Computer Oral History Collection, 1969-1973, 1977

Interviewee: B. Holbrook
Interviewer: Uta C. Merzbach
Date: May 10, 1969
Repository: Archives Center, National Museum of American History

MERZBACH:

Do you mind starting out by giving a little basic background as to your early interest, training, and schooling, how you came to go to Bell Labs.

HOLBROOK:

I was educated to be an X-ray physicist, and I came to Bell Laboratories in 1930 expecting to work in some branch of physics, and I have worked in almost everything except physics ever since. I was in transmission research for a good many years. I worked on voice-operated devices for transatlantic radio and things like this for awhile. Most of the War I spent in a group that was developing electrical analog anti aircraft fire control equipment for the Navy. And then I worked for several years in the Switching Research Department, and in 1957 became head of what was ultimately called the computing systems research department, where I was for the remaining eleven years at Bell Laboratories. I never had anything officially to do with computers until 1957. I was interested in what was going on and thereby acquired some knowledge of it.

MERZBACH:

Could you tell about the early activities which I guess were taking place just about the time that you came to the Labs?

HOLBROOK:

Well, of course, from the standpoint of the history of computers of the major advances was made in 1927 or 1938 with Harold Black's invention of the feedback amplifier which changed an amplifier from something which had gain into a precision measuring instrument which was the essential part of building an electric analog computer and was extremely convenient in handling signals through electronic digital computers. And it was several years after that that people really found out how to use this. It was not until H.W. Boda published his classical paper that we knew not only how to build but how to design to specification feedback amplifiers. This work of course was all intimately tied up with the early development of multi channel telephone systems. Apparently prior to 1930 some people at AT&T invented the fundamental circuit of the electronic analog computer although they didn't realize that they had a computer at all. It was part of an automatic terminal for transatlantic radios, and it happened that they had to have an approximate difference between two signals in order to control the switching of the voice. So this had nothing to do with subsequent developments, but it does indicate that people were working in these lines.

Now from about 1933 one two or three groups at the Laboratories rank into computing problems that had to be solved essentially by computing means, and developed what you have to call do-it-yourself methods of doing them involving various kinds of apparatus not very well chosen for the purpose in mind. But at least it got the problem solved. In the meantime, our transmission development people, the network development people, had long experience in calculations involving complex numbers, and they knew how to do them, but doing them with desk calculators is at the very best a laborious and tedious method of getting the answer. And there were a few other groups who had similar types of calculations, and when our mathematical research people began to consider seriously means of improving their calculating abilities the network people were also interested in anything that could be done. And it was in this frame that Stibitz first proposed the binary relay calculator. His first proposal, as a matter of fact, was for a binary machine. Before anything was built he invented the binary decimal notation, I think for the first time. And when it was decided to build this machine it was developed mainly by Sam Williams, who was a man of very long experience in the development of telephone switching systems. And he was the chief engineer, in effect, of the first two or three at least of the Bell Laboratories' relay computers. As a matter of fact, he stole a very large part of the circuitry out of standard telephone central offices. If you had to register a number of let us say 8 decimal digits, you can go and steal the circuitry out of a panel central office because for years we had been having to register seven digit numbers as dialed by the subscriber and the circuitry simply was taken over from one to the other.

Now this machine was completed and placed in service in January of 1940 and it was as you know a machine which was accessible only by remote terminals. If you went to the machine itself you couldn't get a problem in or out even. You had to go to one of the remote terminals to know what was going on. Now we are getting back to this finally. In the summer of 1940 it was decided to demonstrate this to the mathematical society at their Hanover meeting, and it was first proposed the output of this machine...the input of it was on a keyboard that was especially built, and it had as I recall maybe 16 or 18 keys, the digits, and enough control keys to make it operate. The output of it was on a standard teletype and in nominal use in the Laboratories someone who wanted its facilities went to one of the stations which were distributed around the place--I think there were three of them--if the red light was on she waited until the red light turned off and the green light came on and then she had the machine. She keyed in the problem, and this was usually a complex multiplication or division...not always. As she keyed it in, the input was printed automatically on the teletypewriter. She then pushed the equals button, which was essentially to execute and waited until the machine printed out the answers.

