An Interview with

RICHARD BLOCH

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Conducted by William Aspray

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Charles Babbage Institute
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Richard Bloch Interview
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Abstract

This interview describes Bloch's work at the Harvard Computation Laboratory and his subsequent career in computing. Bloch begins with his early life through his undergraduate degree in mathematics at Harvard. He entered the Navy in 1943 and recounts how he first met Howard Aiken while giving him a tour of the Naval Research Laboratory. Aiken had him transferred back to Harvard just as the Mark I was being shipped from IBM, where Bloch was involved with programming and maintenance of the machine. He describes the architecture and operation of the Mark I, including a discussion of the improvements made after the machine arrived at Harvard. He also discusses a number of the problems solved on Mark I, including one for von Neumann on spherical shock waves in an atomic implosion. He also describes Aiken's personality and attitude toward computer commercialization.

In 1947 Bloch left Harvard for Raytheon, eventually heading their computer division. He discusses the government RAYDAC and commercial RAYCOM computers, as well as his own contributions to the development of parity checking. Raytheon sold its computer division to Honeywell in 1955, and Bloch became director of computer product development there. He describes the 200, 400, and 800 series of Honeywell computers, the development of an error detection machine which he claims opened the field of fault tolerant computing, and competition in this period between IBM and Honeywell. In 1968 Bloch joined General Electric as division general manager to develop large computer systems to compete with IBM. When GE left the computer field, Bloch moved into private work on venture capital, acquisition-divestiture, and high-level corporate consulting in the computer industry. He recounts how he became chief executive officer of Artificial Intelligence Corporation, a company developing a product to use natural English to query databases.

RICHARD BLOCH INTERVIEW

DATE: 22 February 1984 INTERVIEWER: William Aspray

LOCATION: Newton, MA

ASPRAY: This interview is being conducted by William Aspray on the 22nd of February with Mr. Richard Bloch in his home in Newton, Massachusetts. Why don't we begin by having you tell me something about your early education and home life. Where were you born? Where did you grow up and such?

BLOCH: I was born and brought up in Rochester, New York and went through the public school system there. I expected to concentrate as time went on in my favorite subjects, which were science and mathematics. Just what profession I was going to go into, specifically, I didn't know; but that was my leaning. I went to Ben Franklin High School and entered into a competition in my senior year for a scholarship to Harvard offered by the Harvard Club of Rochester. I was fortunate enough to win that competition. So I was due to go to Harvard in September of '39 as a Harvard Club of Rochester scholarship recipient, which would hold for four years provided my grades were satisfactory. They had to be more than satisfactory. I might add that in my youth, before leaving Rochester for Harvard, my activities included quite a bit of music training; I studied at the Eastman School of Music as a flutist and I also played in various orchestras and bands in the city -- high school orchestra and they had an inter-high school orchestra that I also played in. I was also pretty active athletically and I tended to take seriously anything I went into. I played table tennis to a point where I gave demonstrations in which I played against some Chinese experts that were making a tour and I got to the point where I was considered to be one of the better table tennis players in the vicinity.

ASPRAY: What about scientific hobbies, ham radio for example?

BLOCH: No, I did not, interestingly enough. I didn't engage in ham radio. I was a chess player and I actually started learning chess at the age of three. There were articles put out and I was reminded that I knew how to play the game at three years of age. I knew the moves. But I didn't really have strong scientific hobbies at the time. I was a very active participant in high school affairs, was a class president and an editor of the yearbook. All those things were coupled with my being a very obedient and avid student and with a fair amount of athletics. I also played golf on the high school team. That took up most of my time.

ASPRAY: What did your parents do?

BLOCH: My dad was in the advertising field, in direct mail advertising. He was engaged in his own operation called the "Rochester Letter Service". That had been his own company for many years. My mother was active in various groups, primarily in such things as the Eastern Star of which she was a matron. She was fundamentally an at-home mother and spent most of her time with me and a sister and a younger brother. This occupied her time.

ASPRAY: How were the public schools; you went to the public schools, right?

BLOCH: Yes.

ASPRAY: How were they in terms of the education?

BLOCH: Superb, as a matter of fact. I don't know that they are of that grade today there, but at that time it was generally conceded that Benjamin Franklin was so highly regarded that to be a teacher in that school required advanced degrees. The caliber of teaching was extremely high. I just don't think I ever encountered as strong a scholastic level of teaching since that time in any school systems that I've seen which, including Newton here, which is considered one of the best educational systems in the country. I might add I was a pretty strong student. I guess I still have a record that at least at the time was unchallenged in Regents Examinations of New York State. I think I missed one question on four different exams and ended up, I think, at 99.8 as an average. I guess they're still trying to beat that. I'm not sure whether somebody may have gotten 100 since that time. But I had an almost all A record along the way which, combined with my other activities, I guess helped me in winning that scholarship.

ASPRAY: Let's turn to your Harvard days. You planned on majoring in what area?

BLOCH: I decided to major in mathematics with some minor emphasis in physics. However, I also was involved in courses in economics, philosophy, and psychology. But the concentration was in the science area. Since it was for a B.A. degree and Harvard had no engineering of any consequence, I never took any engineering courses.

ASPRAY: Do particular faculty members stand out as either special mentor figures? Were there courses that you remember as outstanding or that you took to especially?

BLOCH: Yes, I can name several. I was impressed by the brilliance and competence of many of the people there at the time. I should mention people in the math department such as Garrett Birkhoff and in particular Garrett, Jr., both of whom are well known. Garrett, Sr. died some time ago. Hassler Whitney, who I also took courses from--his emphasis was in topology at the time. William Quine in logic and Saunders in physics. Also Irv Kaplanski in probability who I think is now at the University of Chicago. As a matter of fact, I became what is called a reader in my latter years for the math department, correcting exams and papers for the department. In Kaplanski's case, we got to be pretty good friends as well as having a great deal of mutual interest in contract bridge at the time.

ASPRAY: I see.

