these doesn't succeed, then the cost are quite negligible, and the duplication of effort on that basis is certainly well justified.

STERN: Plus the atmosphere for invention always seems to be greater during a war because things are funded --

TRAVIS: Oh, the scientific advances throughout history have occurred during wars.

STERN: Just for this reason I am not concerned with just sheer duplication of effort. I meant - were the lines of communication between this more or less academic group of men, and the Naval Ordnance complete enough so that everyone knew what everyone else was doing?

TRAVIS: Pretty much. There was surprisingly little that dropped between the cracks; that somebody was doing something nobody else knew about. We exchanged reports regularly. The reading that one had to do in those days was fantastic. And whenever something was interesting, you called the guy up and went to see him. So, considering

the vast amount of work being done, I think that the exchange of information and the liaison was as good as one could hope for. At least everybody tried. Everybody that I was associated with, I felt, understood the importance of keeping each other informed. If there was duplication of effort, well, let's don't worry about. Let's exchange information. And the important thing is not going to be who's going to win this particular piece of research rights, but who's going to win the war. That should be us.

STERN: There's been painfully little written on OSRD and NDRC except for the participants themselves, but my reading of some of the correspondences is that those organizations tended to fund the large MIT kind of Universities or the Bell Labs or RCA, giving very little consideration to smaller projects by smaller inventors.

TRAVIS: That is true. And there's a simple reason for it. They didn't have the staff to adequately evaluate the small project, the unknown. They spent the money - like the horse bettor that bets on favorites to show. They can't afford to bet on a dark horse to win, no matter what the odds are. After all, the standard saying in those days was "There's a war on, you know. Don't bother me."

STERN: Well then, when you take it back to the ENIAC, I mean they really tried to stand in the way, it seemed to me. They didn't want reports to go out. They tried to convince Army Ordnance that this was not a project to fund, and it seems to me that is going beyond simply saying "We're going to stick with the less risky projects."

TRAVIS: Well, human nature what it is, there are jealousies and competitions. There always are. And individuals who feel this competitive urge strongly sometimes go beyond the bounds of wisdom in what they do.

STERN: So do you think part of the reasons for that was that many of those people were analog men from MIT, and here was coming out the computer that was digital, and therefore there was some competition involved?

TRAVIS: I don't think that the competition was so much a preconceived idea about the technology involved. After all, Gordon Brown who was an analog man from the word "go" was the guy who called the shots in the Whirlwind

project in the early days.

STERN: Sell, it took a long time before Whirlwind became digital though.

TRAVIS: Well, that's right. That's right.

STERN: And there is a book out on Whirlwind which claims a lot of money was lost because of MIT's predilection to go in terms of analog.

TRAVIS: Well, let me defend that point of view in the sense that I had some attitude. But I defend that not because I was prejudiced against digital. I didn't think the time scale of the war would permit much attention to digital computation, so I didn't think it had a chance of really helping in the war. There wasn't anything wrong with it.

STERN: Yet, when Brainerd asked you for reports -- you were the one responsible for giving the Moore School the reports they needed about RCA and the NCR; whereas NDRC stood in the way of that. So despite your feeling about the war, the competition was not there; it wasn't that same kind of feeling.

TRAVIS: I'm trying to recall: was there an MIT man on leave in one of the services that might have -- you've got to remember, there was a personal relationship here. Grist Brainerd was an old friend of mine. And we worked together for years. And so he asked me for something and I gave it to him. I didn't weigh "Is this going to be a good thing for the Moore School or for the Navy? This is a smart guy who wants it, so I'll get it for him." So there was a personal relationship there that may not have existed. I don't know.

STERN: I don't think anyone in the Moore School had a friend on that board. so maybe you're right. That could very well be the reason why it was so difficult. But it was a question of everybody working together for the government effort. It seemed to me, just from looking at it, that it should not have been so hare to get the information asked for. But to get back to your earlier statements about how you thought that a digital computer would not have

provided much for the war; it would take too long. Other than that, did you think it had a chance of succeeding?

TRAVIS: Oh yes.

STERN: Eventually.

TRAVIS: Well, I looked in digital methods of solving differential equations when I was consulting with the GE company.

STERN: I was going to ask you about that.

TRAVIS: In '39 and '40. As a matter of fact, we had a small working task force at the Moore School in 1940 - the fall of '40 and spring of '41, looking toward this additional project to NDRC, to Warren Weaver. and it was a fire control project. It had two approaches: one was analog, and the other was digital. And although I didn't favor digital too much because it was too new, and I felt that urgency should indicate priority on the approach that we knew would work, nevertheless, I didn't feel it was wrong to pursue it as a secondary effort in a digital approach. Matter of fact, I wrote a memorandum to the GE company, I think in 1940, discussing the possibility of a digital differential analyzer. I still was interested in solving differential equations, so I always had sort of analog end result, because shooting a gun is an analog effort. It's in real time, and the angles of orientation with respect to the deck are not digital. So however you do the computation, you convert it to an analog signal in the end. And I always felt, well, unless the digital is far superior, you've got to spend the money to get it out, and the telescopes and the radars all give you analog information. So that the digital-analog conversion, and the digital computer had to pay for that. So here's an overhead that's built in to a digital approach that has to be paid, and back in 1940 and '41 and '42 it wasn't so obvious how that was going to be accomplished.

