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MERZBACH:

All right, shall we start then about the time that you left high school?

MAUCHLY:

Yes. Well, sometime during my senior year in high school, I had a little change of direction, in that I'd been expecting to go to the engineering school at the University of Cincinnati. The reason being, for one thing, my father had gotten his doctor's degree from the University of Cincinnati, and I had learned from him that they had an engineering program there, which allowed you to work part of the time while you were studying your engineering subjects. You'd go out six weeks someplace and then you'd come back and study for six weeks. This was known as a cooperative plan. It had some educational advantages, in that you got acquainted with the use of your engineering in, you might say, the real world. It also had some monetary advantages in that you would be paid, you would earn some money while you were doing this. It sounded as if it was a way to get through school without a large amount of financing. That was what I was thinking about when one of my English teachers suggested that, since I lived in Maryland, I might possibly qualify for a state scholarship to the Johns Hopkins University Engineering School. I guess it was the first time that I knew Johns Hopkins had an engineering school. Lots of other people have known Johns Hopkins as a medical school, and didn't know anything about the rest of the school. I went into this, and found out that it was possible to take an examination and apply for an engineering scholarship. I went to Baltimore and took the examination sometime, I guess, in the spring, but well ahead of the enrollment time for the college, and I was told by the senator to the State Legislature, who had to recommend and approve you, that I'd done well in the examination and that he would gladly recommend me. So, indeed I got this scholarship, and went to Johns Hopkins School of Engineering on the state scholarship for, I think, the first two years--l925-26 and l926-27. Somewhere in there, I got a bit fed up with the engineering. This was partly because the early basic courses in engineering seemed to be, largely, what you might call cookbook style courses, where you learn to design something by following a formula or table in a handbook, and I didn't seem to learn much about the fundamental background of natural phenomena. In part, it was also due to the fact that some of the other people that I knew through my father's work were in physics. In fact, some of them were employees of the Carnegie Institution, Department of Terrestrial Magnetism, where my father worked, who were taking graduate work in physics at John Hopkins and were instructors in the undergraduate physics work. These were such people as Merle Tuve and Lawrence Hampsted, who were working part of the time at Carnegie Institution, and also doing degree work at Hopkins. So I naturally swung over to the idea that what they were doing in physics was more interesting. This was where the real fun lay. This was where the action was, you might say, so at the end of my second year of engineering, I made arrangements to transfer to the physics department and started working toward a Ph.D. degree. At that time, I believe, the President of Johns Hopkins was a man named Goodnow, and he had announced a plan known as the Goodnow Plan, which made it unnecessary for you to get any intermediate degrees. If you wanted to get a Ph.D., you didn't have to stop for a bachelor's degree, or for a master's degree. You could just work right through. So, I undertook to do this. Of course, I couldn't have done this without some financial help, and it turned out that the Department of Physics had something known as the Quincy Scholarship, which I was eligible for. I also could earn part of my tuition as a laboratory assistant, sort of a junior instructor. The more senior graduate students would actually teach sections of the physics classes, while the junior ones presided in the laboratories. So, I did this for the next two years, at the end of which time, l929, I would have been graduating with a bachelor of engineering if I had stayed in engineering school, but in this case I wasn't getting any degree at all. I was merely going on toward fairly advanced work in physics. The only way I added to my record, you might say, was in l929. I was elected a member of Phi Beta Kappa; and also to Sigma Xi, the two honorary societies which normally don't select their membership from the same sources. Usually, if you're a Phi Beta, you're not a Sigma Xi and vice versa, but somehow I managed to get both of them.

MERZBACH:

Was there anything during this period, as far as the training was concerned, that you think was particularly useful, relevant or, on the other hand, might as well have been dropped from the curriculum?

MAUCHLY:

Well, I don't remember particularly. I guess the most useful thing from my point of view, was just the general atmosphere. Here I was in the Department of Physics essentially, with other people who were working with me, and some of them were more advanced students, and it was more of a scholarly and research atmosphere than I would have had in the Engineering Department. I remember that somewhere along the line in there, we developed a thing which we called the "three hours for lunch club", I believe. We hardly ever took three hours actually, but it was a nice name for the club. The new physics building had been built on campus, whereas my first few years of physics were actually within the engineering building, where we used to conduct our graduate laboratory underneath the pitched roofs, where the head room was rather slight. When we got the new physics building, we had more than enough room for a while, and so some of the rooms at the top floor of the new physics hall were available for having our own little lunch club. We brought an electric grill and a few things like that, and a number of us would go up there at lunch time and fix our grilled sandwiches. That seemed to be the age of pasteurized cheese products; Old English and Velveeta Cheese had just come on the market. They'd get a big loaf of those things, and slice it up with bread that we'd bought at fairly standard rates at the store, and you'd had a very cheap lunch at the same time a good conversation with the people that you were lunching with. There were also some interesting ways of passing lunch times. Somewhere along the line I met some girls who were in the Psychology Department and who were working with Johnson Institute, was it, of Child Psychology, on Hopkins campus. They had a nursery school there to practice with. One of the ways they kept their budget down was to go out and buy some bread and other things, and come back and fix lunch in the Psychology Department's nursery school quarters. I'd go over there and visited them, and be accessed thirteen cents for lunch, because that's what the materials cost.

MERZBACH:

Did you pursue any particular projects in that time, or within any specific areas of physics, do you remember anything that seemed to occupy your thoughts?

MAUCHLY:

I don't think there was anything terribly compelling or exciting in the way of new ideas. I mean I was anxious to get into some things which sounded interesting. Professor Murnaghan, for instance, was well-known for his courses on tensor analysis and things of that sort, and so I tried to fathom these, and was, I'd say, mainly impressed by the delicacy and accuracy of his handwriting at the blackboard. He would write very small script letters on the blackboard with precision of alignment, and everything exactly right. His chalkboard calligraphy was wonderful. Murnaghan was an interesting lecturer, but I can't say that I really took to the use of tensor analysis as the powerful tool it could be if you use it right, but it was an interesting course anyway. I guess one of the things that fascinated me as much as anything, was to watch other people doing glass blowing in the student glass blowing shop and try my hand at that too. At any rate, somewhere along the line, I made the decision, if you want to call it that, not to make a decision. Namely, I decided that I didn't want to stick exclusively to theoretical work or exclusively to experimental work. Whereas it was the rule of the game in those days practically, that you made a decision somewhere that you were going to be a theoretical man or you were going to be an experimental man. My various compatriots along the way were dividing up that way. Some of them would say they were going to work in infra-red, and they would patiently build some apparatus and get a salt crystal, or something to refract in an infra-red spectrometer. Then, they would patiently sit taking galvanometer readings which now-a-days are, of course, done in fifteen minutes with an [?] machine or something. Over months, they would collect enough data that they'd draw a spectrum and say that this was their thesis work, you might say. With that much theory attached to it, it's merely a new set of experimental records that had never been taken before to add to the literature. Then there were other people, like another friend of mine, named John Wheeler, who's now a professor at Princeton, who would sit in the library with a slide rule and compute Schrodingor's wave equations, or something, and was doing entirely theoretical work with some practical numerical calculations on the slide rule, you see, as a basis for his studies. He didn't have to go near a laboratory, and all of his work was quite abstract from that point of view. I didn't think I wanted to take either of those routes and so I was looking for something which combined experimental work and theoretical. Just at that time, a new member of the staff joined the Physics Department, Dr. Eachus [?], who had been in Japan on an international research fellowship and was the first member of the staff who specialized in band spectrocity. So instead of worrying about the spectrum of a simple atom, I now would look at the spectrum of molecules, which is much more complicated, and with hundreds of more lines in it to try to analyze. The only way you can get the spectrum is to do the experimental work, to photograph it, and after you have the photographs, you had the theoretical work of trying to make some sense out of it to the point of view of the energy level of the molecule. With Dr. Eachus [?] on the staff, I presented myself to him and said that I would like to do some thesis work in band spectrocity with both experimental and theoretical ratifications, and I became his first student at Hopkins. That meant that I had to do a lot of glass blowing to make the apparatus, which was to produce the light, and then use the large concave grading spectrograph which Dr. Wood had ruled and was his pride and joy in the way of spectrographic accomplishments. Then he'd sit down, and first of all, he'd sensitize the plates, and then he'd go into the dark room and develop the plates, and the plates had to be sensitive to both the infra-red and the ultraviolet end of the spectrum. When you got through, you had long pieces of glass which had these lines on them, some of which were what they called spurrow lines, that come from other elements which you weren't interested in, but most of which came from the molecule which you were studying. In my case, it was the carbon monoxide molecule. Dr. Eachus [?] noted, since he was much more familiar with the literature than I that someone in England had published a paper which was a result of their thesis work. I believe it was an East Indian who had done the thesis. The result had been, by their analysis, that this spectrum could be analyzed with what they called quintuplets. This looked highly suspicious to Dr. Eachus [?]. He didn't believe that carbon monoxide molecules could produce these quintuplet structures in the spectrum. When we got it all done, we indeed found a much more reliable analysis, which was checked in many ways, that showed that the lines occurred in triplets rather than in quintriplets. This finally got published as a paper by Dr. Eachus (?) and myself, about the same time that I had my thesis ready, and went through the oral examinations and so on, and got my doctor's degree. This was a pretty long drawn-out and grueling bit of work with all the experimental work and to do all the analysis work, too. This involved a lot of computation with a desk calculator, an old Marchant thing which was known, at least to the Marchant people at that time, as the "Lima Bean Computer", because it had a handle on to shift the carriage, and with your left thumb, you would push on this lima bean-shaped handle if you wanted the carriage to go right or left. Your right hand was occupied pushing the plus over the minus bar by which you could accomplish semi-automatic division, and if you were careful of just where you stopped or started the plus and minus, you could accomplish multiplications, of course. There also were little multiplier buttons on the side of it, which, if you wanted to multiply by 6, you punch 6. Then you'd shift the carriage and punch 8. If you shifted the carriage the right way, you'd get 86 as the multiplier, or if you shifted the other way, you'd get 68, of course. So you could get very adapted to trying to do your computations fast on this thing. You couldn't speed up the motor, at least I didn't try to, because it being a purely mechanical machine, if you ran it twice as fast, you'd probably wear it out ten times faster. So I did a lot of calculations which had to be done, to reduce the measurements on the photographic plates eventually, to the wave lengths which were equivalent to this in vacuum, and correct for the changing wave length which occurs when the plate is in air, which it was in the room. The further results were, of course, to take these wave lengths and convert them to wave numbers, since we were taking reciprocals, and then taking proper differences and so on, to get energy levels, and finally figuring out what this all implied with respect to the characteristics of the molecule. Well, when I'd done all of that and got my Ph.D. in l932, why there was a depression. I was told, "Don't look forward to coming out and getting a well-paying job. In fact, you probably won't find any jobs. What you'd better do is to weather it out here for awhile until times get better, and we can pay you as a research assistant". I think it was 50 [?] an hour. And there are plenty more calculations". Dr. Eachus [?] had other research papers of his own being written, you know. "You can just work with the faculty on their research now, and they can pay you 50 [?] an hour." Well, this also meant measuring more plates as well as calculating, but there is an awful lot of calculating to do. I got one assignment, which was to calculate theoretically from published formulas, what the energy level would be for a molecule which was known as an asymmetric rotator. In other words, a molecule which had no symmetry about it, but its moments of inertia were different in all three perpendicular directions. The theoretical results were derived by quantum mechanics and were published, but no numerical solutions had been made. Dr. Eachus (?) was wanting to apply this particular thing to formaldehyde spectrum. He thought it might be useful to have all the calculations made as to what the various energy levels would be and the resulting spectrum, if you assume certain characteristics in the molecules in its moments of inertia, etc. So he sent me to work computing that. Well, at least some of the pertinence to this to the rest of my life, I guess, was that I sort of got fed up with computing with computing machines. There is a certain joy in seeing how fast you can push the buttons to get the right answers, there's a certain joy in being able to check the answers by doing the calculations in some different way, and finding if it agrees or if it doesn't, and finding out why and remedying the troubles. But you get to feel that you are making haste slowly and what can you do to speed it up. Well, the first thing I could do to speed it up, was not to buy a new computing machine that worked faster or build one, but just to see if there were any tricks of numerical analysis, you might say, which would get you there faster. I suggested to Dr. Eachus [?] that if I spent some of my time doing that, that I thought it would be very beneficial. He said, "Oh no, you just go ahead and do it the way I told you". So I decided that that might be the best advice from his point of view, but that he wasn't looking at these calculations the same way I was looking at them. All he really wanted was the answers, and why should he dictate how I was going to get those answers, you know. So I decided that it would be fair enough if I spent some of my own time without charging 50 [?] an hour to him for it, you know, to see what I could do to speed up the calculations. Then, if I was successful in that, I could recoup that time I'd spent by putting in the charge for it in proportion to how much I'd saved. (I had my own little formula for keeping honest about this. It made it more exciting and more adventuresome. The first thing I did was to go back to the library to check where these formulas had come from, and I located the original articles that were in German, with eigen values of matrices, and all that. Then I found that there was a symmetry in the problem, which nobody had made use of apparently or at least it hadn't been made use of in the calculating formula. Presumably, it wasn't a symmetry unknown to all the mathematical physicists in the world, but it hadn't been made use of. That immediately cut the labor in half, because you only had to compute half as much, and the rest was derived from that symmetry.

MERZBACH:

At this point, how much formal training had you had in, say, matrix, vector theory, or this sort of thing?

