An Interview with

ROBERT V. D. CAMPBELL

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Conducted by William Aspray

on

22 February 1984

Concord, MA

Charles Babbage Institute
The Center for the History of Information Processing
University of Minnesota, Minneapolis

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Abstract

Campbell discusses his work at the Harvard Computation Laboratory and his subsequent career in computing. The interview begins with a description of Campbell's early life, through his graduate education in physics at Columbia and Harvard. He recounts how Howard Aiken chose him to work with IBM on the latter stages of design of the Mark I calculator while Aiken was on active duty in the Navy in Virginia. Campbell describes what he learned from Aiken about the plans for the Mark I in the late 1930s and the arrangement reached with IBM to build the computer. He assesses the relative contributions of Harvard and IBM to the Mark I project based on his own experience at IBM's research facility at Endicott, NY. He then describes the formation of the Harvard Computation Laboratory, the operation of the Mark I there, and the work beginning in 1945 on the Mark II calculator for Dahlgren Naval Proving Ground. Topics covered include the controversy between Aiken and IBM, Aiken's personality, Aiken as an educator, and Aiken's attitude toward the computer industry.

The second half of the interview covers Campbell's later career at Raytheon (1947-1949), especially the search for adequate storage devices and RAYDAC installation at Point Mugu, CA; at Burroughs (1949-1966) in his position as director of research and in a staff position for program planning; and at MITRE (1966-1984) on long-range planning with the Air Force, and project work on a data processing system for the state of Massachusetts and the city of Newton, MA.

ROBERT V. D. CAMPBELL INTERVIEW

DATE: 22 February 1984 INTERVIEWER: William Aspray

LOCATION: Concord, MA

ASPRAY: Bob, could you tell me something about your early days? What did your parents do when you were born? What was your elementary and public school education like?

CAMPBELL: Well, my father had a rather unusual name, Percy Alphonso Campbell. He was the fourth child in the family. Two older boys and a girl. He was born in Hastings, MN., but grew up mostly in southern New Hampshire. My mother's name was Mabel Van Duyne. Two words. A Dutch name. In fact, when we were in Amsterdam we saw a lot of Van Duynes with that spelling in the telephone book. She grew up in Newark, New Jersey.

My father went to MIT briefly and didn't like it too much. So he dropped out and later graduated from Harvard. He went to work for the General Electric company and was concerned with the design of lamps (of lamp bulbs). He was a lamp development engineer and worked just outside Newark, New Jersey. (He was concerned, for example, in the change from outside frosted to inside frosted bulbs.) He and mother married about 1914 and I was born in 1916. I went up through part of fourth grade in Newark, New Jersey. My mother taught kindergarten before they were married. She had quite an artistic bent. She built a tip-top table, built metal trays, did some painting, and so on. During the time I was in fourth grade we moved to East Cleveland, Ohio. My father was transferred out to Nela as it was called -- a facility of General Electric Company just outside of East Cleveland. We lived there through my graduation from college. I graduated from Shaw High School in East Cleveland. During the time I was in 10th grade my father and mother separated. Later they divorced. I lived with my mother after that. I went to MIT and graduated in Physics in 1938.

ASPRAY: Before we move on to your college days let's talk a little bit more about your earlier activities. Were you interested in Science when you were young?

CAMPBELL: My father got me a 3" refractor on an equatorial mount tripod when I was about 12 years old. I've been interested in astronomy as an amateur ever since. I guess I had a general interest in things scientific and technical. We did chemical experiments and had salamanders, a chameleon, and an opossum as "pets". Also, I went to art school at the Cleveland Museum of Art. There was drawing and clay modeling and so forth; but I never became terribly proficient at it.

ASPRAY: What about music?

CAMPBELL: No background in music at all. No. I enjoy listening to music, but I never played instruments.

ASPRAY: How was your public school education? You went to public schools, is that right?

CAMPBELL: Right.

ASPRAY: Did you think that they were particularly strong or average?

CAMPBELL: Well, the high school in East Cleveland I thought was quite good. I had a year of physics and a year of chemistry. And mathematics throughout the period. I think it was quite a reasonable education. At that time, quite differently from today, you could get into MIT without examinations by being in the top fifth of your class in an accredited high school.

ASPRAY: Was this true in your case?

CAMPBELL: Yes.

ASPRAY: Why did you decide you wanted to go to MIT?

CAMPBELL: I think my father's brother, Arthur, influenced me a little bit. My father wasn't too keen on MIT. As I said, he transferred to Harvard. My uncle, I think, influenced me substantially.

ASPRAY: I see. So then you matriculated as a freshman in what year?

CAMPBELL: 1934. That was a good year.

ASPRAY: I see. Did other people from your high school go to MIT with you?

CAMPBELL: I don't think so.

ASPRAY: What was your major?

CAMPBELL: Well, I started with the idea I was going to major in chemical engineering. I don't know why exactly. But at the end of my freshman year I changed it to physics. So I got my degree in physics.

ASPRAY: What courses in mathematics and electrical engineering did you take?

CAMPBELL: I got the basics in electrical engineering, including the old electrical machinery laboratory work which has long since been disbanded at MIT. The laboratory made measurements on motors and generators. One got a pretty good background in basic electrical engineering and, of course, one had to have a reasonable background in math. In fact, one took calculus concurrently in those days with using calculus in physics. Sometimes the calculus notions were developed first in the physics class and sometimes in the calculus class.

ASPRAY: I'm aware of that. That happened to me in college also. Did you do any numerical computation? Numerical analysis?

CAMPBELL: Not at that time. No. (Only slide rule calculations.)

ASPRAY: The engineering courses that you took, were they primarily power engineering courses?

CAMPBELL: Well, of course the field of electronics had not developed to the extent it now has. I had just broad electrical engineering, I guess like power engineering now. (I don't remember taking any course in communications engineering.)

ASPRAY: Do any of the faculty members stand out as being important?

CAMPBELL: Well, Phil Morse. I don't know if you've heard that name or not.

ASPRAY: Yes.

CAMPBELL: I had a course in acoustics. No, vibration and sound I think it was. Slater who was chairman in the Physics department. I had a philosophy course under Norbert Wiener.

ASPRAY: A philosophy course under him? What did he do in that class?

CAMPBELL: Oh, we talked about some of the major philosophers' ideas and so forth. That was one of the "general studies" that they had at MIT. MIT had a somewhat more narrowly focused education in those days than it does today I think. I think one now gets a somewhat broader education at MIT than I did. I had a general studies course in philosophy. I had one in genetics and one or two others. These were only a couple of hours a week. Not a terribly intensive course, but they required them.

ASPRAY: Was it typical for scientific faculty members to teach these general courses? After all, Wiener wasn't trained particularly in philosophy was he?

CAMPBELL: Well, he was a mathematician with fairly broad interests. There are a good many people who are both mathematicians and philosophers, I think.

ASPRAY: I guess that's right, but today it would be quite uncommon for a mathematician to teach a philosophy course at an undergraduate level. Not unheard of but certainly uncommon.

CAMPBELL: Perhaps, they did tend to use some senior faculty for that purpose. Of course, Wiener was sort of a law onto himself anyway. As you probably realize, he was known as the classic absent-minded professor. You possibly know the stories about that.

ASPRAY: Despite that fact was he in some way special or inspiring? Did you enjoy going to his class?

CAMPBELL: Yes. He was stimulating.

ASPRAY: Alright.

CAMPBELL: I noticed in the paper today that Frank died. Nathaniel Frank, who was one of the people in the physics department when I was there. In fact it seems to me Slater and Frank was the basic mechanics book used. He died at the age of 87.

ASPRAY: Did you work at all while you were in college?

CAMPBELL: I had summer jobs. After my freshman year I had a summer job as a helper in a little hotel. Not hotel, boarding house really. In Scituate, Massachusetts. Just a general handyman. Then the next summer I got a job with the Lincoln Electric Company in Cleveland. They make small motors and arc welders. Then I had a job with the Addressograph Multigraph Company. These were all summer jobs.

ASPRAY: What sorts of things did you do in the summer jobs with the electric company and with the Addressograph?

CAMPBELL: Oh, various services. At Lincoln Electric I was sort of an engineering assistant. At Addressograph I was just a helper in the mailing room. Then, after I graduated from MIT I had a summer job with Bell Telephone Laboratories on West Street -- 463 West Street in New York. I don't know if they're still in the building or not. That was before they had moved to Jersey.

ASPRAY: I think they moved some time just after the war.

CAMPBELL: I helped design and build an apparatus for measuring specific heat to investigate order-disorder transformations in metals. One of the things that we wanted to investigate was the changes in specific heat depending upon the order-disorder process. Then I went to Columbia Graduate School in Physics.

ASPRAY: This was immediately after the ?

CAMPBELL: Yes, right.

ASPRAY: Why did you choose Columbia?

CAMPBELL: Well, let's see. I was in New York because I was working for Bell Telephone Laboratories. My mother was in New York; in Brooklyn actually. I guess that was the main reason I applied to Columbia. I got quite a stellar set of professors at Columbia. Enrico Fermi was there, as well as I. Rabi. Hans Bethe and George Uhlenbeck were visiting professors there. (The first three of these all became Nobel laureates.) I had some quite excellent professors at Columbia.

ASPRAY: Was this for a Masters or Ph.D. program?

CAMPBELL: Master's program.

ASPRAY: What course work did you do for the degree there?

CAMPBELL: It was mostly quantum mechanics, statistical mechanics, electro-magnetic theory, advanced mechanics, thermo-dynamics.

ASPRAY: Did you work at all while you were in school there?

CAMPBELL: I did some teaching in the Extension courses in the evening. Then after a couple of years I got a job with an outfit called the "Normal Child Development Study" at Columbian Presbyterian Medical Center, uptown at 168th St. They were studying the development of children during the first couple of years of life. Well, during childhood actually. I did some statistical work at that time. I utilized various rotary calculators and ran analyses of variance and correlations. Plus, I suggested a special approach for comparing children's rates of development.

ASPRAY: Was that the first time you had used a calculator?

CAMPBELL: Well, to any great extent, yes.

ASPRAY: What did you teach in the evening courses?

CAMPBELL: A number of Physics courses. I don't remember exactly. I think it was lab courses.

ASPRAY: O.K. So you completed the Master's program at Columbia?

CAMPBELL: Yes. It took three years. This must have been 1941. Then I applied at several places. I wanted to go ahead and get a Ph.D. I applied at Harvard and Cornell and a couple of other places, and got a teaching fellowship at Harvard in Physics. I had about a month off between the end of my normal job at Development Study and starting at Harvard. No I guess it was two months off, July and August. I spent that time studying in the New York public library, reading statistics.

ASPRAY: Then you went to Harvard in the Fall?

