INTERVIEWEE: Irven Travis
INTERVIEWER: Uta C. Merzbach
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Merzbach:
Perhaps the logical place to start is with your birth. I believe you were born in Ohio.

Travis:
Yes, I was born in McConnelsville, Ohio, in 1904. McConnelsville is a small country town, population then about 500. It's a big city now, maybe 2,000. I went to elementary school there but moved to Philadelphia prior to going to high school. I went to Northeast High School in Philadelphia and took a combined mechanic art, that is manual training and academic, course. When I think back on the curriculum, I worked through then, some of these high school kids make me feel they have a wonderful vacation. I think, as a matter of fact, my interest in science began at a very early age, because my grandfather was a sort of amateur scientist. He was a Civil Servant on the Muskegon River who was responsible for building dams, building barges and general maintenance of the river for navigation. He reported to the Corps of Engineers who had cognizance of inland water ways, as they still do.

Merzbach:
This was your grandfather on your father's side?

Travis:

My father's side, my father's father. My father traveled a great deal, wasn't home much. I never knew him very well really. My grandfather was the person I looked to for inspiration and I became interested in "scientific curiosity" as a child. At Northeast High School I had the privilege of having some very inspiring teachers, particularly in mathematics. I guess having an inquiring mind as a child and having had quite outstanding training in mathematics was the thing that got me started in what I've been doing the rest of my life.

Merzbach:
How far did the mathematics go in that high school?

Travis:
Only through what I think we call now college algebra, trigonometry and solid geometry, no analytic geometry. Unfortunately. I think that ought to be in all high school courses. It wasn't when I was there. I guess a good many have it now. It wasn't when I was in school.

Merzbach:
But you did have trig and solid.

Travis:

Yes, trig and solid and fairly advanced attitude on algebra.

Merzbach:
What about science courses?

Travis:
I took the usual things. High school physics, biology, zoology, the usual things. Here again I had a physics teacher who was very dedicated and a pretty good physicist, rather better than most high school teachers, I think. I think I even got some rudimentary thoughts then about the fact that physics is made up of the real world that we don't know much about and experiments that we sort of correlate by means of a mathematical framework, and that if your mental picture doesn't match up with the mathematical framework you shouldn't worry about it. That's a pretty fundamental concept that I got at a pretty early age.

Merzbach:
You say you were pretty theoretically inclined?

Travis:
Yes, I always have been from the first I knew what the concept meant I think, I have been theoretically inclined.

Merzbach:
Did you play around with gadgets at all?

Travis:

Oh yes, I was a gadgeteer of the first water. I built amateur radio sets as a kid. I heard the Dempsey-Carpentier fight in 1923 on a crystal radio I built myself. It was a crystal detector, but it had two stages of the DeForest amplifiers. You wouldn't remember the DeForest tube but it's a little tube about so big with a grid and plate just like you draw them in the diagram. You know, the plate looks like that and the grid looks like that and it really looked like that inside the tube. The filament looked like that too. Of course, we had plate power supply then, we didn't even have what you call B batteries, but what you did was solder together ordinary flashlight batteries until you got a sufficient number of them to give you the desired plate voltage. So, I was interested in that sort of thing at an early age. I was always fooling with gadgets. I had a product when I was in about the sixth grade that was a best seller. It was a wooden device about six inches long, hollowed out with two prongs on the back. If you took a piece of wood and put them back to back, you'd get a four-paddle wheel. You'd take heavy rubber bands thrust through the four-paddle wheel and you'd wind it up and it would run. Now on the front end you have a nail through a hole, and ordinary cap like you have in a cap pistol and about three ounces of black powder. This was a magnificent torpedo and the older boys used to buy them from me. The material cost me about fourteen or fifteen cents, and I sold them for half a dollar. The mark-up was quite satisfactory. So, I've always been kind of gadgeteer. I used to delight in making Tesla coils. Ultra-high frequency high voltage that produced nice purple sparks about so big. So, I

guess, in terms of experimental work and in theory, by the time I was out of high school, I was well on the way to being an electrical engineer. I had the interest in the gadgetry and the physics and the hardware, and a pretty good mathematical background. So, I think the die was cast by then pretty firmly.

Merzbach:
You must have gone to high school in about 1922? 1923. Then you went right into Drexel.

Travis:
I went to Drexel and got a bachelor's degree in electrical engineering.

Merzbach:
Was there anything particular about electrical engineering rather than some other--

Travis:
As I've indicated I was interested in the physics and hardware of electrical gadgets. It seemed like the natural thing to do. I got a scholarship to Drexel, and, since I didn't have very much money, I got through as fast as I could. I graduated from high school in February and went to summer school, so I was a sophomore in September. I got through in 3.5 years. I graduated in February 1923, but I got my degree in June 1926, so I went through college kind of fast. Then I worked for Bell Telephone Company of Pennsylvania for a year.

Merzbach:
Doing what?

Travis:
Transmission engineering, outside plant engineering. Then I went to Moore School in the fall of 1928--in the fall of 1927 and got my master's degree in June 1928. I sort of never got away. I stayed on as a graduate's student, as an instructor, and about ten years later I got a doctor's degree. I almost didn't make it. I got to the point where the statute of limitations was eating credits off the one end as fast as I could accumulate them on the other end. There was always something more important to do than write a thesis, you know. I probably have more credits for my doctor's degree than anybody ever did, because it took so long and I had to keep it current, you see, until I finally got around to writing a thesis.

Merzbach:

When you entered the Moore School in 1927, what was it like?

Travis:
The Moore School then was an undergraduate school. The graduate program was just beginning. I think I was in about the second class. I believe there was only one class before I went there. The Moore School then offered four fellowships. That's the way they got the graduate students. There were only four. The year preceding there were four, and the year I went there, there were only four who were given "Moore Honor Fellowships," and I was one