MAUCHLY:

Well, of course, vectors were pretty much the stock in trade, you might say, of the mathematical physics courses and things. A lot of analysis was done through vector analysis. Matrices had only become of interest to the physicists, so far as I knew, by reason of Heisenberg's quantum analysis, quantum mechanics. So the ideas there were not too hard to assimilate, but assimilating the general ideas doesn't make it any easier to calculate the groups of, let's say, a 30x30 matrix, even though it has come symmetrics and is not just an entirely general matrix. The thing was, we were having to do these calculations for increasing size of matrix. You started with 2x2, and then a 3x3, then a 4x4, then a 5x5, as the quantum numbers increased, and even though the numbers you were computing reached a discrete point in the quantized energy system, they all fit on continuous curves. So you could regard the results as having a continuity, and therefore do interpolation and extrapolation. So I found that if I solved this thing for several points, that then, I could extrapolate and find out approximately what the solution was for the next, and if you know the approximate solution to something, then you are a lot further ahead then if you just had to guess all over again as to where to start. So that was a big help in speeding it up. Actually, what I found was a set of formulas which pretty soon gave the approximation so well, you see, that you could just solve a few of these things, and then interpolate the others. For instance, you might do it for the quantum number J of, say, 4 and l0 and l6 and 20, or something like that, and then interpolate what it would be for all the intermediate numbers. With things like that, plus the symmetry business, I considerably speeded up the ways of getting these numbers, and that made me feel that maybe I was on the track of doing this in a more wholesale way and not just for a molecule, but let's put out a bigger table, so that anybody could get the approximate results just from entering a few parameters. I later started to do that after I'd gotten the job, but I found out that I had some competitors who were better armed than I. Before I talk about that part, maybe it would be interesting to note that somewhere in there, I suppose it was the summer of l932 or l933, since jobs seemed to be scarce I decided that maybe I ought to get a little bit of what they called education courses for high school teaching. If you wanted to be a high school teacher, the educators of long ago agreed, apparently, that you ought to have a teaching certificate and that you ought to have courses in how to teach as well as what to teach. I had also married in l930, and my wife was a mathematics major from Western Maryland College. She had helped to support us in the first year or two by working for the Family Welfare Association, it was called, and there were a lot of people on relief. She also, for a while, worked as an assistant to an Actuary, I believe it was the Maryland Casualty Company. It was an insurance company in Baltimore, which was distinguished by the fact that it had one of the few, if not the only woman actuary in the world. But at any rate, these two jobs came to an end--the actuarial one and the welfare one. She was free to do other things, and for one year we ran a restaurant at our house, which was on Wyman Drive, and it was the street right next to the Hopkins campus on Homewood. I'm not sure whether we made any money, but at least we ate. In other words, we took in enough money from other people eating there, that I think it paid for our food. So with that kind of experience, we were facing this apparently endless depression, and the summer school at Johns Hopkins had education courses, and in particular there were two of them that sounded interesting. One was teaching high school geometry and the other one was teaching high school algebra. They were both given, I believe, by a man named Hart, who came from the New York State school system. We enrolled and both of us took these two courses. I found them rather interesting, I couldn't say I could discourse on that subject now, but if we had to have courses in education, these were probably the most interesting we could find. Also, as a part of that, I had to carry my wife to class for a while. At night time I'd be down in the laboratory working on experimental things. One night my wife came over, and we had a sort of one of these garden chairs that extends out, sort of a steamer chair I guess you could call it, with the canvass and wood. She went to sleep while I was working, and all of a sudden, she woke up or maybe I called her, and she was startled or something or other, and she twisted her ankle and sprained it. Well, they taped that up and put her on crutches. This was during this period when we were going to summer school. I'd drive the car up to as close as I could to the entrance and then I'd carry her up the steps, and put her on her crutches. We never got an assignment or a job to teach in high school, either geometry or algebra or anything else. Instead, sometime before the beginning of the academic year l933, Ursinus College was looking for a physics professor and the Physics Department suggested that I go after it, and I did. It was a church affiliated school, Reformed Lutheran, and so they seemed to be interested in character references as well as anything else. It was not only what teaching courses I had had but rather what references I could get, that seemed to be important. As I remember, I got a reference from the President of the Carnegie Institution, whom I barely knew, but apparently, he must have thought my father must have a good son. I got a reference from the pastor of the church and the principal of the high school (McKinley Tech). I've forgotten what other references. I was told later, after I joined the faculty of Ursinus, that the faculty had been appraised of my qualifications and things and had a part in saying whether they wanted me as a member of the faculty, and that they were mighty impressed and that they had never seen such references before. But, as I say, these references had nothing to do with whether I was a good physicist or not. At any rate, in l933 I joined Ursinus College as Head of the Department of Physics. In fact, I was the only person teaching physics there. That was when I thought that I could, in addition to teaching work, get something done on research. It seemed to me that since I had no longer any contact with the instrumentation facilities, the spectroscopes and measuring machines and do on, that I had had at Hopkins, that from now on I was going to have to do my research work more in theory, in calculation. What better way to do that then to exploit what I had already learned about molecules' energy level, and how to compute them more efficiently. So, all I needed then was a desk computing machine and a desk calculator. Although the depression produced a lot of bad effects such as bank failures, there were some good effects from all this, you see, because it was possible then to acquire for $75, a second-hand calculating machine, which was owned by a bank that had gone out. I got this calculating machine, and started to work on the energy levels of the polyatomic molecule. I found out, as I made friends around Pennsylvania, that there was a man at the University of Pennsylvania, named Dr. Witmer, who had a government grant from WPA or something of that sort, which was again a depression organized instituted agency. The grant to him was being used to pay some people, using exactly the same calculator, a Marchant with a green handle, the same kind I'd used at Hopkins, the same kind I'd bought second-hand. He had someone else doing the work for him and they were working, you know, six or eight hours a day at this, and I only had spare time with teaching duties. So I thought that even with the edge that I had on, using the symmetry and other things to go faster, the race wasn't going to be necessarily in favor of me. This impression was deepened considerably when I found, in a recently published article, that a man named Dr. Gilbert King, working up at the Arthur DeLittle Laboratories, was using punch card equipment to compute the same things. I got a little more interested in what you could do with punch card equipment, and found a book which had recently been written by Dr. Wallace Eckert, an Astronomer at the Naval Observatory, on how to do scientific computations with punch cards. The methods he discussed were already well provided. You had IBM punch card equipment available to you, which I didn't, and that was expensive, too. I had several different ways to go from there and one of them that I started exploring was in the area of statistics, which I had known nothing about when I got my doctor's degree. Of course, when you're getting a Ph.D. in physics, you've been very heavily exposed to what is known as statistical mechanics, which you might call deductive methods of statistics. There was nothing at all centered about the inductive methods. It was not until after I'd got my degree, that I was even aware, I think, of the work of Robert A. Fisher and a lot of other people, on what you could do with statistics in the way of making inferences from experiments, and statistical design of experiments and any of those things. In fact, all I'd learned about statistics up to that time, had made it sound more like the engineering that I'd shied away from. It sounded like handbook stuff, you know. You just went to a handbook and you found whether you compute the standard deviation by this formula, and you computed correlation by this formula. When you get through, you reach the pinnacle of success if you could compute a correlation coefficient. In fact any person that took statistics in a business administration course, I'm sure, felt that the correlation coefficient was the acme of all, the real quintessence of statistics. If you could get that far and just compute one, just once, why you'd practically done the equivalent of Moses getting the holy tablets down, you know. So, all of a sudden, this new world of statistics opened up. It opened up just because I was interested I in comparing how my teaching and my students in physics might stand, with respect to other people's students and other people's teaching. When I tried to get some information on standard testing procedures, about which I had learned nothing in Hopkins Graduate School, why, I found out that here was a realm of whatever you want, call it psychometrics or something, in which they were throwing around such terms as validity of a question, and the reliability of an examination, and so on. There were a lot of things that you could do with quizzes and examinations besides just giving a student a mark, and this led to a rather extensive exploration into what statistics was all about, and what people were doing.

So I began to ask myself questions, and I'd say, "What else could be done with statistics in view of the developments that I was reading about"? I guess the most helpful thing at that point, was finding that there was a journal called Econometrica which had a sort of a tutorial summary on the progress in statistics and mathematical statistics each year. This digested, in very brief form, some of the things that were in the forefront of statistical investigation and development of that time. This made me feel that with a calculating machine, the desk calculator I had, the $75 bank thing, and a source of data, that one might apply some of these new methods to available data, and maybe get farther in analysis of that data than anybody had before, because here were these powerful new tools to use. In particular, I knew that where my father would work at the Carnegie Institution, that there was lots and lots of data. So I took a summer job at the Department of Terrestrial Magnetism; I suppose this was around l936. I tried to see whether I could apply some of these methods. The job paid very well, a hundred dollars a month, to allow me access to this great wealth of data.

MERZBACH:

What data did that entail exactly?

MAUCHLY:

Well, I don't remember just where I started first, but I think that it was probably in my father's domain of atmospheric electricity. After my father died, the Director of that work, a man named Oliver Giesh, felt sympathetic toward me, I guess, and decided that they could afford to pay me a hundred dollars a month if I wanted to putter around there. In particular, the problem I was trying to get a grip on was that they had measurements of what was known as potential gradient, in the number of volts that you could measure between two points of different altitudes in the atmosphere, divided by the distance between them. It comes out in volts per meter, then, which would vary with time. They had measurements of this sort for three or four different observatories around the world. If you plotted the yearly average of these measurements for the different stations side by side, or above each other, you might say, in time, the curves look somewhat similar. Now the question is how do you measure this similarity? While these traditional methods of correlations that I was speaking of, for instance, will tell you how similar, you might say, one variable is to another, but here you have say, three different stations and therefore you can pair-wise, take three different correlations. You can correlate [?] measurements in Paris; you can correlate [?] measurements and with, I don't know, Australia, oh, I don't know, maybe three different places. Now, if you want to get the most out of the data, how do you combine those three comparisons into one, and say that there is evidence for these things all varying simultaneously, or there isn't. That was the central problem as I saw it. There didn't, at the first glance, seem to be any method of statistics developed to do this. Well, I found out that the educational psychologist had already considered a similar problem, which is known by the name of Sherman's "G" factor. Sherman was a psychologist who was trying to evolve and test a theory of general intelligence, and "G" stood for general. This "G" factor was something you would, presumably, attribute to general intelligence, and if people got scores, for instance, on arithmetic tests, and they got scores on English tests, and they got scores on other kinds of logical manipulations, you see, and so on, then you have a number of tests on a group of people. Can you then some way evolve from this a measure of general intelligence, as distinguished from their aptitudes or intelligence, in particular, things like arithmetic or English or logic? So this was an ongoing problem to which there were various solutions. In particular, there were some which involved finding that score which was most predictable as they called it. A professor, name [?] of Columbia University, had written a paper and worked on that. Among other things, I went up to see him, and he told me about some of the things he had just recently been working on, which he called Canonical correlation. His paper was not yet published, but he had written the paper, and it was just going through the phases prior to publishing. He described to me right then and there what the paper was about and how you did this, and I immediately solved some applications for this, I thought, in geophysics. So, one of the next things I was doing then, I guess, the following summer at Carnegie Institution, was to write some papers, which I presented to the meetings of the International Geophysical Union, on the possibility of applying these new statistical methods, first to this potential gradient problem that I already described, and secondly, to the problem of finding out whether any evidence existed for there being more than one main variable in solar variations which might affect geophysical variable. In other words, in the past, from the time of Wolf and Wolfer in Switzerland, sunspot numbers have just been sunspot numbers. You have one series of things with sunspot numbers which was suppose to measure the variability of activity on the sun, and anytime that anybody wanted to correlate that with what's going on on earth, why, they always used sunspot numbers. Some of the classical things that people used to write about, sometimes with conviction and sometimes with high skepticism, were the studies, for instance, on the variations of the price of wheat with sunspot numbers, or the level of the Lake Victoria Nianza with sunspot numbers and things of that sort. So there were great arguments developing in statistics, as to whether these correlations were spurious and purely accidental or whether there was something behind them. I'd found out through prowling in the libraries of the Carnegie Institution, particularly, that there were a lot of other arrangements made on the sun in addition to sunspot numbers. So, with three or four different sets of measurements made simultaneously with the sun, it seemed as if there might be some possibility of discovering that what people had treated simply was really a little more complex. If you take an x-ray examination of the sun and also infra-red, they may vary differently, and some things are more responsive to the x-rays and some things are more responsive to the infra-red, let's say. The mixture of the two effects then could be quite confusing unless you separated them. At any rate, I wrote these papers and presented them to the American Geophysical Union, I guess it was. I don't know whether it was an International meeting or not. I was trying to illustrate the use of these new statistical techniques, not deriving anything definitive and conclusive from the amount of data that I analyzed, but rather list as examples some techniques that they ought to be used for and a start at trying them. As a result of that, I got more and more interested in statistics. I actually wrote two more papers on the developments which I'd made myself in the field of statistical inference.

Without describing those right now, the other thing that happened as a result of this interest in statistics and my attempts to apply it, first to atmospheric electricity and then to other geophysical variables also at hand at the Carnegie Institution, was that I got lectured at by the people who had been my father's colleagues there and who regarded me as just a young up-start, you know, who really hadn't had enough experience yet to do anything. Some of them working in geomagnetics said right away that the study of the earth's magnetism and the measurement which we make on the earth's magnetism are really proper scientific things. You know, you can measure those things in a more or less pure form, but this atmospheric electricity is all effected with this meteorology. Everytime a dust storm blows, it affects the atmospheric electric measurements. So really, there's nothing very scientific about atmospheric electric stuff. You can't expect to make sense out of that.