STERN: And like you said, with the urgencies of war, I guess that wasn't to be a primary consideration.

TRAVIS: Well, you had to weigh the adequacy of the result and the time that it had to be in service. Now, if an analog approach will produce a good enough answer, and if it can be gotten into the field on a timely basis, who cares if there's a better way to do it? You don't even waste a week to get a better way to do it, if this is good enough.

STERN: Did GE ask you to look specifically into digital differential analyzers?

TRAVIS: No. Well, I was consultant to GE for a couple of years -- well, let me back up a little bit. GE came to the Moore School to use the differential analyzer for a couple of urgent problems which GE had run into. We'd had a crisis out on the west coast -- I think it was Washington Water Power Company. They were using series capacitors to compensate for transmission line drop -- voltage drop. And they discovered when they closed the circuit breakers, instead of getting normal...transformers, they got full load current; instead of being 60 cycles, it was 20 cycles. And it was a real crisis. The GE capacitor business was in jeopardy. And they conceived the bright idea of trying to set up the differential equations for the problem and solve them on a differential analyzer, and comparing the analyzer results with oscilloscope records they already had of what actually happened out there. Several people from GE came down and I worked with them, Corgy Weygardt worked with them, and we solved the problem. And we save the GE company's capacitor business. That was the beginning of a great friendship with the GE company, Corny Weygardt and the Moore School and I.

So, about a year or two after that, some of the people that I knew up there were wondering if the GE Company shouldn't have a differential analyzer -- didn't they have a big enough load to justify it. So my first task was to come up there for the summer and talk to all the engineering departments and all the research divisions that might conceivably have problems that were amenable to solution on a differential analyzer; and make a recommendation: do you build one or don't you? And I came up with a recommendation that they do. I could drum up a 50% load for it just from my cursory examination of their needs, and I felt certain once it was available, the load would more than double, which it did, of course. So they said, well, all right; if we should build a differential analyzer, we want you to come up and help us decide what kind to build.

So for the next year and a half, my job was to work with the Central Station people - the people I reported to - and the General Engineering Laboratory, to come up with a tentative design of what the GE Company should build. Of course we drew on all the things that GE Company had done that would bear on the subject. One of the most important things was the polaroid light type of torque amplifier, which we incorporated in the Moore School machine later on.

But except for that very great improvement, the rest of the analyzer was pretty much like the original design that Bush had done some good many years before that.

But during this period of time, after it was decided that GE would proceed to build the machine, I launched off into several by-passes to see - should I recommend something other than simply an up-dated version of the differential analyzer of classical concepts? And we finally pursued the conservative approach - we built what we knew would work. And GE wasn't too much interested in research on analyzers, but it wanted the tool. And so these other possibilities weren't pursued with much vigor, and I didn't press them. But I did come up with some other thoughts that might be pursued.

STERN: Like the idea of a digital machine --

TRAVIS: Yeah - for instance.

STERN: Let's get back to the ENIAC -- you were already in the Navy when the ENIAC project was approved.

TRAVIS: Well, I was in the Naval Reserve -- I was commissioned in 1931. I went on active duty in June of '41.

STERN: June of '41, that was even before Mauchly had come to the Moore School to begin with.

TRAVIS: Oh, yes.

STERN: He came in the summer of '41, I believe.

TRAVIS: Well, I knew John Mauchly - John Mauchly was Assistant Professor of Physics at Ursinus. He'd come down and talk computers to me.

STERN: Oh, even before he was at the Moore School.

TRAVIS: Oh, yes.

STERN: I didn't know that.

TRAVIS: Oh, yes. John Mauchly used to come down and talk computers to me, because he knew I was interested in computers. He and I would sit in my office and chew the fat for half a day you know, asking what we were doing in this field?

STERN: So you knew about his digital work, then, at Ursinus?

TRAVIS: Oh, yes. John Mauchly and I knew of each other's interest in computers and we compared notes and had many conversations before I went in the Navy.

STERN: And in '43 when this project was approved for the Moore School, did you think Brainerd was an appropriate choice for Project Supervisor?

TRAVIS: Of course, in '43 I was in the Navy. I was in Washington. I was a very hard-worked, overloaded guy, and I wasn't thinking about the Moore School at all!!

STERN: You were not into that. I see.

TRAVIS: I didn't even know of the application and approval of the project until it had been underway for some time. I don't remember when I first became aware of it. But I was not aware of it at its outset at the Moore School. It might have been almost a year later, that I became aware of what was going on there.

STERN: Brainerd requested that you send him documents in the summer of '43, which was a few months after it had been approved.

TRAVIS: Summer of '43?

STERN: Yes.

TRAVIS: Well, that's probably about the time I first became aware of the fact that anything like that was being done at the Moore School.

STERN: Now, having known Mauchly prior to this, what was your first recollection of this project? Did you think it had a chance of success, based on Mauchly's plans?

TRAVIS: Of course, I didn't know what Mauchly's plans were then. However, I knew Pres Eckert pretty well. He was a student of mine. He was a very brilliant young man. And his creativity and his knowledge of electronics I knew pretty well. And although Mauchly was more of a dreamy-eyed mathematician, and Brainerd was more of an academician than a research and development man. If you had asked me then, with these three guys are involved; will it succeed? -- I would have said, "What does Pres Eckert think?" And if I was told Pres Eckert thinks that he can make it go, I'd say "Yeah, he probably can." I wouldn't have trusted either Mauchly's or Brainerd's judgment on that subject: Will the ENIAC be successful? as I would Pres Eckert's. You've probably heard this from others.