CAMPBELL: I went to Harvard in the Fall of 1941. I guess I was a lab instructor teaching mostly freshmen. I thought I would do a thesis under John Van Vleck: I had talked with him in New York City before starting in at Harvard. That was when I was considering going to Harvard. I guess he was visiting there. I took my qualifying exam for the doctorate I guess that Fall.

ASPRAY: Was it typical to take the qualifying exams so soon after you got there? Is this because you had a master's degree?

CAMPBELL: Well, it was because I had a master's degree.

ASPRAY: O.K.

CAMPBELL: We were down in New York. My wife, Winifred (Culik) stayed in New York for a while to finish up a job at Columbia. She was in library school at Columbia. She had graduated from high school in Detroit. I met her when she was at Radcliffe and I was at MIT. I met her at a social event at Radcliffe. I was a sophomore, I guess, and she was a freshman. She graduated in 1939 from Radcliffe and went to Columbia to get a library degree. She stayed a little longer in New York than I did to finish up. She was working on a book. We had been married just shortly after she graduated from Radcliffe in December 1939.

So we were in New York over Christmas holidays in 1941. I got a call from a faculty member at Harvard I didn't know by the name of Howard Aiken. He wanted to see me. So we met in Grand Central Station. He was on his way to Yorktown, Virginia where he was teaching magnetic mine technology. He was a Lieutenant Commander in the Navy, later a Commander, and was teaching in the Naval Mine Warfare School. We met in Grand Central Station. He told me about the computer. He called it "Calculator". It was a large scale calculator which he had conceived and was being built at IBM. Then he had been called up by the Navy. He was in Naval Reserve, and wasn't able to continue working with IBM during the final stages of design. The machine construction was well along. Some aspects of the design were still incomplete, especially those relating to the LOG, EXP and SIN functions. He told me Harvard needed someone to continue the liaison with IBM. This sounded sort of intriguing so I said I'd do it, after perhaps about an hour's conversation. I had never met him before. He had picked my name out of a file at the physics laboratories. I don't know how whether it was because I had done some statistical calculations or what led to his decision.

ASPRAY: Were there a large number of physics students at the time that he might have chosen from?

CAMPBELL: I don't have a recollection. So early in 1942, in January, I first went out to IBM. I spent part of my time at IBM for the next two years. During 1942 and 1943. About that same time Bob Hawkins was recruited from the Cruft Laboratory machine shop at Harvard to work at IBM full-time until the machine could be completed. His job was to learn how to operate and maintain the machine, and perhaps to help with the final stages of construction.

ASPRAY: How did this affect your degree program at Harvard if you had to keep running off to Endicott, New York? It was a fairly lengthy trip by train at that time, wasn't it?

CAMPBELL: Well, usually I'd go down to New York by train and then take the Lakawanna. The Lakawanna would wind through northern Jersey, the Poconos and through Scranton, PA, and then on up to Binghampton. You'd either get off at Binghampton or a little farther on, close to Endicott. Binghampton, Johnston City and Endicott are "triple cities". That was quite a trip in those days.

ASPRAY: How long would you be gone usually?

CAMPBELL: Oh, I guess half a week or a week.

ASPRAY: Were you still taking courses at Harvard at the time?

CAMPBELL: Yes.

ASPRAY: There wasn't much of a problem in doing that?

CAMPBELL: I'm trying to remember. I didn't need to take a great many courses, actually, considering my courses at Columbia. I remember taking a course under Van Vleck in group theory and application to quantum mechanics. I took a course under Kemble in quantum mechanics.

ASPRAY: What did you do on your trips to Endicott? What kind of liaison was there between Harvard and IBM?

CAMPBELL: Well, first off, the whole program was under the direction of Clair Lake who was, I guess, the equivalent of department manager. Frank Hamilton was the working head of the project, pretty much. Ben Durfee was the design engineer who did a large part of the circuitry work. I'd go and check in with Clair Lake and then work with Hamilton and Durfee. At the time I went out there, they had constructed part of the machine. But there was a question of checking over circuitry diagrams and reviewing the procedures which were used with some of the special functional calculations; and later calculating constants to go into the machine for some of the special function calculations. And developing some test procedures. I made two trips down to Yorktown, Virginia to see Howard Aiken. On one trip, Frank Hamilton and I went down, by train, from Binghampton to Lee Hall, VA which was the closest stop to Yorktown. We took the Reading railroad into Philadelphia, transferred to the B & O to Washington, and then took the RF & P to Richmond. Then the C & O (I think it was) to Lee Hall, VA. So it's quite a process to get down there by train.

ASPRAY: Did you feel as though you were more or less carrying out the finer lines of a design that Howard had already put out? Did you feel your responsibility was to see that IBM was carrying out the design plans that Aiken had, or did you feel that you were just a technical person that knew something about some of these areas?

CAMPBELL: I'll have to back up a minute and first of all identify the roles of Aiken and IBM in this whole enterprise.

ASPRAY: O.K. That's a good idea.

CAMPBELL: The role question is a crucial issue as far as the Mark I is concerned. Who is responsible for what. So maybe if it would be alright for me to address that subject a little bit.

ASPRAY: Fine. I think that is a good idea.

CAMPBELL: This is one of my pet subjects. Well, as I think you know, Howard Aiken wrote a prospectus for a computing machine. This was when?

ASPRAY: 1937.

CAMPBELL: 1937. It described functionally a machine which had a rather complete repertoire of characteristics, but said almost nothing about how it might be constructed or what components would be used. What circuitry techniques or what other mechanical techniques as far as that's concerned. It did, however, talk about tape programming I think.

ASPRAY: What did that mean?

CAMPBELL: Well, a sequence control device from instructions somehow encoded in the punch paper (in a tape, not necessarily punched paper). But other than that there was very little dealing with specific components or specific design techniques. So having developed this concept Aiken tried to find a way of implementing it. He didn't have the resources himself. He was an instructor at Harvard at that point finishing up his doctorate degree. He first went to a number of business machine companies. It was at Monroe that he talked to George Chase, chief engineer. Chase was quite interested in the concept and would like to have tried implementing it using necessarily mechanical techniques, but the top management at Monroe was not interested.

ASPRAY: They just didn't see a market for it, or what?

CAMPBELL: Either they didn't want to spend the money for it or they didn't see a market for it either. He went to other business machine companies. I don't know how many. But he was unable to get any interest. Then through Professor Theodore Brown at the Business School and Harlow Shapley in the Astronomy Department he secured a introduction to Bryce -- James Bryce of IBM, who was a senior executive in New York with a long history of engineering inventions. He became interested in Aiken's ideas; and through Bryce it was arranged for Aiken to talk to Watson. Aiken tells a story, probably somewhat embellished, about his meeting with Watson in a management meeting. The meeting started up and two of the members of the management group were not there when he started. There were just enough chairs for the IBM people, so Aiken had taken one of the missing persons' chairs. The first missing person came a little while later. Watson interrupted the meeting and bawled him out for coming in late. He told him to sit down, so he sat down. The second man came in. The same thing happened; but there wasn't any place for him to sit, so he started to leave the room. Aiken says, "Watson bawled him out for leaving the room while he, Watson, was talking." So this may be apocryphal. I have no idea. But Watson did run the company in a highhanded way. There are quite a few stories about him. In any case, Watson became quite interested in Aiken's idea.

ASPRAY: This was fairly immediately upon this meeting?

TAPE 1/SIDE 2

CAMPBELL: A good salesman doesn't sell you. A good salesman gets you to sell yourself. I'm sure Watson was a rather easy person to sell. Of course, he was a very imaginative and far looking type of executive. Of course he saw the sales and publicity. Not the sales, but the publicity potential. So Watson decided that IBM would design and construct a machine to Aiken's concept and would give it to Harvard as a present from IBM.

ASPRAY: This was the plan from the beginning as far as you know?

CAMPBELL: Yes. Now, the original concept was certainly Aiken's. There is no doubt about that. But, it would have been almost impossible for Aiken to realize this concept without assistance from somebody who had the components, the technology, the money, and so on. I think IBM was just about unique among the business equipment companies in having a set of components that were readily adaptable to this type of system. They had electro-mechanical, electrically controlled mechanical counters, electrical relays, cam operated contacts, a concept of a synchronous operation (with internal processes being synchronized with the reading of a card in parallel), and automatic typewriters. They had typewriters that had solenoids to make them automatic. IBM also had a general design approach of electrical-mechanical components and electrical communications between components which very much lent itself to this. Remington Rand had a somewhat similar set of components, but they used largely mechanical communication. In their tabulator they used bowdin wires to go from the card reader to the accumulator. Although you could certainly have built an all mechanical system it wouldn't have been nearly as easy. Bell Laboratories also had a complete set of relay and telephone switching gear capabilities. BTL was working on developing computers under Stibitz and later under Williams. Aside from Bell Laboratories I don't think there was any outfit, in the United States anyway, that had as complete a set of components as IBM did. Aiken has said that if IBM had turned him down he would have gone to RCA; and maybe then Mark I would have been electronic instead of electro-mechanical.

ASPRAY: Do you think he meant that seriously?

CAMPBELL: Well, perhaps. Of course, no one can speculate successfully about what might have happened. If RCA would have been interested and they would have been willing to fund it, it could possibly have been realized starting around 1938, which is before the ENIAC got started. But some new circuitry would have had to be developed. Well, you could put your own probabilities on the likelihood of RCA's interest.

ASPRAY: But knowing Aiken you do believe that he probably would have gone to RCA?

CAMPBELL: Oh, I think he would have gone to RCA. But whether there is any likelihood that he would have succeeded in interesting them is another thing. Even though he was quite a salesman. He was one of the most persuasive persons that I ever met.

Well, to come back to the original point. The design of Mark I was the marriage of the overall concepts of Aiken to the electro-magnetic counters, relays, cam operated contacts, and general design techniques of IBM. Card readers, card punches, electric typewriters with solenoids on it. The only really new device was the program (sequence) tape. The sequence tape unit utilized uncut card stock, so the tape was the width of the card. They had to design a unit to punch that manually and a reader to read it. These used existing IBM techniques, but required unique designs. There were two kinds of tape actually. There was a function tape and a program (sequence) tape. (They used the same type stock, and the same 24 hole positions across the tape, but different coding.) The readers were a little different but they had the same basic techniques. That was a new device for IBM, but used proven techniques certainly. The design was completed before my time out there.

Aiken participated quite a bit in the detailed design. For example, he and Hamilton and especially Durfee were the ones who did a lot of the circuitry design. Relay circuits and so on. I think Aiken participated in quite a bit of detail in that. So there certainly wouldn't have been a Mark I without Aiken's concept. But there would not have been any Mark I without IBM or somebody like IBM. And I don't think there was anybody like IBM in that sense.

ASPRAY: How well prepared were Hamilton, Lake and Durfee for doing this kind of work?