Now the first proposal for the Hanover demonstration was that we should move one of the typewriters over a standard teletype circuit to Hanover and have an open telephone to a girl glued to one of the terminals in New York. And then somebody said well why don't we just move the whole terminal and use teletype in both directions. And this presented a development with very little time to solve it. And I remember that Stibitz and Williams and I met in Williams' office, and we were told what we had been ordered to try to do. And Sam was an old hand at relay circuitry. I was young and not very good one. But both of us went off and sat up most of the night figuring out how we could do it. The next morning we met, and Sam and I cooked up in maybe a couple of hours a circuit that as I recall was about 75 percent Williams and 25 percent Holbrook. And then we called in all the experts. We brought in the relay requirements people, the people who could tell us that if you want the relay with the following combination of contact and it must operate in less than 15 milliseconds, but you can have up to 30 milliseconds to release it, they looked up in their card files and say try the U221. We also had the trial installation people who had complete lists of all the relays of various types that were in stock at Western Electric plants. And Sam read from the drawing we had prepared, and he would say now I want a relay and it's impedance has to be over 400 ohms, and it has to have two [?] and two transfers. And it can be slow in operating but it's also got to be slow in releasing...it's got to take at least 20 milliseconds to release. And our relay requirements man would go through his books and suggest possibilities. The trial installation man would regretfully report that none of them were in stock anywhere.

So Sam would revise the requirements for something else that would do it. And by the end of the afternoon we had ordered all of the relays for this terminal. The orders were sent via telegraph to various Western Electric plants. The relays were shipped in that night by air freight and Sam installers started to put the thing together the next day. And it was built and tested just before the meeting. The meeting went off. People at the meeting were invited to sit down and put their own problems into the machine and see the answers come up. Sam hovered over the thing all day like a nervous hen, and as soon as the meeting was over he had the installer start tearing it apart because somebody might ask him to do it again. And I don't blame him. But his, incidentally, was the first time that any kind of digital computer was operated over a remote circuit. And it was basically a standard teletype circuit but with a considerable amount of design adapted to this purpose.

MERZBACH:

Were you in New York or in [?] at the time of the meeting?

HOLBROOK:

I was in New York. I didn't even see it as a matter of fact. I heard that it went off but I didn't even see the thing. Now this machine was built essentially to see if it would be useful. Everybody knew the circuits would work, but the question was how much use could people with problems actually make of it. So it was built as an experiment, and a lot of things that would have gone into a central office were not in its designs, specifically, contact protection. And unfortunately it worked beautifully and the War came along so that we couldn't reengineer it for continuing service and with the War coming along the need for network designers multiplied so that they had to have the thing in operation to meet schedules on a lot of radar and fire control and other types of jobs. And unfortunately the only man alive who could maintain it at that point worked for me. And every time the thing had no self checking, let alone contact protection, and every time it started getting wrong answers they would call him. And I was working on a job which made essentially no use of this, but every once in a while he would disappear for a day or two to fix the complex calculator.

MERZBACH:

Who was this?

HOLBROOK:

This was Bill Maltahner, who is I think one of the few other people who remembers the Model 1 in the Laboratory. He is a Department Head in the Switching Development area at H_[?]ville how, and he has nothing to do with computers essentially since. But he was during the War the only man who could get his thing back into service. Now the need for it grew to the extent and the relays deteriorated to the extent that we tried as I recall to get new relays. We proposed to get a complete new set of relays, unhitch the old ones, put the new ones in on the same wiring and thereby resuscitate it for the rest of the War. But we couldn't get a good enough priority to do this. And fortunately at this point the Western Electric Co. in response to the needs of telephone companies who were in somewhat the same boat developed a couple of hand tools, one of which you could use to strip the precious metal contacts off the springs of standard type relays, and the other you could use to weld new contacts on to where the damaged ones had been stripped off. So we took the machine out of service for two or three days while some experts in doing this came and put new precious metal contacts on every relay in the place, and then we were enabled to survive the War and keep it in operation. And this was actually my whole experience with the machine which was that my job was suffering because Bill could fix it.

MERZBACH:

How long was it in operation?