of the four. Two of us got degrees and the other two didn't make it. Al Pender was determined to maintain a school of the highest of standards and he did, it was tough going. The undergraduate program back in the end of the twenties and all through the thirties was one of the most difficult and most rigid electrical engineering programs I know of anywhere. The Moore School doesn't have that same rigidity today that it did then. Pender had a philosophy about his young instructors. He said: Get known." He didn't care much how. That is, do consulting work, write a book, do some original research but do something, don't just teach classes, become known for some attainment. He really didn't care much what. Do something. If after two years as an instructor, you hadn't you wouldn't stay. It was just about that simple. The important people in the Moore School in those days, many of whom are still around, were Carl Chambers, who is now Vice President for Engineering Affairs at the University of Pennsylvania, was an important factor; Reed Warren, who is Assistant Vice President for Undergraduate Affairs; Chris Brainerd, who is Director of the Moore School now; Corny Weygandt who is one of the senior professors there; Charles Wyland and Knox MacIllwayne, who were very important formers of policy and architects of the Moore School through the thirties, have both long since passed away but they were important factors too. We had a pretty strong team in those days, and Al Pender was a great if not sympathetic leader. He was kind of a slave driver type, but everybody respected, and I learned a tremendous amount from him. He had a very brilliant mind. I don't know how much you know of Pender's history, but Pender was a graduate student at Johns Hopkins at about the turn of the century, and he worked under Rowland, and conducted with Rowland the Rowland experiment to prove that a moving electron was equivalent to an electric current. The French Academy didn't believe this experiment, so young Pender, who had just gotten his doctor's degree at the age of twenty-one, I think, went to Paris to demonstrate Rowland's experiment to the French Academy, which he did with great success. So, by the time he was twenty-two years old he was known pretty well around the world and on his way to being a pretty great physicist. He was a very brilliant young man. He went to MIT as an instructor. He worked in a consulting engineering firm for a while. One of his early associates was Michael Pupin, who is not unknown. One of his very close friends was Frank Puitt who has also been heard of once in a while. Pender came to the University of Pennsylvania as a professor of electrical engineering I just guess about 1919. At that time the will of Alfred Fittler Moore was languishing in the orphan's court because nobody knew what to do with it. He had bequeathed all of his estate to found a school of electrical engineering but there wasn't enough money to really do what his will said should be done. Pender conceived the bright idea that what could be done with this money was to found a school on a campus of the University where the service departments of English, Physics, History and all the other things were available at a nominal cost by transfer fees. Let the Moore bequest be the endowment that would operate the school of electrical engineering. It was founded as a 2-year school, junior and senior year granting a degree in electrical engineering, the first two years to be taken in the college majoring in mathematics and physics which isn't a bad plan anyhow. So, Pender was the guy who convinced the orphan's court that this was the thing to do with the bequest of Mr. Moore, and that's how the Moore School was established. It was Pender's brainchild. Mr. Moore's not quite enough money and the University's trustees agreeing that this maybe was a good

idea.

Merzbach:
Approximately how large was the undergraduate student body? You mentioned that there were only four graduate students.

Travis:
It was small. The senior class might have fifteen graduates. Total student body of seventy-five maybe. The building at 33rd and Walnut was Pepper Musical Instrument factory, which had gone out of business and the students and faculty practically rebuilt that building. Built the laboratories, built the equipment, and it was a giant research and development project for both faculty and students alike. This was '23 and '24. Brainerd got his electrical engineering degree, bachelor's degree in '25, so he was a student during these formative years of the Moore School, during its beginning years.

Merzbach:
What did you do for your master's degree?

Travis:
I did something very stupid. I built a device for generating a square wave which was a platinum needle that passed through a small pool of mercury, driven by a telechron 200 pole clock. So, I could synchronize an oscillator with this chopper and produce a square wave and a synchronized sweep frequency so I could look at it on an oscilloscope. This was before the days when you had a nice sweep circuit built into the oscilloscope, you know. You could buy an oscilloscope, but that was it. There was no instrumentation associated with it. It was cathode ray tube period. My purpose was to measure the initial emittance of a vacuum tube. I had heard about this bright idea that you could impose a square wave on a circuit and measuring the response of the square wave and using dual analytical of whatever it was, I've forgotten now, you could calculate its response to anything else. Well, I didn't realize that this didn't mean anything at that time, because obviously what I was really looking for was the performance of a nonlinear device, and this scheme doesn't work except for linear devices. So, I measured all right. What I measured was the initial emittance of the capacitor network that's involved in a vacuum tube. I know now that's what I did, but I didn't know it then. So, I wrote a very handsome thesis on this subject. I'm a little embarrassed now because it's all wrong.

Merzbach:
No one else knew.

Travis:
That's right. I wasn't alone in my ignorance but that doesn't excuse it anyway.

Merzbach:
Well, you--

Travis:
My master's thesis was the most unsophisticated thing, I'll tell you.

Merzbach:
Then you became an instructor at the school after receiving your master's degree?

Travis:
Yes, the fall term.

Merzbach:
Going back to your previous comment about Pender's policy, what did you do during your first two years?

Travis:
I'll have to stop and think. I did get reappointed, I know that. I'm trying to figure out why. Yes, I do know why. The summer—I guess the answer to that is when I got my degree in June '28, I went to work for a company that was then called the Audio-Vision Appliance Company. You probably never heard of it because it existed for only a year. The merger of RCA with Victor was in the conversation stage and the Victor Talking Machine Company felt that they were in a weak bargaining position with RCA in this merger, and they felt if they had some knowledge in radio and electrical things generally they would be in a better position. So, they set up a subsidiary, called the Audiovision Appliance Company, and hired a number of electronic types to develop a radio, which they did. They developed a micro synchronous radio. Then a year later RCA and Victor forming the nucleus of the present company. I worked with this company in Camden for that summer, and I became a consultant to that company during the ensuing winter term. I worked about a half a day a week there and the following summer with the RCA merger and I became a consultant to RCA after the merger. So, I did some consulting engineering work during those two years in connection with the development of test equipment to operate on a production line with signal generators and on of the earliest master signal generators with transport of all the broadcast signals to a test station. We had a master standard calibrated test station and run the receivers through this final test, in a shielded cage and the engineering of that test cage and the whole thing was done by a full-time RCA engineer and I as a consultant. I guess I got reappointed because Pender felt I was on the way to becoming a consulting engineer.

Merzbach:
At that time up to 1930 was there any particular activity as far as calculating devices were concerned, that you were interested in or the Moore School was interested in?

Travis:
No. The interest began when I took over the teaching of the power courses. I don't know what year this was, but I think it must have been '31. I had been interested in radio transmission, radio receivers, test equipment strictly in high frequency fields. The