BLOCH: Irv would come over and join us in a game and sometimes they went on into the night. I don't know who was worse off for that the next day, Kaplanski or myself; but we really went at it pretty avidly. Talking about avocations, bridge became a very strong avocation. It was at Harvard that I became seriously interested in chess. Although my mother was a good player, my dad was a superb chess player. He had national ranking and was almost unbeatable. He had matches with some of the leading chess players of his time. I never really took to it that seriously. I played the game but really not that seriously. When I got to Harvard there was a temptation to get into bridge because there were a number of fellows there that were playing and I knew enough about the game so I thought I'd drop in and become a player. But they played for money and I didn't have much of that--all I did was fatten them up because I was really pretty poor. However as usual I got put out with myself about all of that and decided to really learn this game. Aside from learning my course material at Harvard I also had a self-concentration in contract bridge. I pored through books and studied at Widener Library on that subject. I just felt that if I were that bright what was wrong with my bridge? This game couldn't possibly be beyond my intellectual capacity. I improved and finally got to the point that I was playing equal to the others. In the last year my game started to pick up to the point that I was playing seriously in duplicate bridge and more frequently taking their money than vice versa.

I resided in freshman year in Matthews Hall, one of the older halls on the main campus, still standing there. I won the Dexter Award which I guess is given for the top-ranking freshman. I still have the set of books by Carl Sandburg which was awarded to me with the Harvard seal on them. I guess that was considered to be a fairly distinctive award. I didn't have too much time for other recreation. I played in the Harvard band. I met a number of people that were extremely worthwhile as associates during that period. In sophomore year I moved to Lowell House and, at the time, Elliot Perkins was the master. I know he's not the master now and I'm not sure whether he is even alive. I went on through my last three years there holding a pretty high record along the way--which was mandatory because, as it turned out, my family was not well to do by any means and I don't think I would have gotten to Harvard if it weren't for the scholarship. If I didn't hold that scholarship, I don't think my family could have afforded to keep me there. So I didn't fool around very much.

ASPRAY: I see. Did you go straight through in four years then?

BLOCH: Yes.

ASPRAY: You graduated in 1943?

BLOCH: That's right. Because World War II came upon us. In '41 I joined what was then known as the V7 program which was a program under which you committed yourself to go into officer training in the Navy immediately upon graduation. They would allow you to finish your undergraduate career.

ASPRAY: I see.

BLOCH: I joined that and immediately upon graduation in June of '43 I was off, within a matter of weeks, to Notre Dame for midshipman training. I became one of the "90 day wonders" as they called them in those days. It was a three-month course where you started as an apprentice seaman, became a midshipman, and then became an ensign all in 90 days. They used the term "90-day wonder." If it weren't for the war, I might never have gotten into the computer field. It was a strange situation because my abilities in mathematics were known to the extent that just before graduation, or just about at that time, I got an offer from the University of Pennsylvania to join their staff to teach mathematics, but obviously not with tenure. I think it was Klein at the University of Pennsylvania that headed up that department. I couldn't take that offer because I had to go off with the Navy. So I completed my stay at Notre Dame coming out as an ensign and with the war boiling. We all wanted to do our thing and my idea was to get on the biggest ship possible. So I applied for duty as a navigator on a battle ship. When Washington reviewed my background and said "nothing doing." I thought I was going to sea, but I was the only one that thought so. They sent me down to the Naval Research Laboratory to work in what was called the consultant section of the Radio Division. So there I went doing some mathematical work that related to the war activity in that division.

ASPRAY: Computational work?

BLOCH: Analytic and computational work. Some work in antenna design and so on. It was chiefly doing the mathematics involved in this.

ASPRAY: Did you get experience at the time say using a desk calculator?

BLOCH: No. They had some calculators there, which I used, but there was no real heavy use of the machine. I was only there from about September of '43 to about January of '44. A visit was set up for Howard Aiken to the lab, which was in Anacostia. He was visiting on some mission. As I recall the head of the laboratory at the time was Admiral Van Curen. Aiken was to visit him, and I got a call from the top there at the laboratory to escort Aiken around. In the process of doing that he was telling me about some of the work that was going on there. When he heard of my background at Harvard he asked me would I like to get involved. He told me about this fabulous machine that he had designed and asked if would I like to spend my Naval duty up at Harvard? Of course it sounded intriguing to me; so I jumped at it. I didn't understand completely what this was all about other than there was a giant automatic calculator as he called it that he had been working on, and that he was looking for staff. So in March I was ordered to duty at Cambridge, to the computation laboratory, which at the time was in Cruft Laboratory basement. I remember it being next to the acoustic laboratory in which Leo Beranek was working at the time. When I got there the machine wasn't there. It was just being delivered from IBM from Endicott at the time I got there. I helped in an elementary fashion, helped get this thing put up in place, but in the process of course I was beginning to learn what this was all about. The only other officer that was there was Bob Campbell. I guess he had preceded me there by a couple of months.

ASPRAY: Bob Hawkins was also there at the time.

BLOCH: I think Bob Hawkins was there, yes. He was really the construction engineer you might say, responsible for all the debugging and so on, and getting this thing in place. He was a kind of an all around Mr. Fixit as far as the machine was concerned, whether mechanical or electrical. He was there, yes.

ASPRAY: Had you known or heard of Aiken before when you were at Harvard as an engineer?

BLOCH: No, not at all. This to me was a real break because this was much more intriguing than this other type of activity I was engaged in.

ASPRAY: What were you asked to do when you got to Harvard?

BLOCH: Well, the first thing that I did was to get to learn the machine. Of course there was no manual or anything of this sort; so I guess I just picked it up from discussions and basic blueprints of the circuits and so on. But there was very little if anything really written at the time. There were just piles of prints. I picked up this information from Aiken and to some extent Campbell. Then I was given the task of programming for the machine and I ended up being probably the prime programmer. I also got involved in, eventually really being responsible for, the operation of the machine and the solution of problems, some of them being used in the war effort directly. I was...