HOLBROOK:

It was in operation from January of 1940 until I think some time in 1949, when it was replaced by the Bel Mod 6 computer.

MERZBACH:

Was it actually in use until the 6 came along?

HOLBROOK:

Yes. Yes, indeed. Oh yes. It was routinely used by anybody who could get one of the girls who regularly used it. It was in routine use by anybody who took the trouble to ask somebody how to work it for at least 9 years.

MERZBACH:

Were there any modifications?

HOLBROOK:

Merely to keep it running, so far as I know. As a matter of fact, it had been designed on such a case design that there wasn't very much you could do to modify it, I suspect. Now meanwhile, the existence of this and the fact that it worked, and some of the ideas that Stibitz had not been able to get into it, such as the use of paper tape as input and intermediate storage became known and the Mod 2, Mod 3, Mod 4, and Mod 5 machines were all built during the War for delivery to government agencies by various people. I would have to look to see who built which but Ernie Andrews built at least three of them and possibly four, I don't know. The Mod 2 was an un-enterprising machine called the relay interpolator. It was delivered somewhere, I think in North Carolina to Army Ordnance and it is possible that they subsequently might have had it. The Mod 3 and the Mod 4 were very alike. The Mod 3 was designed for testing of Army anti-aircraft gear and was delivered initially I think--I would have to look up in Ernie's article, to be sure--it was programmable within certain limits, and was extensively used. And later on the Army modified it to give more flexibility but I don't know the details of this. The Mod 4 was built essentially to furnish the Navy with the same facilities. It was--they needed more extensive facilities because the Navy gun is on the deck of a rolling ship so some of the angles can get negative, and you have to do a lot more computation to know whether it is aiming the gun at the right place. It was called by the Navy--it went to the Naval Research Laboratory, and was called the Mark 22 computer. And I believe they also modified it to increase its flexibility later. As far as I know all three of these are no longer in existence. The Mod 5, there were two of them and they were essentially duplicates, and one of them went initially to the Ordnance Proving Ground at Aberdeen, Md., and the other one to what was then NACA at Langley Field in Virginia. And I think you know the subsequent history of these as least as well as I do. Now of these machines, the Mod 1, the complex calculator, was officially not programmable at all, but in effect it had a good many built in subroutines. If you pressed, for example, the divide key, it set itself up to do without further action on your part except putting in the data the division of one complex number by another, and this in terms of its elementary operations turned out to be a sub program with about 12 successive orders. Now the Mod 5's were general purpose computers. They were very slow(?) but they operated, and they operated essentially around the clock. They and some of the other were internally self checking, so unlike the Mod 1, so that if anything went wrong with, let us say, a relay contact they would not give you the wrong answer. They would balk and tell you in some way that you'd better fix them before you go any further. This incidentally was stolen directly from telephone switching systems which mostly in large cities have operated this way since the early'30s. We just stole the circuits and put them into the computer. Now the

Mod 5 was almost completely self checking. According to Ernie Andrews the Mod 5, so far as he has been able to find out, in their many years of service only twice gave any wrong answers at all. Quite often they wouldn't give you an answer, but they would tell you why you weren't getting an answer and give you fairly good information on where to look in the relay circuitry to fix it, so that you would begin to get right answers. The Mod 6 was built for the Laboratory's own use at Murray Hill, and it was put into service, and again you'd have to check Andrews' article, I think in '49 or '50. And at that point the Mod 1 was scrapped, very unfortunately. It shouldn't have been done. If I had been looking it wouldn't have been done. The Mod 6 was a good deal smaller machine than the Mod 5's, but it was designed for maximum ease of programming in that it was possible to have a large number of subroutines in the machine at one time. Now the subroutines had to be wired into the machine, but once you had them in then they could be used in several combinations. There was a hierarchy of subroutines so that you could issue an order on the paper tape that might call for calling six or eight subroutines in succession and using some of them hundreds of times before you went to the next order on the paper tape. And this machine was sued at Murray Hill from whenever it was, 1949 or 1950, until it was replaced by an electronic computer which was some time in 1956. And it was used, I don't think every 168 hours a week, but regularly not very much below this because it again was self checking. It had a second trial feature so that if it miss checked at any one point it would back up two or three steps and try it again. And if it failed two or three times, it would then if it was attended call in the operator and give him a message as to what relay he'd better look at. If it was unattended, and it was built for unattended operation, it would abandon this problem, write a diagnostic message so that the operator knew where to look when attendance began, and go on to the next problem and hope that nothing went wrong with that. And I can remember the girls at Murray Hill every night including Friday they would load it with enough paper tapes carefully stripped(?) together to keep it busy, until in the case of Friday, Monday morning, turn out the light and go home. And the only attendance it had was the watchman was instructed to look and if certain lights were lighted on the panel he was to turn off the panel, and if certain other lights were lighted he was to call the fire department. And the girls would come in on Monday morning and most of the problems would be done. And they would have pretty good indications as what to do about the ones that were not done, that had been abandoned. And I think this is... You know the ultimate disposition of the Mod 5's. The Mod 6, as I said, went to the Brooklyn Polytechnic Institute, and from there in 1960 to the University of Behar in India, and beyond this I have no further information.