CAMPBELL: Lake, Hamilton and Durfee had no concept of scientific computation. Zero. There was some scientific work going on at the Watson Laboratory.

ASPRAY: Columbia? Eckert?

CAMPBELL: Yes. That's right. There was scientific calculation going on there with punchcard equipment and related equipment. But regarding the people at Endicott, I think, none of them were graduate engineers. In fact, in the business equipment field in general they were homegrown more than academically trained. They were very expert in their line doing the functions necessary for tabulating equipment, including the operations of arithmetic. Although I guess they hadn't done as much commercially with division. (IBM had developed the multiply-divide approach later used in Mark I, however.)

ASPRAY: Just so I have a better feeling for this, can you give me an idea of the kinds of things that they weren't very good at?

CAMPBELL: Well, they had no idea of numerical computations. As a matter of fact Aiken showed them how you could take reciprocals or square roots by iteration. I forget what the processes were.

ASPRAY: Something like Newton's method?

CAMPBELL: Yes. I guess it was Ben Durfee who went away and tried it out and was amazed that it actually worked. So they had basically no concept at all of numerical methods and statistical calculation or mathematical functions, trigonometric functions. They were grassroots engineers. Very good engineers, but the horizon was limited at the time.

ASPRAY: Had they been involved in IBM's other projects with cardpunches and punched card equipment? Did they know the ins and outs of electrical-mechanical technology?

CAMPBELL: Oh, yes. The relay was invented. Let's see. The relay that went into the Mark I was invented by Lake and Pfaff. The counters were invented by Lake and Pfaff. The multiplication scheme was invented by Bryce. I might not have the right name. As far as the arithmetic part was concerned and as far as the arithmetic components were concerned, it was IBM. Aiken had no special knowledge of tabulating machine computing components and technology.

ASPRAY: What about numerical analysis? Was he trained in that at all? After all, he was a physicist by training.
CAMPBELL: Aiken was. I ought to be careful how I say this. Aiken was not a mathematician's mathematician. He was an engineer's mathematician. He was very much interested in numerical methods and numerical computation. As a matter of fact, he was, in some sense, more interested in computing than he was in computers. He developed computers as a tool, not so much as an end in themselves.

ASPRAY: He's rather different from many of the other early people in that regard.

CAMPBELL: That's right. This led to the fact that Mark I was used more in numerical computation than any other early computer I'm sure. Aiken was just as interested in using it as he was in building it. The Mark I ran until well into the 1950s. It had been completed and put into operation in '44. So it's quite remarkable that a machine designed, developed and built that early proved to be as effective as it was in such a wide variety of calculations. This was in large part due to Aiken's interest in numerical computation.

Now he was not a terribly subtle mathematician. There's a little story to illustrate this. Andy Arnold, Lt. Commander Arnold, was one of the officers in the Mark I staff organization. He was working on a computational process and decided he wasn't going to get as much accuracy as he wanted. He had a fifth difference approach to doing some interpolation. Aiken said, "Well, take sixth differences." Well, you know as you take larger degrees of differences your truncation error gets smaller but your roundoff error gets bigger. Aiken didn't seem to appreciate that. Also he said that he felt the formula for the error in interpolation, Nth order interpolation, overestimated the error. But, the formula can't overestimate the error! The formula is an exact formula which defines the error in terms of the N & 1st derivative. The only thing you don't know is at what point the derivative is to be evaluated. That's an exact formula. So Aiken was not a mathematician's mathematician. But he had a very good feel for numerical computation and numerical processes and was familiar for with most of the standard numerical approaches to things.

It's a long way around, but to turn back to who did what as far as Mark I is concerned, it was undoubtedly Aiken's overall concept -- his overall systems concept, his functional requirements. And it was at his insistence that there be some program device which would enable the machine to carry out automatically a sequence of operations, and sizable amounts of internal storage. But the specific hardware and specific design techniques which were used to utilize that concept were IBM's. Aiken did make significant contributions to the detailed design, and had a major role in adapting the IBM technology to meet his functional requirement.

ASPRAY: Let me come back to this question that started this quite valuable excursion. What was your role as an intermediary?

CAMPBELL: Well, the design was completed except for some questions, mostly in regard to the built-in functions: sine, logarithm and exponential. (For some strange reason square root was not built in -- this was odd because square root is the most used operation beyond the four elementary operations of arithmetic.) The design of the functional units and some of the other things were under way. Part of the construction was underway. Certainly the overall character of the machine was pretty well determined by the time I came on the scene. What I did after learning how the machine was designed and built was to help trim up the design in some areas, help with the testing and debugging and write programs for five sample problems that we ran at Endicott.

ASPRAY: This would be in 1942 or 1943?

CAMPBELL: The machine was completed substantially I guess in the Fall of 1943. It was moved to Harvard after undergoing tests at Endicott, and a demonstration of five typical problems. It moved in February 1944 and was set up at Harvard. It was finally dedicated formally in August 1944. During the spring of 1944 I was operating the machine along with Bob Hawkins and Dave Wheatland who was there. You know Dave I guess? I also programmed the first two actual problems solved by the machine.

ASPRAY: Yes.

CAMPBELL: Aiken got himself transferred up to Harvard. He was assigned as a commander to head up the Harvard Computation Laboratory which was to be a facility of the Bureau of Ships.

ASPRAY: How did he arrange that during wartime?

CAMPBELL: I guess that's another example of "a salesman doesn't sell you, but you sell yourself." I think the Bureau of Ships was very anxious to obtain some better computing facilities. And Aiken came along at just the right time. That was quite a thing for him to arrange.

ASPRAY: What sorts of problems did they have in mind that needed computational facilities for?

CAMPBELL: That's hard to answer. We didn't really do too much computation for the Bureau of Ships. I think we did more for the Bureau of Ordnance for their research laboratory. As I recall, there was one early problem we did that was for the Bureau of Ships. Solving ten simultaneous algebraic equations, for a multiple correlation. Correlating the properties of steel with the amounts of various small impurities whose amounts could not be controlled. In other words this meant that you could sample the melt and find out what your concentrations of various impurities were and then you could say "OK, this batch of steel will go for this purpose instead of that purpose." Because of tensile strength, hardness and other characteristics were somewhat affected by these small impurities whose amounts you couldn't control. I'm not sure whether or not they had a list of high priority problems that they needed to solve, or whether it was just a general idea that they needed computation. The biggest problem we did was for the Naval Research Laboratory was the Bessel functions. Which was the baseload problem, I guess, for at least five years.

ASPRAY: I see. When you weren't running other things you'd put the Bessel functions on?

CAMPBELL: Yes. Particularly for running at night. The machine was elegantly suited for the Bessel tables because it did have very high precision. Even so, we sometimes had to run double precision. The machine had a precision of 23 decimal digits, but we had to run a double precision -- at least in the early Bessel functions in order to get the accuracy we wanted. Aiken regarded the computation of tables of function as a major role for automatic computers. Other people had a somewhat different point of view. Wiener and others said that computers were making tables of functions obsolete. But Aiken didn't see that, I guess, at that point. The two executive typewriters produced very elegant copy which could be used in a photo-offset process to print the table of functions directly.

ASPRAY: He was quite anxious for that arrangement?

CAMPBELL: Well, we didn't have to retype them and proofread them. If you've ever proof read tables of functions, which I have, you'd realize why this was a major advantage.

ASPRAY: I imagine that the retyping just introduces more errors than there were in the beginning. He produced, if I remember, volumes and volumes of these decimal functions.

CAMPBELL: Yes. Twenty volumes of Bessel and Hankel functions and some other functions.

ASPRAY: Did you check these against other published tables at the time?

CAMPBELL: There were no published tables with comparable precision. Well, there is one instance. I probably told you this several times. In our program, the calculator would compute the Bessel function and then, using the difference relation, check what it had computed. At one point in the run the computer indicated an error. We assumed that the computer had computed the Bessel function incorrectly and that the recursion process used in checking it had shown that there was an error. We had a table of Bessel functions by Hyachi. We had these for comparisons, although they didn't go out to as many places. We compared this particular result with the Hyachi table and the entry on the Hyachi table was different. So we assumed what happened was that the computer made a mistake and the error checking process detected this. Of course, it would not then compare with the value in the Hyachi table. It turned out that that wasn't the case at all. The value on the Hyachi table was wrong. The computer had computed the tabular value correctly, but had thrown up an error in the checking process. All this shows is that things aren't always as they seem. Hyachi's tables were rather full of errors. Quite a few errors.

Well, to pick up the story, I spent quite a bit of time in Endicott during 1942 and 1943 and was also teaching in the pre-radar school at Harvard at that time for Army, Marine and Naval Officers. A three month course in communications engineering prior to a course at MIT in radar. I taught a quiz section as they're called, assisted in the laboratory, and gave lectures. I gave the lectures on filters and once gave the lecture on detection. I was doing this while I was going to Endicott: it was fairly busy in 1942 and 1943. After the machine was moved to Harvard, Dave Wheatland and Bob Hawkins and I were the main people involved for a little while. I programmed up a problem evaluating an integral used in antenna design and of interest to Ronald King, and a ray tracing problem of interest to Jim Baker at the Harvard College Observatory. Things happen pretty quickly during the war, you know. May or June, I think, Aiken got himself transferred up to Harvard to head up the Harvard Computational Laboratory as a facility of the Bureau of Ships. Aiken reported to a naval project nearby. I've forgotten what it was now. By the time of the dedication in August, there was a Navy crew: five enlisted men to operate the machine and do other work, and five Naval officers including me. I got into the Navy almost overnight. I was an ensign. The other Naval officers at that time were Dick Bloch who was the first person to come other than Aiken, I guess. Then Grace Hopper and Andy Arnold. Hubert Arnold. That was all in place by the time of the dedication in August of 1944. The civilians at that time were Bob Hawkins, Dave Wheatland and Ruth Knowlton, secretary.

ASPRAY: During this summer prior to the dedication was the machine being retested out after its shipment?

CAMPBELL: Well, no. We did these two early programs that I mentioned and other programs were being written for it. It was rather unreliable for about the first year. We had quite a bit of trouble. One of the biggest troubles was what we called "singly swedged" (or crimped) contacts", the wires which went to the sockets into which the relays plugged. The sockets into which the counters plugged were wired with a swedged contact. You know, a squeeze contact. Some of the contacts were twice swedged, i.e., in two places. Some of them only swedged once. The single swedged contacts gave us a lot of trouble. We finally had to get them all out of there.

ASPRAY: Did you solder everything?

CAMPBELL: No. I don't know that it was soldered. I think it was double-swedged. There was also considerable trouble with some of the relay contacts. Anyway, after those bugs were out of it and I guess some other bugs which I've forgotten about, the machine worked quite reliably. I would say within a couple of years it worked 95% of a 730 hour month or 168 week.

ASPRAY: As soon as it was reliable enough to keep going, you had it working three shifts?