Why look what happens in meteorology where this fellow, Charles Greeley Abbot, the Secretary of the Smithsonian Institution, always claimed that you can predict the weather with one of these solar constant measurements, where the essence of everything is that maybe the solar constant isn't a constant, but varies. Through the years, he's spent all his time and effort in establishing observatories and making these measurements and there's really nothing to it. He's got it all messed up with meteorological and variables and things and he can't really tell whether the solar constant is constant or not. Well, I hadn't paid any attention to Charles Greeley Abbot at that time. He was a neighbor of ours in Chevy Chase and I didn't really know very much about him. As I learned a little more, I found out that, sure, he had made these claims. He hadn't any good statistical methods on his side, but as far as I could tell, the people who were saying there's nothing to it, had no statistical methods on their side either. That's why, sometimes, they called us "the American love for the underdog", or something. If you're going to fight fair, you know, fight fair, and let's supply the best statistical methods we have to the problem, and maybe we can really substantiate what Abbot is saying, or maybe we can bury it a little more and show there's nothing to it. At least, give it a fair chance.

So, I got hold of a lot of weather maps and again, due to the years of the depression, the students at Ursinus College were being paid out of some government subsidy. I think they were being paid 50 UNK, 300 an hour to rake leaves and do anything else the College might find as useful work. By arrangement with Ursinus College Administration, I got some of these science students, math and chemistry and physics majors to start taking figures on weather maps and getting them reduced to a form where I thought they could apply some statistics to them.

By l940, I guess, I was presenting a paper to the American Physical Society, not to the Meteorology Society of which I was a member, but to the American Physical Society of which I was a member, because I felt that the physicists might give me a better unbiased opinion. The meteorologists were pretty well certain that the sun did not affect the weather. It just couldn't. I thought maybe the physicists would take a little more open-minded approach to this. I presented a paper in which I tried to show that there was evidence for solar effects on precipitation.

In l940, I managed to get that far only by constructing an analog computing machine which did harmonic analysis on some weather variables, because I was looking for evidence of something akin to the solar rotation period occurring in the meteorological variables. In other words, we know that the sun rotates something like twenty-seven days for a full revolution, and the sunspots, more or less, recur with that period, although they may take a little longer or shorter time, because the various latitudes of the sun rotate at different speeds, and depending on whether the spots are at high altitudes or low, you get different results. Also, some of the sunspots grow and become more effective and others die out, so that you get what is known as a quasi-periodicity out of this, rather that a strict and true periodicity.

Well, the methods for analyzing data for this kind of cross quasiperiodicity had already been developed in geomagnetism, particularly by Bartels and Chapman. Bartels was a German and Chapman was an Englishman. I believe Sidney Chapman is still alive, but Bartels died some years ago. These two men who were known the world over as the foremost scholars in geomagnetism wrote a two-volume classic on this subject. For one thing, it was very well known because of the statistical methods which Bartels developed and handled for this kind of a quasi-periodicity situation.

It does seem to me that if you could use that tool to establish the relationship between solar variables and meteorology, that this would really be the best way of convincing the geomagnetic people who said that there was nothing to this meteorology business to use their own tool against them as it were. This is what I did with the help of an analog computer, which enabled one of my student to do one harmonic analysis in about two minutes, whereas doing it with a desk calculator would take about 20 minutes.

MERZBACH:

Did you use a standard machine, or did you have the students put it together, or what?

MAUCHLY:

I put it together myself.

MERZBACH:

What was that? How did you come up with the idea of that?

MAUCHLY:

Well I don't know exactly how, but I, first of all, I was more orientated toward an electrical device...solving these things electrically rather than mechanically. There had been mechanical harmonic analyzers, you probably have several examples of them here. Lord Kelvin, I guess, and lots of people studied tides for instance. The hydrographic office had one that was a tide predictor, and you could synthesize the thing once you know the analysis for it, set it up so that it will tell you what the tides will be in the future at Norfolk or some place. So but I felt that it was within my scope, I didn't have a machine shop or anything like that, but I did have a soldering iron and copper wire and things. I bought some meters from General Radio Company and some potentiometers and things, and I put together something which worked within the limited accuracy of an analog machine built out of more or less commercially available parts, and not too expensive.

MERZBACH:

There were various electric algebraic equation solvers in the thirties, such as the Mallard machine. Had you taken any interest in that development?

MAUCHLY:

I remember hearing about that. No, I didn't. Of course, doing a harmonic analysis is somewhat different than trying to find the roots of polynomials. All and all, I'd say that probably the main result from my point of view of this work leading to the l940 paper on quasi-periodicity in precipitation was, that it had taken an awful lot of work just to get that much out of the data, and that you'd have to have better computation means to do anything. You needed to put more data into the system and maybe compute it in a more sophisticated manner, etc.

So, all of these things, trying to do something in the meteorological, geophysical and solar geophysical correlations, seemed to be driving further and further in the direction of needing better computing equipment. I went to the l939 World's Fair, for instance, with some interest in other things, but my main interest was looking at what Remington Rand and IBM had on exhibit. There were interesting things there, but the one that seemed to be the quintessence of what they had to offer so far, was that Remington-Rand had a mechanical multiplying machine which could make the product of two six-digit numbers and read the factors off a card and punch the answer back, and print out a tape at the end with the answer, all in one second.

Whereas, the sequential multiplier that IBM had on the market then, took 6 or 8 seconds to do a problem like that. Even though this was a step forward doing it in one second, it still was pretty slow, and pretty expensive, because here we had about 40 years of development in punch card equipment, and it wasn't really handling even multiplication very well.

In fact, this monograph by Dr. Wallace Eckert at the Naval Observatory on scientific uses of punch card computing didn't even use an IBM multiplier. He did this by that I think was known as progressive digiting. That is, you'd sort the cards out so that you got each digit of the factors sorted together, and then you do all the multiplications by three, by using the accounting machine, that is the printing tabulator that had adding machines in it. You would add three times, you see, to multiply by three. Then you take all the cards that had 4's on them and add four times, and eventually you solved all these partial products and then you'd come out with the effective product of many digit numbers. This was the way he was computing the nautical ephemerous there. Of course what happened to Wallace Eckert after that was that Thomas Watson, Sr. thought that such diligence in the use of his machine should be supported, and so he put him in charge of the Watson Scientific Computing Laboratory, at Columbia University. He remained there until retirement. So far as I know, he didn't solve anymore problems in this field as to what to do about speeding up computation. I, on the other hand, had during these thirties been reading the physics journals, and in particular, reviews of scientific instruments where various people, some of them I knew at the [?] Research Laboratory in Swarthmore, for instance, were measuring cosmic rays and they were actually counting the things by what they called scaling circuits. These scaling circuits were vacuum tube circuits which were able to respond to counts which might occur as rapidly as a million per second. To be able to manipulate numbers at this speed, to count at a million per second, was far beyond the capacity of mechanical gadgets at all. Not only the adding machines with gear wheels, but the relays which you get to clickety-clack, mechanical motions which had armatures which, even though small, were still massive compared to an electron. So the vacuum tube which merely had to push electrons around, could do things at least a thousand times faster. The economics of it seems clear to me. The vacuum tube and a relay were about the same cost, but one could work a thousand times faster than the other. So, if you had enough to do, why you could essentially make your computations a thousand times cheaper with vacuum tubes. But first, you had to have some money.

I had money enough to buy vacuum tubes, but still that wasn't enough to put a computer together. As I saw it, whether I was going to attack the problems of computing the energy levels of a large molecule, or going to try to do something with solar weather variables, and perhaps improve the forecasting of weather, anyway you looked at it, you had to speed up the process of computation and also, of course, reduce the cost per operation. The speedup has been quite often publicized as the important thing, whereas really it's the economic cost reduction which counts. The progress of computing machines from the time that we did build the ENIAC, with l9,000 tubes, to the present time, where practically everything is done with some kind of a solid state device or equivalent, of thousands of transistors may be in a very small chip.

All this progress has really been progress in reducing the cost of computation. Sure, it's faster, but the thing is, the cost does not go up proportionately. Now you've got to the point where you can do as we do with many other items of technical progress. You can waste it. [?] remarked that when he first considered what electronic television could do, he was thinking about the immense benefits of education. Well, if you look at television programs nowadays, you don't see very many people watching the educational programs even though there are some. Most of the time, we're watching the ones that are really entertainment. There may be something educational in Laugh-In or in Jackie Gleason, but for the most part, people use television to be entertained. Maybe they don't use computers to be entertained, but they quite often use cheap computation as an excuse for computing anything, no matter if it's worthwhile or not.

MERZBACH:

I'd like to backtrack just a minute to get some of the dates straight. You were at Ursinus during the period starting in l933.

MAUCHLY:

l933 to l94l.

MERZBACH:

Now, did the Carnegie association continue through the summers?

MAUCHLY:

I think it continued for about three summers--l936, l937, l938, but I'm not sure. I certainly think that l939 I did not, at least not a paid job with them. I may have had access to their library but they weren't paying me. In l940, I spent an interesting summer working with a long range weather forecaster, who was one of the few who was accredited as being a sincere honest guy by his meteorological associates. That was a man named H. Helm Clayton, who was also a good friend, I think, of Abbott's. He ran a private forecasting bureau in Canton, Massachusetts, and had for many years. Apparently in his younger years, he had been head forecaster for one of the South American countries. He became impressed with the idea that he could analyze the motions of the atmosphere some way in conjunction with solar cycles. He set up a weather forecasting service which tried to make long range predictions, and he wrote many papers which were published, many of them in the, what do you call these little pamphlets that came from the Smithsonian Institution. *Contributions Miscellaneous Publications*, which were in little brown paper covers, etc. Some of H. Helm Clayton's papers are published that way, and some were published in the Transactions of the American Geophysical Union, etc. In l939, when I gave these two papers on statistics at the meeting here in Washington, H. Helm Clayton aid that he would like to have further association with me and could I come and work with him some time, like next summer, and I agreed.

By that time I had built my harmonic analyzer, and so I think it was the summer of l940 then that I filled my car full of all sorts of things I might need for the summer, and sent my wife off to live with her mother here in Chevy Chase, where I'd stayed when we were working at the Institution. I went on up to Massachusetts.

I minimized my living costs by staying with a sister who lived up there, and I would drive over to Canton where his house and office were, and talk with him a little bit. He'd say, "Well now, what do you think you'd like to do?" I'd say, "Well, I'd like to do this". He said, "You go ahead and do it then. I'm not to tell you what to do. You seem to be well directed. You go ahead and do what you think you should do".

So a great part of that summer I spent again with this little desk calculator, the "Lima Bean", and also with my harmonic analyzer, which I had brought with me, computing the effects which the planets had on creating tides on the sun. This was a theory that some people have had which I think has some merit to it. It has not been fully explored, I believe. Some of the variations in sunspots can be attributed to the planets, and if you could just somehow inter-relate the motions of the planets with the workings of the sun, you ought to be able to predict, on a long range basis, because the planet's motions can be predicted on a long range basis what sunspot effects to expect.

I spent the summer doing that, but again computation is slow, so I got enough charts that made me more interested, but I didn't get any conclusive results out of that.

As I remember, Clayton at the end of the summer asked whether it would be possible for me to keep on working with him, and maybe join his business, because he needed some young blood to carry on, I guess. I said that I didn't think I could see that as what I wanted to do right then.

The following summer, in l94l, was when I was accepted into the Defense Training Course for Electronics at the Moore School, and met Eckert, J. Presper Eckert, and no relation to Dr. Wallace Eckert. From then on, of course, the ENIAC developed through the Army Ordnance contract. I'm abbreviating a lot right there, when I say that.

MERZBACH:

Before we get into that whole phase, one thing I think might be of interest, and that is, during the preceding decade, when you were at Ursinus, what about your teaching load, the demands on your time as far as teaching versus research, and the amount of assistance you had, both in terms of manpower of student body and financial and all of this kind of thing?

MAUCHLY:

Well, the financial assistance arranged by Ursinus College wasn't as much as I expected. The only assistance they actually furnished was salary, and in the original commitment that I thought I had with them, they were going to pay me something like $2200 a year in salary. However, when I arrived on the scene, I found out that due to the hard times the College was in, all the faculty had made voluntary contributions to the College of l0% of their salary the preceding year and by golly, they were going to do it this year too. I had to be one of the boys. Maybe it was $2400 and came down to $2200 or something like that. At any rate, I had a l0% reduction there. During the period I was at Ursinus, they finally found it possible to get along without that reduction, that kick-back, and also to raise my salary, so by the time I left there in l94l, I was getting $2800 a year. As a sign of the times, why even then opportunities weren't too good, I was still on the Civil Service roles and...

MERZBACH:

Civil Service?

MAUCHLY:

Yes. One of the things that I had glossed over was that in l930, I guess, l929 or l930, I had earned some money during the summer working at the Bureau of Standards, and the first year I worked at the mechanical laboratories calibrating water current years and testing fire extinguishers. We'd set a fire in an old building we had for the purpose, and then see whether this soda acid extinguisher would last for two minutes or whatever the standard time was supposed to be. If it did, it was okay, and if it didn't, it was no good, and things of that sort.

We also tested numbering machines such as the Post Office uses. Some of them will stamp the number changing each time, and some of them essentially have a binary counter in them, and stamps the same number twice and then changes, and then stamps the next number twice and then changes. They had tests of those going on for the Post Office Department.

I did mechanical miscellaneous type testing one year. The next year I worked with people in the wind tunnels. My boss was a fellow named Hugh Dryden, who just recently died. They had some of the most advanced equipment there for aerodynamics testing. They had a 3-foot wind tunnel, I think, that would get up to perhaps l00 mph. There was one of them they could run up to l80 mph, something of that sort. They had a little jet some place which worked from a compressed air tank, where just for a few seconds, you could momentarily get very small jets of air which were practically the speed of sound. I never used that, but all of these wind tunnel things were very interesting.

