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Interviewee: Association for Computing Machinery (ACM) Annual Meeting
Interviewer: Panel Discussion with Issac Auerbach, Bruce Gilchrist, Sam Alexander, Bruce Schoonover, George Stibitz, Richard Bloch, John Mauchly, Herman Goldstine, Edward Cannon, Maurice Wilkes, Grace Hopper, Jay Forrester, Arnold A. Cohen, E.G. Andrews, Ed Berkley, Leslie Simon, Donald Eckdhal, Herb Grosch, Henry Polachek, Richard Turner, Jan Rajchman, Jerry Haddard, Betty Holberton, and Arthur Burks.
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Tape #1

AUERBACH:

Good afternoon ladies and gentlemen, and welcome to a most unique occasion to celebrate the 20th anniversary of the Association for Computing Machinery. This session, is entitled "In The Beginning, Reminiscences of the Creators." I'm Isaac Auerbach, the Chairman of this session and Moderator of the panel. Before proceeding, I would like to ask Dr. Bruce Gilchrist, who is President of the American Federation for Information Processing Societies, AFIPS, to come to the platform for a special event. Bruce.

GILCHRIST:

Thank you Ike. Ladies and gentlemen of the top table, or the speakers, ladies and gentlemen of the audience. For several years it has been the tradition of AFIPS to present each year the Harry Good Memorial Award to an individual who has made an outstanding contribution to our field. At the last meeting of the Board of Governors of AFIPS, it was unanimously voted that the 1967 Harry Good Memorial Award should be made to Sam Alexander. This occasion seems most auspicious to make the formal presentation. In the audience are many of Sam's colleagues, co-pioneers maybe the better word for it. This afternoon I'm sure you are going to hear many things about machines, programs, which Sam has been involved in.

Rather than go into a long history of his accomplishments, let me just point out one or two and then my good friends to my left I'm sure will fill in much better than I.

Sam Alexander was the scientific officer for the Government contract involved in the procurement of the first three UNIVAC's. Sam Alexander was instrumental in the design and construction of SEAC, which I believe was the first electronic stored program computer to be operational in this country. Following this work on SEAC at the Bureau of Standards, he developed BISEAC, which was the first portable computer. With these

accomplishments behind him, he did not retire, he became in 1954, I believe, the first head of a newly established Data Processing Division of the Bureau of Standards. Over a period of twenty two years, I believe it is now, Sam Alexander has been instrumental in the establishment of numerous computing facilities within the Government agencies and has given almost every agency greatly of his wise counsel and advice. It is therefore with great personal pleasure that on behalf of AFIPS today I would like to present to Samuel Nathan Alexander the 1967 Harry Good Memorial Award for his contributions, his pioneering contributions to the introduction and exploitation of computers in the Federal Government. Sam.

ALEXANDER:

To put it mildly, this is a surprise of unusual proportions. Not only was it kept completely secret from me, even Ida Rhodes knew and didn't tell me. Furthermore, I think some of you know I've been in the hospital for observation for a stomach upset and even the doctor was convinced to pull all the tubes out of me and let me come here for the afternoon. So I had no communication with the outside world that this was going on. I deeply appreciate the personal honor that comes to me with this award, but I want to say I'm standing on the shoulders of an awful lot of people and I want to give credit to them, I don't think it's fair to take time to name them one by one, but I fully appreciate that whatever I've been able to do, it has been as a part of a concerted effort in which at least I had the great pleasure of feeling that I was leading people in a direction and helping to break paths toward the creation of something that will be good for our country, good for our Government and, I hope, good for the world in general. This opportunity at this time to say these few things and express my appreciation for both the honor and the assistance in achieving this honor is gratefully appreciated. Thank you very, very much and I certainly will remember this a long, long time.

GILCHRIST:

Ladies and gentlemen, Dr. Asten who is, as you know, the head of the National Bureau of Standards, had hoped to be here but cannot be.

In his place, we have Dr. Schoonover, his assistant, who would like to say a couple of words about this little ceremony. Dr. Schoonover.

SCHOONOVER:

Thank you. Sam I'd like to say that we at the Bureau today are very happy for you on this occasion and I do regret very much that Dr. Asten could not be here today but I would like to, on behalf of all your friends on the staff at the Bureau to congratulate you and we brought along a little private cheering squad over here.

AUERBACH:

Thank you Bruce and I'm sure we are all deeply impressed with the opportunity that we have had at this occasion to honor one of our own, Sam Alexander.

I would like to set the ground rules for this discussion this afternoon and give you a little background as to why we are doing what we are you doing. As you are aware, for some years now there has been conversation under way to try to start the writing of the history of the development of the digital computer. In keeping with that, we have the very outstanding services right here in Washington of a group of people who are recording this session on audio tape, on stenotype tape and on video tape. The video tape is through the courtesy of Ampex Corporation and Multimedia Engineering. To accommodate them, we have one minor problem and that is the tapes are only one hour long. So that every hour you are going to get a 90 second stretch so that we can try to record as much of this as possible.

I'm going to ask each of the panel members to make a few comments about the era with which he is most familiar. Following that, the panelists may want to engage in some personal comments among themselves and then we have sitting in the front of the auditorium, a number of distinguished individuals who have made major contributions to the computer field. The Program Committee was not able to arrange to have a dais much longer than this and we didn't feel it was possible to fit everyone into the program, but I'm going to ask as many of them as care to in as much time as we have, to add comments appropriate to the situation. Following that, you the audience in general, will have the opportunity to ask questions of the panel.

When the first digital computer, the Harvard MARK I was demonstrated in 1944, the first electronic digital computer unveiled, ENIAC, in 1945. No one could foresee the magnitude of the computer industry or fully realize the potential influence of the computer. Today, only twenty years later, there are over 45,000 computers installed world-wide in a value of in excess of ten billion dollars. The computing industry is the fastest growing industry in the world, growing at the rate of 20 per cent per year, and is destined to become one of the three giant industries, comparable only to the oil and automotive industries. We're all aware that the computer will significantly alter our entire way of life, the way we think, the way we educate our children, our family life and even the degree of privacy of our personal business transactions. It is difficult to convey the speed at which the computer industry has grown and the swift adoption of its techniques and facilities by society. The progress made in the computer industry in twenty years could be likened to the aeronautic industry, if commercial jet travel had been achieved twenty years after the Wright brothers flew the Kitty hawk in 1903. It is therefore fitting on this twentieth anniversary of ACM's founding, that we look backward to see what happened so long ago and what has happened since and to bring this advanced state of technology into better focus.

The first speaker is Professor George R. Stibitz, Professor in Physiology at Dartmouth Medical School. Professor Stibitz has his Ph.D. from Cornell University and has worked as a research mathematician at Bell Laboratories and as a consultant in Applied

Mathematics and research associate in Physiology for many years. He and Mr. Andrews and Mr. Williams, who has since passed on, were responsible for some of the very early relay computers and I would like to now to ask George Stibitz for his comments.

STIBITZ:

When this sort of an occasion arises, I often think of Ogden Nash's little verse at the termination of the story he writes for Saint Saen's Carnival of the Animals. He says "amid the prehistoric wassail, I caught the eye of one small fossil. He closed his eyes and winked and said, Cheer up sad world, it's rather fun to be extinct." I butchered the poetry, I'm sorry to say.

In a way I don't belong on this program because most of the reminiscences I have go back before the twenty year limit that's been set as the beginning of the wassail. However, the few that I have, seem to me to depend mostly on what people at one time or another have thought was going to happen to the computer. And I might say, as you would expect, that many of the predictions were not quite accurate. I recall that 1937, about October, I built a little binary computer which had two relays and could add 1 and 1, Model K for kitchen table I think. I took it to the Bell Labs to show to some of my colleagues and there was considerable interest. But the completely general agreement among them was that the binary adder would never supplant the decimal machine. In that I think you would recognize that they were perhaps a little in error. A little later we drew up a machine, which was somewhat more elaborate, for handling complex numbers. I drew it myself in some rather original types of convention and it was turned over to Sam Williams who has been mentioned as one of the active founders of the ACM. Sam was then with the Bell Laboratories and he looked the circuits over, managed to decipher the peculiar squiggles that I had used in place of the conventional relay type of drawing. Sam was not a generalizer. He took the circuits that I had given him and actually traced through something like ten or twelve complex multiplications and divisions, step by step, through every one of the relay contacts and wires that I had drawn up and he came up with the conclusion that it would work. So the machine was built and it did work about 1939 in the Bell Laboratories. We had three teletype inputs. I think, as I recall, there was a certain amount of sneaky behavior on the part of the operators trying to get to a teletype and turn it on before the other teletypes could be occupied, because what we had then for timesharing did not allow two people to work at the same time. This computer was demonstrated in 1940 and I think introduced the idea of an automatic computer to Norbert Weiner, who was at a meeting, which happened incidentally to be at Hanover, and he played with the machine and was somewhat impressed with the idea that perhaps an automatic computer could be built and that it might be of some significance in mathematics and in applied mathematics.

So then we went on to a couple of other computers. We were very fortunate in the people who were around the laboratories and available to us at that time. The next time we tried to design a computer, which was about 1941, E. G. Andrews, who has also been active in ACM and is here this afternoon, took over the technical work of designing it and having

it built. This machine had a tape program and it would spend most of its working hours, nights and Sundays, loaded up with enough tape to punch output data and enough paper to produce a wastebasket full of information which we were using in the War program.

Slightly later than that, I think '43, '44, probably '43, we started on a more elaborate computer and this one had the honor of introducing John Von Neumann to the idea of automatic computation, so far as I know. John had come to the Bell Laboratories to see some other developments and dropped in to observe the computer. At that time there were steel stairs going down from one floor to the next and I can remember John coming down with a very elaborate hop, skip and jump pattern, right foot, left, left, right and so on, which I think he was using to check the number of stairs by counting the parity of the number of steps that he took. I have to say that Mr. Andrews and his group were responsible for a great many of the developments in this computer and in the ones that followed it during the war, the early days of the war. I think this just about brings us to the end of the target time that we have. I may mention before I leave the air to others that Von Neumann was somewhat shocked to find that we were using a mixed binary decimal notation, the self-checking or error detecting decimal code. He thought that we should have used the straight binary.

Well there were three reasons we did not. One was that we had found it impractical to train the operators to think in binary notation. The second, we had studied economics of the situation somewhat, decided the mixed system was better, and the third was that the machine was already built. We left it as it was.

AUERBACH:

Thank you Professor Stibitz. I'm next going to ask Richard M. Bloch, Vice President of the Auerbach Corporation, to speak. He is former Vice President of the EDP Division of Honey-well Corporation. Dick graduated from Harvard in 1943 with a degree in mathematics and physics, was immediately inducted into the Navy, and was sent back to Harvard to work with Professor Howard Aiken in the University's Computing Laboratory, where he got involved in programming and in design work in connection with the MARK I. In '46, Bloch left the Navy and joined Harvard's Department of Engineering and Applied Physics, where he was in charge of the MARK I, and later the computers that developed at Harvard.

In '47 he went to Raytheon where he developed the RAYDAC computer series and in '55, if you recall, Raytheon joined with Honeywell to create the Datamatic Corporation and then the operation was acquired by Honeywell. Bloch stayed on to develop the Datamatic 1000, the Honeywell 800 and the 400 series. Dick do you want to comment on these days?

BLOCH:

Thank you, Isaac. The date that sticks in my mind is March, 1944, when I fully prepared to become a navigator aboard a BB and thought that would be a thrilling way to close my career. But the Navy thought otherwise and it turned out that I had met Prof. Aiken, who was a Commander at the time at the Naval Research Laboratory and he told me in late '43 of the wondrous behemoth that was developing under his guidance and I was so impressed that I requested a transfer and it was done before I knew what had happened.