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BLOCH: ... involved in the programming side of the picture and also in the maintenance and debugging of the machine. I lived in bachelor officers' quarters next to the Commander Hotel which was within a half mile of the place. I'd be called at any hour of the night by the Navy enlisted men who were on the staff by that time and whose job it was to run in shifts, running 24 hours around the clock. By the way, they were called "I specialists" at the time, "I" standing for IBM. And curiously enough, when I mentioned this to some IBM executives, they didn't recognize the fact that that was true at the time. They were amazed to hear about it. But that was the case. They had an actual "I" on their sleeve. They were people who knew how to operate punch card equipment for IBM; that's where that came from, the punch card equipment operators. We had a staff of perhaps four or five of them. I remember very well they were capable lads who did a very fine job on duty worrying about putting the tapes on the machine and seeing to it that the machine did its thing. Of course this machine was gigantic as you undoubtedly know. It was over 40 feet in length and quite a monster. I got to the point where I knew that machine like the back of my hand, whether it was a plugging or electrical questions, or questions of circuitry, debugging, and programming. Toward the end of my career, which was in early '47, I even started and completed the design of some additions to the machine, in particular a subsidiary sequence mechanism. Because the machine was under unitary sequence, if it did any branching, it had to call in some other segment of the tape. Since orders were not in registers, but only on tape, in order to branch you physically branched by stopping the machine or causing the machine to stop; whereupon the operator would twirl the drum that held the tape over to an indicated branch line and proceed from there. You had instructions that would say "if the machine stops here, move it over to the red line next; if it stops somewhere else, ship it over to the blue line." It became pretty complex and in later years after I left additional sequence mechanisms were arranged. We recognized the fact that this was very awkward. I set the specs and designed a subsidiary sequence which enabled you to have ten different sub-sequences that could be called in. They had on the order of 40 or 50 lines of instruction each. We had subroutines and branching without there being any human interference.

ASPRAY: These were stored where in the machine?

BLOCH: They were stored in stepping switches. More specifically, what happened was that if a call went to one of these sub-sequences which were wired with plug wires, the codes of each line were wired into a series of plug holes for a particular line. What you would do is plug in multiple over from the various codes which served the entire board over to this line. So you were able really to get this three address code plugged into any line, any three address code you wished plugged into any line of coding that you wished. So it was wired literally. The stepping switch served to go from line to line. Once the machine gobbled up one line of coding, it would trigger the stepping switch to go to the next line of coding which was again picked up by the way it happened to be plugged.

ASPRAY: Bob Campbell had asked me to ask you to describe the improvements that were made to the Mark I. Is this one of the improvements?

BLOCH: Yes, this is one of the prime improvements that was made of a major nature. It really was very significant in that it made subroutining possible and branching resemble more what was done later in the field. There were a lot of other things that were done, however. This was the first instance that I know of subroutines in any computer and branching of that nature, being able to go to any of ten subroutines based upon the results that might occur. So I would say that concept really was born right there. I don't believe there was any record of any previous use of subroutines that were able to be utilized in the machine or branching. It was also the first instance of a machine that was able after some modifications to do simultaneous arithmetic operations. The machine had built in multiplication.

ASPRAY: Did it have built in division?

BLOCH: It had built in division, yes, and no programmed division. Of course you could program it through inter ?

. It had a complex divider which actually built up all the multiples of the divisor and the whole bit and subtracted from the dividend. It would build up multiples and keep subtracting until such time as it got an oversubtraction. Then it would revert back and know that the last multiple was the correct one and if it was eight multiple, throw an eight into the quotient, and so on. That represented a big chunk of that machine. Another big chunk was related to the trigonometric functions; there was a built in sine; there was an exponential function, a logarithmic function.

ASPRAY: Was there square root?

BLOCH: No, no there was no square root function that I recall.

ASPRAY: That seems odd. That seems to come up so often. Though you can do it easily by an approximation method.

BLOCH: Yes, but there was no root at the time and that's an interesting point. I do not remember there being any square root in that machine. The machine had the inverse functions so you had sine. I'm trying to recall. It did not have arc sine. This came in through the interpolator functions. But some of these functions would take as much as a minute to run on the machine. It was a very expensive operation. Addition and subtraction were implicit in the fact that the 72 storage registers did their own addition and subtraction literally using these IBM counter wheels. Inside of a multiplication there were cycle times that were not being used by the machine. The multiplication itself had its own sequence of operations within the multiplier unit of the machine. The way the buses were set up you could easily perform additions to your heart's content without disturbing the multiplication - while the multiplication was going on.

ASPRAY: I see.

BLOCH: The way the machine was originally designed, it would bring in the multiplicand through one line of coding on the multiplier and a second line of coding where you wanted the quotient put. But by tricks in the programming, it was possible to force the machine to move to the next line of coding prematurely. We knew that there were exactly X number of cycles before it was going to be time to call the multiplier in. What we would do is force the machine to move to the next line. But at that line was an addition saying add something from one counter to another. We could do that, or subtract. Then, after the multiplier came in, there was a great deal of time before the quotient would show up. So we had a technique of interspersing operations. Some of these operations would consist not only of addition and subtraction. It was even possible to intersperse instructions to move some information into the print counter in preparation for printing, and things of this sort. So we had a situation in which the machine was kept very busy. It was our first example, in fact, of simultaneous arithmetic operations. It was many, many years afterward that it came back in again because it was by just a quirk of the design of the machine that you were able to do that.

We programmed a number of problems for the Navy and the Bureau of Ordinance. Next I got involved with John von Neumann in programming a partial differential equation of the second order, which dealt with spherical shock waves relating to the implosion occurring in an atomic explosion. At the time I didn't know that. It was a very complex problem and von Neumann was there for some time. He obviously knew what the problem was that he wanted to do and started setting up the formulas in mathematical fashion. I had to take that and move them into difference equations and eventually get to program the thing. One thing I do remember is that the first twenty pages of this report dealt with describing a problem and how it was going to be attacked and so on and von Neumann insisted that my name be put in front of his as the author, which embarrassed me a little bit because he was the great mathematician and mathematical physicist of the day. I really didn't feel I deserved that; but that's the way it came out, so I prize that.

ASPRAY: Could you tell from working that closely with von Neumann how much he knew about automatic computation?

BLOCH: I would say this. He knew he had a mind that moved in rapid fashion on any subject relating to mathematics or physics. When he came, he did not know the design of the machine. He knew numerical computation, say the contents of Whitaker and Robinson which let's face it, was well known before.

ASPRAY: Sure.