man, who gave the power courses hadn't been doing a very good job and I knew that Pender was most dissatisfied with the courses in power. Being an opportunist, I thought, "well, gee, here I am competing with MacIllwayne and Brainerd and Wile and all these other guys who were associate professors dug in the Moore School in the field of high frequency. Here's the field of electric power going begging because nobody wants to do anything about it and a poor job is being done, so here is an opportunity." So, I went in to see Pender one day and said look, how would you like me to take a swing at teaching electric power courses. He didn't use these words, but he more or less said you can't do any worse than is being done now, so why don't you take a swing at it. So, I did. I became interested in the subject of power angle swings of synchronous machines which was the key to power system stability. Of course, it became obvious that all of these problems resulted in a whole hassle of nonlinear differential equations. The power field equipment is too big and too expensive to use the experimental approach. You've got to use an analytic approach. So, calculating boards and network simulators, analog devices were being used in the power industry then and it was about this time that Harold Edgerton developed his high-speed photography to study power angle swings of synchronous machines. This isn't perhaps so well-known because he's done so many fantastic things with his photoflash bulbs and high-speed photographic techniques. Sometimes people forget it had its origin in the lowly old problem of what keeps a synchronous machine in synchronism. He wanted to take high speed pictures of the oscillation of the rotor about its synchronous position. You can take a look at it with a stroboscope but that's only if it's steady state. If it is in the transient phase you've got to get pictures. Anyway, I became interested in this experimental approach and about this time Gray and Caldwell and some of Bush's associates built the cinema integraph at MIT and about this same time Bush and his associates built the first differential analyzer. About the time all this happened PWA money became available for worthy projects around the country. So, I went to see Bush at MIT to see what was involved in building a differential analyzer and enlisted the aid of Aberdeen Proving Ground to help get approval of a PWA grant to build the different analyzer. I got a year's leave of absence from the Moore School from my teaching job to take on the job of chief engineer and build a differential analyzer at the Moore School. Building drawings of it, made available to Aberdeen to have a duplicate machine built at Aberdeen. At that time the agreement was made that in time of war or other national emergency if the Moore School machine was needed to supplement the Aberdeen machine it would be made available. Those were the ground rules that led to so many things that you know about since.

Merzbach:
Would you mind expanding a little bit on what caused you to get in at Aberdeen. What made you think of Aberdeen? Had there been previous collaboration or was there anyone in particular?

Travis:
I don't remember. I do not remember how the first contact with Aberdeen was made. Les Simon wasn't yet at Ballistic Research Laboratory. A Colonel Zornig was in charge of the Ballistic Research Laboratory at that time. Les Simon came there some years

later. I don't know how original contact was made with Zornig or--

Merzbach:
Dr. Dederick was already there.

Travis:
Dederick was there. He was the one of key individuals in this picture. When I went up to see Bush he told me that a second machine was being built by Sven Rosseland in Oslo. He may have been the one who said that he thought that the Ballistic Research Laboratory would also have an interest. I don't know. I don't remember. My guess is, probably, the thought came from Bush.

Merzbach:
Do you recall what year you went to see him?

Travis:
'32 or '33. Very early thirties.

Merzbach:
I believe by that time he was aware of their interest because they had put some problems on his machine. Did you do any work—did you use the MIT machine?

Travis:
No. I borrowed Dr. Bush's chief draftsman, a chap by the name of Frost, who had done a lot of the mechanical detail design of the machine up there. I hired a crew of people. There must have been 150, I guess, machinists, tool makers, draftsmen, mechanical designers. It's the only project I know of that was ever operated for the purpose of maximizing the labor content. That was the purpose, to provide work. If you could make it you didn't buy it. If you could buy a piece of hardware for fifteen cents, you could make one on a lathe for four dollars, you made it; you didn't buy it.

Merzbach:
In other words, the construction of the machine was supported by WPA.

Travis:
Supported by WPA. It was done in the Moore School shop. The whole thing, design and construction, was all done right there with the exception of certain things which required skills beyond those which we had, which we subcontracted. We did buy a few things, have them built for us. Of course, the Aberdeen machine was all contracted to a company in Bridgeport, Connecticut. I can't think of the name of the company now. Upon completion of the differential analyzer at the Moore School, Corny Weygandt and I immediately put problems on it that we had been concerned with and about that time the General Electric Company ran into difficulties with their series capacitor installations. The series capacitor was sold to power utilities then to compensate for this inductive drop

and you'd just get the resistive drop and this was of tremendous help in many installations. Well, I think it was Pacific Gas and Electric, anyway, it was a utility on the West Coast, I think in the upper Northwest. They closed the circuit breakers and instead of getting sixty-cycle charging current into the transformers they got three times full load current at twenty cycles. Nobody has any idea why. The transmission engineering group from Schenectady were going crazy. The series capacitor business was on the threshold of extinction because, how can you design around this problem. They never even knew what the problem was. Cell Crary, Charlie Concordia and some of the then very young bright men of GE, now the graybeards up there of course, thought that maybe this brand new toy down at the Moore School could help. So, we set up the problem using the measured parameters that were involved in the series capacitor installation on the West Coast. We ran out the problem on the differential analyzer. We took the oscillograph records of what had happened out there. We still didn't know what was going on, but we knew the differential nonlinear equations that we set based on those parameters produced on the differential analyzer, the same phenomenon had been observed. So we started varying parameters, and more or less cut and try. We determined what ranges of parameters this one objectionable phenomenon would occur and what ranges of parameters it would not occur. We saved the GE series capacitor business. That's why my doctor's thesis is subharmonics and iron cord reactors. Now this is different from my master's thesis which is kind of stupid. This is a real practical problem that arose out of a very basic economic engineering problem with many fascinating theoretical overtones. I got to be a real expert in that subject for four to five years.

Merzbach:
Going back a moment to the construction of the Moore School analyzer, how closely did you stick to the MIT design?

Travis:
Not very closely. Based upon all that had been learned in the MIT machine, we avoided making all the mistakes that Bush had made. He told us what he would do differently if he were to do it over again. We copied some of the things exactly, when he said, "gee I'm glad we did it that way, that seems to be right." We followed his advice quite closely on some of the things he thought should be redesigned and should be done differently. Soon after the Moore School machine was finished, we found to get around these Neiman wrap-end band torque amplifiers which were a horrible mess and used the polaroid disc-type torque amplifier which was invented by a guy by the name of Barry at the GE Company. I don't know if you know how this thing works or not, but it's a very simple thing really. You have a driver which produces light plane of polarization in one direction. You have a follower which consists of two devices with planes at ninety degrees and of course the beam is transmitted through the two are equal when these two are set at forty-five degrees from the driver. So, you've got two beams of light. And the back torque is how much it takes to twist a beam of light with a piece of polaroid which is kind of small. The gain for all practical purposes is infinite gain. All you've got to do is have a suitable stabilization of the output amplifier to maintain stability. You've got a wonderful mechanical torque amplifier. I'm getting my story a little bit out of sequence