The Harvard MARK I actually was a product of many minds. There were several people from IBM Corporation that were involved, Messrs. C. D. Lake and Bryce worked in conjunction with Howard Aiken in developing this machine. I think one of the interesting aspects of this machine was the fact that even though today we contrast punched card equipment with electronic digital equipment, the fact was that the sequence was quite continuous and in fact, the Harvard MARK I used many components which IBM had developed for their punched card accounting equipment in the late '30's and these devices found their way directly into the MARK I equipment.

I don't know how many of you are aware of some of the characteristics of MARK I, but when people refer to it as being a small machine, they certainly weren't talking about its size physically. It provided a convenient bowling facility along the front. I think it ran easily 40 to 60 feet in length and it had 23 decimal digits of capacity. I was just thinking that actually it had more decimal digits than most machines today have binary in terms of capacity and it had some other interesting features and that is that you could do some rather interesting maneuvers and not have to worry about overflow, especially since there was always another column. All you had to do was begin in the middle somewhere and there was nothing to worry about.

The days were very interesting in that great things could be accomplished which were never accomplished before, and there was no one to dispute just how great these things were, especially since they weren't accomplished before. One of the problems we faced and of course that doesn't exist today, was that of a determination as to which had precedence; hardware or software, and it came to be a stalemate, because as another member of the panel, Grace Hopper here will attest to, I had the nasty habit of making changes to the machine on a daily basis, and I think Grace was, as a matter of fact, the first person that programmed successively different machines on successive days. The only problem was that I worked into the night and then arrived at about eleven. Her hours were eight in the morning, and when she put the program on and it stalled, all hell broke loose, especially if you were well acquainted with Professor Aiken, who didn't look at these things with any great humor and perhaps rightly so.

I think that I could mention the fact that these machines did some astounding work. I would like to recount only one aspect and that was involving one of the great men of mathematics and physics who has been referred to by George Stibitz, John Von Neumann, and the problem he brought to the machine dealt with the solution of partial differential equations in the area of spherical shock waves. This was in late '44 or very early '45, and it wasn't until some time after that certainly I was aware as to just what this

problem was all about. But the most interesting aspect of Von Neumann's collaboration with us was the fact that I saw something which we still have difficulty in accomplishing today and that was human interaction with the machine in quite an optimum way. The machine could only add at the rate of 3 of these 23 digit numbers per second and its multiplication was the order of four seconds, and with speeds of this sort to attempt to solve partial differential equations of these complexities was almost an impossibility, but not with Von Neumann's guidance, because we would put test parameters into the machine, work on a particular grid which was very coarse, and then when he suggested that I make a change in the program I acceded to this, although pointing out it would take a couple of hours to do. He pointed out that we might save two or three weeks of running time and that really it was quite important. I didn't realize how important at the time. And indeed, he was right, as he usually was. I think the attribute of using the computer as a tool rather than as a final authority, as a tool for a bright human to use, was the most outstanding feature of that experience.

I simply want to say that I was asked to say a few words about RAYDAC, which was developed at Raytheon. I think it is most apropos to point out that this came about directly because of Dr. Alexander's great insistence upon bringing the computers to bear in Government activity, and both he and Ed Cannon at the Bureau of Standards, were instrumental in the development of this early computer, which found its way to Point Magoo in 1951 and was used for many years in the data reduction operations for the U. S. Navy. Thank you.

AUERBACH:

Thank you Dick. Before calling on John Von… excuse me. I wish we could. Before calling John Mauchly, I would just like to inject the development of a computer that is not as well known to many of you as it might be. It's a computer called the Atanasoff-Berry Computer. There was a period between 1938 and 1942, when under the direction of Dr. John V. Atanasoff, who was a Professor of Physics and Mathematics at Iowa State College, he with the assistance of Dr. Clifford E. Berry, a graduate student, developed a computer and the characteristics were such that I'm going to take a minute and bore you with them.

This machine had 300 vacuum tubes. It could compute 30 simultaneous algebraic functions with 30 unknowns. The mathematical scheme for solving these equations was the systematic elimination of coefficients by combining pairs of equations linearly. It worked in the binary notation. It used IBM cards in decimal notation for input. The memory was particularly interesting. It was a drum nine inches in diameter, eleven inches long, in which were mounted 1,632 capacitors in 32 tracks with 51 capacitors per track. A blank space was left in each track so that the rotational speed of one revolution a second, the clock pulse was sixty cycles per second. Now on this drum they had brass studs so that they made connection with contacts as the drum rotated and the bit was represented by the polarity of the charge on the capacitor, with the charge being regenerated or being reversed, as called for in the computation cycle, once each revolution.

Now the interesting link between this machine and later work was through John Mauchly, who met Atanasoff and visited him at Iowa State College in 1941.

Another interesting facet of this point in history was that the funding for this was through the Agricultural Experimental Station of Iowa State College, with the hope that the computer would be useful in solving problems pertaining to agriculture, and further the assistance came from a group called Research Corporation of New York. It is interesting to note where some of these early things started--on Stibitz's kitchen table and out solving agricultural problems in Iowa.

John Mauchly is founder and chairman of the Board of Mauchly Associates. He is the co-inventor of the first all-electronic computer, the ENIAC, its successor, BINAC and the first commercial computer, UNIVAC. An interest in new statistical techniques for the analysis of meteorological and geophysical data led John to his efforts in the development of electronic computing devices, since he was convinced that reliable weather forecasting was possible only if he could compute his equations faster. He joined the Moore School of Electrical Engineering of the University of Pennsylvania in 1941 and with J. Presper Eckert, built the first large electronic digital computer, the ENIAC, for the Army Ordnance Ballistics Research Laboratories. The Eckert-Mauchly Corporation was formed in 1947. The company was acquired by Remington-Rand in '50 and…from '53 to '59, Dr. Mauchly headed UNIVAC Applications in the Research Center.

One of the interesting developments of this group was the network method of project analysis known as the critical path method. Dr. Mauchly was one of the founders of ACM and has served as its president. He is also a founder and past president of SIAM, and a fellow of the IEEE, the American Statistical Association and was just recently elected a member of the National Academy of Engineering. Let's hear from Dr. John Mauchly.

MAUCHLY:

In the spirit of the conference statement, "In the Beginning," while we will go into what's past, although that's only prologue, I'll have to say that I did fool with American Cam-Can Adding Machines back in my basement when I was a high school kid. They consisted of cams and gears and things which turned out could be made to work, but not very fast. In a split between physics and engineering, I first chose engineering and then got into the computation of band spectra in physics, which gave me a large and tedious background in what calculation is all about and how it needed improvement.

In turn, I was led into statistics and geophysics and that explains briefly why weather prediction to me appeared a challenging problem to which computation and statistical techniques were highly applicable. However, before I got to the Moore School where things really began to happen, I'll have to confess that I built an analog computer. This is

not widely known. It was a harmonic analyzer for 12 ordnance data and it cut the time of computation down from a manual desk calculator method of 20 minutes down to about two minutes, a great gain.

I also invented a digital computer but a very special kind, for ciphers, which was the first place I know of where the memory or storage features of neon tubes or gas tubes was used for a digital computer. It was also the first place where such a set of gas tubes was used as a function table. Those were what you might call the early days. These were the 1930's we're talking about now.

I became acquainted with more powerful means of computing then existing through Wallace Eckert's book on punched card computation and went with eagerness to the World's Fair in 1939 to see what those punched card companies had to offer. Thorough exploration of the exhibits there convinced me they were great, they were dandy, but they too were slow and were not yet a computing system.

So one other area was an already existing relay computer, which perhaps antedates Dr. Stibitz and that was something called the Tote, used for Parimutual calculations at the racetracks. Strangely enough, it was the Totalizator Company who financed us for a while, between the period when we tried financing ourselves and the time at which Eckert-Mauchly Company was acquired by the Remington-Rand Corporation.

I want to corroborate an incident in history just referred to by George Stibitz too. My interests led me while I was using my little analog harmonic analyzer on some weather problems, to a meeting at which there was remote teletype terminal at a mathematics meeting and while everybody else was absorbed in the colloquium lecture, there were just two people out in the hall looking at that terminal. I was playing with it and putting problems through it and thinking, "gee this is wonderful but I still think it could be speeded up with vacuum tubes." The other man in the hall I didn't know, but a small, sort of interesting fellow who kept telling me that "computers were great and this was the beginning of something that would turn out to be revolutionary. Don't you think I'm right?" said Norbert Weiner. I don't know how long he'd been playing with the machine before. At any rate, those things led me into a lot of work on what could be done with vacuum tubes to speed all this up and in the meantime, having met Atanasoff at a physics meetings where I was reporting on meteorological results, because I didn't think the meteorologists were ready to hear about these things yet, he told me he had a wonderful computer and indeed I did visit the Iowa State College premises at his invitation, and looked at something which at that time was very clever, but wasn't working. And later at the Moore School, where I went because I thought that I would be an engineer again instead of a physicist, I told my new friend, Pres Eckert about this Atanasoff computing machine. So what did we do? We said, "well, for what we think should be done, this isn't the right way". Of course his was a special purpose machine, but we wanted a general purpose machine. That's one thing. But we decided to use the vacuum tubes for the high speed storage elements, as the best available thing, and we went ahead and designed a lot of things which were based on the experience I had in computing. Part of that experience

told me that you had to have a subroutine facility and an often forgotten but very necessary part of the ENIAC which resulted, was called the master programmer. That was the sub-routine facility in all its glory you might say. Strangely enough of course, we were unaware of the work of Aiken or Babbage or a lot of other people who would have been very interested in what we were doing and we would have been very interested in what they were doing. But at any rate, the greatest surprise I had when I did visit Aiken's facility was to find that with all that machinery, there was no facility for sub-routines. Those were installed much later.

Now I've talked about what's influenced us. There's a little bit the other way round. I think it's an interesting part of history, not well known perhaps, that it was, I think, through the efforts of Dr. Von Neumann, who became interested in our work, and Herman Goldstine and Pres Eckert and myself, that the Whirlwind computer became a digital computer. That project started out analog. It was also through Pres Eckert's and my later work on the BINAC in the Eckert-Mauchly Corporation, that a group of people at Northrop were educated and apparently, as I've confirmed, the original lectures I gave at Northrop in the beginning of that contract, steered them away from analog solutions into something which later became the MADDIDA.

Although the ENIAC was built to accommodate the idea of a stored program concept, we weren't able to achieve that until we adopted the gin tank to storage. As Professor Wilkes may not have known, the original delay lines were used in moving target indicators using a mixture of water and anti-freeze, which might have been alcohol, and it was Eckert's suggestion that mercury, matching better the impedance of the quartz crystals driving such tanks, would be an improvement. He was not thinking from the human side obviously or maybe there were better uses for gin. At any rate, with mercury storage tank, we were then able to really make practical the stored program concept.

I would like to finish this by saying that there are a few others little known things such as for instance, that Ed Berkeley, was not only the real instigator of ACM in the beginning, but I think that it was from him that we first heard about, "wouldn't it be nice if we had timesharing". He wanted us to set out immediately to build a computer which could accommodate consoles all over the Prudential Life Insurance Company, such that everybody could be using this computer at once because its speed would accommodate all these consoles. At that time we said "it's technically possible but as yet economically not feasible." Apparently we're now arriving at the point where it is economically feasible.

And finally let me say that I sometimes tell about my interest in computers and weather and then later I get asked, "well what ever came of it?" The answer is "practically nothing as yet." The computers are used in weather calculations. They are used primarily for dynamic calculations. My interest was in the study of the statistical aspects of this for long range forecasting and that remains pretty untouched. The only thing I can add to that is that in 1953, I released work on the influence of the moon on weather. It took until 1963 before anybody else bothered to confirm this effect and the average forecasting

meteorologist doesn't even know today that the moon influences the weather. We did have snow in the UNIVAC once but that was due to a computer influence on the weather, that was the weather influencing the computer. Thank you.