The work we were doing mainly was with, what was called a hot wire anamometer, which attempted to measure the rapid fluctuations in air velocity close to the surface, boundary layer effect, and you get periodic oscillations in this.

We measured these with a hot wire which would be cooled more when the air velocity passing through it was high. Then you can measure the resistance of that wire which had changed with its temperature, and you would infer then what the oscillations in air velocity were near the surface.

I forget whether I spent one summer or two summers in that wind tunnel, but at any rate I was what at that time called a junior physicist or something or that sort, and I earned like $2400 a year. When I got my PhD, I went to take the Civil Service exam again, because I didn't know what the chances might be as to what jobs I'd get, and there were no offers at all for senior physicist or whatever I was called. Supposedly, you were able to earn $3200 a year or more as a senior physicist with a Ph.D.

After some years at Ursinus, with the depression only easing gradually, I finally got a query from the Civil Service roles, asking if I would be interested in applying for a job if offered, all the ifs, at Ft. Monmouth or some place like that, Signal Corps in New Jersey. It turned out that the times were such, you see, that they could get PhD applicants who were eligible as senior physicists to take jobs as junior physicists at $2800 a year or something like that. It didn't seem very profitable to change from getting $2800 a year teaching 9 months a year, to getting $2800 a year in Civil Service, where I guess you got 30 days' vacation, working ll months a year. So I turned that one down.

MERZBACH:

This was about the mid thirties?

MAUCHLY:

That was somewhere, I don't know exactly when, but it was well after the time I had gotten my degree, the time when I was beginning to think about leaving Ursinus because the money wasn't too good, you know.

Finally in l940, somewhere in that era, I began to think very seriously about the possibilities of building electronic computers. I was also thinking very seriously about how to make more money because of the question you asked a little while ago about the college supporting any of this, and the demands they were making on me. As I say, the only way they supported me was to pay me salary. I had no budget for any research, and no means for tools at all. I bought this $75 calculator out of my own pocket.

MERZBACH:

So there were two of you?

MAUCHLY:

There were two of us who were distinguished from the others by the fact that we had Ph.D.'s. I think some of the others had master's degrees in some subject, but there were two of us who were Ph.D.'s, which meant that we were further along in age and training and were accustomed to a higher starting salary maybe. In particular, I had a family. Dr. Burks, who was the other Ph.D., in mathematical logic, from the University of Michigan, was as yet unmarried, I believe, so maybe he didn't have as big a problem as I did. Wherever I would go for employment, I wanted to be sure I was making enough to take care of the family. I couldn't take any job just for the love of it. Obviously, it would be a little bit more difficult to take something in another city.

So I was very pleased to find that Dr. Chambers selected two Ph.D.'s to invite to be members of the Moore School staff, for teaching purposes primarily, on a temporary basis, that is, for the duration of the War, if nothing else, because at that time they were just then losing, I guess, two members of the staff to war efforts. One of the persons who either had left or was leaving, was Knox McIlwayne, who left to take charge of some manufacturing for the Napleton [?] Company in war work.

The other one, to my consternation, was Dr. Travis, who was that member of the faculty who was supposed to conduct the course in the design of computing devices which was offered in the catalogue. I had already learned, of course, that even though courses are in the catalogue, it was not necessarily given, and I guess I had tried to enter that and found out it wasn't offered at one time. At any rate it turned out that Dr. Travis was leaving for active duty with the Navy, and so the Moore School was temporarily short of teaching staff, and they looked to the two Ph.D.'s here as suitable candidates for replacement during the war period.

Both Burks and I accepted the appointments as instructors on the Moore School staff, with the assurance by Dr. Chambers, that even though the pay was not great, it was always possible in an engineering school to supplement this by varous other contracts on jobs which needed to be done. This indeed turned out to be the case, especially with the war developing. They began getting some contracts from the military agencies, such as the Signal Corps, and secondary contracts from the Radiation Laboratory at MIT, which was working on war contracts. It looked like there would be a build up in their contract work in association with Ballistics Research Laboratory in Aberdeen. So all in all, there would be opportunities to collect a small salary for the staff teaching work, but also some additional salary for working on contracts at the same time. As I say, that turned out to be the case. Burks and I got employed on some contracts thereafter, so the salary that started out looking no better than what I was getting at Ursinus, turned out to be considerably better.

I don't think I delayed very long before giving my answer to Dr. Chambers. If they wanted me on the staff, why I was willing, and I sent my resignation off to Ursinus

College with regrets, but said I thought I knew a replacement which they could get, who would be quite satisfactory, which also turned out to be the case. So they hired a man from the teaching staff of the University of Pennsylvania's Physics Department to go out to be head Professor of Physics at Ursinus in my place. We swapped, you might say.

During that first year, l94l-42, of the work at the Moore School, we got pretty well swamped, you might say, just with teaching. I did, for that matter. Somewhere in there, I don't remember the exact period that this built up, in addition to the regular students who were not in service as yet, they had also taken contracts for training certain Army students in technical subjects and certain Navy students in technical subjects. So, we had a student training course there, in which we also gave typical engineering courses. We had that additional work on the teaching end.

Then, at some point, I don't remember the exact time, I began to get involved in a project which had been obtained from the Signal Corps in New Jersey. They wanted calculations made on a theoretical basis as to what radiation patterns you would expect in various kinds of antennas. These were antennas that would be used in radar work where you have a small antenna somewhere near the focus of the large reflecting dish, as they called it, and so there were actually two teams working on correlated projects there. One set of people tried to make experimental measurements of the apparatus that I was trying to get the calculations made on. These were essentially parabolic reflectors, but with different portions of the parabolic reflector removed, which would affect half the distribution of radiation away from the target, and might conceivably make a little more energy available at the target, but certain, although the pattern of how, what they called the side lobes of the antenna pattern, behaved.

In doing that, they came up with some pretty serious implementation problems. Nobody seemed to know just how to arrange the calculations actually to do this. I was given, by the project administrator, Dr. Brainerd, a book from MIT, Stratton's "Electromagnetism", or whatever it was, in which there was a nice integral and some vector functions and said that this is the way you do it. I was also given an assistant, I can't remember his name now, who was an emeritus professor of physics and, I guess, he had for many years probably taught the first year physics course and been retired. So far as I can remember, I spent considerable time in trying to tell him what we were trying to do, and finally had to report to Dr. Brainerd that I was wasting more time trying to educate him than he was doing helping me. I didn't want to have that drain on the project any more.

But that was not the only problem. The problem was also that at the Moore School, what calculating instruments existed besides the differential analyzer? Practically none. Every engineer, of course, had a slide rule, and every engineer was expected to know how to use a slide rule. Slide rules were good enough for his homework, daily class work, laboratory work, etc. But there was to my knowledge, only one digital desk computer, an old Friden, which was available around the Moore School to do multiplication with, and of course, it could do division if you used it right. They had an adding machine in the main office for the secretary to add up the bills or something. But there was nothing available to me but one desk computing machine, which was there for a very singular purpose; namely, to perform test calculations, as I understood it, to see whether the differential analyzer was correctly set up. If you had the wrong gear ratios into that thing it might produce results which would go wild and not be what you wanted, so you would precalculate on the desk calculator what you ought to get as the first few steps on the first part of the run. If you didn't get that, then you'd know there was something wrong, and look further.

I didn't need that for that purpose all the time, so presumably we could, most of the time, use this desk calculator. There was one desk calculator for what we wanted to do. There were literally hundreds of thousands of calculations to be done for each pattern that we were going to develop and they wanted a whole experimental set. So the question was how to get more calculating force to bear on this.

The people who were going to do the calculation under my direction were, in the main, students, although we got a few other people. We got a part-time housewife and later a girl that had been trained to teach high school history, but it turned out she could very meticulously punch the keys on a calculating device and read out the numbers and write them down correctly and hardly ever make a mistake. She was capable of doing this in a very accurate fashion. We just laid out a form and said to take this number and multiply this number and write it down here. She would do that all day long and hardly ever make a mistake.

So we had a small corps of maybe a half dozen people who were part-time workers from outside, and then we augmented this at times with students who had some extra time to spare, especially during vacation. At least during vacations, we were able to borrow the complete amount of desk computing equipment which the Whartman School had for their business students in what they called their statistical laboratory. I guess we must have kept those on loan as much as we could, and only given them back a few or something, because as I recall, we had at least half a dozen desk calculators working most of the time, and sometimes more. After months and months of this, we came up with some nice antenna patterns.

MERZBACH:

This is still l941-42?

MAUCHLY:

This is, I believe, in l941-42. During that time of course, I was painfully aware that if you already had an electronic automatic calculator of some kind, presumably, you could short circuit this. You could get the results that we were laboring months over, in maybe half a day or less. The problem was then, of course, that everybody believed the War was going to be of short duration, but you better get something done right fast, in order to implement what they needed for the military. There was no thought of spending a year or two developing electronic computing devices when the contract with the Signal Corps said this had to be done in six months, say. All you had to do was just stoop down and put your brawn in trying to get it done in six months.

As I remember, we were always late in the performance, but this is a somewhat chronic thing which develops whenever you have the deadline set by somebody who doesn't understand all the work that's involved to do it. So when you're out after a contract with the Signal Corps and they say: "We want this in six months", you say: "Aye, aye, sir", and take the contract. You worry about it later, whether you can get it in six months. So that was exactly So that's the main organizational mechanism by which we got things started. As we went along, then we had to instruct what you call production. First of all it was the problem of design.

The office which I occupied during that time was actually Dr. Travis' office, and had drapes in it all over, for acoustic treatment and a thing called a sound prism. A big chasis, a mechanical oscilloscope was part of it, which some student had built primarily to analyze violin tones, for instance, to find out what the spectrum of various violin notes were, and see what you could do with that. That was one of the earlier sound prisms, of which there have been many since. You can buy it commercially from General Radio now, and other places. All of these things that had been done in the way of special calculating devices by graduate students never became useful. So the only useful large calculating machine we had around was the differential analyzer, which, up to the time of the serious back load of work for the Ballistic Research Laboratory, was primarily used for engineers from the General Electric Company. Those people were using this for the design of large 60-cycle equipment in the GE systems. I guess this was a source of revenue to the school. They got paid for whatever use was made of that analyzer by GE.

Of course, there were some research problems, I guess, which Wagy [?] and others had on their own, which also involved the use of the analyzer. As soon as the work from the Ballistic Research Laboratory started building up, the analyzer room was used usually at least l6 hours a day in 2 shifts by Aberdeen, to compute trajectories for the various guns which Army Ordinance had ordered, and sometimes, already had many useful elements of, but didn't yet have firing tables for. So the story was given to me, at any rate, as the War progressed, they even delivered some of these guns to France, that had no firing table with which to aim them.

That situation becoming known to me, as well as to others around there, made me think all the more that maybe the source of funds for getting the computer built was the Ballistics Research Laboratory, if we could just convince them that there was a real need for something better and enlist them in supporting this project. To that end I had an ally in the differential analyzer room, a fellow named Joe Chapline, who had been a student at Ursinus College and often came down to see what I was doing in my laboratory. He had started out, I think, as a preministerial student, and somewhere switched over to get a bachelors with business administration or economics, or something else, because he didn't think he wanted to go into the ministry.

In the talks that I had with him, I found that he had a fair amount of mechanical ingenuity and he also was split on another facet of his career, in that he was an organist, a musician. He not only knew how to play an organ, he knew how to take it apart and put it together and fix it and tune it, which exemplified his mechanical combination with the musical end.

Corny Weygandt and those people said they needed somebody down in the analyzer room to keep the thing running and make sure that the slack is all taken up and various things which get out of adjustment get readjusted and everything is properly calibrated, etc. They wanted to know where could they get somebody with enough knowledge of mathematics and enough knowledge of mechanics to do this, and I said I knew of a guy that might. Well, I think they were disappointed to find out he had never had calculus, because preministerial training didn't include it.

I claimed that he had sufficient ability to understand what was going on there and he'd pick it up quickly, which he did. I hired him, and he was working down there. Like a lot of us, he was around and on call more than accumulator is this, and what you've got to do in a multiplier is this." Of course, there were special things like the multiplier and the divider and the function table, which didn't occur anywhere else, and we gave attention to helping the engineers on those, to try and get their design so it would be compatible with respect to reliability, etc. We tried to regulate the number of voltages that had to come from a power supply to make everything work..

In a simple radio you may essentially have just a ground and some voltage, and all the tubes work from that. But in this kind of a logical circuitry, it is sometimes necessary to cascade things, you might say, so this tube will operate another one which had to be a different voltage, and that one operated another one which had to be a still higher or lower voltage. This meant that pretty soon you'd find that everybody said they wanted 67 volts for this circuit and somebody else would say they wanted 72 for this, and somebody else wanted 59 for this. So again, we dictated that no new voltages could be called upon by any design engineer to be available to him unless we had sort of a little round table conference on this and said, "Well couldn't you do this? No, why can't you do that? Well, Okay". The decision would be, "Yes we'll have that one more voltage because you need it badly enough, or no, you don't get that voltage. You can do it this way."

Even with that, I guess, the total number of voltages available from the main power supply ran into 30 or 40 anyway, all the way from some very negative ones to some very positive ones with respect to ground. In technical language, some of it is necessary because some of these circuits had to be direct coupled, and they couldn't all just pass the information along through insulating devices, like condensers. Third place, had to advance some place. It might have gone backwards still, but there were other things in the circuit, of course, which made it predominantly consecutive, so that the 2nd tube flip-flop turned on the 3rd, the 3rd turned on the 4th, etc., and in so doing, of course, each one extinguished itself.

This became a very reliable counting circuit. It was one which we adopted then after testing several others which turned out to be nowhere near as reliable, and you couldn't figure out how to redesign them to make them so.