here. I should point out that based upon this work I had done with GE, GE asked me to come up as a consultant for a year to survey all their mathematical problems throughout the company and make a recommendation did, they need a differential analyzer of their own. I spent a summer and a half the ensuing winter a day a week in Schenectady interviewing engineering departments, research departments, I toured the company where technical work was done, to come up with what kind of load was involved if the GE Company did build a differential analyzer. By their own admission they could load it up about 30 percent of the time. Of course, everybody who was involved in the project knew it would be 200 percent of the time if we ever built one, which of course it was when we built one. I was retained for a further year to assist in designing and developing a differential analyzer for the GE Company. one was built just out the beginning of the war. I was consultant to GE for '39 -'40, those two years. During this period of time a lot of ideas came up as to automatic solution of differential equations and things like counters came into the picture. For example, a couple of people I was working with there were also working with Aberdeen Proving Ground on timing devices. Aberdeen seemed to get back into this picture from time to time. Timing devices whereby when a shell goes through one loop it triggers the counter off and by means of counting pulses between these two intervals the velocity of the bullet for this interval can be measured. By having a succession of these and a succession of triggers you could plot a curve of velocity versus distance over a considerable course of the bullet's travel. Well, this counting device you know had intriguing thoughts with respect to computing. We had the bright idea (I have a report in my files you might be interested in having to do with the interconnection of a whole gang of desk calculators to solve differential equations) of converting differential equations into difference equations and solving them by a bank of interconnected desk calculators, where you had a bit matrix of interconnections and you kind of plugged up the interconnection of the desk calculator the same way you interconnected the differential analyzer. The analog computer thinking was there and yet it was a digital device that we had in mind. We soon came to the conclusion with intervals such as one must have to get proper accuracy that the number of cycles involved with a desk calculator would wear the thing out; about one solution of a differential equation you'd have to put in a new desk calculator. So, this was clearly impractical from the standpoint of hardware life, which leads one to things like Geiger counters. The same thinking was involved in the velocity measuring group and the differential analyzer group and there was a little cross-fertilization of ideas here. Some of the very early rudimentary thoughts about an electronic digital calculating device emerged from this consulting work with GE. As a matter of fact, in 1940 I submitted a proposal to Section D-2 of NDRC, to Warren Weaver, to build an electronic fire control system. I had taken the shakedown cruise of the Nashville as a Reserve Officer in 1938 and I had gotten a short tour of duty at the Ford Instrument Company around this time in analog fire control equipment. My interest in fire control and interest in naval shipyard analog computing devices was born about '37-'38 so when I went on active duty in '41 of course at the Ford Instrument Company and later at Bureau of Ordnance, I already had some background in this field from being in the differential analyzer business, from being on the shakedown cruise of the Nashville and being familiar with conventional naval fire control equipment. I had some background there.

I submitted a proposal to Weaver and this thing hung fire for almost a year, I guess. We had a project organized, Braynard, Chambers, McIllwayne, quite a few of us, were going to be involved and we had a lot of ideas about what you would do in the way of building electronic equipment to do what was then being done by strictly mechanical devices. I'd hate to hang a basic mathematical misunderstanding on such an esteemed gentleman as Warren Weaver, but we emphasized the fact that what we wanted to do was build integrators. We emphasized the basic research should be on integrators. He pointed out that integration was a relatively small part of fire control problem. What he didn't know was that we weren't driving at integrators for integrators' sake. But with two integrators you've got yourself sine and cosine conversion devices which are very important in fire control. What we wanted was, to multiply by integration by parts--uv is the interval udv plus vdu. If you can integrate you can multiply. If you can integrate you an generate sines and cosines. Therefore, you can resolve components. Therefore, you can transfer from desk coordinates to inertial coordinates, to line of sight coordinates. You can do all the things you have to do in naval fire control, if you can integrate. That's all you have to do. You can do everything else. So, we were going to build integrators. He didn't appreciate how fundamental that was to the whole scheme of things. I think one of the reasons he took a dim view is he thought we didn't understand the fire control problem. If we had wanted to build resolvers that would convert to polaroid to rectangular coordinates, he would have gone for that in a big way. But we didn't phrase it that way; we wanted to build integrators. The project finally fell through, whereupon I applied for active duty in the Navy.

Merzbach:
Now this would have been 1941?

Travis:
Summer of '40 I guess.

Merzbach:
A while ago you mentioned the application of device for the torque problem, this was something that you added to the Moore School analyzer?

Travis:
It was built originally with the Neiman wrapping band torque amplifiers. About a year later while I was a consultant to GE the Berry device came up and it was decided then that the GE machine would be built with that type torque amplifier and that time, we'd convert the Moore School machine to that type of torque amplifier.

Merzbach:
Concerning the GE machine, I understand there was a twin that went to Schenectady and is it right that the one at UCLA was a twin of the Schenectady machine?

Travis:

I don't recall, it could be. I don't know.

Merzbach:
Something else I'd like to take up that went on before. You were involved in some other calculating machine activities. I'm thinking now of that equation solver that was in the Franklin Institute. Would you elucidate on that a little bit?

Travis:
It came out of the study of serial complex variables and things like Nyquist diagrams and the interest in finding stable designs and devices and it's obvious you need to know where the roots are, if you want a stable device. So, we got to thinking about what practical mechanical way can you find the roots of a polynomial since in connection with the stability of networks this is so important. Here again I was hipped on the subject of power stability and network stability and I was interested in what's with roots with positive real parts. They are a bad thing and you need to know how to deal with them. I don't know how we happened to hit upon this idea of rotating machines. That was part of my interest then. So, if you advance a machine theta and two theta and three theta, etc. and if you've got a cam that gives you exponential multipliers for each of the terms, you can scan the complex plane and you come up with the roots of the polynomial. And we built such a device. It wasn't very accurate, and it had some theoretical interest. A scaling problem, like all mechanical devices, was a tough problem. You've got to put the origin up in the middle of your polynomial so you can add it up. You've got powers and roots and you balance the thing so as to get the best accuracy you can get. The device wasn't a very practical gadget. It had some theoretical interest.

Merzbach:
Were any practical problems solved on it?

Travis:
Yes, some. I don't recall just what problems were solved. We built the thing and it was in the Moore School laboratory for several years. We did use it occasionally. Harry and I had a lot of fun building it, but I can't say it was any very great success. Harry Hart was with Bell Labs as a patent attorney for many years after that. I hear from him once in a while. He's retired as I am. I built another gadget in connection with my doctor's thesis, very special purpose gadget for solving a very special equation involving this general subject of subharmonics. I've always felt that if you want to get an engineering numerical answer to a problem that you've got to forget what the mathematicians tell you about solving in closed form. They select all the ones you can solve in closed form and ignore the ones that engineers run into all the time. So, you've got to have a gadget that operates like the thing you're trying to study and reduce it to some mathematical formulation and get numerical answers. So I've always been interested in finding a way to get numerical answers to problems.

Merzbach:

The one you're referring to that you did in connection with your doctor's thesis, what precisely was it supposed to solve?

Travis:
I broke out that thesis not too long ago. I don't understand it very well now. I knew a lot more then than I do now.

Merzbach:
Is it described in your thesis?

Travis:
It is. It's not important. I just mentioned it in passing because it was. . . I needed to solve a specific problem.

Merzbach:
There is something else I'd like to take up, and that is your association with Ford Instrument Company. You say that came about, about 1937?