AUERBACH:

Thank you John. We are next going to turn to Dr. Herman H. Goldstine, who is Director of Scientific Development, IBM Data Processing Headquarters in White Plains. Dr. Goldstine was a member of the mathematics faculty of the University of Michigan and the University of Chicago in the late '30's. Came the war and he was inducted into the Army and wound up at Aberdeen Proving Grounds where he became the man who identified the brilliance of the concept of Mauchly and Eckert and enabled them to obtain the funds to begin this project. Finally after the war he became a permanent member of the Institute for Advanced Studies in Princeton, New Jersey, and was there until 1957, when he joined IBM as a member of their Research Planning staff. Dr. Goldstine collaborated with John Von Neumann in the design and development of the IAS computers and I was wondering if he might give us some insight into those days.

GOLDSTINE:

Thank you. I think I'd like to start by telling you that the thing that made the ENIAC and all the subsequent computers possible was the courage, the confidence the farsightedness of a gentleman who is sitting down in the first row, General Leslie Simon. Les, I think you ought to take a bow. It was General Simon, then Colonel or Major, I've forgotten what, in 1942, who was good enough to bring me to Aberdeen Proving Grounds, to give me freedom along with everybody else in the place to work on the things which we felt were important and in this case it happened to computers. That group that General Simon assembled at Aberdeen was a really very remarkable one. Among other things, he had a Scientific Advisory Committee, the likes of which I don't think we're going to see in the near future. Among others, it had a very nice young Hungarian mathematician on it named, as you all know, Johnny Von Neumann and Johnny split his time between Aberdeen and Los Alamos. And it was on one of these occasions--I think it was late '42 or early '43 (apparently in August 1944).

I was waiting on the railroad platform at Aberdeen to catch the train to go up to Philadelphia. Along come Von Neumann with a very abstracted look on his face and he and I are the only two people on the railroad platform. I had met him once before in my life at a meeting at Michigan and I was faced with this big ethical problem. Should I leave this man along with his beautiful thoughts, or should I egotistically go ahead and bust in on him and start to talk? Well you know how it happened. My egotism got the better of me. I went over, interrupted whatever beautiful thoughts he was thinking and he made polite chit-chat and said, "What are you doing?" I told him we were involved in the development of this electronic computer at the University of Pennsylvania. And he immediately got exceedingly interested. I didn't at that time realize why but he spent the

rest of the time of that day and several days subsequent to that, in quizzing me about this machine. That was really the start of Johnny's interest in computers.

His interest in computing--well he had it for very many reasons. He was profoundly interested in economics. He was profoundly interested in hydro-dynamics. He was profoundly interested in combinatorics and he was interested in weather calculations. He was interested in just enormously many things.

At this point his great interest lay really in helping the people at Los Alamos to do by calculation what they were unable to do by experimentation. So this triggered him off to a lifelong interest in the computer. Now he was himself quite a man at computing and I'd like to tell you a little anecdote about that. Once at Aberdeen, a very, very good mathematician showed me a problem that he was working on that happened to involve the parameter N and he couldn't get anywhere with it. He talked to me about it. I couldn't get anywhere with it. So he said, "I think I'll take a computing machine home tonight and work on it and see what the first few cases look like." I said, "fine." Next morning he came in with bags under his eyes, looking terrible. I said, "What's the matter?" He said, "I was up till four-thirty this morning but by gosh I got the first five cases and here they are." He proudly showed them to me.

He had just erased them from the blackboard when in bounced little Johnny from Los Alamos and he said, "What's new?" I said, "Well so and so here has got a problem, such and such is the problem." He looked at it a while and he said, "Well it's very tough to see what to do with this analytically. Let's try it numerically." So he threw back his head and he said, "Now for N equals 1"…and he would talk to himself and you could hear the numbers going as he was talking…about one minute later he said, "17.53." fine. He said, "Now let's see what N equals 2 is like." And after say a minute and a quarter of churning this over with his head up in the air and his mouth hanging open, he said, "26.51." Then he knocked N equals 3 off in say two minutes and my friend was very obviously getting pretty agitated at this point. Von Neumann knocked N equals 3 off, N equals 4 and he came to N equals 5 and this was the thing that had kept this man up from say I don't know, two in the morning till four-thirty. So he was really out for blood and he watched carefully as Von Neumann was going through this mumbling and he go to a number, Von Neumann did, which this chap recognized and so the fellow immediately said, "67.51." Von Neumann's mouth dropped open, the machine kind of ground to a dull halt. He looked down and he said, "What?" This fellow said, "67.51." Von Neumann's head went back again, the calculation went much faster now. Half a minute later he said, "67.51. That's right." This fellow ran out of the room and so I tried to make little idle chatter with Von Neumann just to plague him. He was pacing back and forth across the room madly, jiggling the keys in his pocket. You could hear him saying to himself, "How could that guy have done that?" A little later in the day we told him how it had been done. So his interest in computing was great even before the electronic computer. I think the anecdote serves to show the kind of lovely sense of humor he had.

Another sort of anecdote of that sort occurred when we finished a very big calculation on the ENIAC for Los Alamos and we were exhausted. My wife then got hold of a big carton that used to hold IBM cards and we took the chaf out of the reproducer, you know the holes, the junk that is been cut out of the holds. We carefully dumped all of this till we filled a great big box, sent it out to Los Alamos with the statement, "here are all the results."

Another sort of thing which I think well describes Von Neumann's intellectual interests lay in some of the things that he did very late in his career…well for a long while but they were sort of the last things that he kept working on and this was automata theory. When he got interested in this, he did something which only Johnny would have done. This is the business of self-reproducing machine and he wanted to see and visualize how these would be built. So what does he do but he goes out and buys the biggest Tinker Toy set that money will buy and started producing these cells by means of Tinker Toys until somebody was able to show him that you could do the things in two dimensions instead of three, which was what he thought it had to be done in. So Oscar Morganstern's son got the greatest Tinker Toy present that probably was ever given. I think my time is sort of running on and I probably had better finish up and tell you just by way of contrast an anecdote about Fermi which kind of indicates the difference between Von Neumann and Fermi. Johnny had a joke for everything and during a conversation, however technical, however serious, he was one of these persons who always said, "That reminds me of..." And he was off on a story. Now Fermi learned about the computer from Von Neumann and asked me to come to Chicago to talk with him about this for a couple of days. Well when I got there, he brought me into a little room which looked like the basement of a police station in a tough precinct and sat me at one end of the table, he sat at the other end, and for two days the questions just went back and forth...I mean back and the answers insofar as I could do them...went the other direction, without any humor, without any pause, until he had finally understood everything he wanted to know about it. It was a very interesting contrast between the two people. I think perhaps on that note I ought to end. Thank you.

AUERBACH:

The time has come for one of our 90 second breaks. I would like to just ask if you will stay right in your seats so we can proceed in literally 90 seconds.

Tape #2

AUERBACH:

Our next speakers are two. One, Dr. Ed Cannon, who is Chief of the Applied Mathematics Division of the Bureau of Standards. Dr. Cannon has his Ph.D. in Mathematics from Johns Hopkins and he joined the Bureau of Standards in 1946 and with Sam Alexander, worked on the computer projects about which you've heard so

much. He has been very much involved in numerical analysis and computational techniques and is one of the big users of computers at the Bureau of Standards.

He will be immediately followed by Dr. Samuel N. Alexander, who is now Senior Research Fellow in Engineering Sciences at the National Bureau of Standards, which is one of the highest levels that an individual can attain at the Bureau, and Sam is one of the youngest men to have been so elevated. You are all already familiar with Dr. Alexander's contribution to the field and so rather than repeat them, let me turn first to Dr. Ed Cannon.

CANNON:

It does seem appropriate because of our early and close association continuing over several years, for us to be introduced as a pair but I'm going to take advantage of my spot on the program and this bit of open space on my right, Sam, in putting you on the spot. Because I'm going to say what I please and leave you the task of making up for its differences which I'm sure you can and will. I'm going to say very few words about the very beginning of the National Bureau of Standards Computer Program, in keeping with the theme of this conference.

By the mid-1940's, with the support of the Office of Naval Research, under the able representation of Dr. Mina Rees, who is here, the Bureau had extensively planned what was called a National Applied Mathematics Laboratory which evolved into the present Applied Mathematics Division. And ONR had become interested in supporting the establishment by the Bureau of the Federal Computing Center and it was soon to support a Bureau Computer Procurement Program. Now you came to the Bureau at about that time and I think I followed a few months later. There was this juxtaposition of events which was rather unusual and significant, I think. Your coming to the Bureau, and before you were lost in a permanent assignment, you were available for a new and demanding task. The Science Committee or Council of the Department of Commerce asked the National Bureau of Standards, well, to tell them and the Bureau of the Census, if this computer that the young men Eckert and Mauchly were talking about was worth anything, could be usefully employed by the Bureau of the Census and should be procured. You received the assignment and participated in that which led to a pleasant association with Eckert and Mauchly and procurement of UNIVAC's, and you of course were involved there. Now I think this may have preceded, I don't know, you think it did or think it did not, but Bob Mantoon who was there very much on the scene then and was our boss during this computer effort, thinks the timing was reversed. But I think a bit earlier than this work in connection with the Census computing interest which came the way of the Bureau, Army Ordnance had a little money left over from the ENIAC, which John and Pres and/or the Moore School of Engineering...oh correction...they had some money in connection with the Fuse Program, which could be made available for computer development and they came to the Bureau and they asked if the Bureau would be interested in a contract for components development, the first assigned pass being to work with the School of Advanced Studies on the MANIAC to develop input and output

equipment. This also led to the same kind of work development for the EDVAC Program at the Moore School of Engineering. And again you were available. The other assignment didn't tie you down enough so that you couldn't be also useful in this Bureau program and it was decided to accept this undertaking.

Now I think it was the combination of the two interests of the Bureau, the one epitomized in the establishment of an Institute for Numerical Analysis under Office of Naval Research sponsorship and the effort along the line of development of computer techniques and for the representation of the user viewpoint, the combination of this with the hardware development or with the computer development which lent strength and effectiveness to our program and this is all I want to say by way of introduction to you.

AUERBACH:

Sam.

ALEXANDER:

I'd like to pick up Ed's narrative a bit. Certainly the Census Bureau assignment was one that to put mildly, shook me, realizing that the assignment was for me to help advise the only people that I knew that knew anything about the business -- Eckert and Mauchly. Fortunately, they shared their knowledge. In the summer of '46, the University of Pennsylvania sponsored an intensive summer study at which some of the pioneers of this new technology attended. Eckert and Mauchly and their associates at the Moore School gave many fine lectures. Herman Goldstine gave lectures. Indeed, everybody that had been associated in any way with Government-supported computer work were brought together to lecture to the neophytes of that day. I had the honor of attending those lectures and getting a start so that I would not be an ignorant uncooperative collaborator with the people in this program. At that time I think I remember meeting Maurice Wilkes who attended for a short period. Isn't that correct Maurice? And many people in the audience I believe either were partly in attendance or worked with the men who did attend. It was a contribution from Eckert and Mauchly that many people don't fully appreciate. It really gave the field a tremendous spring, a tremendous boost.

Another little bit of history from my memory. I believe that the BINAC, in the laboratory, was probably the first internally sequenced machine that operated. Is this probably correct John?

MAUCHLY:

As far as I know it is, yes.

ALEXANDER:

However, it was an incubator baby. It never really did the job that it was intended to do as far as I recall, and certainly never served as a basic computing instrument for anyone. The first machine in the world that turned out what I considered useful output was Professor Wilkes' machine in Cambridge, and I remember painfully one day when John Curtis came in with some printout that had been mailed to him, listing a short tabulation on part of the Airy function generated by the machine, and in effect said, "you're not going to be first with the SEAC", the then to be SEAC, he said, "but damn it, be second."