With that counting circuit design, you may say we solved a great many of the problems which occur all over the logic of the machine because counters were used not just for the data but also for the controls. So then, with the problems of switches or gates, electronic switches, if two things are on, that one puts out a signal. If either one is off, you get no signal out. Those things were subject to the same kind of analysis: how can you do it so the results are reliable, even if the two characteristics and the other components in the circuit change rather drastically with a factor of two or more, during the lifetime of the apparatus?

Well, all these design considerations then, of course, were communicated to the engineers who had to work in separate parts and just like this fundamental flip-flop, they were told that when they needed a counter, this is the counter they should use, and they shouldn't redesign one. If they needed a gate then, there were maybe 2 or 3 ways in which they may have some flexibility in this, but, way number one, you design the gate this way; way number two, you design it this way, etc. So you have a whole bunch of building blocks essentially, to give to people, and then the rest of it came down to the block diagrams phase of saying, "Well, what you've got to do in an linear, but rather when you put a negative voltage on the grid, supposedly to cut it off, how much current still flows? How good is the cut-off? At the other end, if you put the voltage up on the grid in a positive direction, how good was the saturation for the full currents that were flowing then going to be adequate? If they were more than adequate, you needn't worry. Really, all you had to do was to work on thresholds. When a tube was supposed to be cut off, the circuit must not respond to a little current that flows, although it might be microamperes or something of that sort, and when the tube circuit is supposed to be on, why you didn't care if the current was l0 times what you designed it for, as long as it came across the threshold and said that was enough.

With that kind of a design concept, it was perfectly possible then to take lots of tubes which were highly variable, both in their original characteristics and how they aged, and make them all work the same circuit.

I say it was easy, but there were a few other little things which helped to do that. We found a way of introducing what is known as feedback in the ring circuit, so that the normal way of one of these ring counters counting, is that you have ten flip-flops and one flipflop is in set condition or on condition, and the other nine are off. So we arranged the circuits here so that the on condition fed through a circuit which would only be properly balanced if there was only one on, and the off tubes, the circuit that characterized that, was fed through something which would be properly balanced if there were nine off. It was practically impossible for us to get into any situation which was ambiguous. This helped in turn, of course, to pass on its indication. A single pulse which was supposed to advance it from the second to the together, or too far apart or something else. If you got a pulse, why, the next one wouldn't be along until a little later, and you knew how much later. So you could design these things then, so that it would produce a respectable pulse with certain properties every time that the pulse came, and you knew they weren't going to come head on heels and trip over each other.

There were still some other problems, though, because the vacuum tube was a very sometime thing, as some people, say women, are, you know. This vacuum tube business can have tubes that vary by factors of 2 or 3 in their characteristics, and yet are put out by the manufacturer as being equivalent. You could pretest them and reject all those that didn't come up to a certain thing, but that wasn't the answer either, because they change with age. The real design problem here was to try to make the circuits, including these ring counters, respond by some kind of logic in the same way, regardless of whether the tubes had aged or not, whether they gave out a big healthy signal or they gave out a much weaker one.

All of this, of course, was actually possible because of the fact that we were operating these things, we would now say, as switches. We weren't requiring that the tubes be faithfully responsible to every little gradation in input signal, and therefore give an output signal proportional to it, which is what practically all the tube design had been about, and circuit design. The large electronic manufacturers, etc., were always after fidelity and linearity, and we wanted exactly the opposite. What we really wanted was a tube that would either be on or off. Nobody cares what happened in between as long as you could distinguish these two states.

So our criteria for the variable of the tube really was not whether it was but they had no preconceived ideas about how to build electronic computers as far as we know. We had to lay before them what we thought the thing was going to be like, how it would be organized, and say, "Now you do this, you do that, etc." In order to expedite this and keep from doing the same thing a dozen times, we told them that there are certain elements of these machines that are going to be in any part of it, particularly flip-flop, bistable circuit here, which is essential to all kinds of parts to this and there's no use redesigning it every time we come to it. We have already arrived at the design which we think is adequate here, a reliable flipflop to work at something better than the l00,000 pulses per second we planned to use. So every engineer who has a need for a flip-flop uses this. This tube and these voltage levels and voltage differences, etc., these resistors, a predesigned component you might say... So it was with a few other things like that. We did that to make sure that much of the basic element of this thing was going to be a ring counter which would essentially make the machine perform in decimal arithmetic. So how can you build a reliable ring counter?

Now the people in the cosmic ray work had been satisfied with a standard of reliability which was far below ours. They might have one ring counter, two ring counters,...

A ring counter was not a new concept. There were published examples of this sort of thing. Some people in cosmic ray work and other kinds of possible applications had built ring counters with more than two elements, which is the binary unit, the flip-flop type. In a scale of 5, for instance, with 2 x 5 you could accommodate a decimal system.

So there were 3, or 4 or 5 different models, you might say, which you could pick up in the literature, which were sufficiently distinct to consider them know we went back by train, because we had missed lunch while the other people had gone out to the officers' club or something for lunch. Here we were hungry as bears, and all everybody else was thinking about was catching the train. We asked if we could stop at a little lunch room. We just managed to get something to eat before the train came, and went on back to Philadelphia. Somewhere along the line, I know that even though there seemed to be a lot of sentiment from the ballistics research side, particularly Goldstine, it was highly likely that this contract would come through. Dr. Brainerd gave me some paternalistic advice I guess you might say, "Don't get your hopes up, boy. After all, Lieutenant Goldstine is a fairly young man, and he may not know all the ropes or something, and maybe he thinks it's easier than it really is. Don't be surprised if it flops, that nothing happens." It appeared he was actually not too happy about the possibility himself, from various things that we didn't know at the moment. It seems that he needed assurance, he got one way or another, the way the contract was written, that the Moore School would not be held to absolute performance on this. In other words he had to be assured that all the contract called for was that we would try, not that we succeed. I can't blame him. It was a developmental project, and you just wouldn't want to be bound, you might say, absolutely with a fixed amount of money to produce something which had never been done before. I wouldn't either. But on the other hand, what they were saying was, "This was a research contract, we can't expect you to guarantee that. All we're asking, all the contract requires, is that you try your best and whatever you come up with will become the property of the government whether it works or not. It becomes the property, but there is nothing in there that says if you can't make it work the way you expect to that you are going to be black listed or tossed in Hades or something."

At any rate, I'm not sure just exactly when the contract became official by being signed by General Barnes, who was then Chief of Ordnance at the Pentagon. There was some assurance beforehand that things would go, and as fairly customary, I guess, in one of the things, , you proceed on a letter of intent or some agreement as to intent before the official document comes back, and you've already started to assemble your staff and started to work, which we did. In May, we were already working on some of this, but I think it wasn't until June that the contract became official. We just had a little head start.

Then, the problem was, how do you get enough people to do this? Where are there enough? I'd say that a lot of the assembly of the staff was really due to the fact that Eckert, both as a student of the Moore School and a Philadelphian, knew some people with engineering competence that he could get to come in. The staff of about l2 engineers we wound up with, were mainly people who already were known to him or somebody else in the Moore School, and possibly interested and available, and they got them to come. Some of them brought helpers with them.

It seems to me that Bob Shaw was one of those who came in that way and had somewhere or other got himself another companion named Gayle. I said that I wanted Gayle to come with me, so we got an assistant engineer along with the engineer.

Then there was the job of laying out the plans, telling them what it is they are to do, and how we're going to get it done. That was an interesting procedure really, because these people were, as I say, competent engineering people, but they had no preconceived ideas about how to build electronic computers as far as we know. We had to lay before them what we thought the thing was going to be like, how it would be organized, and say, "Now you do this, you do that, etc." In order to expedite this and keep from doing the same thing a dozen times, we told them that there are certain elements of these machines that are going to be in any part of it, particularly flip-flop, bistable circuit here, which is essential to all kinds of parts to this and there's no use redesigning it every time we come to it. We have already arrived at the design which we think is adequate here, a reliable flipflop to work at something better than the l00,000 pulses per second we planned to use. So every engineer who has a need for a flip-flop uses this. This tube and these voltage levels and voltage differences, etc., these resistors, a pre-designed component you might say... So it was with a few other things like that. We did that to make sure that much of the basic element of this thing was going to be a ring counter which would essentially make the machine perform in decimal arithmetic. So how can you build a reliable ring counter?

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So there were 3, or 4 or 5 different models, you might say, which you could pick up in the literature, which were sufficiently distinct to consider them as different models. The question was which one is sufficiently reliable. Some of them could be disposed of quickly just by analysis of the circuits. You could say that this isn't going to be reliable because. Others, we felt, would be hard to tell unless we actually made them and tested them.

So, one of the first things we did was to build models of these various possible ring counters, and test them and see how they behaved, how they would respond at higher frequencies, etc., and how they would respond if the pulses weren't up to what they should be, you know. One of the first conclusions that Eckert and people working with us came to, was the first necessary thing to make the ring counter reliable, was to be sure that the pulse which was supposed to operate the ring counter was a good standard pulse and not just any old thing that comes in. Many of the cosmic ray circuits, etc. didn't need absolute accuracy. If you miss a count here and there, it's all right. If two counts came close together, you counted them as one. But here was the necessity for absolute accuracy so far as you could get it.

The first requirement really, was to not let the counter be subject to all kinds of riff raff pulses, you might say, but be able to design it so that it would respond to a particular kind of pulse which might vary in amplitude or breadth a little bit or something, but which would at least not be confused with a double hop or a spread out or a blurred pulse. This meant that we incorporated into the design of the ring counter what we call a pulse shaper. Of course, one of the things that we knew we were going to do with respect to this computing machine, was to have it work on an organized set of timing pulses. So, from that point of view, there was no question that you would have random pulses in a constant grade unit, two pulses getting too close together, or too far apart or something else. If you got a pulse, why, the next one wouldn't be along until a little later, and you knew how much later. So you could design these things then, so that it would produce a respectable pulse with certain properties every time that the pulse came, and you knew they weren't going to come head on heels and trip over each other.

There were still some other problems, though, because the vacuum tube was a very sometime thing, as some people, say women, are, you know. This vacuum tube business can have tubes that vary by factors of 2 or 3 in their characteristics, and yet are put out by the manufacturer as being equivalent. You could pretest them and reject all those that didn't come up to a certain thing, but that wasn't the answer either, because they change with age. The real design problem here was to try to make the circuits, including these ring counters, respond by some kind of logic in the same way, regardless of whether the tubes had aged or not, whether they gave out a big healthy signal or they gave out a much weaker one.

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In a simple radio you may essentially have just a ground and some voltage, and all the tubes work from that. But in this kind of a logical circuitry, it is sometimes necessary to cascade things, you might say, so this tube will operate another one which had to be a different voltage, and that one operated another one which had to be a still higher or lower voltage. This meant that pretty soon you'd find that everybody said they wanted 67 volts for this circuit and somebody else would say they wanted 72 for this, and somebody else wanted 59 for this. So again, we dictated that no new voltages could be called upon by any design engineer to be available to him unless we had sort of a little round table conference on this and said, "Well couldn't you do this? No, why can't you do that? Well, Okay". The decision would be, "Yes we'll have that one more voltage because you need it badly enough, or no, you don't get that voltage. You can do it this way."

Even with that, I guess, the total number of voltages available from the main power supply ran into 30 or 40 anyway, all the way from some very negative ones to some very positive ones with respect to ground. In technical language, some of it is necessary because some of these circuits had to be direct coupled, and they couldn't all just pass the information along through insulating devices, like condensers.

So that's the main organizational mechanism by which we got things started. As we went along, then we had to instruct what you call production. First of all it was the problem of design.

MERZBACH:

Before leaving that, what could you describe as the stages of the general design developments? For example, you mentioned the modified . Where along the line did you open up the function tables?

MAUCHLY:

That's a good point to get at there. Perhaps we have to go back first to the proposal made in April. At least one of the appendices was concerned with how you could get a set of counters and logic circuits and things. We were proposing to actually carry out a simple calculation for a firing table, with a lot of time spent learning what the equations were that the people on desk computers were solving and how it might be adapted to the method of successive steps from a stepwise difference calculation. I finally achieved a layout which might not have made a very fancy firing table, but it was logically capable of doing something like a firing table. That had in it certain devices which we call accumulators, which had the capacity for storing a number and for adding another number to it. So this thing we called an accumulator, was both a storage and an adder.

I could count up for that simple job how many accumulators we needed. We also postulated that since there was going to be one device which would do multiplication, you could take the numbers from two accumulators, put them together, and the product of them properly cut down to size, could be put back into another accumulator of the same type. It was obvious, of course, that if you multiply ten digits by ten digits, you're going to get something like l9 or 20. So if you kept on multiplying without cutting numbers off at the right hand end, why you soon had nothing you could manage. You had lots and lots of actually useless digits.

So part of the art of the computing, of course, is to know how to truncate these things, where you think it should be rounded off, etc. If you assume that that were known, then the product could be put back in the same types of accumulator that the factors came from, and likewise with division. You can divide out ad infinitum quite often and keep on getting new digits, but the significant ones will be confined to about the same number as those in the dividend, and the divisor's significant digits will limit the number of significant digits you can expect in the quotient.

In general, you plan this computation so that the numbers are all properly placed back into accumulators the same size. I don't think in that report ... we didn't make any final design decisions at that point, how big the computer was going to be, but if you made them a reasonable size, 8, l0, l2 digits, something of that sort, decimal digits, you achieve those calculations, because that is about how big the digit size was on a desk calculator, for that matter. Some of them had 8 digits in a bank and some of them had l0, but they couldn't conveniently enter anything over about an 8 digit number on most of those machines. So we had this kind of layout in the appendix to this proposal which showed in a primitive way, you might say, how a calculation of interest to the Ballistics Research Laboratory, namely a firing table, could be done.