CAMPBELL: Oh, we had it working three shifts way before it was reliable. Aiken always set very tough goals. Whoever had the duty would get calls in the middle of the night quite regularly. I lived just off Cambridge Commons on Follon St, and Dick Bloch, as a bachelor, lived in sort of a rooming house next door to the Commander Hotel. So we were unfortunately or fortunately, depending on how you look on it, quite accessible. In fact, everybody lived pretty close, I believe. Oh, we'd get calls anytime of the night. Two of the most interesting programs that went on the machine were, first of all, Bessel functions and secondly, the partial differential equation that had something to do with implosion of the atomic bomb -- although we never knew what it was all about.

ASPRAY: Isn't it true that von Neumann had come up at one time?

CAMPBELL: Von Neumann, Loevner and Bargman; a group of three noted mathematicians, of whom von Neumann was the most notable, came up. They came in to talk to Aiken. They hadn't completely formulated the computational problem or completed the system analysis. So at some point Aiken said, "Why don't you go into another room and decide just what you want to compute and for what ranges of the variables?" So they went into another room and developed a method for it. Dick Bloch was the programmer on both those problems (Bessel Functions and Partial D.E). Dick was really an elegant programmer. He was the best programmer as far as I'm concerned. He didn't understand that you didn't have to program in ink. So he programmed in ink and he never made any errors as far as I know. You have to do this as a programmer. Sometimes I can do it, sometimes I can't. You have to be totally immersed in the problem and get your mind around the whole problem as you're doing it. It's something that requires intense concentration if you're going to keep track of all relationships which are involved. He did a very elegant job. You have to say that basic operational times were measured in seconds, not in microseconds or milliseconds or nanoseconds. The storage capacity was only 72 words and multiplication took up to six seconds. Division took almost 15 seconds. Today it just seems incomprehensible that such a machine would even produce useful results on complete problems. But it did. I was involved in the programming of the ten simultaneous equations -- not too successfully I'll have to admit in that case. I was doing several other things at the same time. There were initially quite a few bugs in the program. In the fall of 1944 Aiken and I wrote a prospectus for the Mark II. The project was approved in the winter of 1945. It was a project to build another electromechanical calculator, this one for Dahlgren Naval Proving Ground.

ASPRAY: The reason for building a new machine was you had a second installation that wanted one, namely Dahlgren?

CAMPBELL: Dahlgren. Yes.

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CAMPBELL: After the various possibilities were reviewed, it was decided that this would be another electromechanical machine. This seems a little strange in that ENIAC was about to be demonstrated at that point. I think it was demonstrated in 1945, although not yet in useful operation.

ASPRAY: That's right.

CAMPBELL: After it was moved to Aberdeen it took two or three years to get it into effective operation. There was a lot of trouble with the 18,000 vacuum tube.

ASPRAY: How seriously was electronics considered for the Mark II? When you and Aiken did your examination, how far did you go in it?

CAMPBELL: We looked at it. The whole thing was a pretty quick project actually. Aiken was, justifiably I think, somewhat suspicious of the reliability of electronic circuits, circuits and of a really large number of vacuum tubes. One obviously could have been either electronic or electromechanical at that point. As far as getting something into effective operation quickly at Dahlgren, I think they made the correct decision.

ASPRAY: Not worrying about the correctness of the decision, but just the process of making the decision, did you talk to any of the people or had you in the past talked to any of the people at the University of Pennsylvania?

CAMPBELL: I didn't. Whether Aiken did I'm not sure. We didn't have too much contact with the people at Penn until later.

ASPRAY: Is there some reason to believe that Aiken may have? Through his own previous experience did he have a bias for electromechanical over electronic technology?

CAMPBELL: Well, he felt comfortable with electromechanical technology because he had a computer operating with electromechanical techniques. In any case, he was not terribly anxious to push the state of the art in computation.

That was not his main effort. A lot of people in the computer field -- most early people in computers, I think -- were principally interested in pushing the state of the art in computer technology. That was entirely secondary with him. He was interested primarily in providing the capability for computation. He was not as innovative in the machine area as some of the other people were. Let me say he was a little more conservative.

ASPRAY: But also it seems to me that there's a real difference in the background between the Moore School people and Aiken in the sense that, for example, Eckert had training in electronic control. Radar and such. That's not part of Aiken's background.

CAMPBELL: In fact, Maurice Wilkes made this point: people who pioneered in the electronic computer work came largely from either the radar or television background. They were familiar with high speed circuitry. At Harvard I think that only Harry Mimre was very familiar with that and he wasn't part of the project. No, you're quite right. Aiken was primarily an electrical engineer, not an electronic one. He was an applied physicist I guess; but as far as engineering was concerned, he was primarily an electrical engineer not an electronic engineer. I think he felt less comfortable with electronic techniques. I've been told this shows up in some of the designs of Mark III, Mark IV.

ASPRAY: Anyway, it was clear that reliability was an issue. If you were trying to build a machine quickly...

CAMPBELL: That's right. Now, it was an entirely different set of components because Aiken had a falling out with IBM, particulary with Watson, over the publicity at the time of dedication of the Mark I.

ASPRAY: That's such a famous story. Do you want to recount your version of it since you were there?

CAMPBELL: Yes. Well, you talk about references. I think the article. Who's the historian at IBM?

ASPRAY: Who do you mean?

CAMPBELL: He has a group developing the history of IBM.

ASPRAY: Oh, Charles Basche.

CAMPBELL: Yes. Charlie Basche. He may be the author or co-author of an article on the SSEC that also traces the early history of Mark I. I think there's a pretty good account there -- I think from a reasonably unbiased view at that -- if you're looking for reference. But a press conference was held the day before. Aiken held a press conference the day before the dedication was to take place. IBM was not part of the press conference, whether by design or whether inadvertently I don't know. Watson and company came up from New York on the train the night before. When the papers came out the next morning everything was about Harvard and Aiken, and IBM was, in effect, a little footnote at the bottom. Well, for one thing, IBM wasn't all that big a company at that point. It wasn't all that well-known really. Harvard was much better publicity than IBM, particularly in the Boston area. But the cost of Mark I at IBM had been chalked up to advertising. Watson certainly had in mind capitalizing on the thing in a big way. When you put that much money into it and that much time into it, you think that that would be a reasonable thing. So, Watson was deeply upset. In fact, there was some question as to whether he would even go through with the dedication. Now, I understand that Aiken claimed that this was all inadvertent on his part; but once Watson took such a strong stand, Aiken wasn't going to back down. Watson did go through with the dedication, did give a $100,000 check to Harvard to help with the maintenance of the equipment; but he was deeply disturbed at what appeared to him a very inequitable partition of the publicity. And, indeed, I don't think it was equitable. From my viewpoint, it wasn't a very fair partition of the credit. But in the Mark I users' manual President Conant has a preface and Aiken has an introduction in which both bend over backwards to give IBM as much credit as possible.

ASPRAY: Why do you think that Aiken wouldn't back down once he realized what the situation was?

CAMPBELL: I don't know. They were two very big egos. Now, I'll give you my own personal viewpoint on the thing. My feeling is that Aiken was a little unhappy with the idea that he had to depend on IBM so much to realize his machine. That, I think, rankled him a little bit. Also the fact that the IBM people really didn't know anything about numerical methods. So there was a little culture gap there I guess. I don't know how big a factor that was. As a result of the falling out, Mark II couldn't be constructed with IBM components. So Aiken had to find other components.

ASPRAY: Well, he could still have bought them from IBM, couldn't he?

CAMPBELL: Well, he didn't want to deal with IBM.

ASPRAY: I see.

CAMPBELL: So, we developed a set of electromechanical relays with the Autocall Company in Shelby, Ohio. We had written a letter to a number of relay companies and said that we needed a relay which would be operable for 1058 operations, I think it was. The only trouble is that this became a little muddled in the typing and it turned out to be a relay which would be operable for 108 operations!

ASPRAY: And some of these companies still couldn't comply?

CAMPBELL: Well, Autocall was interested in working with us. Paul Seton was the engineer as I remember. He spent quite a bit of time at Harvard and built and tested some prototype relays. There were two kinds of relays needed. One was an ordinary relay. The other was a latching type relay which had either of two stable positions. It was used for storage purposes so you didn't have to keep the current on for the storage units. So the relays were by Autocall.

Other components were no particular problem to find. Western Union supplied punch paper tape and teleprinting equipment. There were also high speed tape readers designed by Bob Wilkins at Harvard which used this narrow Teletype tape. I forget whether it was six hole or seven hole tape. But this reader read a large number of rows of holes all at once.

ASPRAY: I see. So that input and output were completely different for the Mark I and II?

CAMPBELL: The devices were all different. However, relays were used for switching and control in both cases. And both were synchronous machines in which you had cam operated contacts providing the pulses which timed the whole machine. But latching type relays were used instead of the mechanical decimal counters controlled electrically which were in the Mark I. So the storage devices in Mark II were not accumulators. In Mark I they were accumulators. Mark II had two complete machines which could be operated either together or separately. It had four program tape units and I think there were 48 storage registers in each half.

ASPRAY: What was the purpose of having two complete units? For checking one another? Running two different problems simultaneously, or what?

CAMPBELL: Yes.

ASPRAY: Both of those?

CAMPBELL: Yes. Actually, at Dahlgren I think they frequently ran the left half of the machine against the right half for checking purposes, particularly when they were trying to find intermittent errors. The intent was you could solve two small problems or one big problem.

ASPRAY: What was the mean time error? Do you have any idea?

CAMPBELL: You should look at the paper written by Dahlgren. I don't know. The history of Dahlgren's participation in computing has been written up by Ralph Niemann (report dated September 1982).

ASPRAY: I think I can find that out.

CAMPBELL: It also describes all of their early computer work, including Mark II, Mark III, and NORC. The NORC was a very high speed computer which IBM built for them. As far as I can see, Mark II worked out pretty well for Dahlgren. Mark II was begun in winter of 1945. It was delivered in February 1948. I left Harvard in the winter of 1947. I participated in the conception of the machine and the system design, and contributed to the circuitry work. I was also involved in component selection, and wrote a first draft of a programming (coding) manual. It was well long in construction by the time I left.

ASPRAY: Let me come back to this question. My reason for asking about the mean time error was really about a design principle in the machine. Namely, if the machines weren't very reliable, was it a specific design to be able to check one against the other. After all, many of the other early machines didn't have that feature.

CAMPBELL: I don't think that was the main intent. I think it was more just for flexibility in solving problems. We went to a lot to trouble to avoid some of the problems we had with Mark I. Mark I relays were built during wartime when there was a shortage of materials. This may not be terribly accurate, but I think the contacts were piano wire against brass. They were not very good contacts. Subsequently, I think, the relays were all replaced in Mark I with relays which had a lower contact resistance. But we had trouble with contact resistance when we had circuits in which there were a lot of contacts in series. So Mark II was designed in such a way so that you never had a very large number of contacts in series. As a matter of fact, we split up the basic addition process into two steps so we wouldn't have to send current through as many relay contacts in series. But I think that was perhaps unnecessary because we had much better contact materials in the Mark II relays.

