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Interviewee: Association for Computing Machinery, General Meeting
Participants: Walter Carlson, Henry Tropp, Ed Berkeley, Harry Hazen, Dick Clippinger, Betty Holberton, John Mauchly, Ross ?, Chuan Chu, Bob Campbell, Franz Alt, Tom ?, and Grace Murray Hopper
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CARLSON:

…before the full event of the evening gets started. The first thing I would like to do, the important thing to do, is thank Honeywell for providing the equipment and staff to put this particular section on tape. We are very grateful for their participating with us in this event. The history project started in 1967 as a joint effort between AFIPS and The Smithsonian, and has been undertaken at various levels of activity since then, and is now at a vary high level of activity under Henry Tropp, who is the principal investigator at The Smithsonian, and will be sitting in this chair in my place a little later. The project at the moment is concentrating very heavily on the work that was done at the Bell Laboratories in the late '30s, work that was done at Harvard in the late '30s and early '40s, and the work that was done at NASA, at Iowa State, and at other locations.

While that intensive research into those areas of activity is underway, we have been engaged in some events, if you will, among the people who had a lot to do with computer activities, and we've had one that some people in the room I recognize participated in out in the West Coast in December. Another series has been conducted at SHARE meetings. This particular session was put together to put in front of the cameras four of the people who are being honored tomorrow night as founders of ACM, and four other people who had a great deal to do with technical activities of the timeframe in which ACM was formed and who in fact participated in various ways and various levels of intensity in ACM's various affairs.

The actual discussion tonight will center on some of the technical advances of the time frame of 25 or more years ago, and will spill over into some of the things that happened after 1947. Our purpose in holding these discussions is to identify individuals, ideas, technologies, and their flow back and forth across what has now emerged as a profession, and was interested in beginnings of that profession, beginnings of the sense of the need for communication within the people, and that serves as the basis for the project. There are some people in the audience who are actually going to be named tonight by the people up here, because they themselves were part of that same era, and I am extremely pleased to see the representation of some of the pioneers in this business that we have here tonight and are not here at the table.

But enough of the introduction. The meeting from here on is going to be in the hands of Henry Tropp, and I now turn it over to him.

TROPP:

Thank you, Walter. The ??? ??? is being held for me to occupy much of the video time, and I would like to start right off by asking each of the members of the panel that are here, starting with Ed Berkley at the far end, to identify themselves in the proper timeframe—essentially what you were doing in approximately 1947; how you got into this computer field at a time when there wasn't a computer field; and as an afterthought, what each of you are doing today. So Ed, we can begin with you.

BERKELEY:

Well, in 1947, I was in the methods division of the Prudential Insurance Company of America in their home office in Newark, New Jersey. I had returned from a period of service in the US Navy, and the most outstanding part of that period of service were the months of August 1945 to May 1946, where I had the good fortune to be stationed by the Navy at the Harvard Computation Laboratory in Cambridge under Commander or Professor Howard Aiken. The machine that he had there working that time was the Harvard IBM Automatic Sequence Controlled Calculator. This was a most impressive machine by standards of those days. It handled numbers of 23 decimal digits; it added them in three-tenths of a second; it multiplied them in a time ranging from four seconds to six seconds; and in a good Navy installation it was working seven days a week, 24 hours a day; and every now and then, when it wasn't doing rather useful work for the Army and Navy and the US, it was calculating vessel functions. Bob Campbell, I am sure, will soon be talking about vessel functions, because he was there when I arrived, and so far as I know, that was the largest and most ardent production of vessel functions that ever occurred.

Prior to that time, I had been interested in computers and in the application of symbolic logic to making machines operate. I had the good fortune in 1939 to visit Bell Telephone Laboratories, and to see in operator the Complex Computer, which had been designed by Dr. George Stibitz. I can't say one more sentence without asking Dr. George Stibitz to stand up—because he is right there—and acknowledge to him the debt that I incurred at that time from seeing that machine operate.

I went back to The Prudential and wrote a memorandum in 1939 to the people that I was reporting to at The Prudential about the importance and the significance this machine which is multiplying and dividing complex numbers of ten decimal digits.

TROPP:

Excuse me, Ed, is that ??? still in existence?

BERKELEY:

Yes.

TROPP:

It is possible to get a copy?

BERKELEY:

Yes.

TROPP:

Thank you.

BERKELEY:

Actually, we published that memoir, I believe, in one of the issues of *Computers and Automation* not so long ago. *Computers and Automation* is the magazine which I have been publishing in 1951. It is the first magazine in the computer field in point of origin, and well towards the last of the magazines in point of number of subscribers. I have been interested in computers, though I did not call them that name, I have been interested in machines that handled operations of mathematics and logic ever since I have noticed in the punch card machines being used at The Prudential in the late 1930s such things as X punches and no-X punches, which were the direct analogue of one and zero in truth values in ordinary mathematical logic. And here were the machines that incorporated the ideas, and none of the people who designed the machines or used the machines realized that what they were doing was mixing up the truth values of logic with numbers for the purpose of doing useful calculation. And this was extraordinary to me, and I drew some attention to it, and then published a chapter in a book of mine called *Giant Brains of Machines That Think*, which came out in 1949, which drew attention to the fact that here were mathematics and logic being mixed up together, being handled as part of one mathematical system inside of punch card machines.

Now ask me your second question again.

TROPP:

The second question, I think, was what you were doing in 1947 and how you got into the business. I think you really answered both of those. The last one was what you are doing today.

BERKELEY:

Well, the main things that I am doing today are publishing a magazine called *Computers and Automation*, editing and publishing a second publication which I call the *CMA Notebook on Common Sense: Elementary and Advanced*, trying to raise a degree of interest in the social responsibilities of computer people and the necessity of paying attention to the ramifications of computers, and an attempt to prevent some of the bad results that are occurring from the computers, such as the invasion of privacy and the steady deterioration of the public estimation of computers because of the bad, wrong, incorrect, annoying results that poorly programmed computer systems produce. In addition to that, I am trying to get finished my 14th and 15th and 16th books in sort of a confused way of working at them. And in addition, we have a very exciting, rapidly working computer of our own, a Digital Equipment Corporation PDP-9, in which we are investigating computer-assisted documentation of computer programming under a contract with the Office of Navy Research.

TROPP:

Thank you very much, Ed. I would like to re-state something that Walter Carlson said earlier, and during any of these short descriptions, anyone who feels like interrupting, I hope you will. Harry?

HAZEN:

[Inaudible] originally presented where I started out in the field, where I was in 1947, and what I am engaged in now. I feel like I am coming home as I answer the question how I got started in the field. It was less than a mile from here at MIT right across the river, in the Mathematics Department, which is the wing closest to us. I had as a freshman in calculus Professor Artie Douglas, who was one of the most enthusiastic professors who I ever had, an unforgettable person. His particular passion was mathematical instruments. One day he would bring a polarimeter to the class, and the other day a harmonic analyzer, another day, another kind of polarimeter. This kept going for most of the year and fascinated all of the people in the class, and I guess especially me, because I was hooked on computers from that time. And Ed was right; we didn't call them computers at that time. They had all kinds of strange names. If you looked up in the Library of Congress Catalogue under "computers" you wouldn't find anything. If you looked up under "calculators", you would find a little on ready reckoners.

TROPP:

That's right, computers were the people who did the calculations.