BLOCH: You might have to do it with your hand or with a desk calculator; but the point is he knew how to apply numeric difference methods and things of this sort. Exactly how to do this on the machine was left to me. I would say in all fairness that he neither had the time or the desire to know the details of how the machine operated. I'm sure he picked up a fair idea of that and later on, of course, got himself involved in the area of machine design.

ASPRAY: Fair enough. Can you describe some of the other problems you worked on?

BLOCH: Well, one of the major assignments I had involved the need for more exact tables of Bessel functions. I got involved in producing a gigantic series of books. Some of these volumes were issued later by the Harvard Press and stand to this day as a monument to the machine and to computation as used to put out tables of functions to accuracies never before really achieved. We put out Bessel functions, J0, J1, J2, and so on. The results were done to 23 decimal places. In order to achieve that accuracy we operated at 46 decimal places, which required chaining registers and working in double precision, you might say, in the machine. This was a huge project that was one of my babies and, interestingly enough, the printed results were not transcribed so we never lost anything in the process of it going from the machine to paper to transcription to being printed.

ASPRAY: They were photocopy ready?

BLOCH: They were photocopy ready. They were printed on IBM typewriters with paper carbon ribbons that put out a beautiful print of the digits. These were taken, cut out, pasted-up and photoed directly. That's the way the tables were done and I think to this day--I could be wrong--that they have found hardly a mistake in any digit anywhere in any of these tables. In fact our tables show that some of the previous results, some of which were done by a Japanese by the name of Hyashi who had some errors that occurred in the sixteenth or seventeenth place of decimals. He had done this by hand and I rememb er Howard Aiken jumping for joy at the fact that mistakes were found, especially since it was a Japanese that was involved. The tables were useful. A few years afterwards, people would say: if you need the function, calculate it when you need it. Who needs tables? But they were extremely useful apparently in some of the calculations that were made.

ASPRAY: Let me ask the same sort of question a slightly different way. At the time the Navy was giving fairly strong support. They were paying for all these staff members. What practical benefit did they think they were going to get out of having the computer there?

BLOCH: Well the computer was at various times being sponsored by the Bureau of Ships and the Bureau of Ordnance. The advantage to them was that certain computations were going to be made with the machine. We did some ordnance tables for example with the Bureau of Ordnance. We also did some special calculations as I recall: curve fitting, polynomial curve fitting, that were wanted. They came out with a few problems that they wanted to have solved, and I would say that that was the prime motivating factor. I must say that probably a second motivating factor was Howard Aiken himself. He was quite a salesman. Furthermore the machine was, as you know, quite a prodigy in its day and there was a lot of prestige involved in the thing. It was the fastest way to get things calculated, and the mere fact that the Navy had called on the machine was regarded as quite important. There was nothing else that was available for some while.

ASPRAY: I see. So the Navy was quite happy with this.

BLOCH: Oh yes, they were. I won't say that they made full use of it necessarily, but I feel that they got their money's worth out of it.

ASPRAY: Okay.

BLOCH: There were other changes that were made in the Mark I from the time that it was originally brought in to enable various things to be done. They had arrangements later, after I left, for the interpolators which were meant to actually give some automation to interpolating functions which were not regular functions. But they could also be used for regular functions by having differences or derivatives given and then using difference techniques to build up the function for any value between the two gross listed values. This was not used that often. I made some use of it when I was there; but since they were essentially dormant, a lot of the calculations was decided to make one or more of them a sequence mechanism. So that you could now call in a full tape sequence the same way we had called it in from the main sequence mechanism. That was done. There were many other changes that were made that had significance in the technical fashion that would be appreciated by somebody programming the machine. But it won't make too much sense to speak of them here. They enabled you to get away with certain coding stratagems that would otherwise not be possible.

ASPRAY: Were you involved in the design of Mark II?

BLOCH: Not really to any extent. Bob Campbell was involved in the Mark II along with Aiken. As a matter of fact, I spent almost all of my time on Mark I and was responsible for all of that during the period--that of course being the only machine that was operable ______________?____________ during my stay there. I got to be the resident chief of operations, you might say, of the Mark I. I knew what was going on in the Mark II and I may have chatted with Bob some, suggested attacks and so on; but fundamentally I was not involved.

ASPRAY: Does that mean then, for example, if somebody from another department at Harvard came and had a problem in mind or somebody came from outside, you were the likely person to work with them on trying to see if it was a feasible problem for the machine and working on a strategy for solving it?

BLOCH: Oh yes, there was no question about that. I had other officers in the Navy that I would bring in to do programming under my wing. As a matter of fact, one of the first ones was Grace Hopper. She came from Vassar and, as I like to remind her, she didn't know a computer from a tomato basket at the time. She does know the difference between them now, without a doubt. But this was her first experience in the field and I remember sitting down, I think, long into the night, going over how this machine worked, how to program this thing, and so on. Later we got additional staff in and when either Harvard or the Navy--Harvard had some contracts also with the Navy and so indirectly those things might filter through--I was involved. We had correlation, multiple correlation problems. We had those curve fitting problems. We had ordinance problems. These various problems were brought in and were some of the primary problems that we dealt with. There was a great deal of activity in this business of curve fitting and using polynomial coefficients of polynomials, and we spent though an enormous amount of time on those Bessel functions.

TAPE 2/SIDE 1

ASPRAY: Were there some problems which were suggested for computation on the machine but were deemed inappropriate for some reason or other, whether they were too large or otherwise inappropriate?

BLOCH: Actually, since the machine was obviously able to perform all the elementary arithmetic functions, and since any problem if it's solvable at all numerically requires no more than those functions, the machine could solve any problem. The question was time, and the only instance I remember in which time became a tremendous issue was in the problem on the spherical shock wave which I worked on with von Neumann. There the fineness of the mesh really determined the accuracy of the solution. In some instances, if the mesh was too coarse, you would literally get an incorrect solution. This is generally true of mesh- type attacks because there were discontinuities and ? points and so on. The time required went up as a square of the mesh fineness, and we found that we were compelled to stop at a certain point because, if we went one more degree, if we tried to even have the mesh beyond this point, this would require four times the time. And we were getting to the point where this thing was operating on a 24-hour basis you know, around the clock. So I remember that, out of expediency, we had to stop at a certain level because it was just going to be too long. I guess the war would have been over by the time the machine would tackle it at a higher degree. I don't remember refusing any problems that were brought there because of inappropriateness on the machine. There was a fair filtering, I guess. It was known of course that it was a digital machine, and any problems were fundamentally analog in nature never showed up for the machine. They might show up for the differential analyzer.