STERN: Well, I've certainly heard about his engineering talents. There's no question about that. Part of the thing I'm

trying to sort out in my mind is the contribution of these other people. Eckert's contribution is clear from the engineering point of view, although I wonder about how the engineering staff was able to work with him, for example, in those years. I mean, I'm not sure. It's difficult to determine those things. Do you have any sense of that, knowing the kind of person he was, and is?

TRAVIS: Well, Eckert, like many creative people, is more aware of the technology. He worried about that than the people around him. And interpersonal relationships don't tend to occupy too much of his attention. Some researchers have absolutely abrasive personalities. It's not uncommon. I wouldn't say Pres was abrasive. He was more aware of the need to keep interpersonal fences built. But as an inspiration to others -- everybody respected his technical ability, and in any research project, you can overlook a lot in deficiency in management ability, or the usual classical means of influencing people, if the technical leadership is there. And I know in my good many years of research management with the Burroughs Corporation, I always had the problem of weighing the impact of a new Project Manager, new Division Manager on the effort going on in that segment of the company, and on the morale of people, and the ultimate wisdom of making such an appointment. It's difficult. I mean, you can't say because a guy doesn't understand people very well, doesn't really care too much about people, he'd make a lousy Project Manager. This isn't necessarily so. If he's got technical ability and creativity that is recognized to the point that he gains the respect of all the people around him, they will overlook a lot of other things.

So, that's a little piece of philosophy which I've always felt -- you don't necessarily look for a good manager to head up a project. It's nice if you've got both. And people who have both are great people. But there aren't too many of them.

STERN: It's just when you get a project like this one, which is so new, and someone has to take on the supervisory role of design and then others do, in some cooperative sense, design work too -- how is that managed when the one person in charge? The interpersonal relationships are not as firm as they might be. Does it mean it's a cooperative effort, or does it mean it was a Pres Eckert effort where everybody did what he told them to do?

TRAVIS: I've never known. When the ENIAC patent was issued, it came out a Pres Eckert effort. I've testified in some of the several litigations I've been involved in, that although on the surface of it, that's the way it looked, I wasn't willing to believe that's the way it really happened; there's no question that his technical contribution was dominant. But I never believed it was exclusive. I don't believe any technical effort is ever exclusive. No matter how good the guy is. If he's got a bunch of other smart people working with him, they're going to make contribution too.

TAPE 1/SIDE 2

STERN: Well, particularly in light of the fact that there were so many good engineers on the project - people like Sharpless, Shaw, Burks --

TRAVIS: Oh, there were many. In terms of logical design and mathematical intuition, if you will, Arthur Burks was one of the most brilliant men we've had in that field -- ever. He made important contributions, I'm sure, that never showed through. I knew Art Burks pretty well. And here again, it's very difficult for me to answer questions like this without the answer having a good bit of subjectivity in it. I wasn't there when many of these things occurred. What I can do is infer from the knowledge of the people - from my own evaluation of their relative abilities and their personalities, what is reasonable to believe happened. And when I give you a conclusion, it's based upon that kind of reasoning. Not because I was there observing. I wasn't.

STERN: Which in many ways makes you less subjective than some of the other people who were there and were just so much into their own way of doing things.

TRAVIS: It could be I'm more objective than some of the people who were there, particularly those who were directly involved.

STERN: Exactly.

TRAVIS: And who came out unhappy about how things ended up.

STERN: But from you recollection, there was no discussion about precisely how things went on in the laboratory; whether it was a group effort or not? I have not been able to get any answers on that.

TRAVIS: Well I guess Brainerd is probably the best authority on that particular question. What were the debates as to the likelihood of success, who believed it would be successful and who was skeptical. I don't know these attitudes because I wasn't there at that time. And by the time I became close to the project, so much had been accomplished, that people's attitudes weren't the same as they were. And I couldn't back up and say what they had been two years before. So I don't really know.

STERN: Well, in line with what we were saying about coming out as a Pres Eckert effort, I've not interviewed the man, but judging from what I've read and people I've spoken to, he doesn't seem to be the kind of person that would demand all credit for himself.

TRAVIS: No, no. That's right.

STERN: Can you account, then, for the sequence of events that led to that sort of thing?

TRAVIS: No. No, I can't. I find it difficult, almost impossible to believe it really happened the way the record looks as though it happened. On the other hand, I'd be the last person in the world to accuse Pres Eckert of taking unfair advantage. From what I know of him, he's not the kind of a guy who would do that. To say that he may have done it by inadvertence - that would accuse him of not being as smart as I think he is. So, I can't really answer your question anymore than I have done.

STERN: What about Mauchly's role in that? There seems to have developed from '43 on hostility on Brainerd's part towards Mauchly. How much of that do you think is related to things like Mauchly's commercial interest, whereas

Brainerd, more the academician, felt that was out of place?