HAZEN:

The computers originally were the people that did the computing, that's correct. My interest continued as a sophomore, we had a computer as a preceptor—a graduate student who helped the students—and our preceptor was doing a doctor's thesis using a differential analyzer to compute cosmic ray trajectories, so I became a steady onlooker on his progress, and I became fascinated by the analyzer, which was the earliest of the big brains, giant brains. It was widely publicized and satirized by cartoonists in newspapers. ??? ??? did determine a link to the field, the computer field as a career, and I chose courses in many of the departments at the institute that contributed something to that. In 1938, I did have the good fortune to subscribe to the earliest course, as far as I know, on automatic computation, which was Samuel Caldwell course called "Mathematical Analysis by Mechanical Methods", and it was principally in a differential analyzer, but it was at that course in the Winter of 1938 that Samuel Caldwell put before the students in his course the ideas that Vannevar Bush had been pursuing for the previous years. So putting to work the electronic counters, already well know to physics, to put them to work for computing purposes. And that really fried me up again like nothing had since I saw Douglas's polarimeters. And early fall [inaudible] for a bachelor's thesis. Now, ??? ??? computing was actually done with the use of matrices as a prominent stated method to make easier the use of computers. We referred to it ??? just as a footnote.

In the Fall of 1939, I went to work in the Center of Analysis, organized at that time by Vannevar Bush, and the first university organization devoted to the development of computers. So ??? ??? been launched. In 1947, I had leave from MIT. I was on the staff at MIT through the war, but then became a member of the Office of Labor Research Special Devices Center, the head of the computer section at Sands Point, which in 1947 was the ??? out of the ??? of four major computer projects. The ???est of those was Whirlwind, which had gotten launched by degrees at MIT during the war ??? as a major analog computing effort late-1944, early 1945, but then let us switch in '46 and into '47 to a digital machine project, and as projects went that were supported by the government, it was by wide margin the largest and most controversial of those projects. Let me say that the principal controversy had to do with things that were the size of the budget of the project.

Another one was the project was the Cyclone, which was launched not at Reeves but was established in 1947 at Reeves, a major effort on ??? computation devoted primarily to simulation, primarily to guided missile simulation. It was that project that gave birth to the REAC, the first of the general purpose electronic analogue computers.

The other 1947 project was Typhoon, which was a parallel of the Cyclone project, electronic analogue computation at RCA under Art Vats, and a project pursuing the highest levels of analogue computation, and in fact, achieving extraordinarily high levels. In 1947, there was another major project being negotiated by that part of the Office of Naval Research. It was a project at Kennedy known as the Hurricane project, and during '47, our negotiations were in progress as nearly every major-- well, not every major, but every possible source of a possible organization that might be interested in undertaking the developments of what became the first real digital range instrumentation, missle range instrumentation, including digital data transmission and digital computation in early 1948. Raytheon and Bob Campbell, one of the principle involved, was the recipient of the contract for that work. So that was the '47 stint, and it was a busy time. You might say it was ??? at that time, too, and it was only [inaudible].

The present situation [inaudible] early work both at Center of Analysis and the Office of Naval Research was a concentration on the problem definition side of things. My bachelor's thesis was on the formulation of problems by matrix methods; my principal work with the Navy was the formulation of functional requirements governing these projects, definitions of what the machines involved were to accomplish for the Navy. And during that period, it seemed clear to me that it was in fact in the area of the definition of the problem that the key to the future was to be found. And since roughly the middle '50s, I have been focused very nearly directly on that problem, and it is a tough problem, as my neighbor on the right will attest. Dick Clippinger was a member of the language structure group at CODASYL, formed at the same time that the COBOL group formed, and formed for the express purpose of laying the foundation for a machine-independent problem-defining language. Then the introduction to the LSG report published in 1962 in the *ACM Communications*, Dick and his colleagues drew a well-formed picture—a similar picture, but nonetheless a very clear picture—of the difficulties with the approaches being pursued at that time ??? ??? procedural languages, and painted a good summary pictures of the steps that had to be taken. And his group undertook those steps, maybe with a limited success. In the period since that time, there have been ??? efforts, some that have been called implied programming, implicit programming, problem-statement languages, and the like, but the progress has been slow, although there is evidence now, perhaps even widespread evidence, that the range of progress is been picking up. So results or some consequence may be forthcoming in the not distant future.

But it is in that area that I have been working, and I just have a general impression concerning that whole area of problem definition, that somehow it has been growing as a technology, problem definition, at the same rate as the computer field itself. It came on the scene right at the beginning in a pretty consequential way, not only in the work that Ed referred to involving the use of symbolic logic for problem formulation, the work of Couffignal and the work of Aiken in that area. In other words, in the beginning, somehow results were never really forthcoming. The results could be put to use by the practitioner, but we may well be on the threshold of a time, of an era, when those kinds of approaches can be taken systematically, comprehensively.

TROPP:

Thank you, Harry. I guess I would like to ask one more question in terms of the background, your MIT period, and that is your master's thesis at MIT and its subject, which I think has some bearing on that initial era.

HAZEN:

That is right. The master's thesis was in The Use of Electronic Digital Computers For Automated Control. It was written in 1942, and in the introduction to that thesis, there is the result of a design study of an electronic digital computer for automatic control, which I believe stands as the first recorded design study of such a computer. The thesis itself dealt, again, with the use of that computer, that time for fire control, particularly for prediction and target positions.

TROPP:

Thank you very much. Dick?

CLIPPINGER:

Well, at 1949, I was at Aberdeen Proving Ground, working on the problem of developing an instrument to compete with a wind tunnel for the measurement of the pressure, temperature, and velocity of the air around a supersonic projectile or body or revolution of one sort or another. At that time, Bob Kent, who was the civilian director of the ballistic research laboratories, he was also director of the exterior ballistics laboratory in which I was, he called the staff together, and he asked us if any of us would go up to the University of Pennsylvania, and see the ENIAC, which was nearing a running condition, to see if it could be used for anything besides the firing tables for which it was originally conceived. So I went up to the University of Pennsylvania, and I remember Adele Goldstine showing me how the ENIAC worked, and then I started trying to see if I could formulate this same problem, that I was working on empirically with the instrument, which was eventually built and called a firing chamber, if it could be solved mathematically. Well, I succeeded in formulating it as a problem in hyperbolic partial differential equations with boundary conditions, and it developed that there was not enough equipment in the ENIAC to handle it straight in the way in which it was designed. So I found myself struggling with the constraints of not enough memory, not enough transceivers which were used to control the flow of program, not enough cables, not enough anything. And I found a way to use the function tables to store a program, and when Adele Goldstine discussed this with Johnny von Neumann, he invited me to come to Princeton to work with him for a while to see if we could evolve it into another form that would be even more useful. And this we did. And that became the standard way, eventually, of using the ENIAC. And then using that method, I got back to my problem and got it formulated and got it programmed and eventually solved.

It took me two years to evolve the mathematics and the method of using ENIAC. It took about ten million years to program it. It took about six months to get the ENIAC working well enough to run it. It took 100 hours to run off 100 cases. To put it in its proper perspective, about a year ago, this problem is still of interest to aerodynamicists. I was requested to get some of the information that was in that original report, and not having the figures, my wife and I reprogrammed this program in FORTRAN for a modern computer, compiled it ten times, and ran off all the original solutions plus the one that was asked for in elapsed time of 30 seconds.

TROPP:

Is this the problem involving supersonic airflow ??? ?

CLIPPINGER:

Yes. And as a result of that, I was chosen by Colonel Simon to become the head of the computing laboratory to succeed Dr. Dederick, and I picked up the job to start it as his assistant. So in that position, then, I saw by 1947, we had the ENIAC at Aberdeen, and I had the interesting experience of sweating through trying to make the ENIAC work long enough to the get a problem done, which was a fascinating experience. The ENIAC had run better at the University of Pennsylvania than it did after it was delivered to Aberdeen Proving Ground. It had an order of magnitude of 15,000 soldered connections, and not all of those were still soldered when it was delivered to Aberdeen. So for a long time, Homer Spence had to go around with a hammer and try to find the bad joints. Eventually we soldered every joint in the new machine. We developed new techniques for pre-cooking all the vacuum tubes so that they wouldn't have to fuss with any new tubes that were going to go out before they had been in the machine very long.

