Interviewee: Perry 0. Crawford, Jr.
Interviewer: Richard R. Mertz
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MERTZ:

This is a second interview with Mr. Perry Crawford of IBM, conducted in the Yorktown Heights ASD installation of IBM on the 27th of October 1970.

CRAWFORD:

Concerning the naming of Project Whirlwind. It was done in the summer of 1945 by members of the Special Devices Center, then Special Devices Division, contingent are responsible. principally Peter Gracio, and Ralph Mark were involved, and I was involved. I was then a consultant for the Center, and Whirlwind was one of perhaps half a dozen names that had come down to the finals in Gracio's name contest. At meetings in which I was present, Whirlwind was selected, and that was the first of the windy names. Cyclone came along a few months later, then Breeze, then Typhoon a few months later, into '46 now at RCA. Then Hurricane came on in 1947, having been preceded by Zephyr. That was the original name of the project at Point Mugu, but when the magnitude of the job became apparent, they changed the name to Hurricane. That was the end of the names actively used. There was a project called Tornado at the Special Devices Division, by then the Special Devices Center, which referred to a proposed simulation facility at the Center, which would have had a Whirlwind II computer as its basis. I might just postscript this by saying that the final proposed project was called Apocalypse.

MERTZ:

Was there any particular reason for selecting Whirlwind since it was the first assigned one of these? The Project had pre-existed the assignment of this name.

CRAWFORD:

Yes. It, of course, was called ASCA in its earlier days--Airplane Stability Control Analyzer, I believe. Then it was known by its SDD code letters for a time. I don't recall them--thirty, or something like it. But then Whirlwind came on and was used continually from the middle of 1945 on.

Concerning the association that Jay Forrester might have had with Herman Goldstine and Von Neumann. There was of course the meeting at MIT in October 1945 when the Rockefeller Differential Analyzer was unveiled. Goldstine and Von Neumann were present, and Forrester was present. I'm sure that was his first meeting with both gentlemen, and I'm sure they had conversations then that led to the later invitation to visit

Princeton and Moore School.

MERTZ:

I thought I might be able to supply you with the ASCA number, but I don't seem to have it.

CRAWFORD:

We'll have it in our notes [?] . Concerning the proposed extensions of the application of Whirlwind beyond real time flight simulation, even during 1945, or even earlier than 1945, studies had been under way of the mechanization of the gaming at the Naval War College. Mechanization of the kinds of operations which were involved in simulating combat from the point of view of a CIC, a Combat Information Center. Both of these application areas presented very formidable problems as far as the application of analog equipment is concerned, and from early '45 on the digital equipment looked like the way to go in those applications.

MERTZ:

Wasn't this involvement during the War?

CRAWFORD:

That was during the War. That was the Specia1 Devices Division getting involved in the War College. They had extensive radar simulation work and CIC simulation work under way and association with Johns Hopkins _____ at Newport.

MERTZ:

Was there also related to this, in any way, mathematical statistics projects involved in the probability aspects of tactical deployment?

CRAWFORD:

I don't recall anything of a sustained nature at the Special Devices Division in that area. There were, of course, extensive studies elsewhere.

MERTZ:

I was thinking, correct me perhaps, Abraham Wald at Columbia University did have, at one point or another, some money from someone, perhaps in the Navy, to do some work during the War years.

CRAWFORD:

It could've been, I don't know.

MERTZ:

... on the gaming aspects of sort of game theoretic but mathematical statistical aspects of the deployment of defensive ships, and then also of target analysis from coastal batteries.

CRAWFORD:

There could have been work but I don't know about it. I don't recall it, but you do remind me about one interesting aside here.

One of the studies that was supported by the Special Devices Division was one conducted by Philip Rouland at Harvard and Harry Goode and Gilman, I don't recall his first name. That was Harry Goode's introduction to the whole area of computers and automatic control, simulation and the rest. He was a consultant, working as a consultant for that first period. He was working on pursuit curves.

MERTZ:

Is Harry Goode around?

CRAWFORD:

No, he was killed in an automobile accident in the late…. He entered the field first joining the Special Devices Division, moving from his earlier association as a consultant. I knew him from the time he joined the Division.

Let's see. I had commented on the extension of the use. Oh, yes. I had mentioned the interest of the Special Devices Division in war gaming in the War College sense and combat simulation in the Combat Information Center sense. The proposal to use digital equipment had been made in the summer and fall of 1945, so that those prospective uses of digital computer played a significant part in the decision making that had Whirlwind eventually taking a turn from analog to digital systems. By the time the turn was taken, the people at Special Devices Center were really focused on combat simulation, and at ASW, it was a point of key interest. Then command and control systems--simulation on a broad basis was also of very great interest.

MERTZ:

So then, in a sense, the Navy really anticipated what was later

on a more generalized application.

CRAWFORD:

That's correct. And the Project Tornado and Apocalypse in '47 and '48 were very direct anticipation of what came later in the way of combat information command and control systems, operation and simulation ...

MERTZ:

Were those projects formalized as such?

CRAWFORD:

Not really. Tornado was certainly in a planning stage and I think there was budgeting for it. Money was being requested, but I don't think there was anything actually put out under that name.

MERTZ:

Well, the Project description ...

CRAWFORD:

Oh, yes. You'll find the material here--project descriptions, proposals, they were the special interests of Captain 0'Rear at that time.