The Census Bureau experience also had some interesting sidelights. The Census Bureau had the enormous sum of $300,000.00 to spend on sponsoring the initial development and the building of an operational machine along the lines of the unsolicited proposal for an electronic statistical machine. Do you remember that proposal John? Many people were so skeptical that they wanted to ring this investment with all sorts of protections and one of them was a horrible one for us. It, in effect, said, "you at the National Bureau of Standards create that kind of a technical resource so that if Eckert and Mauchly go broke and the Government has made progress payments to Eckert and Mauchly, you will claim all of the equipment owned by the Government and you at the NBS Laboratories will finish it for the Census Bureau."

Well this had its salutatory effects. It certainly was a fear-induced inspiration to work hard and to study the work being done by all of the other projects supported in the Unites States. We were able to be tolerated and in some places welcomed, to peer over the shoulders and to visit with and get all of the advance reports of these organizations. This is what I meant when I said we stood on the shoulders of many, many people. Harry Huskey joined the Bureau with a head full of knowledge of what was going on at the National Physical Laboratories and brought this into our environment and when SEAC was finally put together, it was certainly a mongrel dog. It has more intellectual parents than probably any device that was ever put together.

SEAC came to be for a reason that now looking back, seems kind of humorous. We were to acquire...the Bureau of Standards was to represent the Government in acquiring three UNIVAC's -- one for the Census, which was considered a horrible gamble, but because the Bureau of Standards was willing to contract for a UNIVAC, it automatically became a safe investment and the Air Force wanted us to get one for them and the Army Map Service wanted us to get one for them and I'm sure they gave as reasons to their management that it was not an undue risk because the Bureau of Standards had decided it was worth investing in.

Well we were really holding what we thought was a bomb with a tremendous fuse on it, with a short fuse on it, I'm sorry. In addition, under Mina Rees's program, we had to find another source of supply as a means of protecting against possible failure of the fledging Eckert Mauchly Company and we solicited all of the major electronic firms, including IBM, and got not very much encouragement. In fact, we were told by one organization that if such a thing were possible, they would have been doing it on their own money and turned it down. We finally wrote a contract for experimental component development

preceding a machine design with the Raytheon Company. There was a system design and component design. Ed and I were twins. Ed had the problem of matching the machines characteristics to the expected computational needs of the NBS and I had the responsibility of trying to keep track of what they were spending our money on with respect to component developments and it might be of interest that one of the major reasons the Raytheon Company offered a proposal and we accepted is the competence of the Radar Department of the Raytheon Company with advanced forms of moving target indicator equipment that used mercury acoustic delay lines. This was the major resource they had to offer besides the talents of Campbell and Bloch, who were recently arrived from the Harvard University Laboratories. Thus we had this combination. Plenty to do, plenty to worry about and then it became clear that both Eckert-Mauchly and Raytheon were overextending themselves with respect to what could be accomplished in the time period that was originally set and we had the growing spectra of what could we do for some kind of interim facility that would keep the wheels going on the many programs that were dependent on these machines when they came in. And so Harry Huskey pushed us. He suggested some kind of interim machine be built out of the Computer Components Development Group with some of the ideas from the National Physical Laboratory and that we get some kind of interim facility that would keep the Air Force happy, that would let the Census begin experimenting, that would let the Bureau begin operating and may I thank him for the shove. It worked. We got something going and then when we began to realize that the machines were going to be even later than we first predicted, we were authorized by the Air Force to expand it into a full-fledged machine with full facilities for general purpose computation, and that is the peculiar checkered way in which SEAC came to be. It had many sources, much help. It later became not only a productive tool but an engineering evaluative tool, because before it was finished, it was expanded so that it had an operating acoustic memory, it had an experimental Williams tube storage memory and the day we felt we had really arrived was when the switch on the panel which allowed you to put the problem into the acoustic or put it into the Williams tubes, began to always go to the Williams tubes when the operators sat down to run. When it operated well enough, they would risk their problem the slow, sure acoustics vs. the fast but occasionally erratic Williams tube began to win. With this we got enough experience so that soon both sides were reasonably well in balance and people began to get the courage to program for the enormous amount of memory -- one thousand words of memory. Those days seem fresh and green as I say them now, but they were 1952 '53. The rest of it you know. I think my time is probably up. Such it was in the beginning in the Government. Thank you.

AUERBACH:

Thank you, Sam. You've now heard from our second source of financing that enabled the Government to move into the computer field in the early days. It was a major source as most of the people who were there at the time will well remember. I'd like to turn next to Dr. Maurice Wilkes. Dr. Wilkes is the Director of the Cambridge University Mathematical Laboratory and Professor of Computer Technology at Cambridge University in Cambridge, England. Dr. Wilkes joined this laboratory immediately after

the Second World War and has been involved in computer technology as the Director of the Laboratory ever since. He is widely acknowledged as one of the leaders in the computer field in Great Britain. He was the first President of the British Computer Society. He was the first representative of England to UNESCO, which organized the first International Conference and was a co-founder of the International Federation for Information Processing. Professor Wilkes had a very lengthy introduction when he was introduced as the Turing lecturer at the early morning session yesterday and rather than go on, let me turn to Maurice Wilkes.

WILKES:

Thank you. It was in Philadelphia just after that famous and influential course at the Moore School, that I first began to think in detail about the design of the machine that we later called the EDSAC. We started work in earnest on this project at the beginning of 1947, and I remember now some of the landmarks in the development. I suppose they were very similar landmarks to those that other groups, doing the same thing, were reaching, and I was comparing notes yesterday with our Chairman, Ike Auerbach, about some of the early experiences with mercury delay lines.

I remember when we first got pulses circulating rather precariously. Everytime we switched on the light in the room, another pulse went in and that was the only input that in fact we had. Then later we made a half adder work and saw our binary pulses counting up on an oscilloscope screen. Then we got the accumulator working and then the whole arithmetic unit and so on.

We were a small, informal group in those days and whenever we had anything to celebrate, we adjourned to the local pub. It was a pub well known to generations of workers in the Cambridge University Laboratories. It was known as the Bun Shop, though I think they'd be very surprised indeed if you tried to buy a bun there.

We had a special celebration on the 6th of May, 1949, when the EDSAC did its first calculation, which was a table of squares and a few days later it did a table of primes.

Sam Alexander has reminded me about the first substantial program I wrote which was for computing Airy functions by solving the Airy differential equation and in connection with that, I made a discovery. I discovered debugging. Now you probably won't believe me and I don't know whether my colleagues around this table had the same experience, but it just had not occurred to me that there was going to be any difficulty about getting programs working. And it was with somewhat of a shock that I realized that for the rest of my life I was going to spend a good deal of my time finding mistakes that I had made myself in programs.

There were other groups working in England at that time. In particular there was a group very similar to ours at Manchester University and we had a very friendly race with them over getting the first computer to work. I think they gave various demonstrations of an

incomplete machine but I think we just beat them to it in getting the whole machine going. But as the Duke of Wellington said, after the Battle of Waterloo, "It was a damned nice thing."

The Manchester specialty was the cathode ray tube memory which F. C. Williams had worked on at TRE, which was the big government radar establishment where he in common with many other people including myself, worked during the war. He stayed on for a short time after the war and began to work on this device and took it with him to Manchester University. It was an entirely original contribution and made their machine particularly interesting. With Dr. Kilburn, Williams used this memory in a serial mode. Later it was developed for use in a parallel mode and Sam Alexander has already referred to it. I think really it just about saved the situation because a number of parallel machines were being built. There was the Princeton machine and a number of copies and various other machines. There were all sorts of hopes about a parallel memory, fast memory, materializing but somehow they never did and the parallel Williams tube memory held the fort both on the Princeton machines and for example, on the IBM 701 until the efforts of Jay Forrester and his group, led to the coming of the ferrite memory.

Another group in England was at the National Physical Laboratory, where Turing was for a time until he moved rather early to Manchester University. You already heard it mentioned that Harry Huskey was a member of that group for a time. I had hoped he might come on and help us but it appears to be a good thing perhaps that he did come home and help the Bureau of Standards instead.

The NPL group built a machine called the Pilot Ace which came into action somewhat after our machine and the Manchester machine. To complete the picture of the work that was going on in England at that time, I ought to mention Dr. A. D. Booth's work at Burkbeck College. He was interested in relay computers at that time, though he later of course moved to electronic computers and his work I think was particularly interesting as representing the same sort of work as Dr. Stibitz had been doing for a long time at Bell Labs. (End of side 1 of copy of tape in Project holdings.)

AUERBACH:

Thank you, Dr. Wilkes. Our next speaker is Dr. Grace Murray Hopper who has got three jobs at the present time. She is staff scientist for System Programming at the UNIVAC Division of Sperry-Rand. She is Associate Professor of Electrical Engineering at the Moore School of Electrical Engineering at the University of Pennsylvania and she is recently retired as a Commander in the Naval Reserve but has gone back to active duty in the Office of the Special Assistant to the Secretary of the Navy. Dr. Hopper was an instructor and Associate Professor of Mathematics at Vassar College before going into the Navy and wound up at Harvard University where she learned to program the first large scale computer, MARK I. She later joined the Harvard faculty as a Research Fellow and worked on the MARK II and MARK III computers there. She joined the Eckert-Mauchly Computer Corporation in 1949, and continued there as a senior programmer and

her work in the field of programming, compilers and active work in this entire field of software is sufficiently well known that I literally should just say Dr. Hopper.

HOPPER:

Thank you Ike. I guess I'm here as the representative of the something better than fifty per cent minority of the citizens of the United States. I've also come complete circle because I got into the computer business by being ordered straight from Midshipman School to the Harvard computer, to be greeted by a large and appalling Commander known as Howard Aiken, and to be three hours after I arrived there, instructed in the art of programming the MARK I computer by a very tall and definite young Ensign known as Richard Bloch. He frustrated me quite regularly because even then I wanted to keep my software and use it over again. I didn't want to keep reprogramming things. But unfortunately, every time I got a program running, he'd get in there at night and change the circuits in the computer and the next morning the program would not run. What's more, he was home asleep and couldn't tell me what he had done to the computer. We found ourselves continuing one of our debates at lunch time today. They were frequent in the old days. Today we started to finish one we've started twenty-three years ago. A few elements were missing. Bob Campbell wasn't there to try to arbitrate. Ed Berkeley was at the next table instead of noting down what we were saying and date stamping it. And I very much missed Howard Aiken because he would have been there domineering over all and saying, "well are we making any numbers?" I am not sure he was ever happy with one of the best demonstration routines which was ever written. It was written by Dick Bloch. It exercised everything that showed on the computer. All the lights flashed. The typewriters ran. The card feeds fed and the cards punched and all the interpolator units moved, but it didn't actually do any computing at all. It was always used for admirals and generals. I must be polite to him though, he's the only person I have ever known in my life in computers who actually wrote a complete program in ink and never made an erasure and it ran the first time.

My primary interest throughout has been, not in making programs, not in designing languages, not in running computers, but rather in making it easier for people to use computers. And it was that led me into the whole field of trying to store libraries of sub-routines and here I had acquired the first hints from Dr. Wilkes in the first book on programming and in Howard Aiken's desire for a library of routines and then when I got to Eckert-Mauchly, in John Mauchly's great encouragement that we try to use the computer to help us make our programs.

It was loads of fun in those days but the frustration continued because we insulted the programmers when we treated their programs as if they were data and let the computer manipulate them. They resented it. It was certainly against natural operation. We went to languages to try and simplify things and the mathematicians objected when we let the poor data processing people say add instead of writing a plus sign. Thank goodness we did have help all the way along the line and I can't name you all. Many of you are here, the people that helped, that listened to the arguments, that scolded, and advised and

counseled. But when it came to trying to teach a computer to understand English instructions, I shall be forever grateful to Albert E. Smith of the Navy Department, who gave us the chance and listened to us and let us try it out.

