



## Computer Oral History Collection, 1969-1973, 1977

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**Interviewee:** Jay W. Forrester  
**Interviewer:** Richard R. Mertz  
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### **MERTZ:**

This is Part II of an interview with Professor Jay W. Forrester of MIT, conducted on Thursday, the twenty-second of January 1970, at 2 p.m. in his office. Professor Forrester, would you like to describe your early involvement with the Servomechanisms Laboratory at the point of its inception. What problems you were involved with and how you originally got involve in the Servomechanisms Lab?

### **FORRESTER:**

Yes, in my first year at MIT I was initially a teaching assistant working in the electric machinery laboratory. At some point I had begun to carry on research and acting as a research assistant with Professor John Trump in his high voltage engineering. This carried up until sometime in November of 1940, at which point Gordon s. Brown, Professor—Assistant Professor probably at that time, in the Electrical Engineering Department, asked me if I would work with him on a program that was getting started. I think he had already made arrangements with Trump and with the people in the electrical department. I don't recall whether I had a choice in this, or whether I was invited with a choice and it seemed interesting, but in any case I joined with him, Albert Hall and with Jack Silvey in an undertaking to develop a hydraulic control mechanism for the 40 mm antiaircraft gun that was being designed at that particular time. This was my first exposure to the field of feedback systems.

Gordon Brown had had some background in this by having worked on the differential analyzer, on a machine called, I believe, the cinema Intergraph which, I think, was involved in his doctoral thesis, and in some ways it was a natural evolution for him; it was completely new to me. We had a very small room assigned to us in the basement of one of the main buildings at MIT. We brought one of the 40 mm guns into this room. It was not big enough for the gun to revolve if the barrel were down in the horizontal position. One had to be sure to elevate it into the corners of the room to make it, in fact, traverse around the entire room. In this room we attempted to develop a piece of equipment which would position the gun in accordance with a predicting gun sight. This involved us in the design of feedback systems. There had been work done in England. Gordon Brown had some ideas that came from some English developments that formed the basis for the design of some hydraulic equipment. We were at that time working with the Sperry Gyroscope Company on this particular development, and it evolved eventually into a design of a hydraulic mechanism with various interesting feedback features for

which Gordon Brown and I filed a patent which was issued covering the hydraulic control system, which later during World War II was used in a number of applications. It was used in the gun for which it was designed, it was used for perhaps an experimental unit for a 75 mm gun mount where we did some of our early experimental work that may not have gone into production, and it was used for some radar sets, the antennas of radar sets during the war.

**MERTZ:**

What was the data input that came into the device itself? How did it correct?

**FORRESTER:**

The signal came from a predicting gun sight—and there were various types of these—and it was transmitted as a synchro signal, a three-wire position of an armature that was transmitted to the control unit. A corresponding signal came from a synchro on the output of the gun mount, and the difference between these went into the differential synchro which positioned a hydraulic valve, indicating whether the gun mount was ahead or behind the position that it should be. The problem is to get stability, to get close following and tracking of the input signal without the oscillations or the velocity lags that are apt to be encountered in ineffective devices.

**MERTZ:**

Were there some error limitations in terms of the margin of error in tracking itself which were imposed upon it by the development of the defense?

**FORRESTER:**

One had of course limitations on accuracy, had to achieve a certain precision if it was to be useful. Above all, it had to have reliability under very difficult field conditions. I think that it might be worth mentioning here that MIT has pretty much throughout its history been an institution with a much closer liaison, much closer tie to the end use of its ideas in society, than many academic institutions. This particular program illustrated that willingness to bridge from theory to practice because the people involved in this kind of development carried it right into production. They worked in the companies in which the devices were produced. They went into the field when there were field troubles that were not understood. I myself, in 1943, flew to Hawaii where the Navy was having trouble with one of the devices that had been built in the laboratory for the control of interceptor radar used for directing fighter planes against the Japanese attackers. And, having not completed my work on the device in Pearl Harbor went with the Navy during the invasion of Tarawa and the Marshall Islands. This kind of experience, from theoretical research, mathematical analysis, all the way through to the field problems of reliability, was a breadth of experience obtained by people in the MIT research laboratories at that time, that their counterparts in most other institutions and most industrial organizations

do not have an opportunity to acquire. This is important because—

**MERTZ:**

May I interrupt?