One of the elements of the whole proposal, and one which was interesting to them, was, of course, this achievement of general purpose. The firing tables weren't all it could do. If they hadn't spent their money on this thing, after the War was over, there were a lot of other things that could be done. Even during the War, if somebody from the wind tunnels which were studying the aerodynamic characteristics of say, a particular kind of bombshell, wanted to do a calculation which didn't at all resemble the trajectory calculations, it could be done.

They had problems of internal ballistics, also. How fast does a powder burn, and how does the pressure build up inside the barrel of the gun? The equations, etc., which you have to work out for this interior ballistics problem about burning of powder with different shape pellets and different size holes, and different chemical, different pressures and temperatures, etc., is an entirely different problem, of course, from the mathematical point of view, than the external ballistics problem. So what they wanted and what we wanted to make was a general purpose computer, not something specific to a particular equation or calculation.

Part of it, after we made this proposal, showed it could be reasonably set up to do an exterior ballistics trajectory. Part of the concern which we had and they had was how much do you want in this thing to suit your purposes, from the point of view of general purpose computer? So after the contract was actually signed, we still had conferences with Aberdeen, and particularly a couple of astronomers, who were very familiar with rather precise calculations for their field. In particular, somebody like Leland Cunningham, who was an authority on the computation of orbits for comets, would come and discuss with us what we ought to have, and in particular, how big should the accumulators be and how many of them should we have and how many devices which we call function tables should be available so that they could put arbitrarily specified functions in, as for instance, the drag on a shell.

So the original prospects we had were something which was a little smaller than they finally decided they'd like. We finally upped it to 28 accumulators, although we started with a lower figure, I believe. I think we originally proposed two function tables and we finally came out with three. This, of course, added to the cost of the thing, but the main purpose of this thing was to make for them something which would be of general utility in any kind of calculations that you may incur, and yet be within a reasonable budget and not go wild.

The actual number of accumulators, the actual number of function tables, may not have been tied down in the original proposal in April l943, but there were amendments. The first contract only went for, say, six months for a year, and then there was an amendment which carried it further, and during this course of amending the contract, you amend the specifications, etc. So that's what took care of that part.

Now you might say the concept of what the modular elements were to be was really in the original proposal. In other words, we defined something called an accumulator, we defined something called a multiplier, we defined something called a divider, we defined something called a function table and maybe we didn't define it exclusively, but we described the necessary connections so you could have an input and output, which we felt would be card punch and card reader, because we didn't want to be saddled with having to develop all new equipment for that purpose.

After all, Aberdeen's main computational work was done on punch cards. They had already implemented this with some special purpose multipliers and things which IBM had built for them, so they had a fairly top notch stable flow, at least punch card machines, and they wanted to be able to take punch cards and feed them to our computer, and wanted to be able to take cards from our computer and list them in their listing machines, and punch cards were the obvious answer. Although strangely enough, in the proposal, and even through the patent specifications, I guess the output was quite often referred to as a printer. So they keep talking about the printer, when it's really a card punch. I answered part of your question. Did I answer all of it?

MERZBACH:

I think that's the general...

MAUCHLY:

I started to say we had the design problems first, of course, testing things to see whether we had to design any more, and later, of course, there were production problems. These overlapped, because as soon as we had gotten what we considered to be a reasonably reliable ring counter, which we were going to use for storing the data and other control purposes, we actually designed them, physical layout, etc., of two kinds of plug-in chassis, one of which was the data storage counter and the other of which was a program ring counter. We ultimately had 2 or 3 other variants on counters. We had some 6-stage counters in what you call the master program control panel, in which counters essentially acted as 6-way switches in which you could initiate any one of six different subsequent programs depending on the outcome of the results of the calculation up to this date.

We thought that six was a more livable realistic number than ten. You could always get 36 by concatenating through these, of course, not two of them, but one of them feeding six others. Once we had said that the data counter, which also had to receive pulses and transmit data to other places, etc., was going to be exactly like this, we could physically lay it out and have people start building it.

Now, if later we had to make a design change in this, due to some kind of phenomenon that turned up that we couldn't have foreseen, then all you had to do was to make a small design change, and actually that's what happened.

We produced over 200 of these data counters, because there were 20 accumulators each with l0 of these in it, so you had to have at least 200, and you had to have some spares, because part of the business of keeping this operating presumably, is that when something goes wrong, you don't fix it in place, you take the plug-in unit out, put another one in to replace it, and go back and fix that somewhere else and test it out. So we had to have over 200 of these decade counters for data, and it did in fact turn out later that there was a modification, at least inside of this pulse shaping circuit for the counter. We found out we could get a more reliable operation by changing the pulse shaper, so you pulled out 200 inductances, and took 200 other inductances each one in its place, etc.

At least in the meantime, you had all this built, and it was a small matter to change it, compared to the main labor of building it. So we could set a lot of girls who were again, I guess, part-time housewives and things in a production laboratory in another room there, and start putting together these electronic plug-in units. Then, when we assembled the main frame, in other words, about an 8 foot high structure that went around the room in which we put panels, and then in the panels, we plugged in these units. We were ready to start some of the interconnection wiring.

We had, fortunately, from our point of view, telephone men who were very expert at cable wiring to do all the cabled wiring around the room on these things. The telephone company was like most industries, occupied very much with supplying defense items which were being produced in plants, which would normally produce telephone equipment. They were applying their facilities to producing something different. It might have been a Signal Corps telephone or something else, but it wasn't standard telephone equipment. As a result, there was a sort of freeze on installations of telephones during the War. They had crews of people in every city who normally were doing new telephone installations and installing new exchanges, automatic exchanges in the cities, etc., and these people had practically nothing to do. The telephone company had them on stand-by, and they'd use them part of the time for repair work and service work, but they had no new equipment to install. So it turned out we made a nice arrangement there, where we got a sufficient number of these men who were well equipped to do expert artistic workmanship, you might say, on this wiring, and they knew how to make good solder joints, etc. They came in and did a lot for us. It was an excellent professional job.

Then, to go between the vacuum tube circuits and the punch card reader, and between the vacuum circuits and punch card punch, we had what we called a buffer storage, an intermediate stopping place for the data, to store it, to accommodate the difference in slow speed mechanical stuff and high speed electronic. That storage was telephone company relays. We found the telephone company, because of their needs and reliable communications, exchanges, etc., was actually the best suppliers of highly reliable relays, so we ordered the relays we needed from the telephone company, Western Electric factory. Again, the telephone men were best to install these things and see that they worked properly. It worked out very well that way.

A lot of these things were going on in parallel then. Add to this, the girls who had done all these plug-in units for the decade storage, why then there was a whole slew of plug-in units of smaller type, which went into the control circuits.

Then, there was a certain number of plug-in units of ring counters, for instance, for this master programmer. Then there were other kinds of plug-in units which were a certain coincidence or gain circuits and things which had nothing to do with counters, but they had to be there to fulfill the function of being able to select one piece of data instead of another. When they were supposed to be getting data, you'd have to have essentially a little switching circuit, which was a vacuum tube, to tell it to now respond to what's coming into this line and don't listen to the others.

One after another of these units got designed and this set of girls were wiring up these plug-in units. In the meantime, telephone men were in there wiring up the cables from rack to rack, and wiring up the relays, and at the same time, there were other engineers still working on the design of the multiplier circuits and testing them out and getting them in there and doing whatever they could delegate to somebody. "Here's a wiring diagram, here's a chassis; we need l0 of these."

We had an insurance man from the Philadelphia area who was also a ham radio man or something. At any rate, somebody knew him and brought him in, and he said he'd like to do some of this. He was good with a soldering iron, and he knew how to make the little right angle turns to make everything look pretty, etc., and do a good job. He, for instance, might do the first of a set of plug-in units. He'd say, "Here is the model", and then he'd give it to the girls and say, "You make this like that." He'd produce the first model, the ideal wiring one.

Of course, with all that, you eventually found there were mistakes. The mistakes the girls made in wiring for instance, maybe just a wire that was supposed to be soldered to this socket of the tube got soldered to the very next pin or something. Most of those were caught either by individual inspection, or because we had a test rack for that particular unit, the decade counters particularly, and you could test it out and see if it did what it was supposed to do on that test rack. So that eliminated a lot of the incidental errors that you otherwise get. But there were some errors that remained until l946 or maybe l947. They were just one of a kind someplace. So a lot of testing by just attempting to use everything, was the only way that you could smoke some of those out.

So this is why even though in one sense the machine looked like it was done long before the Army arranged the demonstration in February l946, in reality, there were still errors being found in the months after that demonstration. It's just like the problem of debugging programs, getting the errors out of programs nowadays. IBM or some other big company will release a program after they have tested it maybe for 4-5 months, and they have had a slew of errors in the meantime. After they released it, there are still lots. Here we are using a computer which we've used for 3 years, it's been on the market for six years, and we are still getting changes in their operating systems, as they call it, to correct errors which they've just found.

Another interesting example of that comes in the year of the Univac, which is beyond what we've been talking about. In l95l we delivered the Univac to the Census Bureau, and then we delivered further Univacs in subsequent times. One of the things we thought very necessary to do was have a program which would sort data into the sequence like alphabetizing a phone book for instance. This was a problem that was certainly present in the Census Bureau, but it's present in every business application, and we seized on this one as a prime one that we had to do something about. So at a time when we were working with the Census Bureau, and the IBM Company was telling its customers that, haw haw, it's absolutely ridiculous for the tape machines they're talking about down there, because you know you can't sort data on tape; you can only sort it with cards because you move the cards around. Who's ging to split up the pieces of tape and put them on different bins for you? We said, "You don't sort the tape, you sort the data, and you've got to move the data around, not the tape.

Well, so what we had as a major project, was mainly carried out by one of the girls who had been a programmer on the ENIAC. Her name was Betty Snyder, who married Mr. Holberton and became Betty Holberton. She worked for us for about two years in designing what we called a sort generator. Now the generator is so called, because this is a program which generated another program, but its function was to accept as input the characteristics of the data that you wanted to sort, how many items, say, how long the items were, and which part of the item you want to use as your sorting key, etc. After it had received that data, then it would produce the program or modify a skeleton program in head start we will say. It would produce a program, which then was adapted, doing expressly that thing to the data for which you specified it.

Well, that thing was intended to, and did, sort reel after reel of data. Univac might have l0 different tape drives on it with each one a reel of tape, and you'd be taking data off several tapes, and feeding it back in a sequence on another set of tapes, and when those tapes were full, then you'd start feeding on another set of tapes. You'd remove the first set, which were now full, and put some empty reels back on there, and when the other set that you'd used in the second place were full, then you'd go back to these and get a third batch, etc. We had to do the same thing with input. You had to keep switching from one to another.

This would enable, for instance, a large life insurance company whose files occupied 500 tapes to sort everything on the 500 tapes. For the program to be able to generate that kind of thing, which would handle tape after tape after tape, for as much data as you wanted to put through, you'd have to have a lot of complicated things. You'd have to prepare the way to do all this in advance.

This program was used for 2 to 3 years after we debugged it, and I don't think there were any complaints. The whole thing worked perfectly fine for anybody. Several years after it was in commercial use and there were dozens of Univacs around, one day it failed. It turned out that the reason it failed was that as it was dividing up data among tapes, it came to a case where there was only one piece of data left to put on something, and it put one piece of data on that tape and that's all. Somehow the program had always assumed there was going to be more than one piece of data on a tape. Just in the way of writing it, you just think of a long string of data, you know, so the thing was testing for what you call the sentinel, the end of data. It likewise was putting these sentinels on when the data was ended. I don't know where the failure was, but Betty Holberton would know. Somehow, in this particular instance, either it didn't put the right number of sentinels on or when it went to read the thing back, it didn't understand what was happening, because there was only one piece of data on that tape instead of the hundreds you'd normally expect. Just by chance, that particular situation had never occurred in its use in the previous three years and nobody had thought to test it.

So there is no such thing as an absolutely guaranteed perfect program practically--well, a small one, you know, if you just want to, say, generate the successive integers. Why, if it works well for the first l0 integers, it's bound to do the work right from then on. But that even may have some limits, if you get up to ll-digit integers and you only provide for l0. You somewhere some across a road block. So what next?

MERZBACH:

I don't know whether you might want to cut here. This seems to be a natural point to start up again.

MAUCHLY:

Order some electronic equipment which was useful for teaching students in physics something about electronics. At the time I came there, there had been no electronic equipment whatsoever, but that's similar to the fact there were no courses in electronics at Hopkins when I was there, except the one that was given by a graduate student. We felt that electronics was here to stay and we ought to say something about it.

Well, the teaching load built up. It built up because I made it build up. All that was required of me when I came there was to teach a standard course in first year physics, because that was required of premedical students. The Biology Department and others made a feature of this in preparing the premedical students. The only other output they had was for high school teachers who came to college from that part of Pennsylvania that Ursinus college served then. Why, about the only two professions that anybody was directed toward were either medicine or high school teaching. Nobody knew that there was any other world but those two. If you wanted to be white collar man instead of a blue collar man, that's what you had to do.

I used to spend a considerable amount of time trying to show some of these students that the world was a little bigger than that; there were other things they could do. I thought that some more advanced physics courses might be helpful to some of them, and so I proposed some, and as far as the college was concerned, why sure, put them into the roster of courses and write them up in the book if it's available.

So the advanced physics courses would have, say, 6 or 8 people in them, and in some places, they won't give a course for 6 or 8 people at the undergraduate level. This depends on how dedicated the professor is, I think. I certainly couldn't make the enrollment in those courses a justification for the College spending money for a second member of the department. So the only assistants I had were what they called student assistants, to help in the laboratories in the afternoon. They would allow some students to earn part of their tuition by helping in the laboratory, and you take your better senior students to run the first year laboratory.