MERTZ:

To what would you attribute the demise of ...

CRAWFORD:

Well, drying up of funds on the one hand, stretch out of Whirlwind. The early dates for the Whirlwind II delivery were probably 1949, and by 1949 the quoted dates would have been more likely 1953, and so forth. So, they were visions more than concrete plans or proposals.

MERTZ:

But by '49 the real problem areas of Whirlwind I think had been fairly clearly defined in terms of what was holding it up and where

CRAWFORD:

That's correct.

MERTZ:

The storage was at the head of the parade ...

CRAWFORD:

That's right. Your next question is the Ad Hoc Committee. Refresh my memory on that.

MERTZ:

I was interested in a description of your participation in the Ad Hoc Committee and some of the people on the Committee whom you feel would be useful to interview, and who might be useful sources, from the point of view of the history of computing.

CRAWFORD:

I would like to refresh my memory on that. Luis de Flores of course is dead. Harry Goode was an active participant in the work of that Committee, and he is dead. The head of the Ad Hoc Committee, Bob Sterns, was then the president of the University of Colorado, but I do not know where he is now. I have not heard about him in many, many years since he left the University.

There were a number of other people involved and I would like to refresh my memory and review the roster of the participants before going further into that. The Committee was formed as a direct result of the memorandum that Louis de Flores sent to Forrestal, outlining the kind of possibilities that had been presented to him in connection with the Tornado proposals and the Apocalypse proposals. It was a very excellent setting up of those technical possibilities and operational capabilities.

MERTZ:

Do you know approximately when this memorandum?

CRAWFORD:

That would have been the spring of '48, perhaps even the late winter, very early in '48. It was a very excellent memorandum and produced the desired result. It arrived on Forrestal’s desk at the same time there were proposals there concerning the

establishment of an integrated operations analysis--operations research activity. He combined these two proposals, and the Committee was known as the Ad Hoc Committee on Scientific and Synthetic Analysis, where the scientific analysis was intended to refer to operations research approaches, and synthetic analysis was intended to refer to the use of simulation to study and analyze command and control systems primarily, and weapons systems more generally.

The integrated systems of scientific analysis activity eventually took shape as the Weapons Systems Evaluation Group, roughly in 1950, I think it was. The Ad Hoc Committee formed one sub-committee and intended to form more than one. It formed one sub-committee on Air Defense System Engineering, and Harry Goode was a member of that.

MERTZ:

Was there any carry-over in terms of personnel between the Ad Hoc Committee and WSEG?

CRAWFORD:

No, there really was no close connection between those activities. The Panel on Air Defense turned up a report with some consequence and then the parent committee, the Ad Hoc Committee itself, came up with a report early in 1951 that set forth a broad range of possibilities for military applications of computers, primarily in the area of command and control systems, with particular emphasis on the capability of these systems to exercise themselves, so to speak, for training, tactical analysis purposes, and operational development purposes.

MERTZ:

Was there any relationship between this committee or contact or overlap in personnel, and the Valley Committee?

CRAWFORD:

No direct connection. The Valley Committee was a little bit later. There was a close association between the Ad Hoc Committee for Scientific and Synthetic Analysis and the Ad Hoc Committee that was formed to review the whole computer field. I have the copy of that report if you have never seen it. It's a very interesting document.

MERTZ:

Which reviews the ...

CRAWFORD:

It's a study commissioned by the Research and Development Board on computer development.

MERTZ:

Was this in any way related to the Advisory Board on Simulation's study of primarily analog computer research facilities in the United States?

CRAWFORD:

No, that must have been later, too. The group that I speak of was formed in 1951 to help resolve some of these uncertainties about the relationship between the Whirlwinds and the other projects.

MERTZ:

That would be indeed a very useful ...

CRAWFORD:

It's a useful document. The study was actually conducted by the physical sciences group of the RDB and suffered from being in the scientific area as opposed to the area of command and control and military applications.

MERTZ:

Are you familiar with the so-called Bartkey Project of Walter Bartkey, on the Advisory Board on Simulation which was related to one aspect which was ...

CRAWFORD:

No, I'm not. The names are familiar but the ...

MERTZ:

Evaluation of both analog and digital computer facilities in terms of simulating MIG aircraft flight characteristics on a basis of combat photography in Korea.

CRAWFORD:

No. Well, that would have been quite a bit later.

MERTZ:

'51, '52. It was fairly early in the Korean War. The Advisory Board on Simulation was charged with, among other tasks, the task of taking the aerial photography of dogfights and then attempting to extract from this the characteristics of the then MIG-15 or whatever the current number of the Soviet aircraft engaged in the dogfights was.

CRAWFORD:

That would have been after I left the RDB.

MERTZ:

To which end they had an extensive film library, the most comprehensive one of that kind.

CRAWFORD:

Fascinating. I'd like to see reports on that. You inquired about the magnetic tape work, and I think I can show you the notes.

MERTZ:

On the cellophane base ... you mentioned also what you thought was a seminal article on acoustics.

CRAWFORD:

That's right. Heinz Lubbock's article in 1937. Well, the bulk of my material on the magnetic recording was lost in the box of materials shipped to New York at the end of the War which I never got. But I do have some notes that were with other material, apparently.