ASPRAY: Right.

BLOCH: So I can't really.

ASPRAY: I was wondering, for example, were there any problems where there was too much input and that caused a difficulty?

BLOCH: No, as a matter of fact, most of the problems had, if anything, too much output, but not input. The machine was considered a mammoth number cruncher for its day. Typically that implies small dimensions in input but huge dimensions in output. That is a great deal of work done on a relatively small amount of initial information. For example, I don't even think there was more than ten input parameters, period. Nevertheless, because the machine in this instance had to put out intermediate results which had to be put in in the next step of the mesh, there was no way that the machine could store perhaps 800 pieces of information on one mesh ______________?____________. So what we had to do was punch this out on cards. This was the machine's sole form of reentrant output. Since there was not any such thing as magnetic tape at that time, we'd have stacks of punch cards, sometimes drawers of punch cards, which the operators had to obediently store and then funnel back into the card reader for the next session. Strictly speaking, these were intermediate steps along the way that we were forced to take because of the inability of the machine to store more than really 72 registers of information. But I don't recall problems of too much input and I certainly don't even recall our having any mammoth problems with time taken to calculate because whatever the machine did was so much better than was able to be done by hand. If it was essential to get the calculation done it was brought there. We also did some matrix inversions, things of this nature, and somewhere in the archives I think we have all the problems that were run. But I haven't gone over that for many years.

ASPRAY: In the development of techniques for inverting matrices, did the staff innovate numerical techniques for this?

BLOCH: I don't believe there was any great innovation of note there that I recall. We did have innovations in performing, using ? techniques, square root, one over the square root, elementary functions of that nature. But I don't recall there being any special numerical attacks that we developed in matrix inversion.

ASPRAY: Along those same lines, aside from the innovations in design of equipment, were there other kinds of theoretical innovations? I know that later Aiken was quite interested in switching circuit design and theory.

BLOCH: That was worked on at a later time than my stay there. Most of the innovations that I knew of were related to the machine itself rather than to general computational attacks--with the possible exception of the von Neumann problem where the attack itself was relatively new, such equations not having been attempted numerically at all. The work that was done on areas such as the switching theory and so on were not really related to the machine itself.

ASPRAY: Fine. I'd like to turn to your post-Harvard days in just a moment, but I wanted to give you a chance, if there were any other kinds of things that you think of about the Harvard days that you'd like to get on tape for posterity's sake.

BLOCH: Well, I would like to say that I think that Howard Aiken accomplished a great deal at a time when the tools were few and far between. He clearly came up with a machine, and probably the first machine really that could be called a computer today, one which was essentially automatically sequenced. He also should be given a great deal of credit for concentrating on what the machine did and for whom it did it and the accuracy with which it did it. Certainly during my time and I think even later, he was not involved in wanting to win any races in machine design. I think that's overlooked. His concentration was that he got a kick out of machine design at the early stages when it was really new to the world. He did some pioneering there. His concentration soon after that was on solving problems and he was a stickler for accuracy. Even though some very important inventions came out of Harvard, some of which are not generally recognized as coming out of Harvard and out of Aiken's group, he did not really want to engage in battles that subsequently came up involving who was the first to do what with which. He certainly was, as far as I am concerned, a great pioneer, a tough task master. I remember that he'd come in at any hour of the morning or night and sometimes he'd show up at four in the morning. I might be there trying to get some bug out of the machine. And he'd show up having had, I don't know, three or four hours of sleep and his comment was "are we making numbers?" That was a favorite expression, "are we making numbers?" His big satisfaction, once the machine was designed was to see this output coming forth. He was very nervous when the machine stopped. That to him was ? . The reliability that he wanted to bring into the machine was there so that those results would be accurate. By the way, as far as checking is concerned, there was no automatic checking at that time, it was done by program. The trick there was to arrive at the solution by two different methods, or by an inversion of the process. I remember we had been spending a great deal of time making certain that if it were done in two different ways that truly were different a particular bug could not possibly be in the machine and show up so as to create the same wrong answer in both cases. What we did was to compare the answers, the idea being that if they were correct, presumably everything was okay. I think the fact that the huge computational job, for example in those Bessel functions, stands to this day, is proof that this intricate checking that went on and filtered out just about anything that could have possibly occurred in the machine.

I knew Howard Aiken very well at that time, and then there was a period afterward when I didn't see much of him, and then I saw him before his death fairly frequently. He had interests in some new developments, interestingly enough in miniaturization of computers for home use and so on. He foresaw the advantages of having not only mini-circuits, but mini-input and mini-output devices. He was talking to me about even setting up some companies to probe this and move into this area at the time. So he kept his aggressive thinking in the field going all the way really to his death. He was a man that kept up with the field all the way through and to his last days was still conjuring up new ideas. I sometimes think that because of his rather brusk personality, as subsequent events turned out, he was perhaps not given the credit that was due him in many ways. He didn't seek it and he didn't seek to patent anything. He wasn't interested in monetary reward. He had a way about him which either endeared him to somebody or made him absolutely ferocious. I think to the extent that he was in the latter mode, it didn't help him at all in later days when people were trying to dole out credits for what had happened in the field. So I really think that it's a little bit unfortunate that he hasn't been given what I believe to be his due place in the field. I certainly know that at the time I was there we were putting out information from equipment when no one else was doing so. It clearly was the first practical application of the computer as we know it today.

ASPRAY: Alright, let's turn to your post-Harvard days. You said you left Harvard in 1947. Why did you decide to leave the laboratory?

BLOCH: Well, that's rather interesting. Aiken thought that I really should. In the latter part of my stay I went into civilian status and upon doing so became part of the staff at Harvard as a research associate in the engineering sciences, applied physics division. Aiken felt that I really should get into professorial ranks there and remain in the academic environment. I tended to feel that I was destined to move into industry and that there might be greater challenges there even though I think I would have been perfectly comfortable in academic circles. The idea was that there might be greater financial rewards at the time. I was given an offer by Raytheon to come and work as a senior engineer on the design of a computer project which they were bidding on. That sounded very attractive. Bob had gone over there about three months earlier and of course that's probably one of the reasons why I was called upon by Raytheon to consider joining them.