ASPRAY: Steel and brass don't make good contacts at all. What did they go to later? Didn't they go to silver?

CAMPBELL: I think there was some silver. Yes. Mark II was inherently faster than Mark I because Mark I had a 3/10 second basic addition or accumulation cycle. The basic relay pickup time in the Mark II was 16 milliseconds, or something like that. It took two relay pickup times to carry out an operation because, first of all, it had to set up the relays and then use them. So the basic time in Mark II was something like 30 milliseconds. Mark II was a parallel-parallel machine whereas Mark I was a serial parallel machine. It was parallel between decimal digits, but serial within decimal digits in Mark I. So, depending on how you figured it, we picked up about a speed factor of 5. Mark II was also a floating point machine. It had a precision of 10 decimal digits and had a exponent which was between +16 and -16.

ASPRAY: In the design of the Mark II was there any thought that the machine should be somewhat similar to Mark I, at least in the way that you would code up problems for it? The reason I ask is, was there any feeling that if one machine went down the other one could be a backup?

CAMPBELL: No. Programming was completely different for Mark II. There was a fixed format in the program. There was four multipliers, two adders, and a set of functional subroutine capabilities. During each second one half the machine could carry out two multiplications, four (floating point) additions, and six number transfers. But it was a fixed format and each second was divided into thirty steps. And in each step you could carry out one particular thing. You either read in a multiplicand, read in a multiplier, read out a product, do an addition or other operation. It was, I guess, a rather unique approach. What we were trying to do was minimize the number of things that the programmer had to write down. So a lot of the program information was furnished automatically by the construction of the machine. What Dahlgren wanted to use it for initially was ballistics calculations which was the primary thing that Aberdeen wanted to use ENIAC for. In Niemann's Dahlgren report he states that Mark II turned out to work reliably and generally successfully during the years, although programming, as in other early machines could not have been early. [Niemann makes no specific comment concerning use of the programming format.] It was six years or so (1954) until the NORC machine was delivered by IBM. NORC was a very high speed machine and took over most of the calculations at that point. But although the IBM electromechanical synchronous design was carried over from Mark I to Mark II, the fact that most of the components were different meant that the detailed design was quite a bit different.

ASPRAY: That didn't seem to bother people? They figured that there were so many advantages in terms of increase in reliability and speed that these were still experimental machines that shouldn't look much like one another.

CAMPBELL: Well, these were supposed to be advantages. Design changes were believed to be advantages.

ASPRAY: In retrospect do you think they are? Were?

CAMPBELL: Well, the machines had to be different since the components were different. We had no electromechanical counters which were the counterpart of the IBM. We were looking for somewhat higher speed, so the parallel-parallel approach had to be taken from that point of view. Also, the precision of Mark I was not really required in the ballistic calculations: the ten decimal digits with the floating point approach were adequate. It should be noted that the Computation Laboratory with the Mark I calculator in place was really due to Aiken plus Harvard University plus IBM plus the Navy. All of those ingredients had to be involved in order to produce an operational computation center. The Navy was the user and of course IBM was the designer and builder and founder of the equipment, per se. Harvard was where Aiken was working and Aiken had the initial idea. Without any of those ingredients it probably wouldn't have been successful. Now, the operation of the Computation Laboratory was funded by the Navy.

ASPRAY: Does that mean that all the staff members were paid by the Navy?

CAMPBELL: Either as civilians or as Naval personnel. Yes.

ASPRAY: Had the staff grown considerably in size over the time that you mentioned before and those three months subsequent to Aiken's return?

CAMPBELL: I can provide a staff list. At the time of the dedication in 1944, the staff was five officers, five enlisted men, and about three civilians. The staff grew rather rapidly. During the design and construction of Mark II, the lab was also operating Mark I and already doing some experimental work towards Mark III. I guess it got up to 40 people or so. We can look at that staff list. So the Navy funded the operation of the Computation Laboratory first through Bureau of Ships and then through Bureau of Ordnance. (It's not called the Bureau of Ordinance anymore; but that's what it was called then, and for quite a few years afterwards.) Later, the Air Force contributed funding for a while. I think it was the Air Force that funded Mark IV. The Navy funded Mark II and Mark III, but only then because they were both used by Dahlgren Proving Ground. So it was a combination of, first of all, Navy funding and then later some Air Force funding that permitted the Computational Laboratory to operate over a number of years.

ASPRAY: It seems to me that IBM and the Navy made the main contributions. What did Harvard contribute?

CAMPBELL: Well, mainly the facilities, some of the staff, and some of the users; also the Harvard reputation.

ASPRAY: I see.

CAMPBELL: It was an anomaly for this type of operation to grow up at Harvard. Aiken was an anomaly at Harvard. He would have been much more at home at MIT than Harvard. I was told that G.W. Pierce had a great deal of trouble when he wanted to construct antennas on top of Cruft Laboratory in the late 1920s or early 1930s. Harvard was not much involved in technology.

ASPRAY: I see.

CAMPBELL: So, Aiken was really an anomaly. He really didn't fit into the Harvard milieu. He was his own person, very individualistic, and wasn't at all typical of most of the folks at Harvard. I think maybe people at Harvard felt that Aiken was too commercially oriented and too promotional in working with IBM and other companies. He made them sort of uncomfortable.

ASPRAY: He had a faculty position in Physics, is that right?

CAMPBELL: I think he was still an instructor at the time he went into the Navy. That's been disputed. I'm not sure of the exact facts. When he came out of the Navy he was a full professor of applied physics. He had a list of graduate students that got doctorate degrees under him. I think there were about twelve or fifteen. Also quite a number of people got master's degrees under him. He was very much interested in education. He was also interested in research numerical analysis and computation, in addition to being interested in machines. The program at Harvard was a very broad gauge program. It wasn't just focused on developing the highest speed machine possible. It was much more focused on using machines. Much more focused on some of the corollary work in research. And Aiken was an excellent teacher and lecturer.

ASPRAY: Before I go any further, I'd like to investigate this in some depth because the educational program does seem important. I'd like to spend a minute more on his relationships with the rest of the faculty. Did he participate? Was he close to the rest of the physics faculty or outside faculty? It seemed to me that I remember he drew in a number of people from around the campus to the Computation Center.

CAMPBELL: A lot of people at Harvard, other professors, put problems on the machine. Leontief and some of his folks put economics problems on the machine; especially the input-output model of the US economy. James Baker at the Harvard College Observatory was interested in lens design. Originally the idea was that the Mark I was going to redo the orbits of all the planets. That was in view of Aiken's contacts with Harlow Shapley. Indeed, the precision in Mark I was premised largely on being able to do this planetary orbital computation job. However, in fact it was never utilized for any work of that kind. I think there was sort of a cooling off between Aiken and the folks at the observatory, except for Jim Baker. But a good many other people at Harvard put problems on the machine and were quite interested in the machine.

ASPRAY: What percentage of problems or time of the machine was used on university problems would you guess?

CAMPBELL: Well, if we're talking about Mark I, the baseload problem was the calculation of extensive tables of Bessel functions. The rest was split between Navy or Air Force problems and problems from some other universities perhaps. Also, quite a few people came from European countries to spend time at the university at the Computation Laboratory. If you count the people in the Computation Laboratory who generated some of their own problems and the other people in the University, and backed off the baseload problems of the functional tables, I would guess that half of the problems would be internally generated.

ASPRAY: Were other people in, say, applied physics interested in the machine?

CAMPBELL: Applied physics and some of the people in applied mathematics were interested in it. Some of the less theoretical mathematicians were interested in it.

ASPRAY: Can you mention some names?

CAMPBELL: Garret Burkhoff was one. I remember Ronald King who was the antenna man at Cruft Laboratory was interested. The first problem for Mark I was a calculation for him related to the mutual impedance of two parallel wire antennas.

ASPRAY: Alright. Why don't we turn to the education issue now?

CAMPBELL: Now, I was not really part of that because I left in the winter of 1947 to go to Raytheon. I worked there for 2+ years and then I worked in the Burroughs Corporation in the Philadelphia area for 17 years. So I'm perhaps not the best person to talk about the educational program. But Aiken was interested in education, very much interested in education, and started one of the earliest programs in the computer field. I guess he started perhaps in 1947 or 1948. I don't know if you have the material that was generated at the time of Aiken's memorial service. It would have been in 1973.

ASPRAY: I'm not sure whether I do. Was that published?

CAMPBELL: Yes. I think Tony Oettinger had an article which covers that in some length.

ASPRAY: Right.

CAMPBELL: So that would be a better source than I would be. Herb Mitchell was the first person to get a Ph.D. under Aiken. I think he got his Ph.D. in 1948 or 1949. I have a list of all the Ph.D's.

ASPRAY: How were people trained to use the machine while you were there? Suppose someone new wanted to come in and use it. Say somebody from applied physics who had no experience. Would they use it?

CAMPBELL: No. The machine was operated by laboratory people. The person bringing the problem in would be the analyst, but generally laboratory people would do the programming, I believe.

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CAMPBELL: One thing occurred to me. I wondered why such a slow machine would be useful in such a variety of problems. But then I realized that if you had a problem that was reasonably within the capacity of the machine, the speed of the machine is not the main factor contributing to the turn-around time. Because you have to do the systems analysis. You have to do the programming. You have to test and debug the program before you can get any productive work. Then the actual productive running of the machine is not necessarily a big factor in the overall turn-around. It usually took a month or two anyway to define the problem suitably. Almost everyone who came in with a problem did not have it fully defined in such a way that you could start programming right away. Usually the careful definition of the problem took the largest amount of time.

ASPRAY: When somebody came in was somebody on the staff, e.g. you or Dick Bloch, assigned to work with them?

CAMPBELL: Yes. So to just finish off my participation in the laboratory. The design of Mark II started substantively in the winter of 1945. We worked closely with Harold Seton of the Autocall Company to design the relays. We went down to talk to Western Union in New York City about the teleprinter equipment which would be used. Bob Wilkins was the chief mechanical designer for Mark's II, III & IV. We had a preliminary concept in the Fall of 1944 and a fairly complete concept by the Spring of 1945. We got into detailed design of the basic additional circuits and other arithmetic circuits shortly after that. The machine was assembled in the old Gordon McKay building which has since been torn down -- sort of an old, almost a barnlike structure; but it was fine, with a lot of open space, for building the machine. I guess the machine occupied about as much floor space as any other computer built anywhere. Because of the relay storage the cubic feet devoted to storage of information had to be pretty high. We had to have four separate relays per decimal digit, you see. The machine had a large front panel almost as big as most of Mark I. Then there were several large back cabinets for relays and supporting circuitry. So, by the time I left in the winter of 1947, the design was essentially completed and construction was well underway. The project was under the supervision of Fred Miller, who subsequently went to Dahlgren with the machine. The machine was delivered to Dahlgren in early 1948.