Concerning the Bush notes. Those were notes written by Bush, not by Caldwell, beginning in 1937 and continuing into 1939. They were transcribed by Professor Caldwell's secretary, maintained by her, both the original notes and the transcripts, in her files.

MERTZ:

Professor Wildes is doing some work in writing a history of the Electrical Engineering Department, and I've been briefly in touch with him. He has very kindly placed at my disposal a number of materials relating to his activities. However, so far as I know, these are not among those materials.

CRAWFORD:

Probably not. These files were the Center of Analysis files. When the Center of Analysis was decommissioned and Frank Rizzou moved to the building out in back, the files moved out there, so they kind of fell out of the Department of Electrical Engineering files and archives.

MERTZ:

Could you make any conjecture as to where you think possibly ...

CRAWFORD:

Well, Frank Rizzou is the one to see. It just happened that he married Professor Caldwell’s secretary. [?] and took over these files, so between the two of them they are a primary resource in locating some of these early materials. Frank's collections are quite extensive, and I'm sure he'd be delighted to help out.

MERTZ:

He does have an interest in the preservation of historical ...

CRAWFORD:

Yes. He has been engaged in writing a book. We don't know where it stands as far as publication is concerned, but he has been making use of his records. and the archives for that purpose. You inquire about the NDRC Study, and I have no records, I don't believe, of that study, or my own inputs into that study. It would be very helpful, very valuable, to get that material wherever it can be found.

MERTZ:

Could we for the purpose of trying further to narrow down where one might ... or what, zero in on the study, the approximate date of the study.

CRAWFORD:

It must have been 1941.

MERTZ:

Was it commissioned by the NDRC?

CRAWFORD:

It was commissioned by Section Five, Warren Weaver's group, and the task is to answer the question, should we be responsible for the development of this digital equipment. The eventual answer was no, but in the course of the study. Stibitz traveled to MIT, Eastern Kodak, RCA, Bell Labs, National Cash, and perhaps to other places, but at least those, and I'm sure that his report contains very valuable accounts of work in progress and proposed at those locations.

MERTZ:

As of about 194-

CRAWFORD:

Mid '41.

MERTZ:

And he did have access then to on-going classified projects?

CRAWFORD:

Oh, yes. The Computron was an active project at that time at RCA, and I'm sure his report referred to the Computron report dated about that time, maybe somewhat later, but the work was underway in '41.

MERTZ:

How about the M-9 ... How about fire control?

CRAWFORD:

Well, of course, that was already going, and the whole task of Stibitz' study was to see whether the M-9 was sufficient, or should they be backing it up with digital approaches.

MERTZ:

And the thrust of his conclusion was that the digital approach was not the ...

CRAWFORD:

Yes. I never did see his final report. I'd like to see it, but as I understand, that was the net of his result.

MERTZ:

Also, presumably, he visited Aiken.

CRAWFORD:

Probably not. Aiken, of course, was in the Navy at that time, I believe. He was already at Norfolk, and I don't think there was much going on at Harvard, although there was work progressing at IBM. I doubt if Stibitz visited IBM. His particular interest was in electronic computers, and at that time IBM was not included in the roster of organizations sponsoring work, although I'm sure it should have been on that roster. There was some work.

MERTZ:

I wonder if you would care to make any comment upon the apparent exclusion of IBM from several groups or activities at this time which were involved in the development or, if not academic, at least some special purpose calculating equipment. For example, I believe at the famous Moore School summer course, no one from IBM was invited. There might be other reasons for that. I don't know.

CRAWFORD:

I think it was because IBM was just not caught up with electronics at that time. There were no IBM people present at any of the meetings that were held. It wasn't until around 1947 or '48 that the IBM field marketing representatives were presenting themselves to the various military groups engaged in these development activities, sponsoring these activities. And actually, it was quite a bit later than that before it was anything sustained.

MERTZ:

However, from the point of view of punch card equipment and certain kinds of computational and comparative reading equipment, IBM was fairly early involved in defense ...

CRAWFORD:

That's right. It's kind of a paradox. IBM was not alone in turning away from these military specialized military uses actually very slow to move ahead except in a very few and specialized way. They regarded their Typhoon project as their entry in this area. I regard IBM's reluctance as of the same kind. GE was very reluctant to get involved in this area. They were just so concerned with getting on with their main line of business that they just didn't want to take time for this. As far as IBM, it wasn't until the Korean War that what was called the Defense Calculator Proposal was put forward by Cuthbert

Hurd that they became a major factor in the electronic area.

MERTZ:

Although I think with the unveiling of UNIVAC and some of the other activities perhaps spurred some involvement and interest on the part of ...

CRAWFORD:

That was earlier. The BINAC came on the scene in 1950, and the UNIVAC in 1951 in a significant way. By the time IBM was moving ahead in a comparable way.

MERTZ:

Earlier the application of punched card equipment as data input, or output, to an electronic kind of computing machine was not looked upon with particular interest.

CRAWFORD:

No. That is correct. The principal interests were in military applications of direct input and direct output, cathode ray tube outputs and the like. It's true that punched card equipment was being picked up pretty fast in the operations research area. Steinhardt of OEG didn't like computers, but he did admit punched cards for data analysis and reduction.

Concerning Orbeck's work. I'm sure that you'll find full, very excellent documentation of that work in the National Cash archives, and my impression is that much of that material is now in the hands of Honeywell lawyers.