But the thing that finally made the machine start to run and enable us to get some answers was, we started working 24 hours a day. Homer worked it about 16 hours a day, and some volunteers that weren't qualified for the job worked it the other eight, and that way, all of a sudden it started to work. And then we started to play with making it work even better, and we made it appear to work very well by the simple technique for the firing tables of computing every point twice, and if they agreed, accept it; if they didn't agree, compute it a third time or a fourth or an 85^{th}—as many as necessary—until it would get the same answer twice and go to the next step. All of a sudden, like a miracle, it appeared that the ENIAC wasn't making mistakes anymore, and it would complete its trajectories most of the time successfully, whereas before that, it was nip and tuck whether it would complete a trajectory. Well, skip over the rest of that.

As to what I am doing now, I am working with the use of natural languages as input to the process of dealing with a database.

TROPP:

Ross?

ROSS:

The question of how I got into computing could be answered in one sentence. I was in the Army, and I was ordered to concern myself with computers. It was really more interesting than that. I had been an enlisted man, and then passed through officer school, and then I was reassigned to Washington in preparation for being sent to Europe. This was the early Spring of 1945. Things were changing very rapidly, and so before I was sent away they informed me that they didn't need me there anymore. And in a rare display of emotion, my superior in Washington felt that I had been cheated and gave me a choice of a next assignment. It happened that I had heard about Aberdeen Proving Ground before, being a place where mathematicians were needed, so I requested Aberdeen Proving Ground, and I was sent there at a time, as it turned out, when their office contingent had been cut and they had to dispense with some of their regular staff. And here I was a new man coming in, and the Director of the Ballistic Research Laboratory, Leslie Simon, didn't know what to do with me, so he put me on a non-existent unit that he was he was establishing at that time called the Computations Committee, of which I became the first member, and our charter was to examine methods of computation in a general way, review the work of the computing laboratory, but really mainly concern ourselves with computers, which were then only really coming up; they were not yet in existence. The Army had two large ones in quarter, and the committee was supposed to study their future use, and what we might call now program for them. It didn't have that name then. The other members of the committee reported in the next two months or so, and they were all civilians: Derrick Lehmer, a number theorist from Berkley; Haskell Curry, a logician from Penn State; and a young astronomer named Leland Cunningham.

That's how I got into computers. I left the Army at the end of the war and went back to my pre-war organization for a year, but I had been bitten by the bug of computing. I was quite fascinated with computers, so I returned to Aberdeen my first chance, which was in 1947, late 1946, maybe, and stayed for two years. It was during that time that the computers were actually delivered to Aberdeen, installed there, and gotten to work. To tie this in with what Dick just said, that was a time when he was in exterior ballistics working on applications of ENIAC to calculate the problems, I guess, and in the process, creating a whole new programming method for ENIAC. I was there as deputy chief of the computing laboratory, but again, my main interests were in computing machines. L.S. Dederick was the chief. I just heard that he passed away a couple months ago. I left Aberdeen a year later in 1948 and joined the Bureau of Standards, where I spent almost the next 20 years. I was concerned with computers.

Five years ago, I left government service and joined the American Institute of Physics, where I am now working on establishing a computer file about the Physics Journal literature. I think that finishes my story.

TROPP:

Thank you. Dr. Holberton?

HOLBERTON:

I think that I was at the right place at the right time, is how I got started. During the war, I volunteered for a job, and the type of the job was called "a computer"—that was my professional rating in the civil service—a computer. I performed hand calculations for trajectories with a group called the Philadelphia Computing Unit stationed at the University of Pennsylvania. Actually, it was part of Aberdeen Proving Grounds. They had hired a group of girls, trained them in mathematics with a speed course of mathematics eight hours a day, six days a week for four months, and then we started working. After I guess it was '42 I joined them. In '45, I had had a day-and-night alternating night shift each two weeks of going on continuing a trajectory of the girl that the same seat left at 4:00, and continued on that until midnight, and there was an opportunity to use half a dozen girls in a project, which we weren't even told what it was. I had been supervising, I think, 13-15 girls with hand trajectories, and I had just about had it, and so I was willing to do anything, and so I volunteered. They took me into a room and they showed me some flickering lights with some numbers going between accumulators, and then packed us up—all six of us, and Kay Mauchly [?] was one of the six, who is Kay McNulty in those days, and I was Betty Snyder—and we went to Aberdeen for the summer to learn tab equipment. That is where I met Dr. Alt in Aberdeen. And after we had had our tour down there and learned the tab equipment, we went back to work on the machine in Philadelphia.

In '47, that's an interesting time for me, because I working at two places at once. I was using up my overtime and annual leave from working day and night on ENIAC. I was working at Eckert Mauchly Computer-- no, it was called Electronic Control in those days, and working out my annual leave there through two days a week and three days a week in Aberdeen until September, when I finally left the government service and joined Electronic Control, and stayed with them through change of name, change of company ownership until '53.

What I am doing now, it sort of culminates a desire I sort of had that goes through our family. My grandfather Snyder was the gentleman who first proposed the National Bureau of Standards at a speech that he delivered in 1884 at a conference he convened at the Franklin Institute of International Scientists, and it has always been my desire to get to work at the National Bureau of Standards. So when the Brooks Bill came forward, and I believed in a concept of unified ethic within the federal government, I worked in the National Bureau of Standards to work in the area of making it easier for man to use computers, and also in the area of human communications about the work in computers. I stayed with the profession since its inception and raised two daughters—Pamela and Priscilla—taking four months off, because if you took anymore off in '57, '58, you were kind of lost. Things were going so fast, I just couldn't afford to take anymore time off. So I stuck with it, and am still with it today.

TROPP:

I didn't realize we had a direct link with the Bureau of Standards.

HOLBERTON:

Yes. Oh, and one interesting thing is, too, he was put on a presidential commission to look into the language called Volapuk which I have to look up now, as to whether it was a satisfactory language for the scientist to use, and he was in some kind of commission to look into this. Someday I'm going to find out what that language was, because when I was in the Navy, they put me in the group to develop the COBOL language, so I thought I was kind of following in his footsteps. Since then, I've been working in the area of FORTRAN.

TROPP:

Well ENIAC has been much commented on as we move around the table, so I think you can fill in some of the holes.

MALE:

Well, to follow your three-step question, I can say briefly, of course, that although I have been interested in all kinds of computing devices as they then existed through my youth and through my academic career and teaching career, my interest got a lot heavier when I tried to do some statistical computations in meteorology without anything even resembling the punch card machines that those with more money had available to them. So I started dreaming about how to use some of the cosmic-ray scaling circuits as counters and the control circuits, which they called guizen [?] circuits, for the controlling, switching, gating functions. It wasn't until I got to the University of Pennsylvania in 1941 that I was able to accomplish anything, because that's where I connected with money. I was inspired, as some other people probably, said by being acquainted with the relay computers which George Stibitz worked on. In my case, I was at a demonstration terminal at Dartmouth, I believe it was. I know it was somewhere in New England in a math meeting, and the computer itself was down in the Bell Telephone Laboratories ??? ???. I don't know whether that was the first demonstration of a very remote terminal or not, but certainly, it was the first one I knew of.

At any rate, I connected with the money at the University of Pennsylvania by being at the right place at the right time, if you would like, in that there was the Aberdeen Proving Grounds work calculating firing tables going on, and there was a differential analyzer somewhat like the Bush analyzer at MIT, which was calculating these firing tables. It was already kind of necessary to supplement this way. The hand calculations that Betty spoke of, where as one person got up from the chair at 4:00 as somebody else sat down to continue work on the same firing table, this might go on for days, and on the same trajectory that is, not on the same firing table, because the table means many, many, many trajectories.

HOLBERTON:

With smoothing in between.