Travis:
I guess it was '39 actually. I took the shake-down cruise of the Nashville the summer of '38 and I was aboard that ship for three months. I almost went to war in that ship. We were anchored in the Thames below London when Chamberlain came home from the second Munich Conference and we thought war was imminent. Our Captain said we will not be another Panai (?), and we got ammunition and ready boxes up on deck. They cut my orders for permanent duty. I sent a dispatch to Harold Pender saying I probably wouldn't be back for classes in the fall. That's how close it looked. You know in the fall of '38. Well, I did meet classes in the fall. I had become interested in ship equipment and I learned more intimately that summer than I ever had before. I guess it was the following year that I was at Ford Instrument Company for a while.

Merzbach:
Who was at Ford then. Was Hannibal Ford still alive? Was he active?

Travis:
Oh, yes. Hannibal Ford was still active. As a matter of fact Hannibal Ford was still alive and still active when I went on active duty in the Navy in 1941. Harold Baker was then inspector at Ford and I reported to Baker. Hannibal Ford was still very active. He was, as you know, an inventor of great talent. I knew Hannibal Ford very well, visited in his home and seen some of the gadgetry. He had an automated household like you've never seen. The Mark I computer, that goes with the Mark 37 fire control system, was in its day one of the finest pieces of hardware that was ever built. Much of the design was partially attributable to Hannibal Ford. The disc ball and cylinder integrator with tapered cylinders to compensate for spring deformation of the parts, to produce a linear output.

He got incredibly large torque out of those things will almost no variation error from no load to full load and then to design a disc ball and cylinder integrator to do that is no small achievement. The subtleties of design in that thing are something that the casual observer wouldn't appreciate. He can write down the equation and see why it works, by why was it so accurate. This was an interesting problem. The spiral bevel gears that ran within 1/10th on a pitched diameter when IBM undertook to build this equipment as a subcontract to the Ford Instrument Company, they wouldn't believe that the error had to be held to within 10/1000 on pitch diameter. No, that's ridiculous. The first 50 machines they built were rejected because they didn't believe it.

Merzbach:

When was that?

Travis:
That would have been late '41. The demand for Mark I computers had risen far beyond Ford Instrument Company's capability, even though Ford had expanded fivefold I guess in the preceding 12 months. See, we had a big load on for British ships at that time. This wasn't too well known then. I guess now it can be told, but many British ships came in there for overhaul of their fire control systems and many of them received Mark I computers. As a matter of fact, the skipper of one of the British ships came into Ford to look over the equipment that was to be assigned to his ship and I was quite taken with this guy, a brilliant man. I was amazed that the skipper of a ship would have the detailed technical knowledge of fire control that he had. I was really impressed. It was Louis Mountbatten. I spent a couple of hours with him showing him around the Ford Instrument Company, and it was a great privilege to have just known the guy for that long. Brainy guy, very brilliant guy. Of course, he's just the Uncle of Great Britain, but he was then just a skipper of a battleship.

Merzbach:
What were your specific duties while you were associated with Ford?

Travis:
I was one of 4 or 5 inspectors whose job it was to handle the correspondence with the Bureau of Ordnance for special designs and modification of equipment and I spent a certain amount of time on the floor supervising the actual inspectors who were running routine final test, before shipment. We specified tests to be conducted and supervised before the final testing and acceptance of equipment before delivery to the Bureau of Ordnance. In other words, Harold Baker and his staff were the on-site representatives of the award, the customer, to the supplier. It was our job to see everything was the way the Bureau of Ordnance wanted it.

Merzbach:
What was the formal arrangement. Were you on leave of absence?

Travis:
Yes, for 5 years. From June '41 until April or so of '46.

Merzbach:
In '41 that would have been after you entered the Navy. What was the arrangement before that when you were with Ford?

Travis:
That was during the spring. . . No, summer vacation. I took the cruise on the Nashville in the summer of 1938. In the summer of '39 I was at the Philadelphia Navy Yard on active duty and part-time at Ford Instrument Company. In 1940 again during the summer I had a tour of duty at Philadelphia Navy Yard and then in '41 I went on active duty. So, I had three summers of part-time duty before I went on active duty in '41. The reason I went on active duty in '41 was because by cruise on the Nashville in '38 and it was obvious to me that war was imminent, and we were going to be in it. I didn't foresee Pearl Harbor, naturally, but there wasn't any doubt that one way or another we'd be involved in a war before the academic year '41-'42 had gone its course. I felt the smart thing for me to do was get ordered to duty in the summer of '41 instead of getting ordered to duty arbitrarily during the academic year. It turns out my timing was just about right.

Merzbach:
Let's take up there. What was the sequence of events?

Travis:
Well I went down to the Bureau of Ordnance and Bureau of Ships, Bureau of Naval Personnel in the spring of '41, shopping around for a job. Among other things they said what you ought to do is go up to see Harold Baker, and it became very clear to me that the very best place I could enter the navy on active duty would be right there. So I made a request for orders at the end of the academic year '41 to be ordered to duty at Ford Instrument Company as assistant inspector, and I was so ordered. I was there for a year. In the spring of '42 Baker had been talking to a classmate of his, Andy Murphy, who was then head of a newly established desk in the Bureau of Ordnance, namely fire control research. It was a new desk. Before the war there was L Section which was in charge of all fire control; research, development, production, maintenance, installation, everything. The Bureau was re-organized into a production division, maintenance division, research division, and Murphy was the first and original head of fire control research in the newly organized Bureau of Ordnance. He was a classmate of Bakers. Baker said in effect I've got a guy up here reporting to me as inspector who's got a technical background that would make him more valuable to you than to me. I know you need people who can do research in this field. He said frankly I think the Navy would be better served if Travis were sent down to you. So, Baker engineered my transfer to the Bureau of Ordnance, which was a great thing for me, naturally. It gave me an opportunity to do many things that were of professional value to me later and it did enable to make a contribution to the war effort that was greater I think than I would have

done any place else. So, in the summer of '42 I transferred to Bureau of Ordnance where I remained until the war was over, except for temporary tours of duty here and there and everywhere, but that was my home base from then until I was discharged from the Navy.

Merzbach:
What activities were you involved in?