MERZBACH:

One of the things that was interesting and I wondered if you might speak a little bit about the change in attitude towards the machines at Bell Labs. Now I gather that in 1937 there was a certain amount of skepticism.

HOLBROOK:

Oh, quite a lot(?)

MERZBACH:

Would you mind elucidating on this a little?

HOLBROOK:

Well, it was the War that changed our attitude as well as the existence of these machines. The War put us into the business of building relay calculators essentially to do urgently required calculation while some other people managed to make electronic calculators actually work. And I observe that the electronic ones were a little late. The… As a matter of fact, even the Harvard Mark 1, which in its first version you cannot describe as being electronic at all really, was put in routine operation in April of 1944, and the ENIAC was put in routine operation in February of 1946. So the fact that we had done this was important. Also the fact that we had suffered enormously from having to do on desk calculators things that we could have done much more easily during very much of our extensive development work for the government during the War. For example, during the War I worked on the design of a very large electronic analog computer, or I guess I should say an electro-mechanical analog computer. And this required an enormous amount of calculation of networks and particularly the shaping of potentiometer cards so that the machine would meet its specifications. And I personally beat an electrically driven Monroe calculator to death and had to finish the job on something that they dug out of the archives which I think had been built around 1920 and it very nearly beat met to death before we got the job finished. And there were a lot of people like me who because we went into fields which we had never heard of until the War started, had a very great appreciation of the advantages of doing things by machinery that we wouldn't have had if we'd stayed in our routine businesses that we were in at the beginning of the War. Well, we spent, I don't know how much, but for the Mod 6 computer which was built for the Laboratory's own use, we must have spent oh, 20 or 30 times what we spent grudgingly for the Mod 1, and I'm sure if the people who were building it could have justified more expenditure they would have been able to get it with no difficulty.

MERZBACH:

I'd like to go back now. You just mentioned your involvement with building analog machinery during the War. Could we pick this up and perhaps go back to talk about the project that you mentioned in [?] that you mentioned in the early thirties essentially turned out to be a [?] computing device.

HOLBROOK:

We were building...we were trying to find out how to build, at that point, broad band multi channel amplifiers so that we could handle, let us say, a hundred or two hundred conversations going through all through the same amplifiers on a line by carrier telephone methods. This came about because of the invention of the coaxial cable which made it possible to do this without too much cross talk to other circuits. Now this required that...these amplifiers we knew would have to have certain inter-modulation requirements which we could handle although with a great deal of trouble by methods we knew something about. But also since they were very stiff circuits in amplifiers if the input voltage which was the composite of all the voltages from all of the channels exceeded a certain positive or negative bound from the good bias point they would bat(?). And when there was a bat, this meant a very sudden change in the change of the output voltage, the plague voltage, when there was about it would appear as a click in all or most of the channels connected to it. Now in order to handle this, we had to determine what the convolution of all of the input to the separate channels was going to be. And we tried to do this mathematically. In those days we always tried to get answers in closed form because that simplified the calculation. But the shape of the single channel voltage distribution curve was such that nobody could do it. And I asked some much better mathematicians than myself for help, and nobody could do it. And we had to have an answer. Now we knew that if we had enough channels simultaneously on the shape the multi-channel distribution would approach the normal. But we had no idea how far we had to go to realize this. Now it happened that we had around some equipment for measuring voltage distributions in things like speech which consisted essentially of apparatus which placed a very small condenser across this 600 ohm circuit, and several times a second it removed the condenser from the circuit, discharged it after suitable amplification into a set of gas tubes successively biased higher and higher, and the gas tubes operated message registers. So that by putting speech or something like speech into this circuit for a suitable time to smooth out the statistical irregularities we could get a measure of the distribution function of the voltage we had put in. And we used this by building something that years later I recognized was a special-purpose analog computer. We also had, I should say, at that time what has now become a commonplace, but at that time the Laboratories were one of the few people who could do it, we had very high quality phonograph record facilities, which we had the first good hill and dale (?) recording facilities at that point. They were very special. We sold the equipment to broadcasters for program use, and it was too expensive for anyone else at that point. So I put together four phonographs, and connected their outputs through wheat stone bridges so that I could get the sum of the voltages simultaneously measured from four phonographs. And we made the necessary and fairly obvious tests to make sure that the thing worked that way. And then what we did, we recorded speech with a lot of subjects, recording conversational speech such as you might find on a telephone circuit, through standard telephone instruments. And we then took these four at a time at random, and made four-voice records. And then we measured the distribution function of these. When we had enough four-voice records we took them four at a time and made 16 voice records. When we had 64 voice records we found that the distribution was essentially normal, and we thereby solved a very important technical problem by what I have to call purely do it yourself computing means. I can give you a reference to the article about this which doesn't mention a computer. I didn't know what I was doing at the point. And as a matter of fact this was my--I said that I once worked on transmission research at the Laboratories. This was my sole ticket of entry to fame for that, and that is that every once in a while if I tell someone, outside the Bell System usually, that my name is Holbrook he will say, did you have anything to do with Holbrook he will say, did you have anything to with Holbrook and Dixon--I will say yes, and he immediately recognizes this contribution that I made in I think 1937 and '38 on a subject that I now am completely ignorant of, except that I now know what I was doing from your point of view. Now nobody saw how to do anything more with except what I had done, you see. But when the War came along we knew, the Laboratories knew very little if anything about fire control.