MAUCHLY:

Yeah, with lots of work done, and we have smoothing after the individual trajectories have been prepared. So at any rate, the two eventful things that occurred there were first that I met Press Eckert [?], who had not only the technical know-how to realize some of the things in the way of circuitry and operating system, but also the, you might say, open-minded approach to these things, because I found that not everybody could do more to a suggestion of my sort than just say, "Well, it sounds interesting, but I am afraid it would awfully difficult to do." Well, the man that said, "It's more than interesting, and it is difficult to do, but we will try, and the Army can support it," was Dr. Goldstine, who was sent to the university to try to expedite the work going there. And so with the help of many people that he knew in the department, the contract was made in 1943 for the ENIAC, and they supplied not only the money but the name. We just called it an electronic doojigger or something or other. They had to give it a selling name.

MALE:

Excuse me, John. I seem to remember that one time you referred to it in one of your memos as an electronic differential analyzer or difference engine.

MAUCHLY:

This is the selling name I'm referring to. So the first selling name that you're talking about may have been Bruce Branard's [?] concoction, along with maybe Dr. Goldstine, that they would-- in fact, the early copies and the early drafts of the proposal on this refer to it as a "DIF. Analyzer", leaving it up to you to take your choice as to whether it was differential or difference. But at any rate, as the contract was processed through the ordinance, they decided that Electronical Numerical Indicator and Computer (or and Calculator) was a proper long-winded way to talk about it, and this, of course, had to get shortened to ENIAC.

Well, it's interesting that in doing this, it occurs to me we're very much performing on this contract. We were very much impressed with the need for getting something done quickly, but at the same time having a general-purpose device. Many of the things which came to pass in the way of how that was designed and built reflect exactly that atmosphere. Much of the programming was communicated to the machine through plugging in wires and turning switches. Why? Because we didn't have an adequate memory capacity to store all the programming information with vacuum tube memories, which were at the order of 20 or more vacuum tubes per decimal digit. After you got through controlling those decimal digits, it turned out to be 50-100 tubes per decimal digit. At any rate, so the machine was what you might call a partially stored program machine, because all the things that had essentially to be done in the way of altering the program at electronic speeds were provided for—mostly transceivers and in units that we called programming units. The Master Programmer was the name of a couple panels which we did the main elements and iterations and branching and so on. So I was in stored program in part, but only to the extent that we could afford to do it with the then existing methods of information.

The second interesting thing which was brought up by Dick Clippinger's remark was that it was provided for and definitely planned that we would be able to store the programs as numbers in a function table, and that there were cables provided so that you could do the programming that way. This was supposed to be written up and well-known to everybody, and this is where the history of the times, of course, intervenes again. But while we were building this, we were still intending in making a general-purpose computer, we were definitely heading towards the program that they were so excited about and trying to expedite as much as possible—mainly, the computation of firing tables where the setup or particular firing table would not be accessibly taking time if it took a couple hours or a day, because that same setup would be used for weeks. So, that is to say we hurried along trying to get the machine available for the firing table use, but happy with the knowledge that we had also designed these other elements into it so it could be used in other ways. In fact, Leland Cunningham was one of the people that most insisted in dealing with us that the machine we built be so general that we could handle, at least in principle, any kind of problem: exterior/interior ballistics, hydrodynamics, what else. Although unfortunately, with limitations on how much money you could spend and how many tubes you could put into the thing, you necessarily had limitations on the storage capacity.

There was a second thing that I am reminded of by the comments that Dick made; this is sort of an inverse analogy to what he was saying. The big program which he then later was able to rerun in 30 seconds, I believe, which was 100 hours on the original ENIAC, I had a somewhat converse thing recently. The question came up, from a historical point of view, you might say, as to what some figures were in a lab notebook which I used in testing out some of the accumulators in the ENIAC before the whole machine was built—just to try and run a few small problems with part of the equipment. I said from memory that these figures were the solution to a second-order difference equation with constant coefficients—about the only thing it could do with the amount of equipment we were testing then. Whatever these figures written down in the book, could you regenerate them? Could anybody show how those figures could possibly have ever been generated? Because it wasn't obvious just taking the figures in the book and trying just simple-minded things, you know? So I said, "Well, what I remember of what we did, I'll try," and I took a modern computer and I simulated the ENIAC accumulators with the modern computer. Of course, I was using FORTRAN as a language. It wasn't as efficient as if I had had the machine built to do exactly what I wanted. When it got through, it took, I think, ten hours to simulate, run, check what had been done in those two accumulator tests in the second-order difference equation in about 30 seconds. Now this brings up the differences, see, because what Dick was just describing a minute ago was what happened when he used the ENIAC in its slower mode, mainly when you used it as a serially programmed machine through the introduction of the instructions in an already wired-up setup, rather than microprogramming, as we might call it today. The ENIAC was really designed to be micro programmed with this big plug port of patch cords and things. And so the scheme that they went through to get greater flexibility in program setup at Aberdeen was a scheme which slowed the ENIAC down considerably to carry output in that way. Now I, in turn, here, took something which had been done on the ENIAC in its own machine language, so to speak, simulated it through FORTRAN, and on a modern computer, took ten hours to run what had been done in something like 30 seconds in the ENIAC panels.

ROSS:

??? ??? impressed us about a computer, it works ten thousand times as fast as ENIAC, but it's taken us 25 years and billions and billions of dollars to achieve this factor of 10,000. You, in one fell swoop, went 10,000 times faster than anyone before that. That is much more impressive to me.

MAUCHLY:

Well, this brings up another question, which you didn't ask but I get asked very often, and that is that if we hadn't built the ENIAC and gotten that first electronic computer, how long would it be before somebody else did, and of course, nobody can know. But I think it's inconceivable that we wouldn't have computers today just about the same as we know them. Somewhere, somebody was going to do this, and we just happened to be, as they say, in the right place at the right time. Maybe that answers where I was up to almost '47.

TROPP:

I think it is important to mention where you were in '47.

MAUCHLY:

Yes, I was going to say. The ENIAC was at the University and running test problems in 1946. By 1947, Eckert and I formed the Electronic Control Company and hired Betty and some other people.

HOLBERTON:

I worked for you; you didn't hire me.

MAUCHLY:

Oh.

HOLBERTON:

I didn't get paid. [Laughter]

MAUCHLY:

Well anyway, we tried to make it fair. But at any rate, the first actual contract for a new kind of computer which would have more adequate memory capacity to be able to put all its program into memory stored, that program was made through the Bureau of Standards to supply a machine for census. It took us more years to do that, of course, than it took us to build the ENIAC. It took us about 2-1/2 years to build the ENIAC. So in '47, it was '51 or '52 before we saw something that looked like a machine with magnetic tapes on them and all the other things that we had thought of as we went along and said how nice it would be to do, but we found you get the engineering done and all the systems thinking done, the time necessarily passes. So in '47, we were still the Electronic Control Company in August. Later that year, we changed our name to the Technologic Computer Corporation in order to be able to sell somebody some stock in this worthless project. And we had difficulties getting further financing, but at the '47 year, we were managing to exist and thinking about the programming just as much as we were thinking about the hardware, because as far as we were concerned, there was no use designing the hardware unless you have some reasonable idea as to how you're going to get the programs and make it do what it wanted, and Betty and I particularly were interested in problems of sorting—how do you sequence data. That was what was going on in 1947.

What's going on now? Well, I have been working on several problems, including the problem of what makes stocks go up and down or how can you make money out of the stock market without knowing anything except what a computer can tell you if it's given the right program. Obviously the answer is, give it the right program, and see that the data that you feed it is also correct. You finally do that, why sure, it works fine, except nobody would believe it now.

TROPP:

Thank you, Dr. ???. ??? ?

CHU:

Well, I think there are three aspects distinguishing me from the rest of the panel. First, I cannot quite remember what did I do in 1947, but I can remember the summer of '42 much better. No one laughs. The second one is I am a pure, very pure, hardware man. The third one in the past decade, I say, and quite a lot in business, management, policy-making had very little to do with-- I shouldn't say little to do; not too much to do with R&D, especially personal participation. At summer '47, so far as engineering is concerned, well [inaudible], everybody is struggling with the ENIAC, so as far as I am concerned, ENIAC is finished. Von Neumann has already published his paper on logic engines, so we are looking forward to building still better hardware, a so-called ??? later on, and also the EDVAC. From von Neumann's paper is obvious we need bigger magnetic memory. We need a better input/output device. At that time, General Electric is already working on magnetic recording, so we got a hold of some early laboratory models of General Electric's magnetic recorder. Eckert and I try to collect ??? solid wires anywhere. We study the property of the wires, study many properties such as retentitivity and what not, in order to get better packing density to record magnetic impulses.