TRAVIS: I think there is a general feeling on the part of Brainerd, and I think I share this too, that in a project born during a war, sponsored by the United States Government, under contract to an academic institution, where individuals are hired to do research and paid for full-time research jobs -- not doing research on their own, as was done before the war, but when one had an academic appointment, and had free time to do anything he chose to do -- I think there was a general feeling that they took unfair commercial advantage. I won't say unethical, because that's too hard to define. Let us say it's a thing that I wouldn't have done.

STERN: This is now still during the war that you're referring to?

TRAVIS: I'm talking about immediately after the war.

STERN: When you came back?

TRAVIS: Yes. Well, the record is clear: I'm the one that precipitated the blow-up.

STERN: I know that. Before we get into that, I want to discuss the period prior to that. I have a lot of questions on that. But to your knowledge, and again, you weren't there, was there any attempt at taking unfair advantage during the war. Now, Mauchly has made a very big point of saying his interest was purely war-related until the war was over.

TRAVIS: The only thing I can contribute to that is a letter signed by George William McClelland, who was President of the University. I don't recall when it was signed, but it had been signed before I returned to the Moore School. It was signed before January of 1946. It's probably in the record when it was signed. But in it, it gives the patent rights to Eckert and Mauchly. The University, by a signature of the President, gave patent rights to Eckert and Mauchly. Now, to say McClelland was naive is one way of putting it. It's perhaps kinder to simply say he was a Professor of

English; he was a very fine gentleman, an adequate president of the University. But when it came to contracts and legal matters and patent matters, he had less knowledge than almost anybody you could have found.

Now, who persuaded him to sign that letter and why -- I don't know. I've always felt that letter was unfortunate.

STERN: Considering the circumstances that led to the signing of the letter - it doesn't seem to me that he had a choice. Eckert and Mauchly, they said they might not finish up with the projects should they not get this patent.

TRAVIS: Well, if there was such coercion, -- I don't know. I would say such coercion should be condemned. I do not know of personal knowledge that there was such coercion. There may have been. I don't know.

STERN: And also the fact that the University was in a very precarious position because they did not have the patent outright, and they had to sign over to the government license free right ? to the ENIAC, which they couldn't do without owning the patent, which led to a kind of problem with the government -- an embarrassment, if you will. So I wonder what kind of choice McClelland had.

TRAVIS: Well, here again - you remember...Dickers' office, the University's legal counsel, was an old, stolid, conservative, Philadelphia legal firm, without, so far as I know, much interest or background in the type of subject that we're discussing here. And, they believed their primary role was to keep the University's good name free of any besmirchment -- thus protect the University; and let's not worry about these little details, you know. So I'm reasonably confident that Dicker's office wouldn't have taken a stand as to what's right and wrong. They would take a stand as to what will save the University embarrassment?

STERN: That certainly seems the way it came out, yeah. That makes a lot of sense from that point of view. What surprises me in all of this is not the letter, but Pender. I mean, Pender, it seems to me, with his background, should have taken more of a role in this.

TRAVIS: Well, that's a new subject. I don't think Pender really had a much different view from that of McClelland and legal counsel. "Let's not make waves." Pender never was much of a wavemaker. It was, "Let's keep thing flowing on an even keel." Well, -- it's irrelevant maybe, but Pender had other things on his mind. In those days, he hadn't been remarried so very many years. He had a young son. The business that he helped found back in the late '20s - International Resistance Company - was coming on hard times, and he had a lot of personal pre-occupations in those days. I don't know whether it's appropriate for me to even mention these things in an interview like this, but you raised the question -- why didn't Pender take a more rigorous role? Knowing the guy as I do, and understanding him, I'm not surprised that he didn't. He had more important things to do, in his system of priorities.

STERN: Are you saying that his company took a large part of his time?

TRAVIS: Well, it didn't take a large part of his time then, because he got a former member of the Moore School staff to come in and pinch-hit and run the company for a year -- Charlie Weyl.

STERN: You mean on paper he was running the company, but at he same time he was dean of the Moore School. Is that correct?

TRAVIS: No. Pender was not running the IRC. He was one of the original founding partners; he was a principal stockholder of the IRC. He was a member of the Board of Directors. He was not actively in management -- no. He was not an officer of the company. He did a certain amount of on-going research following the early work he did to establish the company back in the '20s. He always did that part-time. But he was not directly involved in the management of the company, except as a principal stockholder. He was of course very much concerned with its success.

STERN: Do you think, based on his relationships in his company, that he would have taken a sympathetic attitude toward the commercial interests of Eckert and Mauchly? I'm talking about the period, now, prior to '46.

TRAVIS: I don't know. Certainly Pender as a professor of the Moore School who did independent research unrelated to the University's affairs, and it was considered quite a common practice and quite appropriate for one to do this, along with his two partners who were professors of physics, who founded the company which was a big success. So, he's an example of a guy who, while holding a key job on a university campus, found enough spare time to develop a scientific approach to a commercial product and successfully exploit it. So he would be sympathetic to that general sort of thing. Well so am I. I don't think anybody at the Moore School or in most other institutions would be unsympathetic to this general philosophy.

STERN: Most engineers. I think scientists might.

TRAVIS: All right. All right -- granted. Most engineers, or scientists who have a direct applicational flair -- maybe that's an engineer! But whether in the circumstances of Eckert and Mauchly to which we're addressing ourselves -- whether Pender because of his own background and experience would be more sympathetic to their commercial ambitions or not, I don't know. I don't know for a fact that he was more sympathetic. The environment in which he did it was quite different. Now, how sensitive he was to that difference in environment, I don't know.