Travis:
Well, I was involved in all the activities that had to do with development of--I was assigned to the antiaircraft desk. I reported to Murphy and my responsibility was antiaircraft which meant computing devices, servomechanisms, gun laying devices, radar. Of course, in those days radar was a new thing. It had been developed primarily as a search device and a navigational device and its use in fire control, although it was recognized as important, was something that wasn't too well understood. Fire control devices were designed to use very inaccurate range measurements with optical stereoscopic range finders in very accurate angles which you'd get with a telescope of cross hairs. Radar unfortunately produced very accurate range and very poor angles. So the whole philosophy of fire control had to be changed to take advantage of an entirely different pattern of error, that you had with radar inputs as opposed to optical inputs. This was a point that we stumbled over for about a year. It seems kind of obvious. What do you do about it? Nobody knew quite what to do about it. One of the early problems was resolving this question. How do you build a piece of fire control equipment that will smooth lousy angle measurements and take advantage of accurate range measurements. How can you take advantage of a moving target, how can you take advantage of the fact you know its angular position poorly and its range accurately in order to get the best solution to the problem, to forecast its trajectory, as opposed to the conventional problem we knew all about. What do you do when you know your angles very accurately and know your range poorly, how do you forecast future position? So, the problem was suddenly changed quite drastically. So, all equipment had to be re-examined, what do you do now. Range smoothing was well known, and we did this as routine. But angle smoothing, that was something else again. So much of the very early work was how do you adapt radar equipment or fire control equipment. How do you patch it up on an interim so they fight with it tomorrow, then how do you do it right to replace the equipment later on and how fast. So much of the early work was the compromise problem that you so often have when you're doing something one way and you find that it must be done a different way. So, you've got two things to do: you patch it up and get along as well as you can until you have time to do it right. Since you've got limited manpower it's always a compromise. You spend effort to keep them fighting today, or do you be sure they fight as they should next week. So, this was a trying dilemma to resolve. f course it was clear that mechanical fire control equipment required tremendous amount of skilled mechanical manufacture, fine tooling, highly skilled machine tool operators and assemblers. These people were in short supply. It seemed obvious to replace this precision mechanical equipment with electrical or electronic equipment that would tap a different type of talent and perhaps by using precision electrical components reduce the manpower drag to fight a big war by changing

away from this talent in short supply. What we didn't know was that we were going to run into new talent in equally short supply. We didn't really gain anything; we changed the details of the problem; the fundamental problem remained. We had projects with RCA and Bell Labs, GE Westinghouse, Eastman Kodak. You can go up and down the list of big companies with talent in this area, and we had contracts with all of them. Of course Section T and Marrel [?] tube and the proximity fuse were coming along then, which was another factor in the antiaircraft fire control business. If you don't have to set fuses any more with accuracy, this is a big plus in knocking down aircraft. Of course, there were new concepts that evolved too. The question of target designation was an old fashioned Navy term. Everybody knew what that was. You'd hit the sailor in the seat of the pants and say hit that one. That was target designation. When you've got a radar that gives you position from a search radar within a certain domain, certain angular domain, certain range domain, there is a hunk of space out there and you can say it's within that, then you're going to have a fire control radar that's going to pick up that target. First of all the fire control radar seeing space has to be big enough, bigger than the designated location space or you'll never find it. Well, how much bigger. What should be the relationship between the zone of vision of the acquisition (this was a term we invented) radar as opposed to the designation radar. Walter McWilliams, who is now with Bell Telephone Laboratories, and has been prominent in the digital computer business, was one of my key technical assistants in the Bureau of Ordnance.

Walter and I sat down one day to talk about this general subject, how big the zones have to be, and we needed a word. People used the word target designation, well that wasn't it. You don't designate a target. What you do is, the fire control radar that's going to shoot with its gun against a target has to somehow or other latch on to this target and we finally came up with the word acquisition. The problem is target acquisition, not target designation. You know, it's amazing what slaves we are to semantics, but as soon as we invented the word target acquisition the communication barrier between the Bureau of Ships that were building navigational radar and the Bureau of Ordnance which was building fire control, this communication barrier disappeared. It took the invention of that word to get the point across. We had been stumbling over the concept for a long time. I give credit to that one word that Walter McWilliams suggested as being an outstanding step forward in radar fire control business in World War II. It's silly that I attach so much importance to a single word, but I do. It's now 1943 I guess. . .

Merzbach:
It occurs to me, in connection with McWilliams didn't you have some major project in the preceding year, 1942, in relation to submarine propulsion?

Travis:
Yes, we wrote a proposal. We didn't know about the Manhattan Project then. We were real smart fellows. We thought it would be a fine thing if nuclear energy could be developed for submarine propulsion. We felt the greatest problem was that submarines weren't really submarines. They were boats that could dive. They weren't submersibles in the true sense. We recommended that a project be established to do research in a nuclear propulsion device. How did you find out about this?

Merzbach:
You mentioned it.

Travis:
Anyway, we wrote a memorandum that we thought this would be a real smart thing. We knew enough about physics to know that it could be done. We'd read the literature; we knew about equations and we knew that it was there to be had. So, we wrote a very serious memorandum on the subject. Strangely we never heard a word about it. We were kind of annoyed about that. We felt, by golly we had a real smart idea and we were being brushed off. A couple of years later we found out why.

Merzbach:
To get back to the question of computing devices, were there any particular machines that were considered in relation to the antiaircraft problem?

Travis:
The Mark IV computer as developed by Bell Labs was an electrical analog computer which was really a 1:1 analog of the Mark I mechanical computer. It solved the problem the same way, did the same thing exactly, except it used electrical components. I was

analog, wasn't digital. It used electrical resolvers of the type we now regard as conventional. It used electrical cams, shaped cards, one of them with resistance wire and all the standard tricks we know now.

Merzbach:
Was it developed within Bell Labs?

Travis:
In Bell Labs ... It never went into production. Only one was built. It went to Chesapeake Beach Test Station where the Navy tested much of its fire control equipment. We had two important test stations--Dam Neck, Virginia, where we did actual firing of antiaircraft guns against the sleeves and drone targets, and Chesapeake Beach where we did more theoretical analytical work in the testing of proposed equipment. We had another place, Blakistone Island in the Chesapeake, we did some test work. These field test stations were satellites to my operation in the Bureau of Ordnance and whenever a prototype was to the point where it should be evaluated it was sent to one of these stations. Sometimes we also had a half dozen more built and put on board a ship in an experimental installation. The Mark 50 fire control system that was developed by Arma, now part of American Bausch, was one which was made. . . I guess we made 20 or 30 of them and sent them out as experimental installations. The Mark 49 and 45 fire control systems were built by the Ford Instrument Company, set out in experimental lots and put abroad ships for tests. None of these survived.

Merzbach:
What was the reason for this?