TROPP:

Excuse me, Dr. Chu. I realize we are about to run out of tape, and have to make them change, so can we take about a five-minute break? We'll continue in five or six minutes. Excuse everybody, and stand up and get away from the lights for a moment.

[Break]

CHU:

…great experiment ENIAC behind us. It's obvious we needed better ways to build better computers. It is obvious from our experience with the ENIAC, we get the better memory and the better input/output devices. So Bill Eckert and I, we have some ideas I used to examine the property of magnetic wires, to use it as an input/output device with the possibility and better utilization to replace magnetic cards.

The second is use magnetic cards to build internal memory. These were my activities during summer of '47. Eckert may not know it, but he has a really big influence on my life. At the early part of the '40s, namely '43, '44, I was a graduate student at the Moore School in the University of Pennsylvania. In order to make a living, I was teaching Chinese, laboratory structure in the Moore School, and also doing some research. You know, as you ??? Moore school, you cannot help somehow getting drafted.

I got into computers rather late; late, that is to say, compared to John here. I got into it about early '44. ENIAC about finished except one part. We left the most difficult part last as a divider. So they heard that I had nothing to do and wanted to make a living; how about you try the divider. So I have been ??? with the divider ever since. So I got into it not because of my vision, not because of my interest in computers, but simply because I was there, I know John and Pres.

A second influence that Eckert had on me is after my experience with ENIAC, after my brief tour of tour of duty in industry at the Reeves Instrument Company, I decided the industry ??? ??? to make a living. So I retired to the high mountains in a place called Argonne National Laboratory at the University of Chicago. However, during the Eisenhower regime, the Garland [?] Research Federal Trust had been cut. Eckert ??? took a few million dollars to build a computer called ???. He thought I could be the man who could be the chief engineer. So somehow, I got myself involved in the commercial world.

Right now I am with Honeywell, still commercial. For the past decade, I have not done much to contribute directly or publish directly in the R&D area. Mostly my activities have been with business, marketing and policy making.

TROPP:

Thank you, Chuan. And before Bob wilts under the lights, we'll let you finish our identification of the group here tonight.

CAMPBELL:

It was December of 1941. I remember the time, because it was just about three weeks after Pearl Harbor. I was a graduate student in physics at Harvard, and my wife and I were having a Christmas vacation in New York when I got a telephone call from Howard Aiken, who I had never met. He wanted to see me in New York for a few minutes about a topic that he didn't divulge on the phone, so we arranged to meet at the Grand Central Terminal, and had supper together, as I recall, at the Commodore Hotel next door. Well, it appears that having initiated the project with IBM, which was the design and construction of the Mark I Sequence Controlled Calculator, and having worked with IBM on its design for a year or two, he had been called up by the Navy and was on active duty at the Naval Modern Warfare School. He was looking for someone to continue the liaison work with IBM and Endicott during the time that they were completing the computer. And having gone through a list of the roster doing graduate work at Harvard, they picked my name out. This sounded rather interesting, so I said that I would undertake the job.

During the next couple of years, the computer was completed and put through some kind of test at MIT and put through a few test problems at IBM. The work there, incidentally, was under Claire Lake and Frank Hamilton and Ben Durfee. In the late winter of 1944, it was delivered to Harvard University and set up in the basement of Cruft Laboratory. We put a couple of rather simple problems on it in the latter part of the spring. One was a problem in ray tracing for a multi-component lens system, and the other was the evaluation of an integral of some importance in antenna patterns. It rather suddenly appeared in June of 1944 that Howard Aiken managed to have the computer declared a Navy project, taken over by the Navy, and this Navy project actually started up in the latter part of June, as I recall. One of the earliest problems which we did for the Navy was a multiple correlation problem involving determining the effect of small impurities in the properties of steel. Quite a number of people who were here this evening joined the project about that time—Ed Berkeley, Grace Hopper, and Harry Goheen, who are in the audience; Dick Bloch I guess is not in the audience.

After working with the Mark I, I was concerned with the system and logical design of the Mark II, which was a relay calculator. At the time that the first ACM meeting took place in 1947, I joined the group Raytheon, which was working on the computer which Harry Crawford mentioned, which was eventually called the RAYDAC computer, and was eventually delivered to the naval air and missile test center at Point Mugu, California.

At the present time, I am at the Miter Corporation at Bedford, Massachusetts. Incidentally, Mitre is out of Whirlwind via Lincoln Laboratory, I guess you would say, concerned with system analysis and system engineering, principally working in the area of information systems for state and local governments in the area of human services, courts, and police.

TROPP:

Thank you, Bob. Now that we have completed the brief vita of our distinguished guests, I would like to return, I think, to the focal point of the occasion, the 25th anniversary of ACM, and just start off with a few comments, and just throw the discussion open hopefully to some argument of debate. It strikes me, looking backward in time, that 1947 was a highly unlikely time for a new professional organization to have begun. As I look back to what was available in 1947, the only machines that are operating are still ENIAC; the Bell Labs relay computers; the Harvard Mark I and II, and I guess possibly III. The institute's machine is still in its conceptual stage. EDSAC in England is in its design stage, possibly—it actually began running in 1949. EDSAC is still under design at the Moore School. In terms of the Electronic Control Company, or Eckert Mauchly, you've got the BINAC, the UNIVAC contracts. If I am wrong with these, I hope somebody will correct me. I'm not sure whether they RAYDAC machine was being begun, but it's approximately that time, '47. Turing, I think, has begun work on ACE. Williams is beginning to work on the Williams tube memory. But all of these are still in the beginning stage. The beginning thinking in terms of substituting mercury delay lines for memory. I am reminded of a quotation—I'm not sure if it's accurate—of Turing, who advocated the use of gin, which he says that they contain alcohol and water in just the right portion to give a zero temperature coefficient of propagation velocity. This is the beginning thinking.

I would like to read from a couple of things in the current anniversary issue of the Communications, one from an article on the first 25 years by Lee Revins, in which the following quotation appears: "The original modus and organization of June 1947 included the following paragraph: 'The purpose of this organization would be to advance the science, development, construction, and application of new machinery for computing, reasoning, and other handling of information.'" The last quotation that I will read and then I will get out of the way is an article in the same issue by Eric Weiss, entitled "Publications in Computing and Informal Review", and the section where he discusses the non-publication attitude of the ACM at its formation—that is, the desire not to start a new journal. And he says, "To some extent, this non-publication philosophy may have been adopted because some early ACM leaders saw the society as a declaration of independence from IBM, and, by extension, from all commercial considerations like the sale of publications and the solicitation of advertising." With that beginning, I will turn it over to the group and let you discuss how ACM, this problem of communication, which I am sure you were faced with in that period, and what it was that led the group here and others, some of whom are in the audience, to consider the formation of a professional society.

HOLBERTON:

I think you just stated why. I think in reading the list of things that were under development at that time, the enthusiasm of all of us to know what was going on, and to share in that, just bringing to life, again, the massive things that were going on in different little places, I think, is the thing that really put it together. If you went to a meeting at the Harvard Symposium and heard what was going on, for some of us, for the first time, and wanted to keep up with those people who were at the threshold of developing a new science. And I, for one, realize that it was historical. I really did. I knew it was something new, and I had tremendous enthusiasm, and you didn't want to lose contact with these people that you met for the first time.