MERTZ:

If I might just go back a little bit to MIT and to Stibitz' study. He presumably then would have had full knowledge and access to the Bell work on the analog anti-aircraft.

CRAWFORD:

That's right. He would have been thoroughly familiar with all of the projects under way for the Section Five, which would include the M-9 work and work on dynamic testers at the University of Texas and other places. His particular interest after this study was in the area of dynamic testing. In connection with his work on dynamic testing he developed or invented his hybrid type of digital-analog components, the tapes with the notches that would permit functions to be represented by the notches or distance along the tape, variable spacing the notches. He was very enthusiastic about that approach. I’m not sure whatever came of it, but I do recall seeing him late in the War, and he was pushing it very hard.

From the Overbeck point, you inquire about problem formulation in the early days. I made reference earlier to the article by Louis Couffignal the *Bulletin Astronomique*. If you're looking for anecdotes, did I ever mention going to the Harvard Library to get that and discovering it was charged out to Howard Aiken? I did mention that. Somebody named Howard Aiken in Cruft Hall.

There were other articles. Couffignal had more than one. "Calcul mecanique" was the principal one I recall, but there were others developing the same line of thought. You will find references to other work in Europe on symbolic analysis of computing problems, in the science abstracts for the late forties. Of course, at that time Shannon was doing his work on the application of symbolic logic to the analysis and synthesis of relay circuits, and this included calculating circuits. He had this published article in 1939, I think it was, the prizewinning article in the *AIEE Journal*, which described some elementary computing circuits that had been synthesized by his methods.

At that time also, Shannon was very actively working on the formulation of information theory, what was finally published after the War with Warren Weaver. It had its beginnings in '39 and '40. His work would refer to the work of R. B. L. Hartley on the transmission of information in a *Bell System Technical Journal* article which I read, read and reread many times. I was very zealously pursuing, as I think I mentioned before, notions that we would now count as having the ability, establishing an information theory that would apply not just to communications but to computations, which would include the area of problem formulation and matching of computer to a given task.

MERTZ:

I was wondering in that regard, did you get involved very much in the problem of switching theory and also the hardware, such as the number of iron crossbars and so ...

CRAWFORD:

No, I did not.

MERTZ:

Switching equipment which later formed some of the basis for Stibitz' ____ when he had people assigned from the switching laboratory to provide him with the actual engineering counter-parts to some of his concepts.

CRAWFORD:

No. That area was never one of my hot buttons, and I never did pursue it with any zeal. The Rockefeller Differential Analyzer used experimental versions of the Number Five cross-bar switching equipment for the interconnection of computing components so

everybody around the project had some substantial familiarity with the equipment.

MERTZ:

Was that an innovation over the earlier differential analyzer?

CRAWFORD:

Well, it was the RDA, which was the third major step. The product integraph was the first one, and the so-called Bush Differential Analyzer was the step two, and then what was originally called in 1939 the Differential Analyzer became the Rockefeller Differential Analyzer, or RDA, was the third major step. It represented kind of the end of that line. Mechanical equipment turned electromechanical and beyond that it was the electronic analog and the electromechanical analog that took over.

MERTZ:

I guess concretized in things like REAC.

CRAWFORD:

The REAC was one.

MERTZ:

Was this switching equipment one of the innovations in that...

CRAWFORD:

Oh, sure. The early differential analyzers had used a mechanical switchboard which took all day to collect in a problem of any complication. It was a difficult job to maintain and keep it operating. It was held together by set screws and every hour on the hour you were tightening up the set screws. So that the RDA was an enormous advance in that sense. You could run a tape through a tape reader, and it would set up all the crossbars, and you'd have a complete change in the set up of the machine in a matter of minutes. So that was a complete transformation. But the RDA was in a sense transitional. It made very, very extensive use of electromechanical equipment for the switching, for the setting of shafts on integrators. For example, servo interconnections on all the components used analog-digital, digital-analog conversion equipment of pretty advanced design. But it was still basically a mechanical computing machine, and the next major step, of course, is where the electromechanical computing came in directly.

MERTZ:

Did this mean that with the advances in the third phase of this type of analog equipment

any new classes of problems could be run on the machine over ... or was it more a matter of ... more efficient, reducing the time, ...

CRAWFORD:

Well, the RDA was substantially bigger than any of its predecessors. It had eighteen integrators, which is a primary measure of capacity. The earlier one at MIT had six, and I believe the Moore School at Aberdeen had eight, I'm not sure. With that larger number of integrators and with its ample stock of gear boxes and adding gears and the like, it could handle problems of substantial complexity. It was used through the greater part of its active life for dealing with the equations representing the operations of the A-1-gun sight and other of the projects in Draper's instrumentation laboratory. He was the principal customer for the major part of its life, and the Navy was the other major customer.

MERTZ:

What kinds of problems for the Navy...

CRAWFORD:

Primarily ballistics, ordinary ballistics computations. The RDA was also used quite extensively for missile simulations. Some of the earliest NIKE simulation was done using that machine for Bell Labs, with [?]---. and others sponsoring the work. But that was still just a modest part of the load on the RDA. It was, as I say, Draper who became the principal customer into the life of the machine.