All the way through, after I had learned to program from Ensign Bloch, when I got to Eckert-Mauchly, I was taught a further advance on the art, I learned to flow chart and that was one of the finest things that ever happened to me and it was Betty Holberton who taught me how to flow chart and how to really think logically about programs. There were many stories I could tell -- funny events -- and I'll have to put in one because I always loved it. We ran BINAC day and night because it was going away and we wanted to make the best possible use of it we could and I have a vivid picture in my mind of coming in early one morning -- I had gone home to sleep that night -- and finding the BINAC surrounded by Coke bottles and sitting at it, slightly unshaven, John Mauchly, and both John Mauchly and BINAC singing "Merrily We Roll Along." I'm glad we had the chance to do all the different kinds of programming. I of course am ever grateful to UNIVAC because they gave me the people and the machines to work with to make it possible. I don't want to see the development of language stop. I think we all speak in different dialects. I discovered that way back in 1944. The geologist and the biomedicine men did not speak the same language. They each had their own techniques and their own languages and I think they each still need their own volumes and their own languages and I don't really want to put the whole encyclopedia in my computer. I'd rather take down the volume I need. I don't use my cookbook to do differential equations, at least not often and not very well and I would plead for more languages and more experimenting with languages because you see, I've always heard that the computer industry is on an exponential curve. You know, one of these that goes up suddenly. Most people seem to think we're up here but I honestly believe we are right down here and we are only seeing the beginning of what's going to happen in the next years in this computer industry. We're only at the start.

I think I must say one other thing. I am deeply grateful to the Navy that ordered me to MARK I and gave me the privilege of starting in all of this exciting area of computers, of software and of all that's still going to happen. I have come complete circle because I again have the privilege of serving in the Navy and the privilege of wearing this uniform. So I'm thankful to many people and I must add that on. I think the most exciting part of knowing the computers and my favorites are MARK I, UNIVAC I and the 1108. But I think over and beyond that and over and beyond the hardware, which has now become almost a bunch of big gray boxes which are very pretty, and I wish they'd paint some of them red, blue and yellow. I think fifty percent and more of the adventures of these years has been knowing the people there have been in the computer industry. And I think sometimes when we look at our accomplishment, we look only at the hardware which runs and I think we should look at this audience and I can look at the generations of programmers that I have trained. I think it is down to about six or seven generations now. We've grown a whole group of people, wonderful people and they are perhaps a far greater asset than the hardware. Thank you, Ike.

AUERBACH:

Another indication of the great esteem we hold you in, Grace, the outstanding contributions you've made repeatedly year after year in this field. Our next speaker is Professor J. W. Forrester, who is Professor of Management, the Massachusetts Institute of Technology. Dr. Forrester went to MIT in 1939 and worked at the Servo Laboratory during the war and was moved into the Digital Computer Laboratory in 1946, as its new director, and continued there through 1951, responsible for the design and construction of Whirlwind I. He then moved on to the Lincoln Laboratories as the head of the Digital Computer Division and served from 1952 to 1956, guiding the Military Operational Planning and Technical Design of the Air Force SAGE System for Air Defense. Dr. Forrester holds many basic patents, one that he is well known for is the random access coincident current magnetic store.

He is a fellow in the IEEE and was recently elected a member of the National Academy of Engineering. Dr. Forrester.

FORRESTER:

As you have seen from the earlier talks, the way in which one gets into computers in the earlier days was often devious and unexpected. As Mauchly did, my entry was by way of analog computers. What came to be the Whirlwind Project began in 1944 for the construction of an analog computer to study the stability of large aircraft. This gradually evolved through both application, purpose and technology over into the area of digital computers, aimed at the Combat Information Center, the handling of information in military control situations. So I think it is fair to say that the Whirlwind computer was the first digital computer excursion in a serious way into command and control systems, the computer itself having been designed and built in the general period of 1946 to '50 and its use as an experimental Command Center analyzing radar data, controlling aircraft in the period from 1951 to '56. Possibly it holds some kind of distinction as being, as far as I know, the only computer of this period that in fact is still operating. It is in the hands of Wolf Research and Development Corporation, where it, I think, still has some use.

The beginning of the work came at a time when the ENIAC was nearly complete, right on the verge of running. The Harvard MARK I was running. The work of Goldstine and Von Neumann in general purpose computers was beginning to be discussed. We came into that picture at a time when some of the early ideas in the field with respect to logic and binary arithmetic had been pretty well thought through by a number of people. Picking these up, we aimed at the higher speeds and higher reliability necessary in this area of command and control systems. The detailed block diagrams of the Whirlwind computer, done primarily by Mr. Robert Everett, who is now Vice President of MITRE, published and rather widely circulated in 1947, had I think some traceable influence on machines that runs down even to some of those today.

The machine operated in the 20,000 operation per second range which was a fairly high point for that period. Perhaps the really unique part of this whole exercise was the beginning in 1951 of first computer controlled aircraft interceptions. This involved systems that took radar data automatically from radar sets, transmitted this over telephone lines, analyzed it to generate aircraft tracks, generated control orders that were then automatically transmitted to the auto-pilot of interceptor planes. So that the beginning of on line control systems with computers began around the 1950-51 era. This involved interactive computer use with human beings able to intervene in what was going on, the use of the cathode ray oscilloscope as a display for what was going on, the use of a light pen to give the operator a chance to intervene and redirect what was in process.

I think I would like to particularly mention here a man that I don't see involved in the program for this convention, that's Mr. Perry Crawford, who had a great deal to do with our own work. He is the one who first called my personal attention to the existence of the early work in digital computers, and he was the technical officer and the administrative power that coupled the computer and the whole field of Military Command and Control System for a period from about 1946 to '49. He was one of the pioneers in the Government in the Navy in expressing this interest in command and control systems and had a great deal to do with the launching of the Whirlwind computer. Probably without him, it's fair to say there would not have been a Whirlwind Project as it came to exist.

In the area of devices and techniques, I suppose the outstanding one has already been mentioned, the magnetic core storage, which was developed in the period from '49 to '53, went into the memory test computer, a full scale digital computer of about the same characteristics as Whirlwind, which was used as a test vehicle for the full scale testing of storage in the spring of '53, and the transfer of that to replace the particular beam deflection storage tubes that we had been using in the summer of '53, those being, I think, the two full scale computer applications of the coincident current storage.

Other things -- Dudley Buck's work on the cryotron that traces down through a great deal of effort coming into the cryotron work of today, was never in fact used in the Whirlwind computer, but was launched by Buck whom many of you know about.

A number of interesting procedures first...... well, I'm not sure that they are first, but were used in the Whirlwind computer. The marginal checking system was very extensively developed there, a system that was under the control of the computer program for checking the possibility of circuits being near the margin of operation, which at the same time running trouble identification, trouble detection problems for isolating particular weak spots in the computer. This, coupled with the development of the 7AK7 tube jointly with the Sylvania Company, brought vacuum tubes up to a reliability level that I think had not been known previously. Vacuum tubes in the late '40's were still showing the imprint of the entertainment industry. Very little was known over the 500-hour period. When vacuum tubes reached the point of failures in service of a tenth of a per cent per thousand hours, this represented almost a thousand-fold improvement in their reliability, something that happened in the computer industry, because of it, bringing them up to the

point where for some time semi-conductors had their task cut out to match the reliability. I presume now they are substantially better. I think I will stop there.

AUERBACH:

Thank you Jay. May I turn next to Dr. Arnold A. Cohen, who is Staff Director of the System Technology at UNIVAC in Minneapolis. Arnold entered the digital computer field in 1946, when he joined the newly formed Engineering Research Associates, Inc., which later became a part of the UNIVAC Division.

Dr. Cohen has held various positions at UNIVAC in Engineering Management and in Systems Development. Among his early responsibilities were developments of certain basic magnetic drum storage techniques and systems design of UNIVAC 1101 and 1103 computers. Dr. Cohen is a Fellow of the IEEE and has been well known for his contributions in many of the technical societies of our profession. Arnold.

COHEN:

Thank you Ike. I feel that I'm in a very interesting and somewhat pleasant position here today. As one grows older, it is less and less frequent that one finds oneself in the position of being the junior member of a group. As you notice from the biographical note, I entered the computer business in 1946, actually at the very end of 1946, and all of the people here and I believe all through the front rows, were well entrenched in the business before I even arrived there and I might say that everyone here in one way or another has been a teacher of mine through the papers they have presented, they've written, the laboratories I have visited, and for which I and I suppose most of the people in this room are grateful.

An interesting phenomenon about the end of 1946, early 1947, was that even though the war was pretty definitely over by that time, it took a long time...sort of a sticky phenomenon there, of the classifications being removed from much of the development work that had gone on during war time. As a result, there was sort of an air of mystery about just exactly where things stood. What is the state of development of this or that thing? I had come from RCA, where I was engaged in the development of electron tubes, gas tubes to be specific, so I had to take the word of many people as to just what targets we ought to aim at, and in the matter of determining which new storage techniques would be worth developing, there was a great deal of interest in applying or looking at the possibility of storing information on beams of electrons or other charged particles. And I was told by somebody who had access to certain classified information that flip/flop corresponding to a kilomegacycle rate had been pretty well achieved. Incidentally, Kilomegacycle that was before gigahertz was invented, and therefore a good target would be to store a thousand bits and that term hadn't been invented yet, it was a thousand binary digits, on a beam of delay period one microsecond.

Well, I spent the first few weeks going through a very careful analysis of all of the physical limitations and aspects of how one would get at this.

Fortunately, we set the project aside and turned to something a little more productive at the time. I think you'll note that we are almost at the gigahertz computing rate in the business twenty years later. Also there probably isn't much need for serial type storage devices because the semi-conductor business happily has developed to the point where logical components are cheaper and faster and it is possible to do a whale of a lot more in parallel than anybody had dreamed of doing back in those days of the expensive and less reliable vacuum tubes.

A little specific history......we out there in Minnesota were groping our way through how one might achieve certain results and with the encouragement of our sponsors in the Navy Department, we had a contract in which there were a number of tasks, many classified jobs. One particular task that was assigned about, I recall, August, 1947, was essentially to really produce a general purpose computer. This was parallel as it developed, we spent a great deal of time working with the customers' people in arriving at the specifications, and we decided in spite of the fact that we didn't have a high speed memory, we would go the parallel route and we developed a 24 bit parallel machine. This was later called the 1101, using magnetic drum parallel storage. We had done earlier development work on the magnetic drum storage techniques that laid the groundwork for this particular development. Looking back on it, perhaps it was a little remarkable that we delivered this machine. Actually we got the go ahead on the construction of the hardware about the spring of 1948. That first prototype of the 1101 was delivered to the customer agency in December of 1950, with all of the doubting Thomases standing around and watching our engineers during the installation process, laying bets on whether these fellows would get home for Christmas or even, I suppose, for the Fourth of July of the following year. But as things worked out, it was put together and plugged in. It ran. The boys stayed there for a week or ten days and got home for Christmas. The machine operated, and for an early machine of that kind, a period of less than three years, it was perhaps in retrospect rather remarkable.

And this brings out a point that I think perhaps should be made on behalf of the commercial types present, that there's a special achievement there whose satisfaction isn't always enjoyed by people working in government laboratories or in university laboratories, that there's an extra step involved, that step of delivering operating equipment to the premises of a customer and leaving it in his hands, turning around, going home and expecting to have it continue to operate. This is something that every commercial company does over and over again. Sometimes it does not succeed. Everybody has gone through the embarrassing experience of an occasional lapse in this type of thing. But by contrast of course, and I hope I'm not offending anyone, in these other situations where delivery is not necessary, I suppose the date at which the computer is declared complete or operational might be a little bit more arbitrary.