Travis:
Well, the reasons varied. Inability to maintain the thing in operable condition at sea was almost always the fundamental thing that killed it. They were ambitious technically, complicated technically, and they were developed under forced draft without adequate understanding of conditions at sea and adequate understanding of the quality of maintenance personnel that would be available, namely very low quality. They were wonderful things if you could have sent a couple of engineers with each one of them, which we did in many cases. We finally got to the point where we brought in junior officers into the factory and put them in the final test for three months with a piece of equipment they tested. They carried it all through the final test. They understood the equipment completely, they trained blue jackets as maintenance men in final test and the whole crew was sent to sea with the equipment. We did this time and again and it was a way of getting new equipment operable at sea which otherwise would never have worked. It's not the right way to do it, but if we don't have time to design it right, at least you can send people who can make it work even though it has design defects. That's what we had to do as expedient. Sadly, all too many equipment's went to sea in that condition. Towards the end of the war we had some pretty fine equipment out there, though. Mark 49 gave way to Mark 51 which was a greatly simplified and improved version. It used Starr Draper's lead computing sight, 14-gun sight. We soon

learned that 20mm and 40mm guns were no good as antiaircraft weapons. We learned that the 5" 38 was the only weapon we had that would really knock down a plane. We learned that Mark 38 fire control system which was the pride and joy of the Navy was no damn good against aircraft, not in a real war it wasn't. What we did do was take the old handle bare director, one-man handlebar director, with Mark 14-gun sight directly connected to a single 5" 38 mount for the paralax(?) computer to correct for the line of sight and gun position. Train the operator there is no sense shooting these airplanes 15 miles away, you're wasting ammunition and will miss them anyhow. You've got this equipment, don't overkill. Don't send 15 bullets after this guy because there are too many coming in anyhow. Give him three bullets and go on to the next one. But local control, Mark 51 director 5" 38 with proximity fuse killed more Japanese aircraft than all other equipment's put together, in the last year of World War II. It was the answer to the kamikaze plane, it was the answer to the terrible threat that we had out there. We lost a destroyer a day, you know, for weeks. The proximity fuse, the 5" 38-gun, the Mark 51 director and local control was the only reason we didn't lose many more than we did. A kind of simple-minded piece of equipment really and here's a very important lesson. It's a reason I fought against the Sage system from the beginning. It had no fail-safe capability. But when you're fighting a shooting war, complicated scientific devices where it's all in one basket is a snare and a delusion. I think many of our present-day people don't appreciate this. Simple-minded fail-safe device that does a pretty good job, not a superlative job, but a pretty good job and always does it, is the kind of thing I'd like to have in my hand if I'm going to fight a war. The very complicated device is a pretty dangerous thing. I shouldn't go any more philosophical or I'll get classified here. I'm on the threshold of being classified already I think in these remarks.

Merzbach:
What about digital devices now?

Travis:
Well, I alluded briefly to some of my very early rudimentary thinking about digital devices when I was consultant to GE Company. Then I didn't think about digital devices again really until much later. I was so preoccupied with analog devices and throughout the war digital devices in the field where I had an interest had no place then. They weren't developed to the point where they had any merit.

Merzbach:
This is a question I wanted to raise. Was any extensive use made of any kind of digital device for these problems we've been talking about?

Travis:
At Aberdeen there were a couple of relay computers built by Bell Labs, a thing called MADM, and there was a digital computer installed at the Naval Research Laboratory. These were not used online but were used actually to analyze field data for evaluation of equipment. This digital relay computer at the Naval Research Laboratory was used as a data reduction computer. It worked almost full time from my section in Bureau of

Ordnance on reduction of firing data that we developed at various test stations. What they did actually was take continuous strip recordings, oscilloscope recordings and other strip recordings, and sample it and then convert it to digital
form, then work out whatever it was we wanted to know about it, on the digital computer. So I'd say that so far as I know digital computation during World War II was restricted to data reduction insofar as military applications are concerned. Of course, these were all relay type computers. The ENIAC was not finished. As a matter of fact the ENIAC project came across my desk when I was in Bureau of Ordnance for a recommendation. I took a kind of negative view, not because I didn't think it was a smart idea, but I thought that we shouldn't divert effort that could be used to winning the war on something however technically meritous it might be that couldn't have an influence on the outcome. I felt that the ENIAC project which I was called up to give a Navy opinion on, was something that shouldn't be given very high priority because I felt this can't possibly influence the outcome of the war and I had a single purpose in mind then; let's win the war and then we'll do something else. So, I gave it a very lukewarm recommendation for that reason. I said this is a fine idea, but gee, let's fight the war and get on with these interesting scientific gadgets later.

Merzbach:
What about the other digital computers that were in operation. Bell Labs you mentioned.

Travis:
I didn't know about it at the time but there was equipment in use in Arlington Annex, equipment's which are the forerunners of some of the things that NSA does. I didn't know about those then but there were some in this application.

Merzbach:
But you were not at all involved with them.

Travis:
I was not. I had no knowledge of them. I learned about them late '46 or early '47 after the war was over and after I had become associated with some of the people at Engineering Research Associates; it was then where a few naval officers landed after the war, Bill Norris among them and John Parker who was one of the founders of the ERA, Howard Engstrom, John Howard, some pretty smart guys in ERA of the early postwar days. Of course they all came out of this very special segment of the Navy during the war who had the need for equipment of this kind and (where) the most sophisticated thinking of this equipment was done.

Merzbach:
How much aware were you of some of the early experiments with digital equipment even before that? The early work done by Stibitz.

Travis:

I was pretty well aware of Stibitz's work. As a matter of fact, I knew Stibitz. I haven't seen him for years, is he still living, still at the University of Vermont?

Merzbach:
No, he's at Dartmouth.

Travis:
Wasn't he at the University of Vermont? The last I heard of him that's where he was.

Merzbach:
He's at Dartmouth. As a matter of fact, I'm going to see him soon.

Travis:
Well, give him my regards. I haven't seen George Stibitz in years. Prior to World War II, I used to see a good bit of him. We shared an interest in the roots of polynomials at one time, back when Harry Hart and I were developing this gadget, George Stibitz was very much interested in that same subject. Indeed, I think he built a gadget not unlike the one we did, a different principle.

Merzbach:
This was really his first relay machine, but of course the motivation is very close.

Travis:
We were interested in the problem for the same reason. Well, I knew Stibitz pretty well in those days and we used to exchange ideas. I think I was sort of aware in a general way of what he was doing all along.

Merzbach:
Were you at that American Mathematical Society meeting where his machine was demonstrated in 1940 at Dartmouth?

Travis:
No, I was not.

Merzbach:
What about, Atanasoff, did you—

Travis:
Not until afterward. At the time I did not have current knowledge. I did not know of his work at the time he was doing it. I learned of it a little later. I became aware of it, I guess, right after the war, 1946, I guess. I'm very vague. I guess all I recall is that he did

some early work in digital devices.

Merzbach:
What about the work of Harvard? Did you have any contact with Aiken and his group before the end of the war?