ASPRAY: Let's tie up some loose ends about your participation at Harvard. What happened to your educational program?

CAMPBELL: Well, that's a sad story. I originally thought I would do a thesis in physics under Van Vleck; but then having gotten involved in the computer field I decided I'd do a thesis under Aiken in applied physics. I had chosen a topic in the design of a multiplier. I wrote a fair amount of manuscript but never really completed it. I completed all my courses and completed my French examination. I hadn't taken my German examination. About that time I began to get a little restless and opportunity came along to go to Raytheon in a newly formed operation. So I went on to Raytheon. I was supposed to become part of the Mark III design team momentarily since I finished up Mark II. But I never participated in the Mark III. I went to Raytheon instead.

ASPRAY: So you never came back and finished your degree program. How did Aiken feel about that?

CAMPBELL: Well, I don't know. He had so many fish to fry.

ASPRAY: There were lots of things going on that had to be paid attention to. How did he feel about you going to Raytheon, leaving the Harvard computation center and no longer being one of his active workers?

CAMPBELL: I assumed he realized that was more or less inevitable.

ASPRAY: Were you among the first to leave to go to industry?

CAMPBELL: Well, no. Some of the people left as soon as they got out of the Navy. So that's not really fair either. Dick Bloch left a month or two after I did. Grace Hopper stayed on a couple of years more, I think. Berkeley had already left (Ed Berkeley). Harry Goheen had left. Andy Arnold left. We had a number of Navy people who didn't stay around. Bob Cope and Ken Lockerby were officers who left when they got out of the Navy. (Actually, Bob Cope asked for a reassignment before he left the Navy.)

ASPRAY: How many of these people went into the computer industry as it was just then forming? I know that Dick soon followed you into industry. Berkeley hadn't gone into the industry?

CAMPBELL: Berkeley went back to Prudential at first, and then he started his publication business and has been involved in that ever since.

ASPRAY: And Harry Goheen?

CAMPBELL: He went back into education. He was doing something in the Boston area in education. I don't remember where he was. Then he went to Syracuse, and later to Oregon, where he's been for 20 years or so.

ASPRAY: So, it may be fair to say that you were the first to leave the laboratory and go to an industrial position in the computer field?

CAMPBELL: Perhaps so.

ASPRAY: You'd gotten out of the Navy in 1945?

CAMPBELL: I was in the Navy for just about two years from June 1944 to June or July 1946. I was an Ensign then, a Lt. J.G.

ASPRAY: Once you got out of the Navy you stayed on as a civilian staff member?

CAMPBELL: That's right.

ASPRAY: Under Navy pay at that time?

CAMPBELL: Well, let's see, I was either an Instructor or Research Fellow. I'm not sure what my title was at that point. The money, I think, came from the Navy.

ASPRAY: Why did you decide to leave the Computation Lab? You said it was inevitable, or at least Aiken should have thought it was inevitable.

CAMPBELL: Well, I completed the electrical design work on Mark II. And Mark III was well underway as far as the design team was concerned. I wasn't sure exactly how I'd fit into that group. I really didn't have any design experience in electronic circuits. I did have a reasonable background in communications engineering from a general theoretical point of view, though I hadn't done any circuitry work. And the program seemed to be pretty well underway. Also, we had our first child and salaries in the academic field weren't all that large. It seemed like a good opportunity at Raytheon to be in at the ground floor of a new enterprise.

ASPRAY: This was Raytheon's introduction to automatic computation, wasn't it?

CAMPBELL: Yes.

ASPRAY: They had come out of the war having grown significantly by making magnetrons?

CAMPBELL: Magnetrons. That's right.

ASPRAY: They also were in the television business, I believe?

CAMPBELL: In a way I guess, yes. They were a small company before the war. In fact, they had been almost on the rocks, but they did an excellent job during the war on magnetron work and grew substantially. Lawrence Marshall, who was the president, was interested in getting into new fields; and one of them was the computer field. It was through George Stibitz, who had been at Bell Laboratories and gone into consulting on his own, and was helping Raytheon. As a matter of fact he had an interest in two companies at that time. He was doing consulting for Raytheon, but also consulting for Barber Coleman out in Illinois. There would have been an opportunity to go with Barber Coleman too; but I thought that, quite apart from location (for I was very much interested in staying in the Boston area) Raytheon paid me a little better.

ASPRAY: Now, how did Stibitz know about you?

CAMPBELL: Well, there were contacts between Bell Laboratories and Harvard during the period 1944 - 1947.

ASPRAY: You were hired for what position?

CAMPBELL: Senior Engineer. Dick Bloch and I. Jim Weiner, Charlie West and John de Turk were all among the early people at Raytheon.

ASPRAY: The other name that I have from my research is Murray Ellis. Was he there?

CAMPBELL: Murray Ellis was also there, yes.

ASPRAY: Alright. What was the plan?

CAMPBELL: Well, I don't remember the exact timing. We got some money from Bureau of Standards. Two computer programs were under the general guidance of the Bureau of Standards, Sam Alexander. One was a machine for the Census Bureau. The other was originally a pair of machines for the Department of Defense. One machine was to go to the Navy, at NAMTC Point Mugu. The other was to go to the Air Force, Holloman Air Force Base.

ASPRAY: O.K. These were machines that they were going to be built on contract to have somebody build.

CAMPBELL: Eckert-Mauchly got the census machine, which I guess was more beneficial to them because the census machine would be a little more like a commercial machine (i.e., more date handling and less computation).

ASPRAY: Raytheon bid on that if I remember correctly?

CAMPBELL: That's right. Raytheon and Eckert-Mauchly were in competition, and as it turned out they got the census job and we got the military jobs -- although the Holloman machine was later canceled.

ASPRAY: Had they bid on the military machines also?

CAMPBELL: I'm not sure. Raytheon considered various approaches. They considered storage tubes, which they had some background in, and mercury delay lines. They decided mercury delay lines would be the preferable storage technique. Mercury delay lines were being used for moving target indication in search radars.

ASPRAY: These decisions were being made in 1947, is that right?

CAMPBELL: Yes. 1947. 1948.

ASPRAY: It was before any of the other university installations had gotten very far in building a machine with mercury delay lines in it. It was somewhat an independent decision, is that right?

CAMPBELL: Yes. The first mercury delay line machine was built at Cambridge under Wilkes.

ASPRAY: Right. It was completed in 1949 I believe.

CAMPBELL: There were three main choices: mercury delay lines, magnetic drums, and electrostatic storage tubes. Magnetic core storage came along later and replaced the storage tubes, the delay lines, and to some extent the drums. Harvard and Engineering Research Associates chose magnetic drums -- relatively slow, but a conservative engineering choice. At Raytheon we felt they were too slow. We looked in some detail at delay lines and storage tubes. Raytheon had a background in both. We made preliminary designs using both approaches. We decided that delay lines, although slower, were a much safer bet. (Univac made the same decision.) Whirlwind (MIT) and Princeton decided on storage tubes.

ASPRAY: So you elected mercury delay lines for the project?

CAMPBELL: Yes. That project was fairly well along when I left to go to the Burroughs Corporation. A new operation was opening at Burroughs.

ASPRAY: Before you go on to Burroughs, let's talk a bit more about the Raytheon project. Can you tell me something about the personnel? Who was project head? What were different people's responsibilities?

CAMPBELL: The whole thing was under the direction of N.E. Edlefsen, who went with North American after I had been there about a year and a half. He went to North American in the Los Angeles area. He had been at the Radiation Laboratory during the war at MIT. He was in charge of the department at Raytheon. I was pretty much in charge of the logical design, working with Dick Bloch and Murray Ellis. Jim Weiner was in charge of the circuitry hardware with Charlie West and John de Turk. After Edlefsen left, Cap Smith was in charge.

ASPRAY: How much money was the company willing to spend on this project? Do you remember the financial situation?

CAMPBELL: We were proceeding on a hand to mouth basis with small amounts of study money from the Bureau of Standards until we got the construction contract for the machine at Point Mugu in about 1948.

ASPRAY: At least one of the two machines were built, is that right?

CAMPBELL: The one at Point Mugu was built and put into operation.

ASPRAY: Name of the machine?

CAMPBELL: It was called the Rayvac.

ASPRAY: I think I have some information on it. When was it completed?

CAMPBELL: It would have been completed 1950 or 1951, I think.

ASPRAY: So, it was one of the first operating machines?

CAMPBELL: Sure. And Dick Bloch developed some concepts for checking the machine using a modular 11 type of check as it was called. You could check your arithmetic operations as you performed them without having to duplicate too much equipment. I think he patented some of these ideas. I think it was a three address machine. There was a lot of talk at the time about one address, two address, three address, four address machines, as you are probably aware.

ASPRAY: Why did you leave Raytheon?

CAMPBELL: Well, I'm not sure. A new operation was starting up in Burroughs and it seemed to me that Raytheon's commitment to the computer field didn't seem to be all that solid. It had been pretty nip-and-tuck as far as getting the Rayvac off the ground. We got an initial study contract with the Bureau of Standards and then there was an interim period when we had to carry a lot of people until we could get major funding. I felt there was an uncertain future as far as Raytheon's continuing at a sufficient level in the computer field. In contrast, Burroughs was in the business machines field and certainly would be committed to it. It appeared like a good opportunity to get in at the ground floor there.

ASPRAY: This would be their first project also in terms of building an electronics machine?

CAMPBELL: In terms of introducing electronics into the business equipment field.

ASPRAY: When did you make that decision?

CAMPBELL: That was in the summer of 1949.

ASPRAY: Did you go right away?

CAMPBELL: I left Raytheon in the summer of 1949.

ASPRAY: You went to Burroughs in Philadelphia?

CAMPBELL: This was in Philadelphia. They had a small research operation that had started up under Irven Travis.

ASPRAY: From the Moore School?

CAMPBELL: From the Moore School. He had been a consultant for Burroughs for some time and finally he was selected to head up a research operation when they formed one. They had rented one floor in a building in downtown Philadelphia, initially. Then as the operation expanded, it moved to Paoli, which is out on what's called "the Main Line of Philadelphia" after the main line of the Pennsylvania Railroad going west to Harrisburg and Pittsburgh. They built a new building in Paoli and subsequently added to it. They also bought up a company and added another building to it. So they had five buildings in the area.

ASPRAY: Who else was on the professional staff in the early days?