STERN: I see. I understand. You say that you had earlier involvement with Mauchly, prior to your leaving the Moore School in '41. What was your recollection of his abilities in the area of building a machine?

TRAVIS: Well, I don't recall that we talked too much about building a machine, and certainly not building an electronic digital machine. We talked about the mathematical aspects of the problems that needed machine solution; we talked about vast statistical problems; we talked about intractable non-linear differential equations -- the class of problem where ordinary mathematical approaches simply are inadequate for any kind of success within a reasonable lifetime. I think we talked more applications than anything else, and more about existing machines. As for his ability at building a machine, I never saw him much in those terms. He was not an engineer. He was a physicist and a mathematician. And as such, he was able enough. But he wasn't a hardware-type guy. He never was. He still isn't, as far as I know.

STERN: Well, he did say that he built a harmonic analyzer, I believe, in '39.

TRAVIS: This I didn't know.

STERN: He never spoke about that?

TRAVIS: I don't recall that he did.

STERN: And certainly not about Atanasoff's ideas on early digital computers -- he didn't talk about that.

TRAVIS: No. I'm aware of the counter-claims between Mauchly and Atanasoff, and all I know is hearsay.

STERN: Well, you were not involved with Atanasoff in the Navy? He also served in the Navy, I believe.

TRAVIS: No. I did not know of Atanasoff's work until after the war was over. I don't remember when it first came to my attention. But I did not know about it during the war. I had never heard of Atanasoff up until the war ended, but I did hear of him shortly thereafter.

STERN: Were you aware of the problem at the Moore School with the von Neumann draft in '45?

TRAVIS: I'm aware of the controversy, if you will. I'm aware of this anonymity of a piece of paper. I know nothing about the facts. Simply that there arose a question -- Where did this piece of paper come from? I gather there's a conflict between Mauchly and John von Neumann, and I gather it got rather bitter. About the facts I know nothing.

STERN: Okay. Brainerd asked to be relieved of the EDVAC project in 1945, primarily because of this patent problem, and I gather, his feelings towards Mauchly and Warren. Is that correct? To your knowledge.

TRAVIS: Between Warren and Mauchly?

STERN: That Brainerd wanted out of that area.

TRAVIS: Yes.

STERN: And those are the reasons?

TRAVIS: I think those are the principal reasons. I think it had kind of gotten to be a can of worms that Brainerd just didn't want to bother with any longer. There were financial problems, there were accounting problems. He and Mauchly apparently didn't see eye to eye on things. It had grown to the point where it was pretty much a full time job, and Brainerd, I'm sure, didn't want to be relieved of teaching to take over that job. He was much more interested in the academic facet of his career, and he'd had it up to here.

STERN: In his testimony he says that the way the patent situation was handled, and the fact that Pender had given him free hand up until that point, and then decided to acknowledge Eckert and Mauchly's right to the patent was - as far as he was concerned, the problem. He just didn't want anything more to do with the project as a result of that. So that he admits to that kind of thing. But what strikes me as odd is Warren's appointment as supervisor, considering the fact that Warren's background was not in this area.

TRAVIS: Well, look around. Who else? I think it was by default.

STERN: Chambers?

TRAVIS: Well, I think there was little doubt that Chambers was technically better qualified. It may have been offered to Chambers. I don't know. I can imagine it being offered to Chambers and his saying, "Not me, buddy. Not

interested."

STERN: Well, it was my sense also that Warren's got the kind of personality which might tend to even things out a little bit, which might have helped, considering the situation.

TRAVIS: Well, Warren has a blander personality than Chambers, certainly. If Chambers had taken over the job, he would have done about what I did when I took it over, I'm sure. I know Carl pretty well, and he and I see eye to eye on things, mostly. And I don't think he wanted to get involved in it.

STERN: Before we get on to the '46 period, would you say that in some sense, the military organizations, the Army and the Navy, during the war, were more willing to take chances with technological inventions, innovations, than some of the scientific agencies that we have discussed? That is my sense -- that the literature has given too little credit to the Navy and the Army, and too much credit to OSRD and NDRC. How would you feel about that?

TRAVIS: It's hard to say whether the allocation of credit is misplaced or not. The Army and the Navy had a job to do and they needed tools to do it. And I think you take chances in desperation that you don't take in the academic, laboratory atmosphere. So, I think that there were a good many officers in the armed services who felt a need to gamble because the stakes were so high, and I don't believe, unless you read the action reports, or else you're actually out there, you can't get the sense of urgency any other way. I had a sense of urgency, although I was stationed first in Long Island City at the Fort...Company, and then for four years in the Bureau of Ordnance -- but I had that sense of urgency, for two reasons: I went to sea for two or three days every month or so with new equipment and I talked to the guys who were being re-assigned to new ships going out. And I heard first-hand what the problems were, what they needed. And those who were heading up sections, as I was, read all the action reports, and you get just as scared sitting at a desk in Washington as you do if somebody's out there shooting at you -- if you know, first-hand. In the fall of '43, before the big push -- as you know that came in the spring of '44; I spent a month in Pearl Harbor talking to the gunnery officers of every ship in the fleet. They were all there. I spent a whole month out there, and went down in a submarine; I went out on practices with every class of ship we had. I went back

to the Bureau of Ordnance with full knowledge of what had to be done in the anti-aircraft fire control business, because I had first-hand knowledge from the guys who had been experiencing the problems. I know how I felt. And there must have been hundreds of others with same sense of urgency. And I'm sure in that environment you do some gambling that somebody less directly exposed to that type of pressure would do. This is all hindsight, but that's the way it looks to me now.