BERKELEY:

I think I was the author of that first sentence that you wrote in regard to the purpose of the Eastern Association for Computing Machinery. I know the main feeling that I had at the time was that there was definitely an exclusion force operating by the number-one people in charge of at least some of the investigations going on. They weren't too much interested in having the echelon-two people find out as much as they really wanted to find out. It was for this reason that the temporary Committee for the Eastern Association for the Computing Machinery became started at my initiative, because, as Betty Holberton said, we wanted to find out what was going on, and the person who was mainly in charge of our laboratories—perhaps I better be specific and say Commander Aiken—was definitely not too interested in spending any time for the second-echelon people to find out what was going on. We wanted to find out new developments. We did not know the new developments. We weren't too interested in publications, because there were perfectly good methods for getting things published in the usual slow scientific society method. There was a definite attempt to set the dues as low as possible—around $2 a year—so that anyone who was interested in meetings could come and find out. I was acting as temporary secretary in the beginning, and I acted as secretary of the Association for Computing Machinery from 1947 to 1953, and one of the things I sought to do was arrange for the distribution of mimeographed memoranda and reports covering the… [end of recording]

[Disk 2]

BERKELEY:

--things that are going on. It seemed to us that that was important, and it seemed to us that the bodies passed with a democratic approach. It was important also that anybody who was interested in computing should be able to afford the $2 dues and be able to become a member of the association, and find out what was going on.

TROPP:

Did you want to respond to the other comment about the purpose of the society as a declaration of independence?

BERKLEY:

The society, it seemed to me, wasn't really a declaration of independence. We did not want to run head-on into people like Aiken and von Neumann. They, very logically I suppose, felt that there was no ground for another society, and so they didn't take steps to make it easy for the second-echelon people to get together in a professional society. The one conspicuous exception to the was Pres Eckert and John Mauchly at ENIAC. They were young people too, and they were really very eager to help everybody who was interested in finding out things find out things. I don't know why things actually worked that way. I suppose when the field is new, the personalities of the persons involved have a very large influence on what particular kinds of turnings are taken. Does that answer your question?

TROPP:

Yes, thank you very much. Harry, you look like you had a comment.

HAZEN:

I would like to hear the founders first. I appreciate very much Ed's remarks, and I think being one of the non- founders on the panel, it would be very helpful to hear from the founders and then perhaps pick up at that point.

TROPP:

Franz?

ALT:

Well, if this kind of feeling of becoming more independent was present at the time, I didn't know about it, and I could share it. I guess we each had our own motives. I didn't even have a very strong motive. Somebody suggested we ought to have an association, and looking at it, it seemed like we had a good idea. If anything, we were imitating the example of the more mature professional societies. We have always looked at them for a pattern. I did perhaps a little bit have the feeling that we needed more mutual information than we had among the few computing laboratories in existence. There were four that were actively working at that time: Harvard, Aberdeen Proving Grounds, and Bell Telephone Laboratories. Those were places where large machines were operating. Depending on where you draw the line of what constitutes large machine, maybe it could be more. I remember once saying to Professor Aiken, "It would be nice if these laboratories exchanged from time to time lists of programs being written." I think we didn't call it programs then, but whatever it was called—list of problems to be solved. And he shrugged his shoulders and said, "Well, we might try to let them to each other once in a while, but I don't think it's worth making a project out of it." There was some feeling like that, but it was a strong factor in founding the association. I think mostly we felt that this was a new profession and we ought to act like other professionals in other fields.

TROPP:

Tom, do you have a comment?

TOM:

Well, I think the credit for really pulling this all together and realizing that there were good reasons, and probably numerous ones, for having an association, and different people might join for different reasons, but nevertheless, they all were good reasons. The credit for realizing and all that and getting something done about it pretty obviously belongs to Ed Berkeley. I think I was just another one of the camp followers, if you would like. I feel somewhat the same way with respect to another organization in which I participated in the founding years, you might say, The Society for Industrial and Applied Mathematics. So were other people, who realized the need more than I did, but when it was explained to me, I said, "Sure, I'll join in with you and do what I can, you know." But it was, to my mind, mainly Berkley's vision, his action, too, which caused the creation of this, and the fact that he was right is evident today, of course. But the society grew, and our modest name "Eastern Association" got a ban for just plain "Association"; it became national.

This matter of independence is a little hard to figure out, but I think that others have spoken adequately about Aiken's attitude, let's say, a shrug on the shoulders and "Why bother?" or something. On the other hand, there was another protagonist, I should say, in the field of computers, who I think must have felt quite otherwise, but who was also quite self-sufficient, and that Dr. von Neumann. He learned about computers very late in his career through, I guess, a chance meeting with Dr. Goldstine, because they were both associated with Aberdeen. Dr. von Neumann was one of the members of the scientific advisory committee or something of that sort at Aberdeen. And once having met Dr. Goldstine, it was like a drag race, you might say—how fast can you get off the ground or something; how fast can you go? Burn the rubber! But immediately having been involved with many computing projects, I asked both the atomic energy and Navy hydrodynamics offices and things. But he had been absorbed in many ways in trying to get practical computations done, but the idea of building an electronic computer had not been one of the things that had occurred to him, apparently. So when he heard that somebody else was doing this, he immediately wanted to know all about it. Well, he arranged the meeting in the Moore School and in fact several meetings, a number of meetings. And the result of this was, of course, that the ideas that he had been carefully working at under a cloak of government security classification were suddenly broadcast to the world by Dr. von Neumann, who was an invited speaker at practically every professional society and was the number-one champion of the stored-program machine, which largely accounts why his name is often associated with that, and sometimes these are called von Neumann machines. He was the first one, of course, to publish anything in this. Schwan referred to some of the publications from the institute. Well, Eckert and I, under the security classifications that we believed were imposed on us, we felt the safest way was to keep busy in what we were doing and not try publishing at that time. So you can't say that von Neumann was reticent about talking about these things; he was undoubtedly the number-one champion of the new kind of computing programs, and he wanted to get everyone to use them. And because he was so competent and so good at this, well, I guess he never felt the need to either recognize ACM or join it or participate in its founding; but some of the rest of us felt this was a good way of doing things, so we went ahead and tried to build up the Association for Computing Machinery.

MALE:

Well I would also like to pay tribute to Ed Berkeley's leadership and the work which he did in establishing the society. I think that all of us at Harvard did feel, as Ed has expressed, a general sense of isolation from other elements of the computer field. I think we did have some fairly close ties, at least I felt we did with the people at Bell Laboratories, George Stibitz and Sam Williams. I think our contacts with even the people at MIT, just down the river, so to speak were rather infrequent, and certainly as far as the people in Philadelphia were concerned, we did not feel we had at all close contacts with them. We actually felt that we would benefit, and hopefully they would benefit, too, if there would be closer contact.

I should perhaps say that although Howard Aiken was not an advocate of the Association for Computing Machinery, he did join. He joined through a subterfuge. I think the dues had increased up $4 at this point, and I asked him for the $4 that he owed me, and he thought he did owe me $4. He paid me the $4, so I put his name down in membership. So he did join the Society. I rather doubt whether he's kept up his dues, however.

I might also make another comment with regard to dues. It fell upon me to transport back to Boston the dues which were collected in the first ACM meeting in New York. I remember carrying $50—it seemed like a lot of money—$50 of other people's money in my pocket back from New York to Boston in the upper berth of the Owl.

ALT:

The dues were one dollar in the beginning. They were set so low. They were raised to $2 sometime in the second year. But at the time of the meeting at Aberdeen, for example, they were still $1, and we set a registration fee for the meeting for non-members also equal to $1, and left to everyone to choose whether he wanted to use that dollar for registration or membership, and I guessed what everyone did, but there was one individual who insisted in calling it registration and not membership fee, because then his company would pay for it.

MALE:

Well the people attending the first were affluent enough that of the 68, 50 were able to put up a dollar.

ALT:

It was not until 1954 that we raised the dues to $6 at the time when we started with the journal, and we knew that we would need more revenue to pay the printing costs. We were quite concerned about the impending drop in membership, but actually, during the first year after that, the membership increased from about 1,000 to 1,300.