MERTZ:

Prior to the activation and full use of the RDA, getting to the very early days of the I Lab, I guess, did Draper or his group have any occasion to run problems on the Bush Differential Analyzer?

CRAWFORD:

I don't think so. I can't remember any work done for him on that machine. The machine was preempted by the Navy early in 1942 for ballistic calculations. There was just one set-up on the machine from '42 to '45. It was modified many times, quite extensively, but it was just the same basic set of projectile equations. Before the Navy preempted the machine, it had been used quite extensively for control system computations for some of the early missile projects, the DOVE missile worked on by Polaroid, and computing gun sights being worked on by Bell Labs and by contractors for the NDRC. That work became quite extensive probably in the middle of 1940 or late in 1940.

Up to that point the Bush Differential Analyzer had just been used in house computing

service for thesis projects and other research and development projects around the campus.

MERTZ:

And about that time--perhaps it's unfair to ask--was the proportion of in house generated projects largely from the EE department or did it service ...

CRAWFORD:

Largely from the EE department. There was a committee formed, probably in 1937 or '38, to foster the use of the machine around the Institute. That was headed up by Phil Morse, but we were not very successful. Some of the people who were dragooned into bringing their problems around really didn't have their heart in it, and you could tell from the records they left behind whether or not anything significant had been accomplished. Often as not, there had not been in most cases. In the summer of 1940, I went through the entire project file on the Bush Differential Analyzer. By that time there were roughly a hundred plus, maybe a hundred and thirty, manila folders containing the residue of records, set-ups, sheets, and so forth, for all the projects that had been run.

MERTZ:

Do you recall where those were?

CRAWFORD:

They were in the machine room at that time. I'd like to know now where they are, because I'd love to look at them again.

MERTZ:

Those I do not believe are included in the documentation available through the Library of Congress and the Smithsonian.

CRAWFORD:

Probably not. It would be very worth-while trying to track those down. It would be regretful if they were thrown out.

MERTZ:

Could you describe those folders just a little bit more? Were they labeled?

CRAWFORD:

It was just the Bush Differential Analyzer Problem File. MERTZ:

And the folders were numbered consecutively? CRAWFORD:

Yes, they were. It gave identification of the person and the name of the problem being worked on.

MERTZ:

Were they organized by problem?

CRAWFORD:

In serial numbered sequence.

MERTZ:

And the documents were fastened into the folder permanently?

CRAWFORD:

There was nothing very systematic about it. Some of them would have the Ecco fasteners, others were just stuffed in there.

MERTZ:

And they were with the machine actually ...

CRAWFORD:

Up to the end. The machine. of course, was shipped to Wayne State University, and the project file was probably sitting there at the time the machine was dismantled. What happened after that, I don't know.

MERTZ:

It predates the establishment of the MIT Archives up there by some years.

CRAWFORD:

Yes, it does. It was extremely interesting to go through those files because you could tell very easily from the kind of records that were left just whether or not anything had been accomplished, or whether the problem had just kind of dried up and faded away. I attach great significance to having gone through those files because it was in looking at the differences between the successful studies, and those that did not appear to be successful

that I got onto the notion that what we now call simulation is the proper mode of operation of analog computers. That's to be contrasted with the use of that kind of computer for equation solution in the more conventional sense.

MERTZ:

Do you recall at that time, was there a fair degree of departmental capability outside of the EE department for special purpose ...

CRAWFORD:

Civil Engineering was the principal sponsor.

MERTZ:

How about Aeronautical Engineering?

CRAWFORD:

No. I don't recall anything really extensive in Aeronautical Engineering. The Civil Engineering Department with Werber in it was quite active. Their simultaneous equation machine was rather widely publicized about that time. The Physics Department had a number of projects that related to automatic computation and automatic control. The very extensive spectrophotometric facilities that they had there bordered on computational capabilities at some points. In fact, they built [?] counting rings in an attempt to record wave lengths as well as wave numbers from the photographic plates. It was a very interesting project.

MERTZ:

And from the problems, we might follow the train of thought and examine the ones that didn’t work or just didn't seem to achieve fruition on being run or set up for solution on the Bush Differential Analyzer. Did this suggest that an analog technique was not appropriate to any of the problems that they ran?

CRAWFORD:

Oh, no. What came through to me in that review was the proposition that to use that kind of machine to represent the behavior of a system over time was one way to make progress. That often time is an independent variable and it's just the dynamic behavior of the electric motor or mechanical system or thermal system that you were studying, the transient behavior.

MERTZ:

Did any of these problems encounter difficulty in error analysis? The degree of precision required in engineering problems is not usually brought into this, but whether the degree of precision is such that what you're doing is measuring is actually the components and not the problem itself.

CRAWFORD:

Yes, I think these problems I would put in the category of equation solution as opposed to simulation. Error problems were very serious, and they were a key factor in the limited progress.

One very extensive use of the Analyzer was for certain wave function calculations. That was the kind of work done especially on the Manchester machine by Hartree and error considerations played a major part in the whole design of the set-up and operation of the machine. But I would count that work as not putting that kind of computer to work in the best way, and, of course, digital machines are ideally suited to that.

MERTZ:

Well, that was what I was leading up to by my question of the lack of success. That was, whether or not some of this led you to surmise that those equation-solving problems and those problems which could be reduced to essentially no simulation solutions suggested a different technique altogether? Or had you arrived at that conclusion in the Business School.