STERN: In our position in the Naval Ordnance -- you funded both university projects and industrial projects.

TRAVIS: Yes. Mostly industrial projects.

STERN: Mostly industrial.

TRAVIS: Mostly it was development work where you needed a model shop capability and manufacturing capability. You didn't want to transfer if from a university to a factory.

STERN: And most universities do not have that capability.

TRAVIS: They didn't have the manufacturing capability. So, unless it was a project that no commercial organization was set up to do, I tended to give it to a commercial organization if they already had a line of communication between the development laboratory and the factory, and could shorten up the lines from the time the developed model was made until you started delivering. So that was always an important consideration. And that favored the commercial organizations over academic, if there was one who could do it.

STERN: Regarding these commercial organizations, some of the things I've read indicate in this area that there was some sense of efforts to exploit development -- commercial exploitation.

TRAVIS: I know of no instance when commercial interest to exploit the results of development was permitted to

compromise a military objective that was supporting the development. I know of no instances.

STERN: You know of no instance where the effort was even made to do that?

TRAVIS: No, I won't say that. I am sure that any commercial organization which had a contract with the military, to produce a military end result, would be most sensitive to the possibility of by-products from this effort, spinning off into their commercial interests. But not to the detriment of achieving the military objectives for which they're being paid to pursue.

STERN: That was primary.

TRAVIS: In my experience -- I never felt that any company was not giving full effort to what they were being asked to do for the military.

STERN: Ok -- let us go on to '46. You came back in January '46.

TRAVIS: Yes.

STERN: Were you immediately appointed as Supervisor of Research? Can you give me the sequence of events as you remember them?

TRAVIS: I'll do the best I can. When I came back -- or when I got out of the Navy, I had accepted a job as consultant to Reeves Instrument Company. I'd been offered a job as Professor of Mathematics at Brown University, which I very seriously considered accepting. But since I had so many friends at the University and knew the Moore School pretty well, I said "Well, we'll see what's going on in the Moore School and if they want me back, why I'll go back there." So I talked to Pender, and he wanted me to come back as an Associate Professor. Well I'd been an Assistant Professor for six years, when I left the Moore School for the Navy. I figured my period as Associate

Professor was done as Commander in the Navy. I said, "Well now, let's not be silly. I would be a full professor if I hadn't been in the Navy." So - this argument didn't last long. I didn't even have to threaten to go to Brown University. So, the first thing -- yeah, you can come back as a full Professor. And then we discussed teaching load, and I don't remember just how it came about. It sort of seems to me as though it came about in Pender's office one day when we talked about the research projects at the Moore School. And he expressed concern about the way they were being managed. He saw a lot of problems, and I don't know how long it had been since Brainerd had been replaced by Warren, but not too long. And I think Pender wasn't wholly satisfied with the way things were going and I think we got to talking about interface between the sponsor and the educational institution. You know, "How did you do this in the Navy? What did you, as the Navy representative on a project you had with MIT, for example -- what was the interface there and how did things go?" And I think it kind of evolved naturally, as I recall. I don't remember exactly the details, but I kind of think in these discussions, I began to appreciate that Pender had a problem, and he began to appreciate that I knew something about the subject, and whether I proposed it, or whether he did, I don't remember. It may well be that I said, "It looks to me like you've got a problem that I could help you with; if you want me to take a swing at it, I will."

STERN: To the best of your recollection, what sort of problems did he indicate existed?

TRAVIS: It seems to me that at that time the patent problems were there.

TAPE 2/SIDE 1

TRAVIS: Another problem was how you manage you research, or how you mange your research/development project from a financial standpoint. You've got to have project control, which means that you have to have accurate evaluation of percentage completion, and cost to complete. And you have to have accurate evaluation of funds committed. Not funds paid, but funds committed. You can be halfway through a project and your back balance is big; you've only paid a few bills. But the decisions you've made already committed funds for maybe the rest of the money available.

STERN: So this problem was the EDVAC, not the ENIAC, because the ENIAC was already completed.

TRAVIS: Well, there was a problem in that the EDVAC project was already underway. They were running in parallel. Some of the technology that was being developed had application to either. So you had three kinds of money: the money you spent for EDVAC objectives; money you spent for ENIAC objectives; and supporting money that you spent that might go into neither, depending on whether it was successful or not. And the books were kept by the Controller's office; not in the Moore School. so far as I know, the Moore School didn't have project financial watchdogs, watching over the engineers' shoulders and saying "All right -- you made that mistake; you make this decision; you've now spent $30,000; and we don't have that money anymore. It's already spent. Even though no orders have been written for material, and no project direction had been written for people at work -- you make that decision, you've spent the money right then." This type of close control was not in existence. And it has to be.

STERN: Doesn't it have to be during the war? Or is it only necessary after a war? Because after all, during the war the support is almost limitless, so they just spend money.