**FORRESTER:**

Yes.

**MERTZ:**

How long were you out in the Pacific on this problem?

**FORRESTER:**

For about a month, I believe. But then similar kinds of experiences when on in the development of equipment. We would put experimental equipment out, as for example on a light ship in the harbor of Portland, Maine, and then would end up going out there in the middle of winter to keep it running, to find out its problems, to carry on the kinds of experiments that we wanted to do before it was turned over to the Navy.

**MERTZ:**

Did any of these problems involve you with the materials research people at MIT such as Professor Von Hippel and his group?

**FORRESTER:**

Our contacts with that group came much later in the development of the computer ferrite cores. We were very much involved in materials, but in this case they were mechanical materials, hardening corrosion-resistant materials, and so forth, and my recollection is that we worked mostly with metallurgists and processes in industrial companies that we were in contact with. The problem of reliability was severe. It's interesting to note that one of the reasons we were working with hydraulic equipment rather than electronic is that in 1940 and '41 the military services almost flatly refused to accept electronic equipment for anything except radio use, feeling that mechanical equipment was more reliable. We were essentially restricted in the control devices we could use to the development of very precise high-gain hydraulic amplifiers, and some of these had not only an extremely high precision and gain but also a very high reliability. I remember that at one point we were concerned as to whether the Navy devices would be affected by operating in a salt water environment. So I personally went down to Boston Harbor and brought back a gallon of genuine ocean water and we poured it half and half with oil into one of these hydraulic devices and ran it all winter to show that on this emulsion of ocean water and hydraulic oil the device would still continue to function.

**MERTZ:**

Were these devices perhaps a family of devices in terms of hydraulic servomechanisms for either gun tracking control or radar control? They were similar, but, I take it, they were each designed specifically for the different types of uses to which they were put.

**FORRESTER:**

They were similar in principle. They had approximately the same central concept. They vary greatly in size, in horsepower, in the particular nature of the hydraulic equipment. Some of the hydraulic pumps and motors were of commercial pre-existing design. One of the devices I worked with included a completely new design of hydraulic pump and hydraulic motor as well as the hydraulic control systems. So there was variety and expediency in picking equipment from wherever one could find it.

**MERTZ:**

How roughly did the division of labor break down in the Servo Lab itself in the early days? Was Professor Brown concerned with one facet and were you concerned with another?

**FORRESTER:**

The Laboratory run by Gordon Brown was a tremendously effective developer of people. At the time it seemed very traumatic, it seemed very difficult. Research assistants right out of college were thrown into major projects. They received a great deal of support from Gordon Brown if they would stand up on their own and take responsibility. There were a multiplicity of projects, not a monolithic structure, a multiplicity of sponsors simultaneously going on and each project leader almost entirely responsible for what he was doing. Responsible, really, for keeping his house in order with respect to the sponsors, responsible for the technical work, responsible for hiring people, for the purchasing, responsible for working with the organizations that might eventually be gearing up to produce the equipment. So it was an environment in which the entire scope, as I said, between theory and end use and field troubles could be experienced by men who were still under twenty-five years old.

**MERTZ:**

Obviously this grew from that small room into something a good bit larger in time. How did the project expand over the years?

**FORRESTER:**

Expanded gradually as it succeeded and as new opportunities and demands came along, moved into what was known as Building 32 on the back lot of MIT, originally I believe built for a warehouse, and step by step took over that whole building and began to work in parallel and in some of its projects in support of the Radiation Laboratory. The one that I referred to involved in my trip to the Pacific was a program in which a new radar set had been developed by the Radiation Laboratory, the first of its kind; in fact, the carrier Lexington had the first experimental model on it. That was the degree of pressure and rush. There was no waiting even for production designs. The first and only one was actually installed on the carrier with the first and only control systems that were used with it. When these ran into some of the technical troubles that are almost unavoidable, then the solution was for the designers to go out and fix their mistakes.

**MERTZ:**

At this point in '43 then there was a substantial increase in number of personnel in the Servo Lab, I gather.