From what was, I suppose, no more than an 8-hour a week schedule to start with, I built it up to where I was having to spend maybe l2 or l5 hours lecturing and 3 to 4 afternoons a week in the laboratory. It got to the point where it was practically a 40-hour week you were on the job. This is apart, of course, from correcting papers, making up examinations and all the other things. So whatever work I did on these other projects was done mostly in the evenings. I had a few students that would come around in evenings and see what I was doing and talk to me, etc., and some of them weren't even physics majors. An example being a preministerial student, who later had an important part in the ENIAC project. He didn't take physics. He first thought he was destined for the ministry, and he thought maybe history or business education or something else would be more like it. He would come around and talk to me at night when I was working in the laboratory on the circuits, testing out what might be done with electronics. I found out that he was not only an accomplished organ player, but he liked to tune organs and fix organs. He also liked to take clocks apart and put them together and fix them, etc. He had a mechanical bent.

Later, when I finished this defense training course in electronics in, I think, l94l, and I was hired on as a member of the teaching staff at the Moore School. I found out they needed someone to care for their big analog computer, called the differential analyzer, a mechanical thing with gears and slip band mechanical amplifiers, and things of that sort that always had to be adjusted and tuned up and tinkered with. I said that I knew somebody that might be very interested in doing this. I didn't think he'd had any calculus, and didn't really know what a differential equation was, but I thought he'd learn and he did like to fix things.

So we got Joe Chapline, the man I'm talking about, to come down there and care for the analyzer and keep it running right. He was my underground man at the Moore School. The differential analyzer was in the basement. While he was working down there underground, he would try to pass the word to anybody that came up from Aberdeen that said, "Yes, this thing is not turning out the results as fast as you would like them, but there is a man upstairs, who says if you did it with electronics, why you could go so fast you'd be up-to-date whenever you wanted". Occasionally they'd say, "Who is this? I want to talk to him". So that's how it came about that in l942, somebody came up and talked to me and told me, "Well, it sounds very interesting, but I don't think it's very practical, because it would take a year to build it, and the War will be over in a year". Then in l943, another man came up, Captain Goldstine, or he was Lieutenant Goldstine then, and he had a different story. He said, "Well, I don't care whether it will work or not. This is a chance worth taking. We spend millions of dollars on developing new tanks for Army Ordnance, why can't we spend a million dollars on something like this. If it comes through, fine; if it doesn't, well that's just one more thing that didn't work". So he was the one then that said, "Let's get a proposal in fast and I think I can sell it", and he did.

MERZBACH:

Let's go back a little bit. Earlier, you said you made the remark that you started applying vacuum tubes at Ursinus and tinkering. Approximately when was this, and what kinds of things?

MAUCHLY:

The interest I had in trying to develop electronic courses for the students was pretty early, and I probably made some purchases of electronic gear for that purpose, maybe in l934 or l935. Some of these ideas are generated by reading the *Review of Scientific Instruments*.

Cosmic ray counters, etc., got me started on the electronic computer ideas, I suppose, a little later, l935-36. Then, of course, by the summer of l939, I had built this analog computer for numeric analysis, but I think it was before that that I built another machine which is a sort of digital machine. It was a cryptographic device, in which you'd essentially scramble triples of letters, and you'd enter with three letters in a sequence, say ABC, and come out with SBQ or something. If you put in CBA, why you didn't just reverse the triple, you'd make some different combination.

MERZBACH:

How did you happen to do that?

MAUCHLY:

It was a sort of interesting by-product, you might say. Again, it was because I was interested in computers and I was trying to see what could be done with a low budget. Whereas vacuum tubes and big key boards and printing mechanisms, etc., were beyond my reach economically, just like an IBM machine would be out of my reach. I thought what could I get that's cheaper, which might serve the purpose? So one of the things that I got interested in was little gas diodes and neon tubes. These things are used all over nowadays as indicator lamps. Quite often some kind of an electrical appliance will have a red glow so you know it's working. This is a little neon globe that takes only a few cents' worth of electricity per year to run.

I got interested in these things, especially when I found they were being used in what are called indicator fuses, sold at the local hardware store. If a fuse burns out, the light would glow, and you know it's that fuse that's burned out. So I went down to the hardware store and said, "I'd like to buy some of those, how many could I get"? Well, it turned out, they came 25 in a package, and they cost 25¢ apiece or something, or 30¢ maybe. So I bought 25 of them. I proceeded to take them apart and throw the fuses away and just keep the lamp. Then I investigated and found out you could buy the lamps without the fuses. If you bought l00 from GE, why you wouldn't pay much more than I paid for 25 fuses. Now that I had l00 of these lamps, what could I do with them?

Well, one of the things that occurred to me, not only due to the properties of these gas tubes, was that you could arrange a network of wires, so that by applying a voltage to two wires in a matrix formation or column wire, you might say, one single light would glow. In fact, I tried to make this cryptographic device work by using incandescent lamps, and found that everything was just a dull glow, and these things had the opposite properties they should have for this purpose. The neon lamp, once it conducts, its effective resistance is low. What happens with an incandescent lamp is that the more current you run through it, the higher the resistance gets as it heats up. So this tended to even out so everything would glow. But with the neon bulbs, it was just the one you wanted.

I guess this also, in part, was occasioned by what was available in the surplus stores. There were lots of multiple decked wafer switches with which I thought I ought to be able to do something, so I bought a couple of dollars worth of these. Then I made some special tools so that I could take the contacts out one place in the wafer, and put them somewhere else, and make the wafer switch in any combination of contacts I wanted, instead of the ones used as actual wave band switches for these multiband radio receivers, I think. With all that cheap gear available for switching, plus relatively cheap lamps, which were both able to serve as an indicator, and also have a nonlinear function to perform in the circuit, I got started on all kinds of ways of using neon lamps.

In fact, one of the things that I developed was essentially a binary flip-flop neon lamp. I built the thing in the form of a railroad crossing signal. A round disc at the bottom, an old cardboard cap from a pint ice cream container, you know, then a glass rod, and the wires went up through it, and at the top was a cross member, and there were two neon bulbs up there. I put a thing across them like a railroad crossing flasher signal. If you put the right voltages on it, why it would stick on one side, and then if you just gave it a little electrical twitch, pulsed it, it would transfer to the other side. If you put the voltages considerably higher, then it would oscillate, flash back and forth just like the flicker lights at the railroad crossings do. I thought it was a nice job. I could just make these up and sell them to model railroad people, but I never did it. Instead, it's become sort of an historic piece, I guess. It appeared in the [?] movie that they made on the l5th anniversary. All during the time that I was on the staff at the Moore School at the University, I kept this thing blinking on a table in my office. People would come in and say, "What is that"? Well, this conversation piece was to open up the idea that you can make electronic computers this way, you see. But no electronic computer really was ever made that way, because although they are cheap, they're slow. So, the various gas tubes I explored, were based on, you might say, low budget economics, where the budget says what you can pay for the gadget, not how fast it will work after you get it done. Yet the economics of the modern computer is the other way around. You can afford to pay a lot for the gadget, providing it will do so much that the cost per operation is low. My preoccupation with these neon tubes was to see whether I couldn't do something quick and cheap, and I tried a lot of things that way.

MERZBACH:

Did anyone become aware of your gadget?

MAUCHLY:

Yes. I tried to sell it, you might say. I tried to interest the government in it. To backtrack a little bit, I mentioned going up to see Hotelling about some statistics, and he told me that if I was really interested in statistics and what was being done today, why the best thing I could do was to join the Institute for Mathematical Statistics. Before that, I didn't know it existed. I remember hearing about the Numerical Statistical Association, which in those days, was publishing papers, which were mainly reports on government economics or how the steel industry is doing, etc., and more or less factual reports of data. here was nothing in there about mathematical statistics.

He clued me in to the fact that there was this other organization, which would get you into the forefront of what is going on. When I joined that, I met a lot of people who were congenial. One of them was a professor down here at George Washington, Solomon Kullback. It turned out if you called Dr. Kullback's office at George Washington, he wasn't there. He worked for the National Security Agency or the Army branch of it, whatever it was called then, Army Security Agency, I guess, over in Arlington, Virginia at that time. He taught courses at George Washington to have a visible reason for living. When I got this cryptographic device put together and working, I arranged with him to come down from Arlington and exhibit it. Their top man then, I guess he's retired now, was Friedman.

MERZBACH:

Yes. He died last year.

MAUCHLY:

Well, Friedman and Kullback and 2 or 3 others assembled to look at this thing, and there was nothing concealed about it, you might say. It was all made on a masonite panel, which had all the lights sticking out this way, and all the wiring below, and the masonite panel on a hinge so you just flipped it up that way. Everything in the box was open and visible, and there was nothing there but wafer switches and wires. There were no clanking gears, nor anything else.

They looked at it and judged that with all those wires and nothing else, it couldn't be very complicated, and so I was glad to see they were surprised when I said, "Well, now you can put the ABC in and you've PQV or something, but put CBA in there." By golly, they expected to see these three letters just turn around, and they didn't. Three entirely different ones came out.

I learned something that day. They gave me a mimeographed piece of paper with what their requirements were, and gently told me that this didn't satisfy their requirements. It wasn't that it could be built small or cheap, but as far as they were concerned, they were interested in methods of secrets in the field, and something which crypted three characters into three characters was very vulnerable to garbled messages. They said, "We expect to lose l0% of our message in transmission from here. With this thing, we'd lose 30%. Sometimes it's pretty hard to figure out what the message is after you've lost 30% of it. So you've got to have a letter for letter equivalent, not a two or three". So that sort of fell between, because it was not simple enough for them; it didn't have the letter for letter equivalents. Presumably, I never tried, and it was probably too simple for the State Department. The essence of secrecy there is that the code not be broken for l0 to 20 years. With something of this sort, especially with computers available, the thing isn't nearly complicated enough. There is no code that can't be broken. It's just a question of how long. So all you can hope to do is maintain secrecy for a period that is long enough so that you don't need it so much any more. In the field, you don't need the secrecy very long, because the action has taken place within 24 hours. In diplomatic circles, it remains much longer.

Well, they allowed it was a very interesting device, but not for them. They gave me reasons which were pretty convincing as to why they wouldn't want it. But it was as far as I know, the first example of which gas diodes had been used in a sort of function table matrix form to actually make use of properties to do something else than just produce light.

MERZBACH:

How did you become involved in that course at the Moore School?

MAUCHLY:

Well, I don't know now how the announcement reached me. It could have been just as a direct mail piece from them, because I had enrolled in an evening graduate course in electronics in l936, I think, and been down there 2 hours, one evening a week, for one semester, in which I learned something about how to use Taylor series to figure out something about the space charges inside of a vacuum tube. It wasn't a very practical course from the circuit design point, at all. The textbook we used was one written by one of their professors, Max McIlwane, and perhaps Brainerd helped write it, I don't know. McIlwane gave the course. As I say, it was a course in how to expand Taylor series.

At any rate, as a result of being registered that way once, they continued to send me announcements every year of what they had to offer in the evening graduate school. Of course, it could be they used the same mailing list to mail out this thing about the availability of an advanced training course. It could have been just an announcement in the newspaper. Whatever it was, when I heard about it, I responded and applied. The idea was to take people in somewhat related technical disciplines, mathematics, physics, chemistry, and try to train them to be electronics engineers. I guess you could say the effort was successful, if you don't measure it just by the percentage of people who persisted, but by what was accomplished by the people who did. I don't think there were more than 5 or 6 people out of 60 or so registered in that course, that stayed in electronics thereafter. Some of them merely burned out their laboratory instruments, and demonstrated they just were no good for this kind or work, and became successful mathematical philosophers and logicians and other things. Others, as far as we know, may not have been successful at anything.

Some of them went on, and one of them worked on the Whirlwind computer later. Dr. Burks, who had come from Michigan, a PhD in mathematical logic, and myself, both had the right union card, the PhD, to be invited to stay on the staff. So we not only stayed on the staff, but Burks worked on the ENIAC, too. There were a few others. Herb Column in the Physics Department, who was interested really in thermodynamics, and later became professor of Physics, but he also was a consultant to Rand in computers for awhile. I'm not sure, but I think there were several others. What remained in and around computers thereafter.

MERZBACH:

Who was it that was ?

MAUCHLY:

Lou Wilson. Those people made contributions which have never been made, just like myself, and may not have made a contribution in electronic computers if I hadn't managed to switch to a place where you could get funding for them, you know.

MERZBACH:

As far as that course itself was concerned, that was sponsored, but did you pay tuition or was that sponsored?

MAUCHLY:

The government paid tuition. We had to supply our living, of course, food and lodging, because they didn't do anything for that. They gave us a course with free tuition.

MERZBACH:

And who taught the course?

MAUCHLY:

Well, the lectures were given by the various members of the faculty, Dr. Chambers, Dr. Weygandt, Dr. Warren, Brainerd, and maybe Pender came in and gave a few, and maybe even Dr. Focett, who was an expert. This was in the field of industrial lighting, and he designed all the electrical supplies for the Pentagon. That was his big job at the time, doing Pentagon lighting. He specified all the electrical supplies and lighting.

In the laboratory, we had a few of the fellows who had just been getting their master's degrees there, and that's where Eckert came in. Once I was on the premises taking this course, it turned out that the one person who was willing and agreeable to talk about the possibility of electronic computers was Eckert. Nobody else really wanted to give it a second thought. Some of the faculty members, when I talked to them, said, "Well, somebody here had already proved that that didn't work". It turned out actually, I think, they were referring to some kind of a report which Dr. Travis had written for the General

Electric Company. General Electric Company was actively using the differential analyzer for design of heavy electrical equipment before the Ordnance people entirely preempted the use of the machine when the War came on heavy.