TRAVIS: I don't know when the financial purse strings began to tighten, but they had already begun to tighten when I arrived back at the Moore School. Budget pressures were being exerted and additional funds were needed in total. It didn't really matter whether that was EDVAC money or ENIAC money -- maybe a lot of money that was supposed to have gone for ENIAC went for EDVAC and vice-versa. And without close financial control by a financial watchdog down there in the laboratory, watching closely what's going on and keeping project records, you don't know. And so we needed money; but was that EDVAC money ore ENIAC money? It was kind of hard to prove which it was.

STERN: The contract was a supplemental contract on EDVAC, so did that mean that it didn't matter from the government's point of view how the money was spent?

TRAVIS: Well, I don't remember. You're asking me to worry about a Moore School budget when I worried about Burroughs' ? budget for 20 years after that.

STERN: Sorry!

TRAVIS: No, I know. Literally, the Moore School financial problems were for 20 years...trivial compared with the problem making a buck for the Burroughs Corporation, so much so that I've forgotten the details. I think that what you say is right -- that the EDVAC contract was a supplement to the ENIAC contract. And how well the work statements were spelled out as between the two, I don't recall. I suspect, without remembering accurately, that it was fuzzy. These things tend to be. So what was the workload allocable to the original contract, and what was the supplementary workload allocable to the supplement; and how did the funds match these workloads -- I don't recall now. I just recall it was not as clean-cut as I would like to have seen it.

STERN: It was your sense that the major problem when you came back was the financial one.

TRAVIS: Two problems -- the patent problem was also an important one because there were no patent agreements in effect. I was appalled to find that the University had a contract with the United States Government in which it guaranteed to turn over patent rights to the United States Government. It had no contract with employees that would allow it to implement this obligation. The University was in an untenable position. It had an obligation under a contract with United States Government that it couldn't fulfill. It had no contract with any employees. So one of the first things I did was to write up a patent agreement, more or less along the lines that I had known about at MIT and Bell Labs. The Research Corporation was the patent exploitation agency for MIT. The Research Corporation founded on the Contrell ? Cotrell patents -- you may know of it. Joe Barker ? was for many years head of this thing. And the MIT philosophy was to give the inventor some of the proceeds; MIT takes some of it; and the Research Corporation who commercially exploited it, would take its operational fee. But it was all sewn up tight, legally, so that MIT could enforce any patent requirements on its employees. So, more or less copying what I knew to be in practice at several other institutions, I wrote a draft of a patent agreement. And laid down the law -- everybody does sign

this. And I had some dissenters.

STERN: Yeah. MIT did have a patent agreement like that once, but it was my sense that it was not unusual for universities not to have any.

TRAVIS: The universities that were experienced in doing contract research all had them.

STERN: I see.

TRAVIS: Applied Physics Laboratory at John Hopkins had it. The University of Virginia had it. MIT did. I'm not sure about Harvard. Brown University, it seems to me, did not.

STERN: Just to go back a little bit on Brown University -- had you worked there, would that have been with Archibald, because those men were concerned with Mathematical Tables and other Aids to Computation and the Journals --

TRAVIS: I was on that committee.

STERN: You were on that committee?

TRAVIS: Yeah. Matter of fact, I've got a file of correspondence with Archibald -- way back somewhere. Quite possibly, since I knew Archibald -- but that wasn't the reason -- my executive officer, Warren McWilliams who is now at Bell Labs, and I went up to see the President of Brown University. I've forgotten his name, now. And we presented a problem. We had an urgent mathematical need. I've even forgotten what the problem was. But it was a crisis. We didn't know where to get the work done. It was a mathematical project that was critical to some work we were doing. And we both knew of the mathematical capabilities at Brown. And we thought - we'll go to the Boss Man and see what he's got to say.

So we presented our problem. It irks me -- I can't think of his name. Because there was immediate rapport and confidence. He understood exactly what our problem was; he was sympathetic toward it. He said, "We'll take in on." And -- so he made a very fine impression on us, and we made a pretty good impression on him, too. And it was that association that led to my possibly going to Brown University. That was very direct personal contact. My association with Archibald was more or less by correspondence.

STERN: All right. Back to the Moore School patent matter. First, you came back as Supervisor of Research. What was your relationship then to Warren who was still Supervisor of the PY...project?

TRAVIS: I don't remember what happened in the transition, but that situation didn't last very long. The people PY and PX projects were reorganized and some new people came in.

STERN: Were you responsible for this reorganization?

TRAVIS: Yes. Well, in consultation with others. I made the decisions. No - from the time I took over Supervisor of Research, it was a line organization; I reported to Pender and everybody else reported to me. And - well, I'd spent five years in the Navy. I was going to run it like you run things in the Navy. There's one guy making decisions.

STERN: How about Chambers? What kind of position did he have?

TRAVIS: Chambers remained active in many respects, more as a consultant than a line decision maker. He wasn't in the line of command.

STERN: But Brainerd and Warren and those people were under you.?