ASPRAY: Just for the record, that's Bob Campbell.

BLOCH: Right. And so I did. Of course I did not know where the future was going to lead. I went to Raytheon in about March of '47. There Bob and I worked on a design with which Raytheon won a contract from the National Bureau of Standards for a machine for the Navy. It was to be used especially in the reduction of data coming from telemetering sources. That work began in '47 and went on until about '49. We proceeded with the building of RAYDAC. I think Bob Campbell left in about '49 or '50 and I took over his section, which came to be known first as the analytic section of the engineering division and then later became the computer division. I was in charge of both the design of the hardware and the software side of the RAYDAC, which eventually went out to Point ? in California in the early '50s, about '52. The thing that I was proudest of was the fact that I came up with the automatic error detection system. That was work I had done sometime in '47, the original notebook work dated I think from '47. What it amounted to was an extended version of today's parity check, but it was much more complicated than that.

The parity check today is a degenerate version, you might say, of the checking that was shown there. It was much more expansive in other ways too. It was patented and involved the ability to check binary to decimal conversion and decimal to binary conversion automatically. It also dealt with the checking of arithmetic operations as well as transfer of information. It still used what I called weighted count techniques; that is, binary weights were applied to various columns of the numbers, and by doing that you had very interesting relationships that (emanated?) when you went binary to decimal, decimal to binary, the checking of a multiplication and so on. It relates a little bit in principle to what you might call casting of nines except that it was much more complex than that.

ASPRAY: Yes. Was that an idea you had in mind when you started to develop this technique of casting of nines?

BLOCH: Well, no not really. I simply wondered what would happen if I applied these weights and I played around with the idea of having something that was a small version, if you will, an abbreviated version of the number itself. And I said to myself, "well, what happens if we overlap five binary digits at a time or four binary digits at a time. I started playing with this and I didn't really think of it as something that might be the equivalent of casting of nines, but it is a technique that does relate really to that. I mentioned in the write-up of the patent that a degenerate form of this would be weighting all the columns' unity where one could have a binary indication, binary counter, which would reflect whether you had an odd or an even number of ones. I kind of stated that that's a simplified version of things. We didn't use that but it was described. Subsequent to that time, of course, the parity check has been used ever since.

ASPRAY: Wasn't it somebody at Bell Labs that suggested that the more simple form of the modern parity check? I don't know the history of this.

BLOCH: No, I don't think so. The first instances of the parity check and weighted column checking was put into the RAYDAC machine. I don't know where that occurred first because we had the more complex version with weighting other than just unity of each column.

TAPE 2/SIDE 2

BLOCH: This technique was controlled by this patent and certainly there was nothing known of it in its use or suggestion or anything ? at the time I came up with it. The patent has held and the fact is that it was used by companies that I've been associated in defending themselves against IBM and others that would come against our company for possible patent infringement in other areas; whereupon they would take the Bloch patent and hold it up and say "now you've got a problem." It was utilized in an honest-to-goodness machine. Later on a machine I developed in 1957 or so, also automatic correction built into it. I called it orthotronic control. It was built into the DATAMATIC machine which came about shortly after Honeywell picked up this Raytheon division.

ASPRAY: So you went with the division to Honeywell.

BLOCH: Yes. It was my division that really was the inauguration of Honeywell into the field. Honeywell had made control devices previously.

ASPRAY: That's right.

BLOCH: This group consisted of some of the best engineers in the field. In size the group was comparable, if not larger than, the number of engineers that were working at IBM in the mid 1950s. Certainly in the early days that may well have been true. It was quite a group that we had there and they were responsible for some of the earliest and finest magnetic tape drives, magnetic tape heads and multiple heads. In fact we produced those for others and I remember that the group at Princeton--Goldstine, von Neumann and so on--who were working on the MANIAC at the time. They wanted to use our tape mechanisms in that machine. Raytheon bowed out of the field in '55 after certain government contracts were no longer available. Raytheon was heavily dependent upon government work and still is to a great extent. I had some moonlighting going on in my operation, looking for a commercial version of RAYDAC which we dubbed privately "the RAYCOM." We did a fair amount of work on this in the attempt to get Raytheon to sponsor a commercial version of this machine with its own funds. We knew what was going on at the time with Eckert and Mauchly and Remington Rand.

ASPRAY: You could see Raytheon as a competitor to Eckert and Mauchly? Could they bid on the same contract, was it the Census?

BLOCH: That's right, it was on the census machine. Because we were binary, we ended up doing the Navy machine. The Census contract was given the Univac, presumably because it was a decimal machine.

ASPRAY: I see.

BLOCH: We were dubbed the scientific version and they the commercial version. By the way, that machine ran for many years and used mercury delay lines for storage. It had, as I recall, water cooling. It was quite a group. They became the Datamatic Corporation in 1955 and were jointly owned. Raytheon didn't let it go completely; they owned forty percent, Honeywell sixty percent. The management at Raytheon that I tried to convince to stay with computers did not believe that there was really a future in the commercial area of computers. Their evaluation was that there may be a market, but not to the extent that it was worth going into it. So they accepted what amounted to something like four million dollars and turned over everything.

ASPRAY: Oh, I see.

BLOCH: People and developments and everything else went over to this Datamatic Corporation, which then became in 1957 the Datamatic Division of Honeywell. Raytheon decided not even to keep its 40% interest in the field.

ASPRAY: I see.

BLOCH: Thus Raytheon bowed out of the field. They came back since but never to the degree of being a chronic main-frame competitor which they easily could have continued to be had that operation continued. So this was the beginning of Honeywell. But in the course of developing this Datamatic machine, I came up with orthotronic control which essentially enabled one, through redundancy, to have the machine correct its own errors. It certainly operated very well. It was a case of being able to spot a single bit error simply by having a two-dimensional array similar to horizontal and vertical checking, and if column 3 did not check and row 4 did not check, then the intersection of 4 and 3 represented your point of error. It turned out that this, when combined with the weighted count, was quite powerful because the weighted count was applied in both directions. Now you could actually get multiple errors literally picked up.