In the early work, I would like to digress for a moment to indicate something about the people in those early days. I'm sure you would recognize a number of them who were associated with ERA back in those times, Tommy Tompkins, John Coombs, Sid Rubins, Bill Kye, Jack Hill, George Hardenberg who long since has left the computer field and is in politics. He's a County Commissioner out in Ramsey County, Minnesota. Frank Mullaney, Arnie Hendrickson and some of our dear friends, the customer people who kept us on the track, Howie Campaign, Joe Eachus, Jim Pendergras, and of course many others. Jay mentioned certain early concepts that were quite durable. I think I'd like to pay tribute to ferromagnetism in general as being an extremely durable physical concept. If you are looking for the storage of information in a two state form, if you wish to have the information alterable and yet non-volatile, permanently available without power, over power off periods, and selectively addressable and so forth, I think we've gone through tapes, drums, cores, films, plated wire and so forth and ferromagnetism, I suspect, is going to be here for a long time to come.

AUERBACH:

Thank you Arnold. You couldn't have timed that one more perfectly because we are now up for our second 90 second breather, after which we would like to call upon some people who are sitting in the front to make some comments, as well as some exchanges right from the panel itself.

Tape #3

AUERBACH:

Before digging into this next session, I'd like to ask that all those people who attended the first ACM Conference twenty years ago first stand up and then later come to the front. We would like to get a picture of them. Will all those who were at the ACM Conference......good. What I'd like them to do with a number of the people sitting in the front to wait for just a minute at the end of this session because I have been advised by our General Chairman that he would like to get some photographs of this group.

May I first ask if the panel wants to get involved in any anecdotes or debates. Sam do you want to comment?

ALEXANDER:

I'd like to add a little bit to the record. We, in our early days, were negotiating with ERA as a possible source for one of the first machines and the difficulty we ran into was one you probably all recognize. In order to build a machine that they could guarantee to deliver, a most admirable attitude, was to build it with the central memory as a magnetic drum. We wanted more performance than that and the study contract which they did for us, trying to develop optimum programming procedures on a magnetic drum, was just not

convincing enough for us to abandon the glean and go of an acoustic delay line memory, which we never got.

AUERBACH:

Jay do you want to comment?

FORRESTER:

Herman Goldstine has mentioned the very broad interests of JohnVon Neumann, but you may not realize how very, very broad these were. As an extremely junior member in computing many years, ago, I found myself seated at the end of a table about this wide at lunch and on one side was John Von Neumann and on the other side was Norbert Weiner and they totally ignored me but they spent the lunch talking to each other and the entire lunch discussing how you would translate modern tabloid headlines into four letter renaissance English words.

AUERBACH:

Herman Goldstine.

GOLDSTINE:

As long as we're doing this sort of reminiscing, let me tell you another Von Neumann story because I think you might enjoy it. He told me this very late in his life at the Institute and it had to do with formal logics, which had been one of his original loves. In fact, he had tried to prove the opposite of the Godel theorem at the same time Godel was trying to prove his theorem. Now the story has a nice sort of Biblical touch because Von Neumann told me that he worked on this problem one evening and he went to bed, and as was his wont dreamed he had the proof. And got up and, this is true, I know he did work this way. He got up in the middle of the night, went to his desk and wrote down the proof and got almost to the end, but not quite, got stuck, turned out the light and went back to bed.

The next day he worked on it again, went to bed, dreamed again how to carry out the proof to the completion. He got up, turned on the light, went to his desk and wrote and got to about epsilon from the conclusion. And he said to me, "You know, Herman, what a wonderful thing it is for mathematics that I didn't dream the third night."

AUERBACH:

Anyone else on the panel want to add a comment?

GOLDSTINE:

Well let me tell you one if I may about Turing, who came over in the early days at the Institute to work with us for awhile. Sam, you mentioned about the reliable old acoustic tanks. Well Alan had a very nice proof which showed us that the tanks couldn't possibly work and convinced us. Of course they did.

AUERBACH:

I'm very glad you brought that out Herman. I made this comment yesterday but I had nobody else who would collaborate that comment. Thank you. Jay.

FORRESTER:

Another similar episode in the early days of trying to develop the ferrite cores, we sent an inquiry to Philips-Einhoven people in Holland who had done a lot of the early work in ferrites for transformer cores, and were told that there was sufficient known in the theory to know that they could not be made rectangular.

AUERBACH:

Grace Hopper.

HOPPER:

One reason, maybe, that information didn't travel so rapidly during World War II was that MARK I was operating for the Navy and ENIAC was operating for the Army and we were all very classified and the Army didn't talk to the Navy very much.

It may have been caused by an event that happened up at MARK I. We found the back door to the Army storeroom and we were very short of graph paper and things like that and the yeoman and I went in to borrow some graph paper. And Aiken loomed above us and said, "What are you two doing?" And we admitted that we were liberating some graph paper. We had started to take an entire carton and he looked at us very sadly and said, "I'll admit that maybe the Army can't count, but they can tell the difference between some and none. Please leave one package."

ALEXANDER:

I have one that I think might be worthy of a few moments. One of our goals in building SEAC was to use as few cathodes as possible. We were convinced, even though Jay Forrester was working very hard with Sylvania on reliable cathodes, our experience had indicated to us that the cathode of the tubes we could get was certainly the weakest link in the system. So we went to all kinds of trouble to use as few vacuum tubes in the design of SEAC as possible and put our reliance on a thing that most people considered absolutely horrendous -- the germanium diode. We build SEAC with as many germanium diodes as possible and as few vacuum tube cathodes as possible.

In the course of attempting to follow the philosophy, one of the designers fell upon the observation that sometimes it paid to build the circuits using the dual notation, making a pulse equal 0 and a space equal 1, and designed the arithmetic unti(?) that way because he was able to save a few diodes. This unit was built and wad demonstrated and as you can imagine, it was the source of great difficulty in explaining how the arithmetic unit worked, particularly to those members of the bureaucracy, both military and civilian, that came by to see how their money was being spent.

Fortunately, one of the fellows was perceptive enough to have determined the fact that the full binary adder is its own dual and therefore all of this nonsense need not be resorted to. The explanation of the operation is the same, whether it is 1's and 0's and 1's, and we hastily pulled down all the complicated diagrams around the demonstration unit and explained it in a normal way, thoroughly embarrassed that a little bit of analysis was not done earlier in the game.

AUERBACH:

Let me turn to some of the people who are sitting up in the front who go back, at least many of them, to the days that the panel members represent and ask if any of them would care to make some comments. Mr. Andrews. This is Mr. E. G. Andrews who was referred to earlier, whose work at the Bell Telephone Laboratories on the early MARK computers is of historic value.

ANDREWS:

I will come up here and say a few words after all the tributes that George Stibitz uttered. They are not altogether deserved because I got into this job like some of the others, but even more accidentally. I had been working with a group on a radar trainer and the Army cancelled it one morning at eleven o'clock and at eleven-ten I was called in and they said, "George Stibitz has got a job for you." I was introduced to computers at eleven-ten that morning and I really didn't know beans about the darn thing. So I really had to sit at his knees to understand how to add binary numbers and quasi-binary numbers, but it was a very instructive course and I must say that my schooling started with the patience of George Stibitz.

On the other hand, my contribution is again trivial because it covers only a part of the span of computer-life developments that the Bell Laboratories has participated in for quite a few years and I will run through and give you a tour in time, so to speak, on some of these, using nomenclature that you are familiar with but not the nomenclature that they were using in those days. In 1919, the value of what was then referred to as common control switching came into being and a principal element was what we would call a computer today but it was then called a sender. This sender did certain code conversions, it controlled switching, it had cyclic access or table look-up, and a short time later it used binary codes. I should say the flip-flop came in there too.

Along about 1930 they came up with a development which provided for direct random access to memory, which was, as you would know in the computer field, was quite an advance. Later on even more sophisticated schemes came into the picture, but at about this time the computers came in and this is where George Stibitz's reference to the complex number computer enters the tour. He conceived the idea and put it down on paper around 1938 I believe. The machine was built and tested out under the guidance of Sam Williams, past president of ACM, and it was put into service in January, 1940. This was, as brought out before, demonstrated on a remote control basis in September of that same year at Dartmouth University where they had a station at Dartmouth and used a data link, we didn't call it a data link then, a data link to the computer which was still at the 463 West Street building.

In 1942, at eleven-ten, I was introduced to George and up until that time George was kind of a big mystery to people in my fraternity at Bell Lab then. He was known as the fellow that smoked a corn cob pipe and did remarkable things with orange crates, and he did. He had the circuits for what later became called or what was later called the Interpolator. This interpolator embraced some novelties that I think appeared for the first time, namely a program tape control of operations. It also had the bi-quinary code which introduced reliability in a way that hadn't been seen before.

Later on the Bell Laboratories went into some electronic computer developments. Maybe you will remember Leprechaun and of course right now the big thing is the electronic central office and this embraces all of the concepts that you can think of in computers plus a lot of control features.

I'd like to spend one moment if I may, about an incident in the early days of ACM. For some reason, my name had been given as one to sit in a small group to formulate the constitution and by laws for the organization. We met at Ed Berkeley's home, I think it was West Eleventh Street at that time, and late at night we came up with the first paragraph of the constitution, namely, "This organization shall be known as the Eastern Association for Computing Machinery."

Now I'm going to just go back a little bit and indicate, while that's only a few words, it did represent a lot of debate. First, why was it Eastern?

The reason was that there was known to be some activity on the West Coast at the time and it was recognized also that there was activity in the Chicago and Milwaukee areas and North and it would be presumptive for this group to try to embrace them. So the word Eastern was put on.

The word Association was debated a great deal. Some thought it should be Society. Some thought it ought to be Institute. Some thought it ought to be Organization. We probably had four or five other names but we settled on the word Association because it represented fraternalism in a way, because this was kind of a small fraternity at that time,

only a few hundred and everybody knew each other and was happy to have some of these early meetings. The next word that gave trouble was Machinery. Now Machinery does not seem to blend with tubes such as were in the Eckert-Mauchly ENIAC. But Ed pulled out a dictionary and we looked up under the word "machinery" and it was such a big dictionary that it had nine or ten meanings and along about the eighth meaning, there was a interpretation that fit us perfectly. It was getting late so we agreed to call it Machinery.

AUERBACH:

Let's call on Ed Berkeley for his comments. Dr. Berkeley.

BERKELEY:

I wanted to face the people I speak to and I'm told I have to face the other way. I have one story I want to tell and three or four remarks I want to make. The story in how the Association for Computing Machinery began. One day in April, 1947, I went to see Archibold and Lehmer on the evening before a meeting of the National Research Council Committee on Large Scale Calculation. The reason I went to see them was because I wanted very much to see an association of people working in the computer field put together. I persuaded Lehmer and Archibold to take the matter up at the meeting the next day, in which Stibitz and Von Neumann and Aiken were, I think, the other three members to meet with them. I found out later that they did bring up the proposal. Von Neumann said there was no need for a society. Aiken was thoroughly against it and as far as I can make out, Stibitz didn't say very much and Archibold and Lehmer sort of retired quickly. That made me angry and the next day I started putting together a small group who would call for the formation of some kind of an association, a temporary committee for an Association for Computing Machinery, and Dick Bloch was on the committee and John Mauchly was on the committee and I was on the committee and a few more people were on also and we rapidly put something together.