TRAVIS: As well as I can recall, some of the projects had been, in my judgement, well managed, and in no need of

very much attention. One was a group of projects that I think were pretty well run by Ralph Showers ? in the communications field. I guess they reported to Chambers. But he was sort of a father confessor and technical consultant and delegated it to Ralph, and Ralph ran that group of projects, with half a dozen people reporting to him. That technically came under me. But I saw no reason to do much abut it. They were getting along fine. I had enough problems with projects that were in trouble. So I sort of segregated out those projects that I felt needed my personal attention and delegated them to somebody to manage so that I could look down on them pretty carefully. And some of the projects kind of went along as they'd been going. And it seems to me that any one that reported to Carl was in that category.

And Warren was soon out of the direct line. I never got the impression Warren wanted particularly to be in the line.

STERN: He was eager to get back to his own work --

TRAVIS: He was quite happy to get out and I was just as happy to get somebody who was really going to put full time effort into it. And so, by mutual agreement, that just kind of faded away. It didn't last very long. The time schedule, I don't recall.

STERN: Who did you have take over?

TRAVIS: Well, here again, I don't know. I think early on I nominally had charge, and that Pender said, "Yeah, you go ahead and do this." But when did I take over? Well, I did it by pieces. As soon as I knew something about a segment of the business to think I could -- I became active in making decisions. So I sort of took it over piecemeal, beginning with the pieces I thought were most urgent.

STERN: But now - these were not the only projects -- the ENIAC and the EDVAC projects that you would take over, but yet --

TRAVIS: Oh, no. there were a good many others -- but they're the ones that I was concerned about.

STERN: That's what I mean. They were the ones that were most --

TRAVIS: They were the ones where there was a patent problem; they were the ones where there was a financial problem.

STERN: How did Brainerd feel about this? Your coming in as Project Supervisor?

TRAVIS: Not unhappy if that's what you mean. You must remember that Brainerd and I had been close personal friends for many years.

STERN: That's why - I was just thinking from the point of view of two close friends, and one comes in and becomes a supervisor - there might be sometimes, some kind of --

TRAVIS: Well, Brainerd continued to do what interested him most -- teach -- graduate courses primarily, and I don't recall there was any feeling of competition or even displacement. I mean,...he didn't particularly want to do it.

STERN: Did he tell you about any of the problems he encountered?

TRAVIS: No specific thing occurs to me.

STERN: From your sense, early on -- what would you say Mauchly's role was in '46? On the EDVAC or ENIAC project?

TRAVIS: The relationship between Eckert and Mauchly has always puzzled me, and still does. I never understood just the relative division of effort there. Nor did I ever understand the obvious loyalty they exhibit for each other,

knowing as much about them as I did - both personality wise and technical ability -- it seemed to me sort of not-too-normal a liaison. I never understood it and still don't.

STERN: I can certainly understand Mauchly feeling that - after all, the engineering that he had in his head but couldn't get down required engineering - but this business about Eckert's using Mauchly as a sounding board, I find difficult to understand myself.

TRAVIS: Well, on the surface it was as though Eckert was an electronic engineer and hardware inventor, and Mauchly was the mathematician and logical designer, and systems conceptual leader. This is the way it looks on the surface. Maybe that's the way it was.

STERN: But from you sense of the situation, from January 1 until they resigned, what did Mauchly contribute to the project...?

TRAVIS: What he contributed was not evident. And however great or small his contribution might have been, it still would not have been evident, but it didn't appear in the hardware. If his contribution was that of guiding philosophical directions for hardware assemblies to go, you wouldn't see it unless you were in the conferences. So how much he contributed, I have no way of knowing.

STERN: Is that an unusual thing for an engineering project? In your vast experience of engineering projects, is it often that there is someone that plays that kind of role?

TRAVIS: It's not unusual. But you have to know him pretty well -- you have to be close enough to the project to see it working. I came on the scene late.

STERN: You came on the scene late. And with Chambers, Brainerd, and Pender...a lot of things that happened -- I asked Warren especially -- he didn't know. Of course Brainerd has his impression about this, which is based on his

subjective attitude toward Mauchly, and Mauchly has his reaction. But I have not been able to get a reaction of anybody outside that particular situation, and I find it difficult to understand. Did he come in and direct engineers? Did he have any authority --

TRAVIS: Well, you must recognize my reluctance to speculate. I could simply answer your question by saying, ask somebody who was there. I wasn't there, and there fore I don't know. This, I haven't done that. I have tried to answer questions within reasonable bounds of speculation. One can speculate only so far. One gets on hazardous grounds if one speculates too much. So, when it comes to interpersonal relationships, who did what when -- when I wasn't there -- anything I say has to be based upon deductions of what I think must have happened from how things came out and what I know of the people concerned. And on this particular thing, I've speculated as far as it's appropriate for me to do. Ok?

STERN: Ok, fine. Let me ask you then - just the couple of months that you were involved...the resignation, did Mauchly come down to the lab -- did he interact with the engineers, was there a kind of, on-the-record...?

TRAVIS: I don't know. The reason I don't know is I wasn't in the laboratory. I didn't. Sure, I'd stop and check in, but generally to see a demonstration..."We've got such and such working today -- come down and take a look. See what you think."

STERN: Well, there were a lot of demonstrations of that sort, too.

TRAVIS: There were -- there were a lot of demonstrations going on. I was working with Don Murray ? ...on financial aspects. ...So far as doing any practical work on direction, I didn't even attempt to. First of all, I wasn't close enough to it. It took me quite a while to get close enough to take any measures.

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END OF INTERVIEW