ASPRAY: That's nice.

BLOCH: In fact, it took some very curious types of coincidences to negate the correction and give a false correction. It really required that a bit was gained in one column and lost in another. Furthermore, they had to have the same weight; so they had to be precisely five columns apart, and so on. We used this to great advantage. I remember one demonstration we had. We had broad magnetic tape, perhaps three inches wide, which gave us 32 columns or rows. One of the rows would be devoted, for example, to name and another row to address and another row to city/state if it were a record of a subscriber or something like that. For the fun of it we took the home address, street and number, and erased it all the way through the tape, literally erasing that channel. At one point we disconnected the head of the ? to emphasize the fact that this was being done. What happened was, we would then go back and read that tape and resurrect every record correctly. The comment, of course, was "well essentially the number and street address must be elsewhere in the machine." In a sense they were right, but not really. That was a very powerful use of redundancy. It was much more efficient in practice than anything like the ? check, which gets very redundant if you start going into double error detection or single error correction and so on. We had double, triple, detection and correction in terms of bit errors with a redundancy of less than 10%. It is true that you could know just the right type of error and beat the system. You could have a double error just in the right spots but the probability of that is very small. In practice it worked out extremely well.

I headed up the Raytheon operation, came over to Honeywell and directed the product development of the Honeywell line of equipment. I was director of product planning first and then became vice president for product development. We embarked on a series of machines that eventually was called the 800 series and the 400 series, and then came the 200 series.   The 200 series machine really was a fabulous machine. It was multi-programmed and had all of the correction and detection features in it. In the previous machine, the Datamatic 1000, we had the ability to hold the results of the previous operation until the machine had checked them and then went on to the next operation. When that operation was finally checked, we could erase memory of the previous operation, move that back over so that at any time an error was detected, the machine automatically reverted to the previous operation as though the current operation had never occurred. Then it proceeded from there. It would give you some blinking lights saying, "hey I've caught an error. I know what to do about it. Don't worry about it." It might print out where the error occurred, but the machine never stopped. This was, in essence, the beginnings of the non-stop or fault-tolerant machines. It was some time of course before that came back into vogue.

It's curious how some of these things I mentioned in the Aiken days come back much later. There were many attributes there that have come up two or three decades later which were really foreseen at that time. So Honeywell became a very strong element in the '60s, but it was based primarily on a 200 series machine. The 200 series which we designed had the capability of gear-shifting into an emulation mode of the 1400 series of IBM or into its own mode. In the emulation mode it had actually ? the IBM instructions, and run them just about as fast or maybe even a little faster than the original IBM machine. If you went into the native mode, it went very much faster. This whole thing was called "the Liberator attack" which said "you don't have to throw your programs out, you just take them and put them on this machine." This was probably the first instance of the transportability of programs. There are a lot of other firsts that I've seen as part of our operations at various times during this period. For example, when we had the correction on tape, we put full-page ads in the Wall Street Journal. I remember getting a call from my friends at IBM who said, "what the hell was this all about?" I got called down by Watson saying, "what is this thing? Does it really work? I mean, is it true that this machine can correct its own errors?" And you know I told them what it was about and they said, "well, okay thanks for the information." All they wanted to know was, professional to professional, what the hell was this all about. So I told them. IBM engaged in a campaign that said, "that is not the way that you want to pick up errors. The way you want to pick up errors is at the source. Therefore, what you should have is what we have--namely, read-after-write-head. You'd write, pick it up by the read head and immediately verify, even though the purpose was to write whatever was written on the tape. That was what you wanted and it was very nice. But it had one defect. Once the tape was written correctly, it is put in a file ? . If a flake of magnetic material or dust gets on the tape and it's read three days later, which is, of course, the purpose of putting this on tape, an error occurs. You're dead at that point. There's no way you know that you would know that. We said, "look, what we're interested in is that the time you read the tape, not at the time you write on the tape, is when you better have the correct information coming in. We don't care whether the error occurs in writing or in reading, but if it occurs in writing, we'll pick it up on the read side by the redundancy check. If it occurs in the read, we'll also pick it up on the read side. We don't care really what caused that error to take place."

Well, IBM's attack was quite successful--as it always has been. They didn't really lose a great share of the market although Honeywell did a substantial job of becoming a major element in the second tier of the market at about this time. There was only one in the first tier. Interestingly enough, more than a decade later, IBM came out with a self-correcting approach on tape. It was praised very highly. It's interesting to note that this has occurred many times. Something will pop back up under a different label or with a different code on it, people will look at it, and unless you have a perspective that goes across all of this time element--which very few people have--you won't recognize this apple popping up in the basket again. It's really quite amusing to see this sometimes. Its happened more than once.

So the Raytheon experience led to Honeywell. I was at Honeywell for some 11 years. We started at zero and, by the time I left, we were at about 500 million in annual sales. The company had it made at that time. I was anxious for Honeywell to not let its guard down. It made great inroads, but I was anxious to move up into IBM land a lot more quickly. I had some differences of opinion at this point with upper management, that is, between myself and those who felt that we really didn't have to go into some of the grand developments that I had been thinking about. It was clear they were going to be taking a more conservative posture, I always like an aggressive posture, and so I left in '68.