CAMPBELL: Well, Ike Auerbach who became notable in the field. He was one of the early technical people. He came from Eckert-Mauchly to Burroughs. He left that to found his own publishing company. Let's see -- Ev Schin. Everett Schin, John Howard from Engineering Research Associates, George Patterson -- I don't know if you've ever heard of him. He was a mathematician at the University of Pennsylvania. He went back to the University of Pennsylvania. Perry Smith came from RCA. It eventually expanded to quite a sizable operation. They had research work for commercial products and it also had a large amount of military work. They built the FST-2 radar processor for the Sage system.

ASPRAY: Was it digital equipment?

CAMPBELL: Yes. They also built the ground guidance computer for the Atlas ground ballistic missile. The Atlas utilized a ground guidance computer. It also had an inertial system, but corrected control was from the ground because they weren't sure whether inertial missile systems would be sufficiently accurate and reliable. It turned out the inertial missile systems were indeed accurate and reliable; so that the fully inertial approach became the major thrust as far as missile systems. These were two early military electronic machines.

ASPRAY: What dates would these be?

CAMPBELL: Well, let's see. I left Burroughs in 1966. So I guess these were started up in the late 1950s.

ASPRAY: Were you involved in either or both of these projects?

CAMPBELL: I started out heading up the electrical design group. Then, for a while, I was doing product planning. Then, for a period, I was director of research under Irv Travis. He was vice-president of research. And later I was moved over to a staff job. I was in the staff job for about the last three years before I left.

ASPRAY: Responsible for what?

CAMPBELL: Program planning.

ASPRAY: What about the commercial machines?

CAMPBELL: Well, this was an interesting area because Burroughs didn't even have electro-mechanical machines really. It had mechanical machines. The question is how do you button electronic techniques onto mechanical machines. It had been obvious from early work that the input-output problem is really the biggest problem in commercial machines. Computation wasn't really a big problem. It was input-output of data. So, the first actual product that came out was an addition to the Sensimatic Bookkeeping machine which was the top of the Burroughs line, the most advanced machine at that time. We put a magnetic stripe on the ledger card, and the machine would pick up the old balance in a bookkeeping operation. Say, if you're doing bookkeeping, you have a ledger sheet. You first of all pick up the old balance then you enter in the new transactions and finally the machine gives you a new balance, right? Well, the transactions are usually two, three, or four digits long typically; but the balance can be a very large number of digits. That could be one of the major sources of error using the machine. However, if you put a magnetic stripe on the ledger card and stored the balance on that, then you could pick up the old balance automatically. You had to key in the transactions anyway; even if you had a completely automatic operation you had to key in the transaction -- unless you had some machine readable documents in which the transaction was pre-recorded. If you just had printed documents for the transactions, you had to key everything in once even if you had a completely automatic computer operation. You had to record the information. So with the Sensimatic you would key in the new data, the transactions. It would pick up the old balance automatically, give you a new balance, and record the new balance on the magnetic stripe. So it is actually a pretty efficient operation for that limited bookkeeping application. Burroughs had always been strong in the banking business. At Paoli we also developed an automatic check sorter using the American Banking Association standards for magnetic character recognition.

ASPRAY: That had already been decided on?

CAMPBELL: Well, that was developed about the same time.

ASPRAY: This was in what year?

CAMPBELL: Oh, I guess that would have been around 1960. We spent considerable time out in San Francisco talking with people at Bank of America about check sorting. Also, at a Chicago bank.

ASPRAY: Bank of America was one of the first banks to automate, weren't they?

CAMPBELL: Yes. Then we were trying to incorporate electronics into the existing Burroughs line. Meanwhile, Burroughs decided to buy up the Electro-Data Corporation of Pasadena, California which, of course, was already in the computer field. Not so much commercial computers, but more scientific computers. They had a 200 machine which was...

ASPRAY: I think it was the Electro-Data 200.

CAMPBELL: Then they came up with the 5500, was it? Well, in those days they had one of the very early compiler-oriented machine designs. They had one of the very early machines designed with a great deal of attention to making it easy to program. They had a push-down stack and a lot of other things which were fairly novel at that point.

ASPRAY: This was 1959 or 1960?

CAMPBELL: Around then, yes.

ASPRAY: Because ALGOL I was 1958. The first version of it.

CAMPBELL: Yes.

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CAMPBELL: Well, Burroughs did get into the scientific computer field through Electro-Data. There was a large high-speed commercial computer development started at Paoli. This was in competition with the computer developments at Pasadena. Eventually the management decided to cut off the one at Paoli and continue the work at Pasadena.

ASPRAY: I see. They kept the same facilities and some of the same staff also?

CAMPBELL: Oh, this was one of many projects.

ASPRAY: Did Burroughs assume the scientific staff from Electro-Data?

CAMPBELL: Oh, yes.

ASPRAY: Do you know some of the people that were out there?

CAMPBELL: Lloyd Cali was one of the major contributors. Robinson ("Robbie") was chief engineer. Jim Bradburn was president of Electro-Data. He eventually went to Detroit as Vice-President of Engineering. Later he went with RCA. For a while I was going out there once a month, since we had our planning meetings there.

ASPRAY: While I'm on the topic of personnel, I asked you about scientific personnel. Who were the management personnel at senior levels who were responsible for either computer activities or getting the company involved in computer activities?

CAMPBELL: At Burroughs?

ASPRAY: At Burroughs.

CAMPBELL: Well, John Coleman was president during the latter 1940s. Then he was succeeded by Ray Eppert. And he was succeeded by Ray McDonald. He had headed up the European operations for Burroughs. It was really John Coleman who was the primary focus for getting Burroughs involved in electronic work. Of course, at that time the company was called the Burroughs Adding Machine Company. It wasn't until sometime in the mid-1950s that the name was changed to the Burroughs Corporation. Of course, that was a very cutthroat field, you know. There was a consent degree that was signed by NCR and Burroughs back around 1910 or 1915. It proscribed certain activities as being unfair competition. So they had to be careful how they operated. T.J. Watson was vice-president or director of marketing for NCR. That's where he cut his teeth on marketing techniques. At that time Patterson was the legendary head of NCR. When I was with Burroughs, NCR was bigger than Burroughs and had a thorough hold on the cash register business. Burroughs made an abortive attempt to get into the cash register business through a Swedish machine, Sweda, in part of Michigan. But NCR just brought their big guns in and that was it. Of course, since then NCR has gotten on hard times. I don't know what there situation is right now. Their current president came from Burroughs.

ASPRAY: So did many of their management people. What projects did you work on while you were there?

CAMPBELL: Well, I was first of all concerned with logical design. I had a small department in logical design. Then, for a while, I was a project planner working with some of the sales people. Later I was an assistant to Travis, helping him run the place. Then, for a little while, I was director of research and in line management. Later on I was in staff work back in research planning.

ASPRAY: The B5000 is the machine which most people look back to today.

CAMPBELL: That was the really advanced concept as far as a marriage of ALGOL and machine design. It was really designed from the software point of view rather than just designing hardware and then figuring out what you could do with the software afterward. It was quite an advanced concept at that time. That was a product of the people in Pasadena.

ASPRAY: I see. Not something that came out of Paoli?

CAMPBELL: No.

ASPRAY: Were there any commercial products that your research directly influenced?

CAMPBELL: Well, in time. There was quite a line of commercial computers which came partly out of Paoli and partly out of Pasadena. I guess the line was being formed in the 1960s.

ASPRAY: A whole series of banking machines, for example, and that I recall. For the most part your responsibility wasn't for designing a whole machine or a system, but rather doing research on components and logic design?

CAMPBELL: Logic design. I was never much involved with direct circuit design after Mark II actually. I was in the logical end of systems and logic end of things and product planning and really good activities with some management functions.

ASPRAY: You stayed there until when?

CAMPBELL: 1966.

ASPRAY: What was your decision to leave based on then?

CAMPBELL: In a reorganization of the research function I didn't have as meaningful a job as I had before, and it didn't look as though things would change. Staff planning was never too rewarding an area: direct results were hard to see. I was interested in really getting back more into technical work. I felt that I was less interested in management and more interested in technical work. So I've been doing mostly project work at MITRE.

ASPRAY: I see. You went to MITRE directly from Burroughs?

CAMPBELL: At first in a planning capacity, but then I gradually shifted to project work, which is what I think I like to do.

ASPRAY: What sorts of things of things have you done?

CAMPBELL: Well, I don't know how much you know about MITRE. MITRE works closely with the Electronics Systems Division of the Air Force. The Electronics Systems Division of the Air Force is one of three so called product divisions. Another is the Aeronautical Systems Division in Wright field in Dayton. The third had various names. At one point it was SAMSO, the Space and Missiles Systems Organization in the Los Angeles area. Those are the three product divisions in the Air Force. Those report to the Air Force Systems Command at Andrews Air Force Base, Maryland (outside Washington). MITRE's concern was really systems analysis and systems engineering primarily -- helping the Air Force acquire new electronics systems. Primarily systems which are concerned with command, control, communications, and intelligence. MITRE originally was concerned with the further implementation of the Sage system, Semi-automatic Ground Environment, which was a radar defense system. The prototype had been developed by Lincoln Laboratory of MIT. Having completed the prototype installation MIT felt that it was no longer appropriate for MIT to continue; so they formed MITRE. MIT and the Air Force jointly formed MITRE in 1958. Originally the purpose was just to continue the implementation of the Sage system. IBM had the major tracking and guidance computer, the FSQ-7. Burroughs had the radar processor, the FST-2. AT&T had the communications. Systems Development Corporation had the software. MITRE was the overall systems engineer. From just the Sage system they diversified to considerably broader activities. The Electronics Systems Division was formed in 1961 from the Command Control Development Division. This is MITRE Bedford. There is also MITRE Washington which does military work for the Department of Defense and also work for the FAA and civilian agencies, and for state and local governments. But basically MITRE Bedford is concerned with helping the Air Force acquire systems through defining requirements, initial systems conception, advanced development, full scale engineering development, production and operational text. MITRE is the architect, not the builder. We did not in general produce hardware or software. So very typically we'd help the Air Force formulate requirements, determine specifications, write requests for proposals and help the Air Force in its evaluation of the responses. Of course, the Air Force wants the competition among potential suppliers whenever possible.

For example, one program I've been associated with for several years started with the conceptual phase. The Air Force got ten proposals for conceptual designs. They selected four people to do competitive designs, then, when they got into advanced development, they wanted to reduce to three, with later two in full-scale engineering development so that they could have a competitive procurement for production. This was the plan, but the program ran into various technical problems and money problems. So the later phases didn't quite work out that way.

MITRE is involved in working with the Air Force in some or all those phases, depending on what program is. We also do some independent research and development ourselves. So that's the complexion of the company. MITRE is a so-called federal contract research center. Its a not-for-profit corporation run by a Board of Trustees. If the company wanted to disband the assets would revert to the Federal Government. Most of the facilities, at least in Bedford, are owned by the Air Force. So it's a special kind of operation.