HOLBERTON:

It was about that time that they were going to set up a family membership also. We kept trying to get involved in finding out about this, since my husband and I were two charter members who had been paying the fee the last 25 years twice in the same family. It wasn't until recently that you were able to get other journals than the same two.

MALE:

Yes, I would like to affirm that for me, as well as for the other people who have spoken that Ed is Mr. ACM, and has been right from the beginning. Those around in 1947 when ACM was getting underway know all the ways he had his influence on the enterprise right from the beginning. I knew the language was his. The Association for Computing Machinery was also his language, right?

BERKELEY:

Well, I suppose that I had a good deal to do with making The Association for Computing Machinery come into existence, and I do think I carried a good deal of the ball a good deal of the time. But it seems to me the biggest contribution that was made to, at the meeting that occurred in September 1947, the biggest contribution that was made was we Franz Alt immediately said, "Let us have a meeting in December at Aberdeen, and really organize it as a useful meeting, and as first regular meeting, planned meeting of the Eastern Association for Computing Machinery." Ever since that time, in my opinion, Franz Alt has been, I would say, a very important main spring of The Association for Computing Machinery.

I might add that I know very definitely the time and place when the impetus to form the Eastern Association for Computing Machinery took place. There was a symposium in February, 1947 at Harvard, and Professor Sam Caldwell spoke of the possible desirability of an association. Nobody made a motion to form such an association; the symposium broke up with no decision taken. The next reasonable place to do something was when a meeting of the National Research Council, which included five people, met in April in New York. The members of this National Research Council committee, in large-scale calculation, were Professor Aiken, Dr. John von Neumann, Dr. George Stibitz, who is here this evening, Professor Archibald Brown, and Professor Lehmer of University of California at Berkeley. I went over to the Prince George Hotel in the evening before the meeting took place and urged Lehmer and Brown to ask to have the National Research Council Committee organize or call for the organization of a computing machinery association. And Lamer and Brown agreed to take it up at the meeting the next day and try to go out and have the decision taken. I found out about a week later that one of them brought it up, the other one hadn't said anything, and George Stibitz and Aiken and von Neumann all decided there was no need for one more scientific society, and they tabled the motion for a year. This made me angry, and I rebelled, and in the next few days, I sent out a notice to the friends I had in the computing machinery field, a notice to about 15 or 20 people saying would they join me in calling for the organization of computing machinery. About eight people said yes. Among those who say no was Grace Hopper. Those eight people, who included Kate Sharpless and Charles Tompkins, who unfortunately has died since then, were the original founding persons of the Eastern Association for Computing Machinery—the people who decided we would organize the first meeting. So you can see, it was an act of rebellion that caused the beginning.

TROPP:

Perhaps we can go back to some of the reason for rebellion. Most of you have indicated in earlier comments about how you got into the computer field—generally one of being in the right place at the right time. Prior to the formation, prior to Ed's rebellion, how were you communicating? How were ideas passed around? Harry?

HAZEN:

Let me just say this, that the IRE and the IEE were both getting very active beginning early in 1947, and at that time, the electrical engineers—electronic engineers, of course—were the most prominent part of the field. They gravitated naturally toward those two organizations, and especially IRE, which took a very aggressive role. Then at this March meeting in 1947 had the first public program—the first program—at which the computer was placed before the public. And that was organized by Weber and the other engineering department at Brooklyn Poly, and Jay Forrester, who actually organized the meeting. Forrester, Reichman, and Hillstein, and Sam Alexander and I were on that program covering storage. I covered applications; the others covered various technological aspects. And from that time on, IRE had a really energetic program going. Many people looked to them for leadership. But it was the field broadened, there was more and more people seeing that the leadership was not to be found from the engineering organizations alone. And also at that time, there was not very much leadership forthcoming-- [end of recording]

[Disk 3]

MALE 1:

…they were kind of ostracized [chuckles]. Not one of us. The organization was formed at MIT to brew up interest of the departments in the use of automatic computers, including mathematicians, but not many successful—

MALE 2:

[Inaudible] There are so many math departments that in their entirety [inaudible]. But there were some people who were interested. Von Neumann is one; Dick ??? certainly was, and [inaudible].

MALE 1:

Okay. But in the academic establishments in our neighborhood, I think it was something else.

MALE 3:

I think what you are saying is that there were individuals who were interested, but the institution—the establishment, no. The individual didn't find anyone else in his own math department, necessarily, having any common communication with him.

MALE 4:

Well, is that so surprising?

MALE 1:

I would like to respond also to the point that Ed made and others made concerning the sense of isolation and feeling a lack of communication means. Military security did play an important part in limiting communications in the early days—limiting publicizing of new developments and new ideas. And when Sam Caldwell got up in the meeting that Ed mentioned at Harvard, he was speaking out very vehemently again military classification of developments in the computer field, referring primary to Whirlwind, what he saw to be the very serious nature of continuing a project like that on a confidential basis after the war. So he did lend a hand very energetically to the formation of the organization—an organization of the kind that Ed was talking about. The ??? security was also a factor in cutting down communications. What you could call academic security, the lack of desire of the academic departments to communicate with one another was a factor. So I think that the time was right for the formation of the group and for Ed's rebellion. I attended the organization meeting at Columbia and the organization meeting in December, and there was a very fine spirit on both occasions, and it was pretty clear that the time was right for the formation of such an enterprise.

TROPP:

Would you say this formation of a society at this particular period in time, with so many new machines on the drawing boards and an attempt to look at some other ideas in terms of storage and memory and an attempt to move ahead, gained some impetus then from the formation of the ACM?

MALE 1:

Oh, I think so, sure. You have to remember that these machines were on the drawing boards, that they were all going to be running next year. [Laughter]

TROPP:

This was about the time when the von Neumann factor, I guess, came in the 18 months.

MALE 1:

It was even shorter in the earliest days.

MALE:

I think the most reports on projects, everything was described in the present, as though it was already running, even thought it was only a gleam in someone's eye. You will never tell from the tense that was used how things actually stood.

MALE:

I could comment something about communications other than publications that were occurring before the time of ACM. There were scientific advisory bodies for every governmental research group in the United States and in England, and von Neumann was the strongest catalyst of all of these—he belonged to all of them. And he was the greatest communicator of ideas about computing machines and about methods of using computing machines before, during, and after the formation of ACM. I think John played down his role. It may be true that he didn't know anything about computers before 1945, but I think he doesn't need any apologist. But nevertheless, I feel that it should be said that he saw more deeply into what they were all about than anybody else, and his incisive report written in 1945 describing the EDVAC was read by everybody, and had made a tremendous impression. And when I say everybody, I mean several hundreds of people all over the world, at least in the Untied States and England. There were others that, like Hartree in England, who had been doing important calculations for years before there were electronic computers, and who were just itching for a tool that would allow them to move ahead rapidly. So the interest was there. You commented that it didn't seem as if the environment was right for the formation of such a society, but the lectures that had taken place at the University of Pennsylvania had been attended by people from many places around the world, and those had stimulated tremendous interest in the field of computers. To those of us who were on the scene at that time, it seemed very appropriate that such a society should be formed.

TROPP:

How about communications between the US and England at that point? I mentioned a number of things going on. There was an automatic relay calculator that was also going on at Birbeck College at that time. The business machine LEO was at the conceptual level then. Other things going on here in the United States, I think Dr. Stibitz also had a business electronic calculator conception, and about 1946, the Barber Colman machine, which was never actually manufactured, but it was built. What were communications like between the two countries, and did the formation of ACM in any way assist in the communication links?

MALE 1:

I think the communication links provided by the Army and the Navy was very important. Both the Army and Navy had very extensive interchanges during the war that continued after the war—scientific interchanges. I know that Hartree and Porter from Cambridge were over in 1945 to view the ENIAC and tour the country to look at everything that was going on and took that back to England. ??? was back here in '47 or '48, visiting again all the projects and reporting his work, and Wilkes was over back and forth. There was a great deal of interchange that way—conferences.