**FORRESTER:**

It grew, probably to some fifty professional people plus draft smen, purchasing department, and so forth, by that time, guards, machinists, a very good machine shop and some very excellent machinists, gentlemen of the real skilled machinists' art that we perhaps see very little of these days. Men from whom a young college graduate could learn a great deal if he would, working with them on how to assemble, how to design, how to manufacture and machine the kinds of devices that we were working on. An excellent model shop with people like a Mr. Gillison that I remember, a man named Byrne who worked with the young engineers who were designing equipment and manufactured the first ones and worked side by side in the assembly, and so forth.

**MERTZ:**

As the lab expanded and took on increased responsibilities, was this reflected in further increase in responsibilities borne by you in terms of various activities undertaken by the lab?

**FORRESTER:**

Yes. I became involved in developing a hydraulic control system that was to go on the Raytheon SG radar set and successors to it for stabilizing radar antennas high up on the masts of ships. This was a program that actually was in production at the end of World War II, but that particular activity never became a major issue in actual end use because it was just in the early production stages when the need no longer existed. Some of the earlier programs had of course materialized into active use.

**MERTZ:**

At that time did Mr. Everett belong to the Servo Lab?

**FORRESTER:**

Yes. He had worked with me from probably about 1941, so he had worked with me during the development of these devices that I was just speaking of. So our association began early in the history of the Servomechanisms Laboratory and continued until 1956 when I left Lincoln Laboratory to come to the Management School.

**MERTZ:**

Were there any others in the Servo Lab period during the war who either worked in other projects under Professor Brown or were directly working with you who later went on to work on the flight simulator?

**FORRESTER:**

Robert Wieser and Stephen Dodd as well as Everett were in that category. There may have been one or two others. Those three are the principal ones that carried—who had a common history through much of the Servomechanisms Laboratory and then were to be found on the aircraft analyzer and the computer work.

**MERTZ:**

At this time do you recall the role that Nathaniel Sage played in connection with the war effort research of the Servo Lab during the war period?

**FORRESTER:**

Yes. We're speaking of Nathaniel Sage, Senior, there being a Nathaniel Sage, Junior, who has also been in sponsored research work at MIT. Nat Sage, Senior, was the Director of the Division of Industrial Cooperation, as it was known at that time. He came into that position sometime prior to World War II, and at that time there was almost no sponsored research activity in academic institutions, including very little at MIT. Sage's background, I know a little bit of it. I think he was the son of an Army officer, I think he had grown up in Army camps in various places over the United States and the world; he had gone to school at MIT. It's possible that he—I believe he did not graduate, though I am not positive. He had been in various activities and I think in the construction industry, being one.

**MERTZ:**

Did he major in Civil Engineering?

**FORRESTER:**

My impression is yes, but he may have been in Naval Architecture or something, but basically a non electronic, non mechanical type, I think. His background in construction in his youth, and so forth, and for whatever other reasons, had given him an extremely good judgment of people and, I think, a conviction that ability, integrity, courage, and so forth were the kinds of things that really mattered. He ran the Division of Industrial Cooperation, or the sponsored research activity, in an extremely effective way which consisted of deciding in his own mind whom he trusted and whom he did not. I suppose my high regard for him is partly because his judgments and mine obviously tended to agree. So there were certain people around MIT in whom he placed an extremely high degree of confidence. Those people include Stark Draper, who developed the instrumentation laboratory in the use of gyro systems for a variety of purposes. It included Gordon Brown, in the Servomechanisms Laboratory. It included me, in the Servo Lab and later in the Whirlwind project.

Nat Sage set the pattern for the country in contracting between the government and the university. There was very little of that prior to 1940. He had an extremely strong hand in establishing the way in which such contracting should be done, particularly the insistence on a degree of freedom that would allow the institution to go ahead and do a good job as the institution saw the need.

The Radiation Laboratory was one of these activities, and a very major one, probably large enough to be in many ways autonomous from Sage's direct intervention. I'm not sure about that, but he was directly influential to a very high degree, in the development of my own career. I believe Gordon Brown feels the same way. I would expect that Draper would feel the same way.

He would give complete moral and administrative support. He would isolate complaints from government contracting officers from the projects. He would talk to contracting officers and insist on certain things and hold his ground until he got the kind of contracts and relationships and terms that he felt was necessary to run a satisfactory program, quite in contrast to the kind of administrator who follows the path of least resistance, who kind of accepts whatever the pressures on him are asking for.