CRAWFORD:

No, I was in the Electrical Engineering Department. Oh, yes, we were working on digital computers at the time, as well as running the differential analyzer, so we were happy to project the use of the electronic computers for this.

MERTZ:

These were hand machines you're talking about.

CRAWFORD:

No. At that time the number one project in the Center of Analysis was the early work by Overbeck and by me on the digital equipment. The operation of the Bush Differential Analyzer was kind of a sideline, a bread-and-butter function.

MERTZ:

During which time you contemplated the other problem.

CRAWFORD:

That's right. Running the differential analyzer was a pretty onerous duty. Your talents had to extend from something close to mathematical talents through knowing where to oil the thing.

MERTZ:

I gather it was also a dirty task as well.

CRAWFORD:

That's right. We had a fascinating time during the War refining the Bush Differential Analyzer and improving it in a number of ways. We brought it to the point of computing the trajectories of 16-inch shells in faster than real time near the end of the War. That was kind of a thrill. Then we started out in life we were following the traditional methods of operation of the machine and we had a scale factor that had 256 revolutions of the time shaft representing one second in time. So that means it's at the rate of about 200 revolutions per minute. It was one minute per second. But, by the end of the War, we had 16 revolutions per second and even 8 revolutions per second so that we could run in almost real time.

MERTZ:

Taking the square root of the square root ...

CRAWFORD:

Yes. We made a complete transformation.

MERTZ:

Reduction by 200 [?]

CRAWFORD:

Yes, it was in doing that that it became clear to me what the eventual ramifications of analog computation were. If you could specify anything, you could simulate it.

MERTZ:

One question in this regard. When you became involved in the Special Devices, the SDD group, were similar limitations imposed by the nature of the beast, as it were? The Rockefeller Differential Analyzer had certain limitations. Did this cause you reasonably early on to opt for at least exploration of digital approach to the ASKA equations?

CRAWFORD:

No. Right at the end of the War we were pretty enthusiastic about the prospects of analog computation as well as digital computation. Within the Special Devices Center, we were 5 0-50, two analog projects and two digital projects.

[END OF TAPE II, SIDE I]

[TAPE II, SIDE II]

CRAWFORD:

I was mentioning that we had a pretty healthy respect for the capabilities of both kinds of machines and backed the analog for the two missile simulation projects, and then backed the digital for the work in the area where command and control systems were under simulation. They were targeted and they worked on missile range instrumentation data reduction. By mid '45 there was work actively under way on the use of digital computers for flight simulation for pilot training purposes and other training purposes.

MERTZ:

Was there any development in the field that stimulated this work? Was it ENIAC? Did it play a role?

CRAWFORD:

As far as digital computing was concerned. Well, sure. It played a big role.

MERTZ:

Towards the thinking of use of digital machines in training ...

CRAWFORD:

Well, very extensive proposals had been put forward as early as 1940-41 for the application of electronic digital computers, so actually ENIAC looked like kind of a special case, taking it over hand computation.

MERTZ:

RCA I believe had put forth a proposal on this [?] considered [?[fairly early on.

CRAWFORD:

Well, sure, work on the [?] and the Computron and related work in '40 and '41. That was targeted on military applications for fire control at that time. The work at MIT was targeted on fire control, too. Remember, my thesis was in 1942, and it was on digital computers for automatic control. It was fire control, but we had envisioned a very broad range of control applications. So, ENIAC was already later in the day, for those who had been around MIT from the mid-thirties. There was some resentment at MIT concerning the ENIAC project because the work had been turned off at MIT after the decision of the NDRC, so the Moore School was seen as having done a successful end around when they got the Army Ordnance supporting their project.

MERTZ:

Did MIT fairly early on promote proposals of their own which didn't receive the support of the official backing?

CRAWFORD:

Not really. I drafted proposals that had to do with the development of digital computing for fire control purposes about the time I was writing my thesis and before. Those proposals had been my input, and MIT's input was in Stibitz's survey. When Stibitz's work was turned down, it meant that the MIT work was turned down, and of course the persons who nominally had led the MIT developments were themselves gone and active with the NDRC program.

MERTZ:

Who? Caldwell ... Bush was.

CRAWFORD:

Caldwell and other EE people. Bush had left in '39.

MERTZ:

How about Warren Weaver?

CRAWFORD:

Weaver had never been actively associated with MIT. He didn't come on the scene ...

MERTZ:

I was thinking of his connection with Stibitz.

CRAWFORD:

He was the head of that committee, that's right.

MERTZ:

And the committee at Bell?

CRAWFORD:

They were quartered in Philadelphia, so they were not visible at all on the MIT scene. I think they were headquartered in the Franklin Arsenal, but I'm not sure.

MERTZ:

We digressed a little bit, I believe you were giving us a brief resume or hopefully amore extended resume of the problem formulation and the impact of reviewing these manila folders, dealing with the problems which had been run, or attempted to have been run, on t he Bush Differential Analyzer. You suggested some ways of formulating the problem of computer use or appropriate types of computer use, as one of the stimuli for pursuing digital application.

CRAWFORD:

Well, to [?] I thought about taking up the problem of problem formulation, thinking there particularly of how do you set forth, how do you write your equations to express it. Then given your statement of the problem, how do you then go about the systematic determination of the best machine means for treating the problem. Those questions were presenting themselves even in connection with the differential analyzer, the best set-ups. They present themselves in just how do you lay out the steps in best computation to handle a given task.