…most about computing machines at the Moore School and if it was possible to construct an integration device to work on digital principles. I never saw this report, but I heard about it. Apparently, the gist of it was that if you took a lot of mechanical desk calculators with their register capacity, etc., and connected them mechanically and tried to solve a differential equation step by step, which is what they wanted to do, for instance, in this electrical machine design that General Electric was interested in, that you'd have to take so many small steps that the thing would probably wear out before you'd solved one problem. Well, electrons don't wear out. Not the same way anyway. The vacuum tubes have their faults, but the electrons don't seem to wear out much.

So it's not known to me at any rate, whether Travis considered electronic methods at all. It just seemed like the other people were jumping at the conclusion that what was proved on a mechanical machine therefore, applied to an electronic one. Of course, Travis wasn't there to consult. That was one of the things that I considered unfortunate about it all. I'd gotten the equipment manuals of what courses were being offered at the Moore School for several years, and the attractive thing about the Moore School was that they offered courses in the design of computers. So when I got down there and asked about these courses, they said that they weren't always given. They were given if enough students appeared interested. They were always given by Dr. Travis, who had designed this differential analyzer downstairs. Unfortunately, the day I appeared was the day he left. He was called to active duty in the Navy. He had been a reserve officer. So actually, when I was put on the staff, I occupied the same office that he used to have, but that didn't give me all his experience.

MERZBACH:

Had you paid any attention to the digital computation outside of the punch cards? In other words, the type of work you and Stibitz had started, Aiken, and ...

MAUCHLY:

Yes. That was an interesting experience. I knew nothing about the possibility of relay computers until that year, which I think must have been l940, when I went up to New England to work with Clayton. While I was up in that area, there was a summer mathematics meeting. Was it at the University of New Hampshire?

MERZBACH:

Was it Dartmouth?

MAUCHLY:

Darmouth, yes. So, I got in my old car and drove up there, but I didn't really time it right, you know. I didn't have a particular reason, and I didn't have a program, but I just knew there was a meeting. I arrived just at the time when a mathematics colloquium lecture was going on, and I've forgotten who it was, but somebody was in there giving a colloquium lecture. Outside in the hallway, directly outside this lecture room, was this teletype machine with some simple directions as to how you use it. This was the Stibitz complex calculator that was being worked remotely from the Bell Labs at West Hampton, was it, and so I tried a few problems on it. By golly, it worked. I didn't then and there verify the accuracy of the calculations, but the fact was that it came back with reasonable answers, so you could multiple and divide complex numbers, and add and subtract, of course.

While I was fooling with that, some short little fellow with a plump contour came up and started talking to me about " Well, that was interesting, but don't you think the possibilities of computers really lie in the direction of cathode ray tubes?" Well, he started talking away at this and asking me every so often if I didn't think he was right. I thought he was right. I subsequently learned, of course, that this was Norbert Wiener. So far as I could tell, every time I'd seen him since, it seemed to be one of his characteristics to sort of ask you to agree with him. I couldn't really figure out what he was talking about except that he had some vision that, someday, there were going to be computers with cathode ray tubes. Whether he thought of them as storage devices or what, I have no idea. Of course, he was a good friend of von Neumann, and it turned out that von Neumann's machine had cathode ray storage devices. But then, von Neumann had already had visits to the Moore School where we discussed cathode ray tubes as storage devices in a very concrete and specific way, whereas, as I say, I got nothing out of what Wiener said.

MERZBACH:

Did you look into the work that was being done on relay machines at all, subsequent to that demonstration?

MAUCHLY:

No. Relays were a pretty poor substitute for the speed of vacuum tubes, and the relays have one thing, that ought to give them a leg up, and that is multiple contacts on one relay. Whereas, in the vacuum tube, the best you can do is put 2 or 3 elements in one envelope so that what looks like one bottle, is really functioning as 2 or 3 vacuum tubes, usually 2. On a relay, even though you have multiple contacts, you pay for them in more than one way. The more contacts you put on, the slower the relay is, the more power you have to put into it to actuate it, etc. So, you better keep the relays pretty simple if you want to make them compute fast. The speed of Stibitz's calculator was, of course, a lot greater than trying to do the same thing with ordinary desk calculators, because it handled both elements of a complex number, and especially complex number division is quite a few operations when you try to do it on a monadic computer, you might say. The next computer that we learned about essentially, after we built the ENIAC, was the Mark I at Harvard.

MERZBACH:

Getting back to the early Moore School stage, you mentioned that you found Eckert seemed to be the only person there who also had some interest in electronic computers.

MAUCHLY:

He was one who took a positive aggressive point of view toward this, rather than a negative wet blanket sort of thing. You always have to design your circuits to work under the worst conditions that can prevail. It's actually easier to do with vacuum tubes and switching elements. Then, you have to depend on any of the characteristics, except for cut-off and saturation point. He didn't care how linear or nonlinear they are in between. Of course, most of the people using tubes are trying to make high fidelity amplifiers out of them, which is exactly the opposite extreme of what we were talking about. We wanted them to be as non linear as possible.

He saw no difficulty in trying to design something which would be reliable as a switch, even though vacuum tubes varied in their characteristics. A cut-off is a cut-off, you know, and that's great. No tube cuts off perfectly. You, let's say, allow point thousandth of what you call its normal current that may be passed even when it's cut off. That's the world to play with, you might say, when you can take a signal that's big and cut it down to one thousandth of what it was, and there is no more trouble in discriminating that against a full signal. If the full signal varies by a factor of two or three, in other words, a tube that's saturated carries sometimes a current of say three milliamperes, and another time a current of only one milliampere, or different tubes vary this much. Again, that's no bother if you are really trying to see whether half a milliampere is flowing or not. Anything greater than that is considered on, let's say. When you go down to one thousandth of this, you certainly can tell you're off.

So he looked at it in a very positive way and there wasn't any difficulty that we could dream up which there wasn't an answer to. Those conversations and things in l94l gave some confidence and assurance that I wasn't just on a wild goose chase. As I say, in l942, there were still people saying you couldn't build this before the War would be over. Until l943, nobody took this seriously.

MERZBACH:

Did you at that point, when you discussed it or tried to get other people interested in it, have any kind of a mock-up or did you ever simply describe your conception of this?

MAUCHLY:

You might say Eckert convinced me that the models and things I had been making with neon bulbs, etc., were not very practical, because they were too slow. If you are going to do this thing, do it right, you see. It's vacuum tubes that will respond fast. In fact, the greatest problem with the gas tubes is not starting the flow of current, but stopping it. In other words, it takes time for the ionization to die out, etc. Getting better than a thousand times per second operation out of them is virtually impossible. Whereas vacuum tubes could go a thousand times faster and so a gas tube wouldn't be much better than a relay, but your vacuum tubes would.

MERZBACH:

Before we get into details, do you want to make some more comments about work of other people who were doing some kind of experimentation?

MAUCHLY:

Yes. I mentioned Stibitz, and I mentioned visiting the Worlds Fair in l939, for instance, and looking at the mechanical whizzes, which didn't whiz too fast. Back, I guess, in December l940, when I was giving the paper at the American Physical Society, it was really one of these Christmas meetings of the AAAS, and all its section were meeting. So when I was through giving that paper, somebody came up to me out of the audience, maybe I was the last paper on the session. Anyway, at the end of the session, this man came up and introduced himself, and he was named Atanasoff, and was at the State College at Ames Iowa State College. He said he had been working on computing machines, and I had described a little about my harmonic analyzer when I was giving the paper, how it made it possible in getting some of these results done, but I thought that faster computers were possible and I hoped someday to get them working on the weather.

When I got into conversation with him, it turned out he really didn't want to say much about his computer out in Iowa, but if I was interested I could come out and see it. I said that was great, and couldn't he tell me more. Well, about all he would tell me was that he was doing an awful lot of good things with an awful small amount of equipment, apparently. This piqued my curiosity greatly, of course, and so I tried to make plans to get out to see him on Easter vacation.

It turned out that the only way I had, with my economics and college professor's pay, to get out there, was to get some other people to go along with me who paid for the gas and oil. Some neighbors living out in Collegeville couldn't go at that time and they said maybe they could go later. At the close of the school year we managed to arrange this. Mr. Schratfler and, I think, someone else rode with me and my son, who was very young then, [?] Mauchly, and he must have been about 6 years old, he was born in l935, got in the old Plymouth and started driving out there. We left them off someplace in Ohio, where their destination was, and continued on to Iowa. We stayed a little while with Dr. Atanasoff and his family at his house.

He did, indeed, show me the computer he was building, and explained somewhat more about how it worked and what it did. It wasn't clear to me from what I saw then, that it was near enough to completion to say it was going to be a reliable computer or that it would work well. What was clear, was that it was a very ingenious thing he was trying to do, but it was a specialized computer. It was intended to solve a very common problem: to solve a system of simultaneous linear equations. When you can do that, there are lots of things you can do with it. So many problems that if you can just solve the linear form, then you're happy. On the other hand, it wasn't what I had been dreaming about in the way of general purpose computer.

It seemed that the hang-up of giving out information about this came about because he and the University were hoping to get some patent rights on it, and they were very carefully trying to see that the knowledge of this machine did not fall in the hands, say, of IBM. So I came away from there with a feeling "here is a guy with good ideas, he's got something going there, and I hope he finishes it, but it still isn't what I want".

It was while I was visiting out there, incidentally, that I got the message, I guess relayed by my wife, that the letter had come saying I was accepted for the Moore school advanced training course. So I might have stayed longer, but we got into the car and hopped back as soon as we could.

MERZBACH:

Did you meet any of the people who were working with him at the time?

MAUCHLY:

Well, there weren't very many people working with him on this. I don't have a distinct clear remembrance of the main person whom I met, but the fellow who was working with him was named Berry, who I think has subsequently died. A fellow named Clifford Berry, I think it was, who, I think, at that point was a graduate student there, and was doing a lot of the work and trying to get the machine completed. Oh, there were lots of people, I guess, that I shook hands with, but I didn't remember their names.

One of the things I had promised to do, committed myself to, when I made the arrangements with him, was that I give a seminar talk out there primarily on the harmonic analyzer that I had built. I don't think I brought the analyzer out there, but I gave a fairly detailed description of what it was and how it worked and what the design principles were. Certain quantities had to be big with respect to others, etc. So from my point of view, I wasn't trying to conceal anything. I didn't regard the harmonic analyzer as a patentable device or anything, and I wasn't worrying about patents. It was pretty clear that all the information wasn't coming the other way necessarily.

MERZBACH:

If I understand you, you didn't really care for the computer in terms of application.

MAUCHLY:

No. I'd say that in a sense, it was something of a let down. In the clues he had given me in December about how much he was doing with little equipment, I got my imagination going, and I thought that I would sure like to know what that's all about.

I guess the biggest let down when I got there, really, was to look at what he was using for his input-output. It was something I think I had thought of, but discarded. But at any rate, whether I had or not, the idea was that he was going to have a spark puncture an IBM card with a high voltage spark, and then if you wanted to feed the same information back in again, a spark of somewhat lower voltage would fail to puncture a good solid paper card. But if the hole had already been made by the spark puncturing it before, why then the spark would be able to read the holes passing there, so to speak. It just didn't seem as if this was going to work out as a reliable input-output device, and I don't know of any indications then or later that it would be a reliable inputoutput device. Nobody has used it as far as I know. So at any rate, once I was there and saw what it really was, it wasn't as great as I had imagined it.

MERZBACH:

Was it sufficiently complete so you could put on a problem?

MAUCHLY:

No. If you could do that, you might say you could work it prior to then and test its reliability. I think he had some push buttons and you could demonstrate what it could do in binary addition. Logically, there wasn't any reason why it wouldn't work. It's a question of what it does and how well it does it.

MERZBACH:

That reminds me of another whole other category of things that were discussed to a greater or lesser extent at the time. Had you spent much time thinking about problems of numbers in strands, and questions that subsequently became more interesting: binary versus decimal machines, this kind of thing.

MAUCHLY:

Well, I don't know. I didn't attach any particular importance, you might say, to number systems. If you have a binary device, in other words something with two stable states, you better use it that way, and don't expect it to work decimally in itself. But out of such high stable devices, you can make any number system you want. It was always my preference to have the machine behave as if it were a decimal machine. The ENIAC was that way. I always felt the computers were there to serve the humans rather than the other way around, so anything you could do to make it easier for the human, good. You lose a little efficiency by not using the machine binary all the way, let's say, but it doesn't take much to make it behave decimally. We made the UNIVAC behave decimally, you know, with a binary code of decimal digits. I always thought of these number systems as a means to an end, with nothing very esoteric about them in themselves.

MERZBACH:

I don't think we've talked about mathematic logic. Were you at any time particularly occupied with that whole area?

MAUCHLY:

No, not particularly. I got interested quite a lot at one time in operational calculus but not in mathematical logic. I guess I was particularly entranced by a book, maybe you know it, Steppins. He's an actuary in Sweden or someplace and wrote the book in English, which is fortunate because I can't read Swedish. He developed a whole theory of interpolation in which every step as it goes, develops the calculus of the operator, etc. That sort of fascinated me. In fact, I tried to teach some of that to some of my better physics students at times, and I'd say, "Hey, look what you can do here. If you think the binominal theorem just works with numbers, well it works with operators, too".

MERZBACH:

In that more general [?] it intrigued you.

MAUCHLY:

But it was merely a tool so far as I was concerned, for instance, with respect to developing methods for integrating differential equations. I wanted to try to get something simple enough that the ENIAC could handle and yet know something about what it was going to produce. I used some of these operational methods to develop things.

[END OF INTERVIEW]