**MERTZ:**

You have perhaps partly answered the question I was about to put to you. What do you feel were the influences on you personally of the person of Nat Sage?

**FORRESTER:**

Well, I think he was great supporter at times when probably the work I was doing would have been terminated by those who, perhaps, were yielding to the pressures and didn't themselves have an insight into what the purposes and the objectives were. I think that

Sage had a good grasp of what one was trying to accomplish, and I think he would hold deep convictions about the importance of it and not pretend really to be any expert on how it was going to be accomplished. I place him in the same ranks as I do some investment bankers that I meet. There are some investment bankers who are better judges of technology than technical people.

Such people can ask questions and can judge whether they are getting competent answers. They can judge whether or not the man is simply trying to snow them, to impress them, or whether in fact there is substance behind what he says. They certainly judge integrity, they judge courage, they judge initiative and devotion to purpose, and probably they check with various other people to see if their own perceptions are confirmed. But they do this without needing to delve into any of the finer points of what is going on. And if they form an opinion, as did Sage, that this is a worthy effort in hands as good as he is likely to find, then he's unswerving and unrestrained in the support and the help and the encouragement that he gives. In Sage, actions were undivided in his acting as an intermediary with other people at MIT, other people within the military, if necessary, and shielding, really, the technical people from things that he could take care of himself which would otherwise have been distractions.

And so, in a form of leadership, in a form of support, in judging the likelihood of a successful outcome and then staying with it long enough to reach that outcome rather than giving up and yielding in the face of some sort of intermediate pressure that is almost certain to develop along the road, I would say that what he provided was an opportunity to succeed enough times that one understood how to succeed. And one understood the process whereby you succeeded, and also had some sense of what the cycle is of carrying out an effective program and how to withstand the pressure, that may arise along the way.

**MERTZ:**

In some respects then his role and that of Gordon Brown are similar, I gather.

**FORRESTER:**

In many ways, very similar. Gordon Brown being far more involved in the technical substance than was Sage.

**MERTZ:**

Let us come to the end of World War II and the last part of your activity which was the mechanism to stabilize radars hydraulically powered, I guess, you could call it, Servomechanisms I work in stabilizing radars on ships. That then sort of bridged the activity that you had undertaken at the end of the war. That's around 1945, I should imagine.

**FORRESTER:**

Yes, I think, actually, if I recall the proper year, right at the end of 1944 and going into 1945.

**MERTZ:**

Well, at that point in your career, what were some of the considerations of alternatives that presented themselves?

**FORRESTER:**

The alternatives were of course not very clear, fluid. There wasn't nearly as well established an industrial research activity as we know it today. I had not actually made any serious examination of what I might do as an alternative. I had entertained ideas of trying to either start or become associated with some form of research laboratory. I had visions of Boulder, Colorado, and the mountain setting as a place for this, but they had never become very specific, because in the process of considering what I wanted to do next and in the discussion with Gordon Brown he had a list of a number of things that had come to his attention that were possibilities for continuing at least beyond the end of 1944. One of these attracted me for reasons that I wouldn't be very clear on right now, but, anyway, one of them seemed a very interesting kind of program to undertake. Having embarked on it, it led in a very, kind of orderly set of sequences, into the later computer work.

**MERTZ:**

Were these number of problems that Professor Brown had ones which had come to his attention in connection with some of the earlier defense work which the Servo Lab had been working on?

**FORRESTER:**

I suppose so. I don't remember actually what was on the list except for the one that I did engage in, which was the aircraft stability analyzer, a program that had been initiated by some interests of Admiral Louis DeFlores, which had been studied somewhat by a couple of faculty members in the aeronautics department, and which involved, as people visualized it at the time, a rather complicated analog computer.

**MERTZ:**

This was building on something, I gather, which had pre-existed in the form of the Link Trainer and some of the other devices which had been developed in World War II, and was, if I'm not mistaken, viewed as an extension of, in part, at least, that activity. Was it then the first time they were concerned with the problem of flight simulation in real time,

where you would introduce the number of parameters which the ASCA equations that are first presented in the project were first considered, or were some of these already done in the way of previous smaller analog simulators?