Now I'd like to maybe join myself to those people who have said we haven't seen anything yet. I predict that the ACM will have 200,000 members before very long. I predict that the time will come when we will be unable to tell whether a certain piece of thinking was done by a machine or by a man or by both of them working together. I think it is true that computers plus software equals giant brains of machines that think and I would like to say again the remark that Lincoln Stephens said when he came back from a trip to the Soviet Union in the early 1920's, I think, because I think it applies to us too. "I have seen the future and it works." We who are here in this hall today have seen the future and it works. Computers are an enormously important part of the future and it works.

AUERBACH:

May I ask if General Simon cares to make any comments?

SIMON:

I will say at the outset that I made no scientific or technical contributions to computers. I was merely an administrator. But I couldn't even be an administrator without getting into troubles every once in a while. I think one of my first troubles was that I had a forward looking view of what was going to happen to firing and bombing tables. I happened to be charged with getting out the firing and bombing tables and my motivation in computers was the humblest only, and that was to get firing and bombing tables done. So I had a careful plot made of demand and I could see the demand going up so I had a bright idea and I sent emissaries all over the country to the colleges and not only signed up the girls who were math majors and about to graduate, but I signed them up all the way back to girls that just had two years of math and were going to graduate. So I felt very smug about that, thought I was on easy street. But time went on and the first thing I got in trouble on that was the Navy came along and decided to do the same sort of thing and found that these girls had all been signed up. Then I insisted I was severely misquoted. I think, to the best of my knowledge, that what I said was that I admitted that I had stripped the East of girls with math majors, but the Navy said that I had stripped all the girls in the East that had math majors.

This ended up that I felt that I was still pretty much on easy street so I agreed then that the Army would do the Navy's overflow in firing and bombing tables.

Unfortunately, in making this plot of demand, I had taken cognizance of the first derivative but not the second derivative. So the result was that before long I found that I was getting crowed again. So I went to the Scientific Advisory Committee, which as Dr. Goldstine has pointed out included Professor Von Neumann, laid my troubles before them and they said, after discussing it, "we have just the thing for you. All you have to do is to do some dirty tricks on IBM punched card machines and you can come out all right."

So that looked fine and except for some more administrative troubles, where I had trouble about getting the machines and getting them in such a way that I could insure them if the war ended and I didn't have the money to continue to have them. I finally got that ironed out, however, and things were going very nicely with the machines and they did a good job until I began to have that curve catch up with me again. But that action got us again into future trouble that came in for embarrassment because after the war was just about over, right at the end of the war, I got a letter from Washington one of these things, "you will reply by endorsement hereon", and it stated that I "had spent a great deal of money buying the Bell relay computer which had served during the war but that the ENIAC didn't come in till after the war and I had spent a great deal of money on that. So obviously, I had wasted a great deal of money either on one or on the other." So that annoyed me somewhat and I made a facetious reply and I said that "the Bell computer was indispensable during the war but it would not have remained sufficient had the war gone on. Question: How would you have known when the war was going to end?" I never got an answer to that one.

The third trouble that came up was when we decided to do the research leading later to the ORDVAC. We had a contract with the Institute for Advanced Study and this contract called for payment of certain monies under certain circumstances but among other things, it provided that the services of John Von Neumann would not be chargeable but in lieu of thereof they would get all of the equipment that was left over from the experimental processes associated with their studies such as their making five arithmetic units and cycling them so many times and as a matter of fact, we didn't want those things to be turned over to the final contractor to make the machine.

But in the due course of time I got a letter coming through which accused me frankly of presenting the Institute for Advanced Study with a computing machine. Again, that required some more thought. So finally I came up with the idea and said, "You overlook the fact that the equipment that went to the Institute for Advanced Studies was given in lieu of payment for Professor Von Neumann's services. We are subject to attack and on these grounds inasmuch as we grossly underpaid them."

AUERBACH:

I heard an interesting tale earlier today that the ENIAC was the most amended contract that the Army ever had. I understand your problems General. I would next like to call on Don Eckdahl. Don is an old pioneer in this field but has done most of his work out on the West Coast. He was one of the original members of the Northrop team that built the MADDIDA and then moved on the National Cash Register Company, where he is now a vice president. Don.

ECKDAHL:

Thank you. John Mauchly mentioned the MADDIDA and I appreciate the opportunity of filling in this piece of the hole. The MADDIDA was one of the early computers and had an influence, a pretty significant influence, on the growth and development of the West Coast computer industry in that a lot of the people that worked around that machine went all different directions later on the West Coast in computers.

I went to Northrop Aircraft as a young engineer in August of 1946, and found two men already working in the field of digital computers there -- Floyd George Steele and Dick Sprague and certainly Floyd George Steele was the man that really......the MADDIDA story I think is really his story.

The era that MADDIDA was involved was generally from 1946 to 1949. Dr. Mauchly was correct. There was a significant influence I believe as a result of the relationship between Northrop and the Eckert-Mauchly Corporation in both influencing that general direction of digital computers towards a guidance system. This was what the MADDIDA work was directed towards. It was a digital computer as applied to a missile guidance system. In addition to that, we also had some of this same course that these other people

were talking about in 1946, except here John Mauchly came out personally and gave us a series of lectures on everything he knew about the computing field. MADDIDA was a digital differential analyzer, not a general purpose computer. It used a magnetic drum as a memory to store the integrators. My memory is that it had around 50, 24 bit binary numbers. The machine used around a thousand diodes, used diode logic and about 50 to 60 vacuum tubes. It was a portable computer. Either in 1949 or 1950, whenever the ACM Conference was at Rutgers University, '49?, we took this machine to the ACM Conference in Rutgers and gave two papers on it and demonstrated it there. We also had the contact here with John Von Neumann in that our corporation in attempting to see the significance of this work, had us take this to him for study which of course was one of the privileges of my life to meet Dr. Von Neumann at that time. That's I think the picture from there. I don't think the significance of the MADDIDA was just the digital differential analyzer. The impact it had on all of us was in moving in the other directions in the computer field on the West Coast.

AUERBACH:

Thank you Don. I had the privilege of being in the hotel room at the Princeton Inn when the MADDIDA was demonstrated to Von Neumann and they waited until something like eleven-thirty one Saturday evening because he was at a party and on his way home he and his wife stopped by. He had never seen this machine. He was asked to give an evaluation of it. So the inventors of it proceeded to tell him something about the machine and how it worked. And in about ten or fifteen minutes he thought he really understood the machine and he asked for demonstration and as was very typical in those days, one never believed the read out. You always felt that these things were somehow being generated but not for real. This was not the only case. My colleague, Sam Alexander didn't believe it when I first demonstrated a five megacycle mercury tank working either and we had to sit and spent four hours proving that the scope was calibrated right, etc. So it was pretty commonplace.

Von Neumann duplicated the exact thing that Herman Goldstine said. They were generating functions. He saw if demonstrated. He wasn't satisfied. He said, please program it for the next zero intercept. They said, "we didn't have tables with them and they could find out whether the answer was correct or not." That didn't stop Von Neumann. He proceeded to mentally compute the intercepts for three more. He sat down and all within the first hour, reprogrammed the machine with the men pointing out what they should do to make it work. It was really a startling demonstration of what this man was capable of doing. Herb Grosch.

GROSCH:

I think no major ACM meeting would be complete without a few gripes. Some of them have been hinted at in the last few minutes. We have a great scarcity of Westerners, although about half of the early work and a much larger proportion of later work has been done beyond the Rockies. We have no representatives of business data processing to

speak of. Very few applications people, unless you count Grace's Bessel functions. And above all, we don't have much representation from a rather well known company in the field. Now I happen to have the unusual distinction of two Purple Hearts awarded by that organization and our Chairman today thought it would be only fitting if I described some of their early adventures, briefly, please, or so he said. I ought to point out that from the turn of the century, the majority of automatic data processing in this country and worldwide has been done on Mr. Hollerith's machines. True, this was a small fraction of what was being done manually in the early days. But right up to the time of the demonstration of ENIAC, IBM felt that it really had the majority position in the field and rightly so. There were however, a few months in which more digits were squirting out of ENIAC than all of the scientific computing adventures that IBM was then sponsoring or participating in. These had begun, I might say, at Columbia University in the '30's with Wallace Eckert whom John Mauchly mentioned. They had continued again with Wallace Eckert at the Naval Observatory here in Washington where the first paying scientific computing installation was installed in 1940, the operator being Jack Beizer who is in the audience and who in that respect, I think, outdates all of us to say we were doing useful work on that machine.

Counting again, in crypto logical and cryptographic work, in the late years of the war, stuff that has not yet been told I believe publicly, adventuring into the Aberdeen area with the relay calculators which they delivered somewhat in advance of the Bell MARK VI, although they were not nearly as flexible nor as far along in the line of general purpose computing, since they still required the plug board as a programming device. Another machine was installed also at Dahlgren that I think some of you in the audience will be familiar with. At the end of the war the Watson Laboratory was established at Columbia and I was the first employee that Eckert took on in 1945. Shortly thereafter, with the beginning of electronic squirting by the ENIAC, Mr. Watson, Sr. decided that it was time to restore the normal balance, a balance I might say that has not been perceptibly upset since. From the time that he gave orders to build a competitive machine to the time that this machine had been specified, designed, built, tested in Endicott, taken down, assembled again in Galactic headquarters in New York and demonstrated to an amazed audience which included among many others, John Von Neumann, Howard Aiken, Von Karman, Richard Courant and many others of the group to whom the Eastern Association of Computing Machinery aspired, was exactly twelve months. This included such novelties as a 78-hole paper tape which was used in lieu of the not yet developed magnetic tape, the largest, I think ever designed, relay storage racks which consisted of I think it was six major sections, sixteen feet high and a total of forty feet wide filled with the C. D. Lake wire contact relays which had just been developed and which are still in the production line of slower IBM equipment to this day, 12,000 vacuum tubes. After the machine had been thoroughly tuned up following the demonstration, a fairly common sequence in those days, the mean time to failure was four minutes and the mean time to repair was twenty minutes. Nevertheless, with the specially trained crew that worked with the machine during its whole existence, it was possible to do very effective advanced scientific work. Eckert did a great deal of celestial mechanics on the motion of the moon. I made some early adventures in automatic optical design. Hillerith Thomas and some

others did work in nuclear physics. David Hill for instance, did a very advanced and well reported job on the hydro-dynamical analogy of the nuclear fission process which was outside security in those days and when the machine was dismantled, it had the additional distinction of being one of the first of the great machines to be dismantled. This was partly due to the fact that it had been installed in extremely valuable space in the adjacent building to Galactic headquarters, then known as World Headquarters.

IBM at that time already had the upstairs offices on lease but the ground floor was occupied by the French Bootery, which still exists on the other side of the street to this day. And since they were doing a fine business in high heeled baby toed shoes in those days and didn't want to give up the space, it was necessary to bribe them to leave. The installation was a very elegant one with giant panels of plate glass, much stainless steel and aluminum, all of which were extremely hard to get in the '47—'48 era we're talking about. But with Mr. Watson's priorities, which mostly consisted of a very firm set of orders, all of this was prepared and ready in time.

Two days before the great event, Rex Seiber who was to be the manager of the installation, Wallace Eckert and I and two or three other people were showing Mr. Watson through the beautiful, shining, gorgeous installation. Nothing like these grubby machines you've had described to you so far. Mr. Watson looked around and he found everything in order, including the inscription on the wall and so forth. But he objected to one thing. In the center of this rather large room were two pillars about two and a half to three feet in diameter. They had been painted an unobtrusive black, in fact shiny black after the Black Widow Principal that had been discovered in the night fighter days. But unfortunately, Mr. Watson's sight was still quite keen and he observed these two and a half foot diameter columns and he said to the entourage which was accompanying him with nodding heads, "Gentlemen, I want those columns removed before opening day." Well the secret of those columns was that there were two large girders in there which held the building up and even for Mr. Watson, it did not appear practical to remove them in two days. Perhaps with a little more time we could have tackled it but we just didn't quite see how to do it in two days. There was, however, a solution which is immortalized in the literatures.