There may have been a few people who knew something about it. But it was obvious that antiaircraft fire control was crucial in Europe, and sometime, and I can't give you the exact date, in '39 or '40, Parkinson and Lovell--Parkinson worked for Lovell--and the story is that Parkinson had a dream in which he saw incoming bombers all going down under very accurately controlled antiaircraft fire. And unlike most people with dreams, he did something about it. He figured out how to do it. And essentially he invented, I think, the electronic analog computer. He didn't use my stunt of using hybrid or [?] Burgess (?) circuits to isolate the input. He built feedback am pliers with an input impedance of one or a few ohms and then fed them through very high resistors to isolate one input from another. And with this he had a device that would add two or more voltages. And since he knew how to design very good amplifiers, the accuracy was a fraction of a percent. And he then, he or he and Lovell, and I'm not quite sure which, how this was done, found that they could multiply two voltages on a servo by using potentiometers essentially the slider on the potentiometer is driven by a servo motor controlled by the amplifier in such a way that what the position of the servo shaft measures the product of the input voltage across the potentiometer by the linear position of the slider. And to make this work since potentiometers do not have either zero or infinite impedance, it was necessary to shape the potentiometer. Now once you shape the potentiometer you can also multiply, for example, a quantity by the sine or cosine of an angle. Or you can get the time of flight of a shell by putting...by building the ballistic function into the potentiometer provided you can shape it. Now prior to the War, and as a matter of fact through the War so far as the Navy was concerned, this was done by very high precision mechanism, ball and disc integrators, scotch yokes, revolvers of that sort, things of this sort. And this worked all right. But the difficulty was it could be only built by a very high precision machinist. And there was going to be a terrific supply problem in getting enough of this stuff built to furnish the needs which clearly had to be met. Now in the Lovell-Parkinson sort of thing you had to have a lot of precision. You had to be able to build high precision amplifiers. [?] had showed us how to do that. You had to build high precision servos, and after all a servo is nothing but a feedback amplifier which is partly mechanical, and in this case partly electrical. And...Oh, dear, my memory for names is not very good...let me just make a reference to something...And Macall had showed us how to build high precision servos. He did it in response to the needs of the electrical gun director(?). And somebody, and it may have been Parkinson, I don't know, showed us how to build high precision potentiometers without having to have high precision mechanics. Essentially what you did was this. You wound the potentiometer on a card which was suitably shaped. And the construction of the shape was something that any mathematician could show you how to do. You wound it on a winding machine which was suitably modified so that it could wind things on cards other than rectangular ones, which is what most people used to think of as the way you built a potentiometer. Then you mounted the potentiometer on a mandrill which was slightly shorter in circumference than the potentiometer and you had marked in or after manufacture certain specific turns on the writing surface of the potentiometer where you knew exactly what the potentiometer coefficient...what fraction of the resistance between the left hand end and the right hand end is the resistance from the left hand end of this turn. And you then hired bright high school girls and gave them rings and wedges and showed them how to lock the card to the mandrill by the use of these wedges at appropriate points so that with their fairly simple instruments they had every check point at the right proportion of the circumference so that the potentiometer would check within oh, a fraction of a percent, all the way around. So this offered a possibility of getting a completely new and relatively trainable type of labor into the manufacture of these things instead of the very high precision mechanics that were necessary by using the prior method. And I don't know how many of these things we built. This turned out to be the M-9 gun director which fairly successfully defended England against the V-1, the buzz bomb. It was no use against the V-2, of course. That was too fast. You couldn't reach that with a shell, anyway. And I don't know how many of these things were built, but an enormous number of them were built for the Army antiaircraft during the War. I happened to work on one that was developed for the Navy, and the development was undertaken essentially because the Navy was also worried about the supply of manpower that could do this. But the Navy had their machines built by the Ford Instrument Co. in Long Island City, who found that they couldn't hire experienced mechanics to do this kind of work. Their usual experience was that a man would come to the employment office with his tools and they would interview him and hire him, and they would then take him out and introduce him to the foreman in the shop, and the foreman would pull out a drawing and show him what he was supposed to make with whatever it was he was an expert on, and he would come down to the lower right hand corner, and it would say Tolerance 0 - .0003. And at this point the man would put on his hat and say that's not mechanics' work, that's tool makers' work. I cant' work to those dimensions. and go out and get a job somewhere else, which he had no difficulty in doing. So the Ford Instrument Co. started hiring boys who had worked around cars and garages and people like this. They got them fairly soon out of high school, and they trained them essentially as single operation men. And they had to train them to work quite complicated machine tools. But they trained them to do essentially single operations. And their instructors were very carefully indoctrinated. For god sake, don't tell them it's difficult. And they were able to make with the...

[End of Side I]

HOLBROOK:

And they were able to make, with the assistance of many subcontractors what the Navy needed during the War. So that the machine that I worked on became late in the War a research vehicle to try new ideas out on and not a prototype of something that was to be put in mass scale production.

MERZBACH:

Now what was the relationship here between the work done at Ford and the work done at Bell?

HOLBROOK:

Essentially none. We were given a contract. As a matter of fact, the way we got the job, my boss was told to bring some of this down to Washington, and in the Bureau of Ordnance we were presented with essentially several copies of the instruction book for using the Ford antiaircraft computer and told to start studying this and build something using the Laboratories' methods that would do the same job. As a matter of fact, it had to fit on the same mounting places and use the same cables as the Ford machine. And none of us had ever heard of a fire control computer up until three days before. We didn't know a thing about it.