**FORRESTER:**

The history, I would see it as two threads. One thread were the flight trainers, the well known Link Trainer being one of these; but actually there had been some much more elaborate, effective and comprehensive ones that had been developed by the Special Devices Center of the Navy that had been located at Sands Point in Long Island. And these devices had been probably designed and they were built by the Bell Telephone Laboratories, and they were used for training for some of the bigger and more complicated airplanes. The airplane existed and the trainer was simply made to duplicate enough of its characteristics, including the feel of the control surfaces, the stick and wheel, and all the various switches. And these things were linked up behind the scenes in a control board in such a way that the airplane had a high degree of reality in its feel. They were tied up to the flight instruments and everything, so that they were quite effective apparently as pilot trainers. But as scientific devices one could simply cut and fix and experiment until it behaved to people's satisfaction because the airplane already existed and one knew what it felt like.

So it was not a predictor of airplane performance, it was a trainer for people and could be adjusted as much as necessary until people were satisfied with the way in which it reproduced the behavior of the airplane.

On the other hand from Vannevar Bush's differential analyzer on through the so-called Rockefeller differential analyzer developed at MIT and the various fire control computers of—gun sight computers that had been developed through the war, there was a tradition of analog computation starting with certain fundamental propositions and computing the dynamic consequences of various sets of equations. These two threads then came together, as I think Louis De Flores saw them, with a possibility not of matching an existing airplane, but starting from the wind tunnel characteristics of a proposed airplane and developing a device that would reproduce its behavior. Now one of the problems that apparently had existed was that even after an airplane was wind tunnel tested and finally built, it would turn out to have quite nasty and undesirable dynamic characteristics, doing various things in the air that the pilot was not easily able to cope with. Various kinds of flight instabilities that might vary from annoying to dangerous. The proposition was to discover these ahead of time by building something that would behave like the proposed airplane.

The only known computational devices at that time were analog computers and the project got started as a development of the computing device that would predict the behavior of aircraft. The reason for our getting into it is that such a predicting device, such a computer, depends very heavily on very precise feedback control devices to carry out its own internal processes. So that the components out of which such a machine

would be built are essentially servomechanisms and presumably would come out of the background of having developed the control devices that the Servomechanisms Laboratory had been working on.

There was a parallel program at that time of very much the same philosophy. Albert Hall, who was one of the people who started in the Servomechanisms Laboratory, had become associated or perhaps was the leader of a project that, I think, was called the dynamic analysis and control computer, which was aimed at essentially the same problem with respect to certain guided missiles that were just beginning to be developed at the end of World War II.

So basically there were two programs aimed at similar kinds of end objectives, both presumably to use analog computers. The other one, the dynamic analysis and control simulator, was in fact built and it was used for awhile and on the whole it did encounter the difficulties that led us to abandon our effort on analog computing well before we really had built any machine.

**MERTZ:**

The number of parameters involved, I gather, in actual simulation was considerably greater than in the training situation. This was one of the considerations.

**FORRESTER:**

Far greater. It is not clear to this day whether a machine to do the job can be built. I don't know.

**MERTZ:**

Nevertheless, initially the problem was explored to some extent from the point of view of applying analog computer technology to simulation devices. Do you recall—the chronology is perhaps not so important but—the major considerations in addition to the knowledge of this parallel project which led to the abandoning of the analog approach?

**FORRESTER:**

We worked for about a year on the components of such a system and we did build a prototype of the cockpit simulator, a device that would provide hydraulic pressures on the control stick and respond to various electrical signals given to it, and we developed some feedback devices that were aimed at producing the integrators necessary to do the computation. The further we went into it the more discouraged I became with the likelihood of ever producing that much machinery with that degree of precision that would do anything but respond to its own interconnected idiosyncrasies. At the end of about a year, I think, we had come to the conclusion that it looked like a futile losing proposition.

Perry Crawford, who had I think—who had been a student at MIT, worked for the Special Devices Center and he had done some work while a student here on certain pulse circuits and flip-flops and computing devices or at least was in touch with some three or four people who had been. That plus his ability to get around and know people that were doing various things led him to call our attention to some of the embryonic work which was then going on in the field of computation. I think it's probably quite clear that he is the one that triggered my serious observation of the emerging work in digital computers. At that particular time the Harvard Mark I computer was in preliminary operation. This was an electromechanical machine, not a general purpose machine as we know it today, because the control tape was not itself under the control of the machine. In other words, the stored program orders were not subject to modification by the machine, so far as I know. So it was not exactly a general purpose machine, but it certainly was a digital computer, largely mechanical, electromechanical in its design.