There was quite a bit of interest in systematizing, what we now call computer system design task, and the thesis that I attempted to write in 1941 was at least in part directed at those questions. How do you formulate the problems and how do you determine the optimum means available for treating the problems?

MERTZ:

At that time, whom do you feel were some of the individuals more actively and [?] concerned with the problem [?] ?

CRAWFORD:

I can't say that I did know anybody. Well, Kufenahl and some of these other people who had worked with algebras and logics for adapting computers and electronic computers, would be counted as working in this field.

MERTZ:

Was there anyone in the Cambridge community?

CRAWFORD:

I wasn't aware of anyone. The work that proceeded later at Harvard by Aiken and others concerned with the use of symbolic logic and other algebras for computer design is part of this general area. I'm not aware of any work to count as bearing directly on the problem statement and formulation without regard to the solution.

MERTZ:

Shannon's work is tangential.

CRAWFORD:

I would say that it is. I had another interesting exposure at that time that had me grappling with the problem. Irwin Shell, who was head of Course 15, the course I was in as an undergraduate, was a pretty innovative, energetic man. He wasn't content to see the differential analyzer being applied only in the established areas of engineering, electrical, mechanical, and so forth. He demanded to know how it could be applied in his area of business.

I fell heir to answering that question, and I was not satisfied with my answers. In fact, I can't say that anything that qualified as an answer was forthcoming. But I wrestled with the problem of, in a sense simulating business operations, at that time in kind of a crude way, trying to envision what would be the counterpart in the business sphere of Newton's equations in the sphere of mechanics and physics. And it wasn't very clear.

MERTZ:

Has it subsequently been clarified?

CRAWFORD:

It's getting clearer all the time. I count that as the root problem in the whole computer field. When you crack that root problem is when you're in a position to take the next major step ahead, the first consequential step after 1947, intellectually speaking, in the computer field.

MERTZ:

You're referring to the business world as a special application of what? Economic theory, or as a phenomenon that's quite distinct?

CRAWFORD:

No, the next chapter in systems theory is the way some people would express it. We talk

about a business as we talk about a management economic system, and if we talk about projectiles and missiles, we talk about them like a mechanical and physical system. When you develop perceptions of adequate generality and the functioning of systems in general, you can take uniform approaches to the one or to the other, or even to any systems at all, including organic systems.

MERTZ:

They both presuppose a very precise definition of what a measurable quantity is.

CRAWFORD:

Both presuppose a very precise definition of everything that's involved in the area of interest.

MERTZ:

I was thinking of ergodic theory in the sense of the word which is fundamental to making analogies between physical systems and other systems on the premise of measurable, ...

CRAWFORD:

We're not making here an analogy between a physical system and other systems, a business system. You're treating the mechanical system as an even more degenerate form of the general system than the business system.

MERTZ:

Well, whether or not one considers that treatment needing some kind of explicit abstraction of an analogy [?] , the essential point that I was thinking of in connection with computation is that you were talking about one can in the definition of the conceptual definition something about measurable ...

CRAWFORD:

To measure in a given situation presupposes an earlier definition and application of systems to that, to defining. The root problem is the adequate systematic general application of a method of definition. Which one you have to apply that gives you the basis for measurement, classification ...

MERTZ:

Did this lead you at all into the area of mathematics known as ergodic theory? The theory of what to measure.

CRAWFORD:

Well, if it has, I'm not really aware of it. I have a speaking acquaintance with the field, naturally, but I'd be hard pressed to establish any connections between the approaches I've been pursuing, and some of these well-established approaches in mathematics. I do have a conviction that what is taking shape in this area of problem definition stretches the traditional mathematics, and something qualified as new must take shape.

MERTZ:

Did any of this lead you to Poincare and some of his conceptions, which are measurable hypotheses?

CRAWFORD:

No. I'll go back to Leibniz in my estimation. (Laughter) I count Leibniz as having targeted the root problem nearly 300 years ago, and as having spent his lifetime working toward a solution, and as having made very important progress. But after Leibniz died the whole task that he had set himself was set aside and people have not even been interested in it until recently.

MERTZ:

Some people say that Gauss had something to say in this area.

CRAWFORD:

His contributions were of the first order but Leibniz's distinctive interests and contributions were in the area that would qualify as language.

MERTZ:

More abstract.

CRAWFORD:

His concept of the universal characteristic has to be related as a strong contender for the original proposal along these lines.

MERTZ:

Did any of this lead you into a problem of semantics and seminionics?

CRAWFORD:

No, not really. I became a devoted follower of Korzybski in 1940.

MERTZ:

The non-Aritotelian ...

CRAWFORD:

The non-Aristotelian systems. I count Korzybski as the precursor, as having provided the original formulations of ideas that are only now coming forth in adequate ways. To speak of general systems theory is to speak of what Korzybski was working on as well as [?] an other of that time, but I count Korzybski's work as having been in the main line of advance.

MERTZ:

His big opus came out ... I'm not sure ...

CRAWFORD:

1933.

MERTZ:

An edition that I saw was later, about '39 or '40.

CRAWFORD:

It's in its 4th now.

MERTZ:

I'm not sure which edition I saw.