There are other things that I participated in which I could mention, especially inventions or concepts that I came up with. One was the use of bar code. There was no earlier computer usage of bar code or any other usage that I knew of. I think there was some marking on railroad cars or something of this sort that would be picked up. My concept would literally be able to put out returnable medium on a printer. At that time, optical character recognition was in its infancy. But, even so, it required very fine printing. You are not going to get that from high-speed printers. You don't even get it out of high-speed printers today with regard to set print type although it's much better. I set forth a concept whereby we would have bar codes actually laid down by the printer. We sold Metropolitan Life on this idea. Their premium bills went out with everything in English and Arabic numerals, but at the bottom they would have in bar code the information necessary to be fed back into the machine indicating that the payment had been made and who made the payment. It had very high accuracy. This was the year in which also the American Banking Association had decided to go into magnetic character recognition. I suggested to them that this be in bar code form. I feel to this day that if this had been done, some very inexpensive sensing equipment could have been developed with great accuracy. Bar code had extremely high accuracy. The one negative was that people said, "my gosh, you can't read it with your eyes." We said, "your eye isn't going to read it. It's the machine's eye that's going to read this." They said, "you know, how do we know that that actually represents the numeric information." The argument had some strength to it, but fundamentally the fact that you couldn't read it by eye didn't matter. Its inherent accuracy was what really counted. Today you find broader use of bar code t/an ever before. When I developed it, it was used on the coupons that were in cigarette packs. We developed a reader that would read a thousand of these coupons a minute. Today, this bar code is being used, as you know, is in standard use in supermarkets and at race tracks. It's interesting because the fact that you can't read it by eye proved to be a big advantage. They don't want you to be able to see it by eye. It's used in lottery systems. When you put this into a machine, it reads the bar code very easily and it's been a big winner in the automatic reading area. I did this after a short stay with Auerbach where I was vice president of corporate development. I've known Ike Auerbach for years.

ASPRAY: You'd worked together at Raytheon, hadn't you?

BLOCH: No, no, Ike I knew from way back. He had been with Burroughs.

ASPRAY: Oh that's right, excuse me, yes it was Burroughs.

BLOCH: I think he had worked with Bob Campbell for a while there. But I knew Ike going back into the late 1940s. He was one of the early pioneers in the field. He was anxious to build his company up and so I joined him. I was there only about a year before some head-hunters arrived on the scene. At first I paid very little attention to them because they were talking about GE. GE's reputation in the field was not one of the best in terms of accomplishment. Everybody knew they had plenty of money, but they weren't regarded as a stellar group in the field. But they twisted my arm far enough by telling me that's the reason they wanted me there was to bring them into a major position in the field, as they had in lighting, motors, and so on. I went there in 1968 as division general manager of their main systems division. I had the role of developing a whole new product line for GE. I worked with small and large systems intended for absolute competition with IBM. The goal was to be number two in the market. We proceeded until GE walked into some financial problems trying to maintain its flow of money into nuclear powered jets, jet engines, and computers.

TAPE 3/SIDE 1

BLOCH: A decision was made at GE to get someone to join forces with them in the computer field. The plan that I set forth was going to require some $500 million dollars of investment in a five year period. GE, as large as it was, was being stung at the time by a huge strike which had cost them a couple of hundred million dollars to amend. The result was that GE exited from the field and, of all things, the buyer was Honeywell. Naturally, that was the end there. We had developed a tremendous plan segments of which Honeywell subsequently used. It was the "master plan", as we called it, for the entire new product line at GE. We were building up a tremendous staff in terms of confidence, bringing in a lot of new blood from the outside, including IBM. It was cut short and GE exited from the computer field in the sense of being a builder of equipment. They retained their timesharing services operation in Bethesda. I next moved into the private sector and got involved in venture capital activities, acquisition divestiture activities, and certain high-level corporate hand holding operations with various companies through the 1970s. In 1979 I took over as chief executive officer of a company in the field of artificial intelligence.

ASPRAY: What company is this?

BLOCH: It is called Artificial Intelligence Corporation. It had a great product idea, the use of natural English for querying a database in actual English. I had done some work with _______________________________ , but the company was not in good straits financially. I guess it was almost gone. I picked it up and brought it up here from Washington to the Boston area and really got a very large contract with ___________________________________ and also with Honeywell. I then brought in a chunk of venture capital. I was there for three years. I've since sold my holdings in the company and it's doing well at this point. Now I'm anxious to grab hold of some other challenge. Right at the instant I'm looking at a very exciting possibility in signature recognition in real time.

ASPRAY: In real time?

BLOCH: In real time, based upon the characteristics of the signature in terms of velocity and acceleration with which the signature proceeds. The work has already been done to a certain extent by a company here. I'm considering lifting for a consideration that work out and setting up a company to exploit this. It's a very exciting area with obvious implications in areas that are very troublesome today; namely, unauthorized interference in networks via jokers coming in on unauthorized terminals, secrecy in terms of networks of the government, and so on; but also in financial transactions, credit card transactions, where there is yet to be a good system for verifying that it's you. The developers have already shown that the characteristics they are studying mathematically here, that are pulled out mathematically and have correlations made, they have shown that if you were to be able to trace the valid man's signature literally and then put it through the system, put his signature in real time as though he was signing it and trace it perfectly (Let us argue that it's a perfect tracing which is highly unlikely, but let it be.), this system--because it's already been shown to operate this way--would show on a scale of 100, 86 say for the valid man and the forger would come through 18 or 20. Unbelievably! The reason for this is that it's neuro and bio impulses that control the speed and various portions of your signature, and these are uncontrollable and unrepeatable by another person. I got very enthused about this thing. So I may, if we can get some substantive funding of this thing, get involved. I've got a group of people that I'm prepared to put on this.

As time went on, just to go back, I started out in engineering--never having taken an engineering course. Nevertheless, I started out really by virtue of picking this all up at Harvard in the early Harvard days. So that everything I learned was really self-taught in this area not the mathematics area, but certainly the applied engineering area. I was very heavy on the technical side, of course, all the way through. Except that as time went on I got involved more in management and marketing and general management, and I would say that 25% of my time at the time I left Honeywell was in technical monitoring and 75% was in more upper management duties, general management. As time went on at General Electric it became wholly General Management and there were you know bright people in the technical area that were making contributions that perhaps corresponded to the kind of contributions I had made twenty years earlier. Although I still like to feel like I can understand these things, I'm not as technically active as I was. I also got involved in the financial area which comes along with general management. As a matter of fact, in concert with a west coast executive, I run a private fund called the Equity Option Fund using a mathematical model which I developed which controls the timing and the quantity of options to be written. It is, worked out very well as an investment medium. I got quite a kick out of developing it because I had to delve back into the books, some of which I hadn't cracked since my Harvard days, in order to come out with some of this. So you know I'm always intellectually active, I guess, in the area of mathematics, applied mathematics, and so on. But now I guess I'm getting to be viewed if I'm not careful as an older statesman. That's a brief history of my career to date.

END OF INTERVIEW