At the same time there was an electronic machine that had been developed at the University of Pennsylvania and it was electronic. The ENIAC contained some 18,000 vacuum tubes and was not at all general purpose in the sense that the program for it was plugged up in various plug boards.

So there were those two existing pieces of digital computation and there were also propositions for what we would now call the general purpose electronic computer in the form of a design of the serial EDVAC computer at the University of Pennsylvania in which Von Neumann and Eckert and Mauchly were in various ways engaged, never very clear to me exactly how all of this was divided between them, but there was in any case, that line of thinking which had developed that was accessible, but none of this was far enough along to make it very clear whether or not it would serve the problem that we had at hand.

**MERTZ:**

Was Perry Crawford following your activities during the year in which you were exploring the possible application of analog equipment to the flight simulator? Had he been more or less riding herd from the Navy side over the project? Or did he enter the picture later?

**FORRESTER:**

I don't know for sure. I think he was present at the time of the transition from analog to digital computation. I'm quite sure of it. Whether he was there at the very beginning or not, I am not sure. There were two or three other people, perhaps young naval officers, who were involved. Peter Gracio was one. I believe there was another named Ludwig that may not be exactly the right name. Whether they were in before Crawford, exactly when Crawford joined that group, I don't know.

**MERTZ:**

He was clearly there during the time when you were making your re-appraisal of the applicability of the analog.

**FORRESTER:**

Yes, I'm quite sure of that.

**MERTZ:**

And what was Professor Brown's view at this point of the idea of reconsidering the whole approach in terms of the type of computational device?

**FORRESTER:**

I don't have any separate or clear recollection of exactly his participation. It was being carried out at that time in the Servomechanisms Laboratory still in the physical space that the rest of the laboratory occupied, presumably continuing discussions of it. No recollection that I have of any difference of opinion and beyond that I can't give you perhaps detail on it.

**MERTZ:**

I was thinking, did he share your view that indeed the analog approach at this point did not seem to be productive, the prospects did not seem to be that hopeful?

**FORRESTER:**

I don't recall. I think it would have been somewhat unlike him to take any strong position to the contrary. His mode of operation was to discuss, to advise, and to let people shape their own successes and failures.

**MERTZ:**

You mentioned space in you location at the Servo Lab. When you then started to consider the idea of the application of digital computing techniques to the problem of flight simulation, did this raise any question in terms of space, personnel, in terms of the shift away from what had been more or less a logical outgrowth of the Servo Lab, namely analog devices, at least servo components.

**FORRESTER:**

As we began to develop the work in digital computers we needed many more people. We were running out of space where we were and this was eventually solved by taking over

what is known as the Barta Building, where the MIT Graphic Arts Department is now located, number 211 Massachusetts Avenue, I think, up the street from the main buildings at MIT. This was a building that had to be renovated and repainted, and which we moved into as the base of operations and in which the digital computer work stayed until we moved to Lincoln Laboratory.

**MERTZ:**

Do you recall roughly when the move took place from the original Servo Lab area to the Barta Building?

**FORRESTER:**

Roughly in the 1947-48 period. I can't spot it perhaps any closer than that.

**MERTZ:**

Some work on the digital problem did predate the move to the Barta Building?

**FORRESTER:**

Some of the early experiments on the very early electrostatic storage tubes I know for sure were done in the Building 32. This must mean that some of the basic computing circuits were started there, but by the time we got to the 5-digit multiplier, so called, the first experimental assembly of computing circuits, we were firmly established in the Barta Building.

**MERTZ:**

I see. Now during the pre-Barta Building period, roughly how many people were involved in your flight simulation project at this juncture?

**FORRESTER:**

A relatively small number. Through the first year of operations there was a kind of a core of about six people who were thinking about what they should do, which met at least one evening a week to try to plan. This just gradually grew without any major discontinuities.

**MERTZ:**

Of those original six you had been associated with the majority of them in the war years at the Servo Lab, I gather?