ASPRAY: What sorts of things did you do within that environment?

CAMPBELL: First I was involved in some long range planning for the corporation. Long range planning with MITRE is a little bit tricky because they have to be responsive to their client. They can jointly conduct long range planning with the Air Force, but there is a limit to what they can do independently. When you're working on an Air Force program you're partly responsive, obviously, to MITRE management; but you're also responsive to the Air Force systems program office you're working for. So it's a dual kind of thing. Then for a while I was involved in what MITRE called the "Diversification program". This was around 1970. MITRE diversified into the civil area, and I worked on some data processing systems for the state police in Massachusetts and for the City of Newton police. Also on a criminal justice information system for the state of Connecticut. A support system for the Superior Court of Massachusetts. Also on some health information systems, partly for the Department of Public Health and Department of Mental Health. We also participated in a program, supported a program that the MIT and Harvard Center for Urban Studies -- that's not quite the proper name -- had with the HEW to look into the health information system for greater Boston. The center was under the direction of Pat Moynihan at that time. That was before he went down to New York to become a senator. This was around 1970. So I was involved in these diversification projects doing basic engineering work and some system concept work. I remember for the Newton Police Department we developed a system that went through five different phases. This was funded by the Law Enforcement Assistance Agency in Washington through a committee in Massachusetts which doled out the funds and monitored things. According to the Newton procurement rules, the city had to advertise all procurements. In the Newton paper you saw the following. Three ads one after the other. The first was for trimming trees on the high school grounds. The second was for washing windows in the junior high school. The third was for building the input processor segment of the Newton police data system. This was a bit incongruous.

ASPRAY: What did the court system, for example, involve?

CAMPBELL: The court system was for the Superior Court in the State of Massachusetts. This involved setting up a court case management system, as we called it. When a case came into the court system the basic information would be entered into a computer and the computer would keep track of all the events that had to take place. It would provide a complete case history and would also generate all the documents which were required to conduct affairs. The hope was that through better planning, scheduling and follow-up, the courts could shorten the time necessary for the judicial process.

ASPRAY: I see.

CAMPBELL: The most successful thing from the standpoint of being actually implemented was the system for the city of Newton. That was because the police chief was very interested and the mayor also supported it, and of course the money came from the feds largely. The city contributed in-kind labor, but they didn't have to put up much money. MITRE wrote the proposals to the funding agency, for each of the program phases. MITRE determined requirements, wrote requests for proposals and specifications, evaluated responses, monitored the contractors, and assisted with installation and testing. After four phases of the program had been completed, the user was able to take over completely. I don't know if you've had any experience with state government, but state government works pretty slowly. There's an awful lot of people involved. Perhaps one of the most interesting jobs was for the state of

Connecticut Criminal Justice Information System. It was an information system pulling together information from the state police, the local police, courts, probation, parole, and all the other agencies that are associated with them. I think we handled the thing correctly. We had a senior advisory committee made up of senior representatives of all the agencies involved. We had a working committee in each area. We wrote the proposals to the federal government for money. But state government is just a very difficult thing to work with.
In the case of the court case management system for the Superior Court we ran into a jurisdictional problem. Some people were hoping to use the state police computer for court work. Other people felt that would violate the separation of power between the judicial and law enforcement people. There was also a competing program coming out of the Attorney General's office. That was never implemented. I think we had a good concept. I was involved in that for three or four years.

The last "diversification" program was an information system for prospective rate setting for hospitals and nursing home programs out of HEW. But the people we were working for really didn't have the ability to implement the information system. There were too many unknowns in the rate-setting business. We were really a little ahead of time working on an information system before the basic rate setting process had been established. So not too much came of that, unfortunately. We weren't really in the right place at the right time to actually implement it. Since then I've been involved mostly in a couple of Air Force acquisition programs. One for a jam resistant communication systems for airborne-to-ground tactical communications. The other an improved communications system for the Air Force security police. One of the things I've been working with quite a bit is life cycle cost models. The idea is to try to control not just the research and development cost and the acquisition cost, but the whole life cycle cost of the program, including operation and maintenance.

ASPRAY: One of the things that we neglected to talk about before was the founding of ACM, which seemed to come out in a way of the Harvard Computation Laboratory.

CAMPBELL: Yes, we always felt that the principal spark plug in the founding of ACM was Ed Berkeley. Ed Berkeley had been, I guess, a Lieutenant. He had been at the Harvard Computation Center and later went back to Prudential before he got into the publishing business. He and several of us had felt that since Aiken handled most of the external contacts himself, other people in the Computation Laboratory were isolated from the rest of the world. That was one of the motivating influences in the founding of ACM. Ed Berkeley, Harry Goheen and I from Harvard with several other people helped get the thing organized. I think Ed Berkeley was the principal instigator of this. A computer policy committee of senior people -- I think Aiken, von Neumann, Forrester and two or three other people, decided that another society was not needed. But Ed Berkeley went ahead anyway. We had an organizing meeting in New York in 1946 or 1947.

ASPRAY: I think it was 1947, but I'm not sure.

CAMPBELL: The organizing meeting was held in New York and I was elected treasurer. Ed Berkeley was secretary and John Curtis was president at the time.

ASPRAY: He was with National Bureau of Standards at the time?

CAMPBELL: Yes. I don't remember who was vice-president, perhaps Sam Lillian. The first real meeting was at the Aberdeen Proving Grounds. ENIAC was really just getting into substantive operation at that time. It was still having some problems. Also a relay machine built by IBM for Aberdeen which I guess was not operating very well at that time.

ASPRAY: What was the value of the early ACM?

CAMPBELL: It offered a very good forum for interchange between the different organizations working in the computer field. Now, I think most other organizations did not feel quite as isolated as people at Harvard did. There was, for example, a summer session in computers at the University of Pennsylvania in...

ASPRAY: 1946.

CAMPBELL: 1946. That was the session out of which emerged the famous notes on the design of computers which led to the stored program concept for instructions. Internal storage and arithmetic manipulation of instructions. I think that was something that came out of that group and was documented by von Neumann. Some people give him credit for originating the idea. Whether he originated the idea or not is another question. I don't think many of the other places felt quite as isolated as people at Harvard. Aiken was always his own man. He went his own way and he had his own idea of how to do things. He wasn't really part of the mainstream in some senses. He was on his individual course. Aiken didn't think there was any need for a society, but I tricked him into joining. I asked him when he was going to pay me back the five dollars he owed me. He didn't remember, but he paid me back the five dollars. I used the five dollars to enroll him in the ACM. I don't think he ever paid another dues though.

ASPRAY: While we're describing this period again, do you want to tell me about Aiken's communications with other organizations? For example, is this at a time when you were at the lab?

CAMPBELL: He started that, yes. He was very interested in spreading the gospel to other parts of the world and developed rather close ties with Sweden, Switzerland, Germany, Spain, and one or two other countries.

ASPRAY: Can you be more specific in any of these cases?

CAMPBELL: Yes. He didn't have much impact on the work in England. It really went along independently. People from the different countries came to the Computation Laboratory, spent time there, and then went back to initiate projects and conduct work in their respective countries. I think it was either in Belgium or Holland, that Aiken went to some kind of an affair which involved the king of the country. After the formalities were over, Aiken sat down with the king informally and "told him all about computer machines". He was very effective in communications with other people. He was a very effective lecturer. A very good salesman. Very effective in getting his ideas across. Some people feel that his influence in Europe was not all advantageous, that some of his biases and limitations came into it. But in any case he did have quite an impact on activities in Europe and made trips over there fairly frequently. Not so many during the years I was there in the laboratory because his nose was pretty much to the grindstone in his early work at the Computation Laboratory.

ASPRAY: Can you point to any particular places where Harvard-like machines were built?

CAMPBELL: Let's see. I'd have to look at my notes here. I think there was a copy in Darmstadt.

ASPRAY: That sounds right, but I'm not sure.

CAMPBELL: I think that's true. In the early days there was a fairly informal atmosphere in the laboratory even though it was a Navy organization. Aiken didn't stand on ceremony very much, and it was quite informal. I think later on after he had been through Mark I and Mark II, had become a full professor, and had quite a reputation in the field there was a little more of a formal atmosphere, I gather. Initially, it was rather informal.

ASPRAY: One of the things that I really didn't ask you as we went through earlier is what the contributions of various people were. You told me all about the development of the Mark I and Mark II, but you didn't identify people as working on particular aspects.

CAMPBELL: Well, as far as the programming for Mark I think Dick was really the primary person.

ASPRAY: That is Dick Bloch?

CAMPBELL: Dick Bloch was really the primary force as far as I'm concerned. He did program two of the most important problems. Grace Hopper came along a little bit later, but her main contributions to the field were after she went to UNIVAC. She had one major task in helping to document the Mark I user's manual, and she was involved in the Mark II user's manual also. But her real contributions to the field didn't come until she was at UNIVAC later. As far as Mark II was concerned, Aiken and I wrote the original prospectus. Of course, it was his initiative. Bob Wilkins did the main mechanical design. Other people also participated in aspects of the design.

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CAMPBELL: I did a good bit of the circuit design for addition and multiplication. I worked with the Autocall people on relay design. I helped interface with Western Union. Then, later on, the project was under the management of Fred Miller. A good many other people participated in various parts of it. The introduction to the Mark II user's manual gives a pretty good indication of all the people who worked on it. As far as Mark III was concerned, of course, I wasn't there during that time and I'm not sure of people's roles. Again, the introduction to the user's manual gives a pretty good indication of Aiken's view of who participated in what.

ASPRAY: Was there a similar document for Mark IV?

CAMPBELL: There was a draft user's manual. I have a copy of the draft of the user's manual that was never published. There is no introduction to it. So there is no way to tell Aiken's view of who did what. Why it was not finally published I don't know. It's in a relatively finished form. Incidentally, reading the user's manuals now is a little bit strange because the concepts and vocabulary of data processing systems weren't very well developed at that time. You find, for example, the first chapter in the Mark III manual entitled "Description of the System", or something like that. But by the end of the first paragraph you're into details of the pole piece design for the readers for the magnetic drums. So, some of the "organization description" sounds rather strange. Actually though, the description of the machine is pretty complete, but the reader has to do some digging.

ASPRAY: The whole systems concept wasn't really all that well understood at the time.

CAMPBELL: No. That's right.

ASPRAY: IBM seemed to have a slightly better idea of the systems approach than, say, UNIVAC did in their earlier equipment. Was there good reason for that?

CAMPBELL: Well, IBM had a little bigger complement of machines I think, a little more diverse complement of techniques than Sperry Rand did. It was Remington-Rand at that point. IBM had to do enough systems work to assemble a group of machines in order to handle an application. Of course, IBM was always very strong in not only marketing but also maintenance and support. So they really had the whole capability for serving the customer.

END OF INTERVIEW