MALE:

It was frequently said that best communication between American research groups was carried on by visits by the English.

MALE 1:

That's right, because they would move from group to group within the United States.

MALE:

That's right.

MALE:

I think that's probably true, because all of these people you mentioned and others visited us in 1946, '47, and '48 era. In fact, one of the things which seemed to us a wrong decision, but who were we to question why, that was when a representative of the Swedish Royal Commission of Computers came to us and wanted to make a contract to buy UNIVAC. We were told by the State Department that computers were munitions, and we could not export them.

TROPP:

This was in 1947.

MALE:

'47 about, yes.

TROPP:

A look at the logbook at Harvard, I think, indicates in terms of visitors almost an international group.

MALE:

And at the ENIAC. In those days, you didn't see a manager of some department of data processing; you saw the Prime Minister of Belgium and the King of somebody else. All the top people were coming over here. The interest was very deep.

MALE 1:

I showed the differential analyzer to a duke and duchess during the war.

TROPP:

I guess my comment about really not the right time was more to stimulate than to indicate the decision. I think it is clear, as one looks at the ENIAC project, the work at Bell Labs, the work at Harvard, that people all over the world from almost every country of any size were visiting these projects to see what was going on. People were interested. The number of projects that were under way, and by 1949 which would probably double in terms of things that were going, indicated that I think Ed's rebellion was at the right time, and the time really was right.

How about this resistance aspect? You threw out a great number of very high-powered names who felt that there shouldn't be a society. Apparently it had no impact on its formation, but how strongly did this affect people who might otherwise had become involved during the early years?

MALE:

Well my guess is that the comment about von Neuman was considerably misinterpreted. Von Neumann might has said he didn't see any need for it right this minute, because there weren't more than 100 people that might be involved right at this moment, but he certainly felt deeply the need for communication, and he showed, I think, considerable interest in it for a long time after that. Didn't he, Ed?

BERKELEY:

Well, I had some correspondence with von Neumann in the summer of 1947, and he did say in his letter to me that he did not think there was a need for a new society at this time dealing with computing machinery, but he said, "If you prove me wrong, I will be perfectly satisfied," or something like that. I think it was in connection-- I can't remember where, but it had been connected with inviting him to join and become a member, which he did not.

MALE:

No, but which we were positive [inaudible; overlapping voices].

BERKLEY:

He said if you prove me wrong, I should not be unhappy about it.

MALE:

Yes, but we invited him to give a talk at the Aberdeen meeting a few months after founding the association. ??? ??? was most cooperative at the time.

MALE:

[Inaudible; overlapping voices] during the other talks.

MALE 1:

He spoke also at the ACM meeting in Los Angeles in ’48.

TROPP:

Once you got started, Ed, there wasn’t really any resistance, I guess.

MALE:

??? came along. He was [inaudible]. He was interested, and I think he probably joined; I’m not sure though.

MALE:

George, did you join?

MALE:

I’d like to make a comment. Since Ed commented about communication in his neighborhood, I thought perhaps I should make some comments about communication in my neighborhood. We’re both in the East Philadelphia area. For people in the Philadelphia area, and I for one, I think many others, perhaps a few this way, we would never have a communication problem. Although Mauchly is not at that time [inaudible], John von Neumann being invited to everywhere to give a speech. But he gives speeches all the time—in the hallway, in his office, after class, in the tavern with the students. His publications is laying strewn all over his desk, working ??? ???, “John, what do you mean by that?” John would grab hold of you; you cannot go home for lunch. Also, John was communicating with these English people, Swedish people, and also some ??? also consulting, stay with us. John von Neumann is there all the time, or most of the time. So we feel that we have so much information sometime it is difficult to digest. So maybe it is one of the reasons that the people closer to the Philadelphia area feel the ACM is not immediately, imminently required. But nevertheless, I think we owe tribute to Ed for him to have this vision to get the things here organized.

MALE:

Perhaps we had the kind of thing that was later called an invisible college. Those on the inside knew quite well what was going on with the establishment. There were a great many on the fringes that did not.

MALE:

Invisible college or invisible university, it's still very much out there today in many fields. But it seemed that with people like von Neumann traveling the world, you had a spokesman for the invisible college in the groups that were working and you did communicate among each other. So there really wasn't a major communication problem as such or per say.

MALE:

There was for the people who were curious and could easily find out.

MALE:

Right. The secondary level people I think that as Bob pointed out, a number of you have.

HOPPER:

I think that's right. I think that's where it was really felt and those of us who were not the in decision making who were merely using this instrument were really at a loss to get information, except looking for [inaudible] over being told about it, but you still didn't know just how far a field you really were grasping the knowledge. And I think that it was a big help to us who were not members of the IEEE or the American Mathematical Society to be in a position of being informed of what was going on. You know, I think I do agree with Ed Mercury [?]. For us in that position, it was a tremendous boost. It really was.

MALE:

Let me throw out another observation. It seems that you have within the upper echelon good communications on the East Coast, good communications with the operations in England. And I guess I'll throw the question at Ed, and anybody can respond to it who might want to. If you look at the next four or five years in terms of computer development, the realization of all of these machines that are on the boards, developments in about the same time period on the West Coast of the United States, do you think that ACM played any role at all in helping to disseminate information or to establish communication links coast to coast level within the boundaries of the United States?

BERKELEY:

I'm extremely sure that the ACM made an enormous difference in communication about people interested in computers. One reason for this to be true is the meetings that ACM organized and planned and held. Another reason is the publications that ACM in an informal way was distributing, mimeographed notices of things that were happening. Some of this was carried on for many years as the Digital Computer Newsletter by, I think, a part of the Office of Naval Research.

MALE:

Office of Naval Research?

MALE:

That's right.

BERKELEY:

Mm-hmm [yes]. I do not think that the field could have developed nearly as well or nearly as fast without the existence of some kind of communication link among all the people who were interested in it and were not top-level people.

MALE:

think there's another point which seems not to have been mentioned, at least I haven't noted it, but seems to me is very important in this development. The two separate engineering societies, IRE and the IEEE, had their own committees on computers. In fact I was a member of the IEE committee for quite a while. But those things were mostly oriented to the kinds of computations that were going on in engineering and were to serve the people who were oriented in an engineering way entirely. One of the major things about the charter of the ACM was that it was to open in its membership to anyone who was interested in using computers of whatever kind for whatever purpose. And people who would never have gone to an engineering meeting and have never known, of course, what transpired in committee meetings of these engineering meetings, which were sort of closed door sessions really, many people joined up from the users group, and the users propelled this as much as anything, and this gave rise to the great exhibit potentialities, too.

TROPP:

Are there any other comments before we bring our discussion to a close?

MALE:

Do you want to say anything about this?

TROPP:

Yes, why don't you, Ed? Why don't you take the last few minutes. While you go on allow me to thank everybody for your time, energy, ACM and Honeywell, and take the last three or four minutes, the two minutes I have now been informed on the machine to your left.

BERKELEY:

One of the outcomes of my stay at the Harvard Computation Laboratory was the decision to make a baby relay computer, which drew some interest. It is called Simon. It was the front cover of *Scientific American* in November 1950. The painting that lent to the front cover is here. It was given to us, my wife and me, by Mr. Patrick McGovern of International Data Corporation. The machine itself has about 120 relays and a stepping switch, which operated by means of a punch paper tape, which fed in both data and instructions. And at the time it was finished, it was guaranteed to do no useful work [laughter]. The purpose of the machine, of course, was to make clear to people with very small numbers ranging up to approximately 16 in the binary notation how an automatic computer could work and make computations. One result was that some four or five other Simons were made and we sold over 500 copies of construction plans for Simon. Have I used my two minutes?

MALE:

I think so. Thank you very much. I would like to thank everyone here for your time and most interesting discussion. [Applause]

[End of meeting]