**FORRESTER:**

Yes. One newcomer to the group, Warren Loud, was from the mathematics department. He is now at, maybe University of Wisconsin or Michigan, I would have to check that. Everett and Dodd and, I believe, Wieser were members of that group and probably by that time Harris Fahnestock, who served as Administrative Officer probably was with us. I think he did join us while we were still in building 32.

**MERTZ:**

And then—

**FORRESTER:**

And then also Hugh Boyd. I'm not exactly sure of the dates at which those various people joined, but Hugh Boyd, Harris Fahnestock, Steve Dodd, Bob Everett, Warren Loud were among some of the first people to begin to meet together.

**MERTZ:**

In the later period I notice that the biweekly summaries tended to be divided fundamentally into two parts. One was more or less the mathematics part and the other was the hardware part or, to use the modern terminology, the software and the hardware. Of the individuals in the original group, roughly how did that break down in terms of those who were concerned with components, with hardware, and those who were concerned with the mathematics?

**FORRESTER:**

Except for Warren Loud, who was essentially a mathematician, and who probably would not have been involved with electronics to any appreciable extent, most of the others did not make any such differentiation because it was a composite problem involving both and they were, all of them, really able to work on either side as the situation required.

**MERTZ:**

Had most of them been trained in electrical engineering?

**FORRESTER:**

During the early planning stage—one has to recall that nothing was known by this group about digital computers and almost nothing was known by anyone. The work that I referred to, work that had been done at the University of Pennsylvania and by Von Neumann, were logical propositions, the description of what could be done, and some diagrams that would show that there existed computing circuits that in principle could carry out the various logical steps. But there were

essentially no circuits that were available. Very little was known about anything that was necessary. The logic, the programming, the circuits, the storage, there was simply very little of this. And so probably the first year was very largely a matter of trying to think through the whole proposition, feeling one's way. The group that worked together tended to work on all aspects of it, tried to define what the problem and the process was, and to define the issues and the tasks and to identify all the various things that would have to be done. Specialization tended to come somewhat later, and we never were highly specialized. A person would work in an area for a while and then perhaps shift to the area of next greatest priority. So that we had actually a group of generalists that predated the beginning of the computer laboratory because they were people who had already had the experiences that I've already mentioned of going from research to end use. They had done this two or three times on various projects.

I expect few people realize how powerful such a group is. I don't think we realized it at the time. But there are very few people who have had the entire breadth of experience from an original idea, and maybe the mathematical analysis of it, through the development, through the research work, through the engineering design, and are still with it when it's gone out into its end application. We had a group that had been through this more than once.

So even in the very preliminary stages they had some sense of the implications of what they said and some feeling for the kinds of difficulties and problems that it could lead into. Now these were not very clear impressions because the background was in hydraulics, the newly emerging task was in megacycle video circuits, so that the overlap was not of course of a detailed nature. But at least they knew how likely mechanical and technical things are to cause trouble, how much you have to over-design and protect yourself if you want the device to really work.

**MERTZ:**

I was going to ask you in that connection, was there any experience that had accompanied, say, the development of ENIAC, which might have stimulated, or any other awareness that might have stimulated the concern for component reliability at this time? Or did that concern come later with the actual hardware development?

**FORRESTER:**

I think the concern came from perhaps two places. One, the group itself had been deeply conditioned with the idea it was designing things that were supposed to be used, not just ideas to write about. And they'd had a lot of exposure to the problem of making devices that would withstand the idiosyncrasies of people and environment.

The other pressure that entered the picture was the eventual use to which the electronic computer was going to be placed. And that was as a combat information center in real life, initially Naval Operations. The first really—at the time the computer was under

actual final development, it was already clear it was no longer going to be used for the aircraft stability analyzer, which has been mostly a scientific tool, but instead was being aimed at a naval task force in antisubmarine warfare in which the device would analyze data and keep track of the relative positions of one's own ships and try to combine in one place the multiplicity of fragments of information that were available from all of the various units in the task force.

Now, this is a—you see, this is a serious real-life situation in which reliability is an absolute must and it was the intent of the group that they would produce something that would serve the purpose.

**MERTZ:**

Did this come in, this consideration, in that formative year when the small group was more or less educating itself as to the nature of the problem, digital computation as applied to the problem?