The archives of our society and many others have copies of the big brochure which was handed out two days later. Note that, two days later. It was an elegant two-color printing job of many pages which must have taken a great deal of time to print. There is a spread photograph in the center of that, showing this beautiful room, the machine, the people standing there, many of them probably sitting behind me in the audience, but the columns have been retouched. They do not appear in the photograph. Thank you very much.

AUERBACH:

This is Mr. Harry Polachek.

POLACHEK:

I am primarily a user of computers and one of the early users of computers, so I fulfill the bill as Dr. Grosch has mentioned. I've used the MARK I. Of course I remember Ed Bloch very well and Grace Hopper and many other people. But one of the computers I used was the SSEC that Herb Grosch has described, the Selective Sequence Electronic Calculator which was the first IBM large scale computing machine which was located at 590 Madison Avenue as he described. I used it with many other people, including Dr. Hyman who's sitting here and I just wanted to relate a little story about the use of that computer. I computed shock wave refraction on it and I stayed up a number of nights with Rex Seiber and Dr. Hyman and a number of others and some of the characteristics of that computer, the SSEC, I'd like to relate. I don't want to doubt Dr. Grosch's figures but my recollection was, at least during the time that I was using the computer, that the mean free path of an error was about three-quarters of a second. But this was my experience at night anyway. The machine used to work and it had a very good feature, it was doubly checked so that any time an error occurred, the machine stopped. We used to pride ourselves on the fact that the machine ran for three-quarters of a second because it used to carry out about ten thousand calculations every second. So in three-quarters of a second, it did 7,500 calculations and a great deal of work was done during those three-quarters of a second.

We managed to stay through those nights and get our work done and we did a very find job on it. But one other little story I must tell about. Mr. Watson, Sr., who visited that installation on a later occasion than Dr. Grosch has noted. On one occasion while I was using that computer, the work came around that no other person than Mr. Watson, Sr. is going to visit the installation. So the work spread very quickly and the machine got shined up and everyone got in his best clothing and ties and everything went beautifully, except one point worried me a great deal. Of course I wasn't working for IBM but it worried me a terrible amount. It was my understanding that Mr. Watson was under the impression that the machine operated at 100 per cent efficiency with no errors. And I knew the machine had these intermittent errors all the time. I was just worried about what was going to happen. The IBM people didn't seem to be quite as worried as I was. Well Mr. Watson came around the next day and the IBM people did a very simple thing. They disconnected the checking circuits. So everything went off beautifully. Mr. Watson visited and the machine was 100 per cent efficient, no errors were made. The machine was clicking and everything went off fine. Thank you.

AUERBACH:

Thank you Dr. Polachek. May I ask if there is anyone in the audience who would care to come up and make a comment. There are microphones spread around the back of the room. Please just identify yourself and your affiliations. If you want to line up behind the microphones, I will identify you.

TURNER:

Ike I was hoping to get a minute.

AUERBACH:

Your name?

TURNER:

Oh I'm sorry. Dick Turner, NASA. I'd like to commend to the people in the audience the neophytes in design. I recommend to them that they go back and read what these people did. I would like then to bring up two examples. One of them is the extremely reliable paper tape which I have used in other things, in Dr. Stibitz's MARK V relay computer. It ran on and on and on and you never got an undetected error.

The next one is a story that John Mauchly doesn't know and this is the history of the development of interrupt. I never told it to him. I told it to Grace a couple of days ago. I was faced in 1954 with having to work with some badly timed devices, I knew about the overflow device and the trick of placing the "are you" instruction pair in location 0 and all we did to get interrupt was change that "you" address. That's all it has ever been.

AUERBACH:

Thank you. Next Dr. Jan Rajchmann who was one of the early pioneers in this field with RCA.

RAJCHMANN:

Perhaps in keeping with the historical part of the discussion, I'll talk about the very early part. I was very fortunate in having been appointed the first man in RCA to work on computers, as a result of a contract we got from the Frankfort Arsenal in 1939. During those days we thought of many of the circuits that actually have been used very widely thereafter, particularly adding devices, and one which was the resistance arbitrary function generator which in today's terms we call a read only memory, which is effectively a huge matrix of resistances. We were very fortunate that Von Neumann was as a consultant to our contracts and when I first told him about his matrix, he said, "Gee whiz, this can't possibly work. It is just a monster short circuit. Everything is really connected to everything else." I must give all credit to Von Neumann because when I explained to him that we should make the impedances right, it really isn't a nonsense type of short circuit but a very meaningful type, he saw the thing immediately.

Some of the work that we have done was in fact used by other people later on. We're very glad for that. We were later associated with the Institute for Advanced Studies in the development of the Princeton machine and during that time I had personally thought that memory has to be of a matrix type and shouldn't have any analog deflection and we developed a vacuum tube which in modern terms would be called an integrated vacuum tube -- technology type -- which was much slower in coming than was required for the

machine. This is incidentally, not so bad when you look at how long it took the integrated semi-conductor technology to come into being, which is a much easier technology. So the tube was not used in the first Princeton machine but was used in the Rand machine on the West Coast and I believe that it was only around '58 or '60 that actually the tubes were usefully discontinued. Not that they didn't work but other machines became more modern.

AUERBACH:

Thank you Jan. I'm just going to call on a few more people. First, Mr. Jerry Haddad, Vice President of IBM.

HADDAD:

I'd like this to be a first. Normally when I'm at a session and Herb Grosch gets up to talk, I never find it possible to follow him. I'm usually in a hole that's ten feet deep. But I figured that this time I'd say a few things that Herb will recall. It was shortly before and shortly after his first Purple Heart from IBM. In his modesty, he didn't acknowledge the fact that he had quite a bit to do with the planning and testing out on paper in an incredibly short period of time of the 701 machine. I think that was just prior to the time that he left IBM for the first time. Shortly thereafter, because it was a fairly high speed program as I recall, Herb was back in the very same building, but this time in the guise of a customer. We had a session with about forty people and we were describing how the machine worked from a technical point of view. In those days it was felt necessary to let the people who used the machine know how it worked. Somehow that eludes us these days.

The Williams tube or electrostatic storage has been mentioned a couple of times and if you recall, the 701 and 36 parallel boxes of Williams tube storage so that we could work 36 bits in parallel. Now we found it necessary to do what we called addressed scattering. We didn't number the spots sequentially, 1 was on one tube, 2 was on another tube and 3 was very far from the 1 and so forth in order to prevent interaction on successive references. And this upset Herb very much and in the session he stood up and objected. He said, "I don't see how it is possible for a human being to program this thing so that the spots on the face of the tube can be made to spell Good morning Mr. Watson." My rejoinder as I recall was that he might hear a rumor to the effect that we had done this address scattering for technical reasons in order to make the bloody thing work, but really it was because we'd had advanced notice form his psychiatrist that he was about to do something like that, which is really why we did the address scattering.

AUERBACH:

Thank you Jerry. For the record, Jerry was the chief architect and designer of the IBM 701 computer. Betty Holberton who is, if not the first, one of the first programmers in the

world, with the Eckert-Mauchly Company, prior to that with ENIAC and now with the National Bureau of Standards. Betty.

HOLBERTON:

Well I was one of these girls that General Simon hired when he put out that advertisement in the local Philadelphia paper, and computed firing tables both day and night on an alternating shift. Well after word got around that there was a possibility of five girls accepting a new job which we were not allowed to know what it was, I volunteered because I'd had three years of this computing day and night. In fact, our classification was a SP something or other computer...that was what we were called. Well I'd like to tell you what it was like to be a programmer in 1945. You have to visualize what the Moore School of Electrical Engineering looked like in those days. It was, according to my story, it was an old piano factory, three stories high and we were on the top floor during the summer when the students were away. Above us they were drilling to put on a fourth floor and the plaster was falling all over the place. We were not supplied with any material whatsoever to learn this beast except wiring diagrams and block diagrams. They told us which direction to follow with our fingers, but if it hadn't been for one of the young engineers who came up two steps at a time to the third floor to see the girls, we might not have been able to get through it. This is the way we learned the machine from the inside out.

When we finally got on the machine after plugging and taking these tremendous long trays about twelve feet long and moving them, a girl on each side, not the fellows helping...in fact I was never in better physical shape in my life than during those two years. Well when we got the problems on which someone said took hours but it really took days, we then went through what...I don't recall if we called it debugging or not. But I think that we did invent the term 'break point.' Because as we pedaled, we called it pedaling, through the time cycles, we would actually pull a wire which left the circuit dead and all the numbers displayed and this was what we called 'break point.' If somebody knows of an earlier use of this word I would like to know, because we'd like to put this down in the history of coining words.

I would like to say what happened later on in the fall of '46, I guess it was, when the new idea of wiring up the ENIAC with sort of semi-permanent wiring with instruction codes...a number of us worked with Dr. Von Neumann in setting up this code and it was to be set up with dialing into the function table which had settings from 0 to 99. We felt we wouldn't need that many settings for all the instructions. We sort of worked along for awhile. But to my astonishment, he never mentioned a stop instruction. So I went to Princeton with the idea that I'd have courage enough to mention a stop instruction. So I did kind of coyly say "Don't we need a stop instruction in this machine?" He said, "No we don't need a stop instruction. We have all these empty sockets here that just let it go to bed." And I went back home and I was really alarmed. After all, we had debugged this machine day and night for months just trying to get jobs on it.

So the next week when I came up with some alterations in the code, I approached him again with the same question. He gave me the same answer. Well I really got red in the face. I was so excited and I really wanted to tell him off. And I said, "But Dr. Von Neumann, we are programmers and we sometimes make mistakes." He nodded his head and stop order went in.

AUERBACH:

The last person I'm going to ask to speak is also one of the earliest workers in the computer field, Dr. Arthur Burks who was associated with the ENIAC and is now a professor at the University of Michigan. Art.

BURKS:

This is in a way a fellow-up of Jan Rajchmann's story concerning the function table but it illustrates very well the pressure under which we operated and the speed with which luckily enough, we were able to finish the ENIAC. The ENIAC was so large and had so many units and we had so much to do in so short a time, that except for the accumulators and the cycling units, we were unable to make prototypes. In the case of the multiplier, Pres Eckert and Kite Sharpless made a cross section circuit on a bread board and tested it out and then I was given the job of directing the complete logical design. When this was finally finished and the other units of the ENIAC were ready so we could test it, we all waited with bated breath to see if the multiplier would work. It worked very well except when any digit of the multiplier was 0, the easiest case. It didn't take us very long to discover that we had used a complement system in the multiplier table, the Rajchmann resistor matrix, and whoever designed it before had forgotten that the complement of 0 is 9. Well fortunately we were able to put in extra tubes and extra resistors and fix this up and the general schedule of the ENIAC was, starting in the summer of 1943, to design the circuitry, testing the prototype of two accumulators in a small cycling unit in the fall of 1944, and finishing the first problem in December of 1945. Later computers took somewhat longer and this leads to my last story. You'd ask somebody in '45 when is the EDVAC going to done? '46. Later it was a Von Neumann computer or any other computers. In '46, when will it be done? Same machine, '47. In '47, when will it be done? In '48. This got to be known as the Von Neumann constant. Whenever you asked when a machine was to be finished, it was always in one year.

AUERBACH:

May I ask again at the conclusion of the session that all those people who were in attendance at the first ACM Conference as well as all those who were in the computer field prior to 1948, please come to the front of the room. In concluding, may I express the appreciation of the ACM Program Committee and all of us here for the contributions that each of the panel members has made to record some historic events that took place twenty, twenty-five and thirty years ago. And again, those of you who participated from the audience, our appreciations and thanks. This meeting is adjourned.

[End of Panel Discussion]