As a matter of fact, I can remember going down once with one of the people on the job and we were very confused about taking...how a ship moved. And at that time the only way you could get to Washington--you couldn't get a hotel room in Washington so we used to take the night train down and spend the day there and come back on the suppertime train the next day. And one morning when I got off in Washington the man with me looked as if he had had an extremely bad night, and I said what's the matter. He said, oh, I didn't sleep at all. I spent the whole night having nightmares about how to compensate for the motion of this damn train. Well, the Laboratories built several of these things. At the end of the War we had one under construction for a seacoast defense job using large guns in case the enemy brought their fleet in close enough to our shore, I guess. But this was not completed, and at the end of the War it was suggested that we had all these parts of this uncompleted computer, by just winding the cards all differently we could make a general purpose analog computer for our own use. And this was the first of the machines that we in 1960 sent to Brooklyn Polytechnic. And we put it in service possibly about 1950--I would have to look it up. And it was so successful that a few years later we had an exact copy of it made and arranged it so that we could use them either as two small computers or one large one when the problem got too large to work on directly. Now we had to have graphic recording equipment and things of this sort for it, and we developed a good deal of this. Some of it we developed during the War for use in testing some of the computers we built for the government. And at the end of the War we sold the drawings and patents to some manufacturer down on the Jersey Coast who started making plotting tables for computers and things like this. And the first one of his I ever saw I wondered how these people had ever got it out of our [?] Laboratory because it looked identically like the one which we had manufactured for our own purposes.

MERZBACH:

What was the name of the manufacturer?

HOLBROOK:

I would have to look this up for you.

MERZBACH:

Now this was when you were a student at Chicago?

HOLBROOK:

Yes. A graduate student in the late '20s. I had to do a good deal of Fourier synthesis, or Fourier analysis, I don't know how you tell the difference. And I used the Michelson-Stratton analyzer which was still there. And found out that since Michelson had built it a design flaw had become unfortunately obvious, and this was that Michelson added displacement by converting them into the strain of small springs and adding the spring forces up against a large spring and thereby added up to 60 or 80, I forget how many, displacements. And the difficulty was that when the springs got old, and by the time I got my hands on them they were like 20 years old, they had different force contents so that you got some startling results out of it. So someone put me in touch, and I can't remember the name of the place now, but it was an acoustic laboratory and a very good one in Geneva Illinois, and they had a Fourier synthesizer which was built out of things that I would now call either scotch yokes or sine and cosine revolvers and used a very fine chain to add up the displacements. And this mechanism was essentially the same as that of the isograph but arranged to in effect do this in two dimensions for a different purpose where the Fourier synthesizer did it in one dimension for its purpose.

MERZBACH:

I'm wondering [?] developed a synthesizer I think before 1920. I wonder whether this Illinois thing may have.

HOLBROOK:

No, I don't know about the Miller one. The Michelson Stratton one at Chicago was done oh, some time in the early years of this century, I don't know just when.

MERZBACH:

The original I think was written up in 1908... Gaertner and Son.

HOLBROOK:

Well, Michelson had one built at the University of Chicago, and it was called the Michelson-Stratton synthesizer or analyzer, I don't know which.

MERZBACH:

But Gaertner produced some of those.

HOLBROOK:

That I didn't know. You mean commercially.

MERZBACH:

William Gaertner, he was at Chicago and I think he himself installed it...some informal assistance to Michelson.

HOLBROOK:

Oh, that could be. That was before my time.

MERZBACH:

...custom built them. The interesting thing was they still did that about 1930, I think they changed the design from the cone to the cylinder, and they made some modifications...

HOLBROOK:

Oh. No, this machine I'm speaking of at Geneva, its base was, I guess, a steel casting roughly the size the shape of a grand piano. And it had gearing underneath it so that it drove on top of it, not eighty, but maybe twenty or thirty discs, each of which had a revolver on it. And it drove them at speeds one, two, three, and so forth, and each of them had a revolver on it so that its motion at right angles to the lead screw was the motion of the ninth component. It didn't look at all like the Michelson-Stratton.

MERZBACH:

This sounds more like the Miller which was an entirely different...

HOLBROOK:

Well, that one I never saw.

MERZBACH:

Well, if you get hold of...

HOLBROOK:

This Institute was in Geneva, Illinois, my memory is so bad that I can't tell you the name of it, but it was run by people who were early investigators of architectural acoustics. They worried about the aliveness and the frequency spectrum of rooms and things like this, and I just can't remember their name. But I remember using their machine. I went out two or three times to use it, and I'm sorry I can't remember the name... Does that fix you up?

[End of Tape]