**FORRESTER:**

The small group in the digital computation would have been approximately the calendar year 1946. They would have been considering a serial electronic general purpose digital computer for the aircraft analyzer. Then the serial computer went the way of the analog computer. It would have been too slow, it would have not been suitable for the aircraft analyzer. The year 1947 was devoted to developing concepts of a parallel synchronous logic digital computer, still with the idea that it was to be used in the aircraft analyzer. 1948 represented the refinement of the design of the parallel computer and I think would be the year in which the transition clearly was taking place—maybe '47 included some of the transition—but '47, '48 covered the gradual giving up of the aircraft analyzer and the gradual firming up of the commitment to a combat information center machine. I say "gradual" because different people accepted this transition at different times.

**MERTZ:**

Now the role of, I take it, the role of Perry Crawford—You mentioned that he did to some extent stimulate your interest and involvement in the first consideration of digital computers. He then became more involved in time with the progress of your group from the Navy side.

**FORRESTER:**

On the Navy side Admiral DeFlores continued to feel the importance of the aircraft analyzer. I think even we had abandoned it in any serious way. Crawford on the other hand, I'm quite sure, was one of the leaders in moving away from the aircraft analyzer and into the combat information center. I don't think that Crawford had any deep devotion to the aircraft stability analyzer. He was, and probably still is, a visionary in a

quite effective way, and I think he saw at least the conceptual possibilities much earlier than most people. It's really not very clear to me at this stage the nature of the transition away from the aircraft analyzer and into the combat information center, except that it clearly came about as a series of discussions between Crawford and myself and we were probably the two who first began to move in this other direction, others then following in due course of time.

**MERTZ:**

Then what had originally been conceived of as a more or less special purpose device to support a flight simulator or to predict certain characteristics for stability in aircraft, the device itself became more or less the end to serve first one purpose and conceivably another and perhaps with a view toward making it a multipurpose device so far as what it could serve. Perhaps I'm anticipating the actual history of what it was viewed as, in terms of its end purpose.

**FORRESTER:**

As soon as we embarked on the digital computer it was, of course, evident that it was a multipurpose device. Everyone else developing electronic computers looked upon them as scientific computing devices, to be used by science for scientific computation. We broke away from that in terms of using the computer for a control device, but, of course, we were well aware that it could be used for these other purposes. So we looked upon it as general purpose, but we were committed to the use of it in the handling of information for real-time control. Now that placed two extremely important criteria on the undertaking. It had to be high enough speed to do the particular task in real-time. In other words it had to process, it had to do whatever it was going to do as real-time marched on. You couldn't save up the problem, take any necessary amount of time and still have the answers relevant. They had to be geared to what was going on here and now as things continued to happen. Unless you could do enough the exercise would hardly be worthwhile.

So there was a large task to be done with what turned out to be an extremely high priority on speed and, of course, one had to do it with a high degree of reliability; otherwise one would simply end up in chaos. I suppose it's impossible for people to go back now into the degree of uncertainty that existed in those days on the question of reliability, because no general purpose computer had operated at that point in time which was able to get access to its own control orders. There was very little known about such things as whether thermal noise would introduce pulses and spurious signals into these channels. And it was quite clear from the logic that a single mistake would lead to chaos in the operation.

So one was talking about a machine that would have to operate perfectly for very long periods of time. Electronics in 1946 and '47 and '48 was far from perfect, and the magnitude of the task was almost unknown.

To find out whether there was even a remote possibility of success, we developed, I think, about 1948 something called the 5-digit multiplier. These were five digits, five positions of the arithmetic element that was proposed, very heavily instrumented with error checking devices so that one could count how many times it made a mistake. We discovered that it would make mistakes several times a day, but that's very hard to find out why if you have two or three single mistakes in something that's going on at two million times a second. A lot of that problem turned out to be simply pulses coming in on the power lines and we introduced a motor generator set to isolate us from the power system.

The technical uncertainties are very hard, I think, for people to look back on now and to visualize, because one knew very little about vacuum tubes, their troubles and their reliability; one knew very little about thermal noise and what it would do. There was no experience. There had been no equipment up to that time of any sort that required this near perfection of performance. If one didn't get it, the whole undertaking would be a failure.

**[End of interview]**