Travis:
Not before the end of the war. My contact with Howard Aiken began immediately after the war, I think spring of '46. I saw a great deal of Howard, I visited his operation up there. In '46 and '47 I was consultant to Reeves Instrument Company and we had the Cyclone Project at that time and in connection with that work I visited people who were doing similar work. I used to drop in to see Howard Aiken pretty often and Jay Forrester and Perry Crawford was at Naval Research Laboratory out on Long Island at that time and he used to come down and talk to us about these things. I guess from early '46 on I was in close contact with Howard Aiken. As a matter of fact, one of the first guys I hired when I started to work for Burroughs Corporation was Bob Campbell, who had been an associate of Howard Aiken's. As a matter of fact, I went up to see Howard, and said I need a good theoretical man in this field, whom do you recommend. He recommended two people. I can't think of the other one right now, you'll know his name as soon as I think of it. He's with Sperry-Rand now I think, went with them shortly thereafter. I went after both of them; I got one of them. I used Howard as a consultant on personnel pretty heavily when I started the Burroughs research laboratory in 1949. He helped me on several occasions by suggesting people I might look up. I remember one of the guys I tried to hire in those early days, I couldn't succeed in dislodging, but subsequent events proved I was after a pretty good man. That's Brock McMillan. In 1950, I guess, I tried to hire Brock McMillan; I didn't succeed.

Merzbach:
You let the Navy in '46 and at that point returned to the Moore School?

Travis:
I was on terminal leave for the last three months of '46 I guess, and I went back to Moore School and was appointed professor. I left as an assistant professor, but I pointed out that I'd been gone for five years and I would have been an associate professor all that time. So, I was appointed professor and also supervisor of research. At that time I undertook to organize more formally the research programs at the Moore School and to put into that program management techniques similar to that which I had learned about at Johns Hopkins which had a very fine organization and MIT and Department of Industrial Cooperation and my Navy experience. I had seen educational institutions that did it right and the Moore School was doing it all wrong, at least in my estimation. So, I undertook to reorganize the Moore School research pattern more after the pattern I had observed in some of these other institutions where I thought they had been quite successful.

Merzbach:

I don't think we'd mentioned the contact with Johns Hopkins before. What specifically did you do?

Travis:
I was aware and kept informed about the VT fuse program. Of course it was very close to the work I was doing in Bureau of Ordnance, and when the VT fuse program was nearing its conclusion was perfected and in production and everything was pretty well wrapped up, Merle Tuve observed quite properly that he had accumulated a very high-powered scientific team to do the VT fuse job. It would be sort of a shame if that team was disbanded, what other important job could it do. It was suggested that the blind firing control system using radar input on mount was something we were working on with some success, but a duplicate second effort might be justified since the project was so important. So, such a project was set up at APL, Johns Hopkins, and much of the team that worked on VT fuse were transferred over to this project, which was a duplicate and parallel project to some work that was being done under the Bureau of Ordnance at MIT. Indeed, there were two projects, one at Radiation Laboratory under Evon Getting who is now on the West Coast at Aerospace Corporation. Getting headed up the Mark 56 fire control system and Starr Draper headed up the Mark 52 fire control system. A third effort, all aimed at roughly the same objective, was set up at APL under Larry Havstadt, who is now Vice President of Engineering at General Motors. He was in the senior researcher under Merle Tuve at APL. It seems to me he was in charge of that project. At any rate I was associated with that work because two jobs reported to me aimed at this objective and this was a third one over which I had no direct contact, but APL had a couple liaison officers assigned to my work and I had a couple of guys assigned to theirs to be sure that duplication was constructive and not just a waste of time. Any bright ideas they had that I could use I took with no apologies and they did the same with the ideas my people had. So, we worked pretty close together although in a sense the projects were competitive, aimed at the same objective. It was a kind of race to see who was going to win. I guess none of those horses in that race really won the race, but it used all the early ideas that were developed in the other projects. So, I was close to the ALP work at that time, both in respect to VT fuse and this fire control system they were developing.

Merzbach:
You mentioned you set about to reorganize the research effort at Moore School.

Travis:
I set out to do some real sales work with potential sponsors and to get organized more formally and to set up a formal relationship between the research program and the academic program, whereby there were some people who were full-time academic employees and some people who participated in both programs. The salary structure and tenure and patent questions and the whole organizational structure needed some formalization. The Moore School hadn't done anything like this prior to the war and it was trying to do sponsored research in a wholly academic atmosphere, which those who've tried it find doesn't work too well. At the same time, I was consultant to Reeves

for a couple of years.

Merzbach:
At that stage of the game what was Reeves doing and who were the people involved?

Travis:
The two key people were Chalmers Dewey who was Vice President for Sales and Harry Bellock who was Vice President and Chief Engineer. The President was--He's an executive of Claude Neon, I've forgotten his name. He was sort of President in absentia. Harry Bellock and Chal Dewey ran the show and the guy whose name was on the organization chart as President was concerned about whether they were making any money, but he never concerned himself about operations. You almost never saw him. I was consultant and the three of us actually, Dewey and Bellock and I sort of ran Reeves at that time. They ran it full-time and I spent a day a week there helping them as well as I could. We had a scheme for bidding on projects which I think is a fine scheme. I recommend it highly. You can set a group of estimators to work and they'll add up all the numbers and work out the whole thing and come out with a number too small of course. It can't be too big because they price out everything they thought of. But what did they forget, well it's what they forgot that it's too small. So, this scheme doesn't work. What you do is take three people who are experienced in the business and then estimate. So Chal and Harry and I used to read the specifications, read the RFQ and kind of visualize what we'd do and we'd each write a number on a piece of paper. Then we'd toss the pieces of paper out on the table and add them up and divide by three and that was the bid. You'd be surprised how many bids we won and how many we made money on, by just that procedure. What did I do for Reeves? I was the general consultant on whatever problems there were. I guess more than anything else I helped them develop proposals, a technical approach to a request for a proposal that came from one of the government agencies and I helped them in the selling. I helped them develop a technical way of going about it, helped them price it out, I helped them organize a team that would do it and I helped them convince the customer that we were the guys which would do the best job for them. I was really in the spearhead of technical procurement business end in that I helped their engineers invent a way of doing it and I helped figure out how much it would cost, how many people, and help sell the government agency that we had a technical solution to their problem. I did that for a couple of years.

Merzbach:
The major project at that time was Cyclone.

Travis:
Project Cyclone. What is more important is what became of REEVAC. That was a product that sold considerable quantities. That started out as kind of a bread board analog device using resolvers and amplifiers and it became the classic gadget for solving the old stability problem that I had worked on years ago in a different way. The REEVAC was a real powerhouse when it came to solving network stability problems and Reeves sold quite a hatful of them, I don't know how many, but that was a very profitable product.

Harry Bellock was involved, more Harry's than mine, but I participated in that program, although I think Harry Bellock deserves the credit for REEVAC. Without his vision as to what that thing could be as a product, it never would have happened. I didn't have the confidence he did.

Merzbach:
I think this is a good stopping place.