CRAWFORD:

In response to your question. When I first came across it I believe it was in 1940, and I read it pretty promptly after I picked it up. My principal impetus in reading it the first time through at least, was provided by what I saw to be thee kindest remarks I'd ever seen in print about Spengler. (Laughter)

MERTZ:

There weren't too many people, at least to your knowledge at the time, of whom you were

aware, who were specifically interested in or at least producing published works on the subject of problem formulation.

CRAWFORD:

No, none at all.

MERTZ:

You touched on this one article in perhaps a series in the *Bulletin Astronomique*.

CRAWFORD:

Yes. I have no clear awareness at all today of my awareness of Leibniz at that time. I know I had some awareness, because both Spengler and Korzybski were thoroughly familiar with Leibniz's work and proposals. The problem that is presented here is essentially the problem that presented itself to Leibniz when he was still in his late teens, of establishing what he called the universal characteristic, the universal science, the universal encyclopedia. It's very interesting that although he pursued these ideas with great zeal and great competence through his lifetime, the extent to which others have taken up his pursuit is astonishingly small. Even the extent to which Leibniz's thinking has been taken seriously is astonishingly slight.

Another thing which fascinates me is that among the few who have paid any attention at all to Leibniz, one was Wiener, and the only account that I am aware of in any of the modern literature of Leibniz's ideas in relation to modern communication theory, computers, and the rest, is Wiener's *Cybernetics* and *The Human Use of Human Beings*. In *Cybernetics*, he says that "if cybernetics would have a patron saint it would be Leibniz," and in *The Human Use of Human Beings*, he says that "Leibniz must be counted as the intellectual ancestor of this work."

Wiener then goes on to review very briefly Leibniz's work, but points out that Leibniz's entire concern was with communication and with language and the relation between language and computing machines. And in spite of all the discussion of language in relation to computing machines, none of it really addresses the problem that Leibniz and later Wiener were addressing.

MERTZ:

Now you might want to follow that [?]. Would you feel that's still the case today?

CRAWFORD:

Oh, very much so.

MERTZ:

Since that time there have been others who have published on the subject.

CRAWFORD:

I'd make a clear distinction between writings in the area of general semantics and writings in other areas known as semantics. I make a distinction between Korzybski's work and Carnap's work, for example, because in my view there's no connection at all. Carnap's approaches do no take you toward the solution of the problem in question.

MERTZ:

How about the logical foundations of mathematics and such fields as that?

CRAWFORD:

No, I don't think it takes you very far.

MERTZ:

There has been some work by Goedel, for example, on the subject of whether or not one can deduce all hypotheses from a given formulation...

CRAWFORD:

No, it's very important work but I don't think it has direct connections with the problems of problem-formulation, the specification of the functioning of systems.

MERTZ:

It might be suggested that perhaps a different approach that might include to some degree this subject ...

CRAWFORD:

It might, but in my own experience the established branches of mathematics are remote from this area of specifying systems in general terms, including [?] systems, organics systems, economic and business systems, electromechanical and physical systems.

MERTZ:

Certain specificity of that sort is not found in mathematics. Who would you day is today working on the subject of computers? Who is doing the innovative, suggestive or and insightful work?

CRAWFORD:

Well, modesty limits the extent to which I can respond to you question.

MERTZ:

Is this what they call an improper subset of the set? That is to say, have you defined the field in such a way that all the subsets consist of Zero and One, namely yourself?

CRAWFORD:

No, not quite. But the number of occasions where this problem has surfaced in the computer field is astonishingly limited. What I regard as the first is the language structure group at CODASYL, the group that was formed at the same time the COBOL group was formed, and formed to establish the basis of a longer range development of what is called a problem, well, a machinery dependent problem in defining language. In the report they published in 1962, there was very clear recognition of the problem area we're talking about. But their work faded away following publication of that report, and there has been no organized effort taking up that work since, at least until very recently.

MERTZ:

If I might just ask you a question in that regard. Would you say in review that the development of such things as COBOL and FORTRAN, in its various forms, has tended to influence those people who might otherwise think about this problem, into thinking about machine languages in a way which is at best tangential or perhaps even quite apart from what you are interested in, in terms of problem-formulation?

CRAWFORD:

It's a difficult question to answer. Nominally they are in the direction of the problem-formulation ...

MERTZ:

I was trying to distinguish between the nominal case and the real case.

CRAWFORD:

It is in my view a clear distinction. In the case of the FORTRANs and the COBOLs, you have languages which in their essentials are procedural languages. They are languages for describing steps that a computer would take or a person might take to solve a problem. In contrast to that, ;there is the notion of language made up simply of declaratives where you define a result to be secured where your definition of that result

covers all of the logic governing the production of that result, but says nothing about the specific procedures that might be used to produce that result. But then given that declarative definition of a result, you can then expect the computer itself, to find the procedures that would in a given situation be optimum for producing that result.

MERTZ:

A serious question has been raised as to just how optimal the given languages we have just referred to are. In fact ...

CRAWFORD:

Well, I'll just make a point that even though COBOL and FORTRAN would appear to be heading toward this non-procedural kind of language, in fact there is an impassable barrier between the two lines of development, the lines of development of the FORTRANs and the PL/Is and the non-procedural. And to get at the non-procedural you're going to have to forget all about the procedural languages and take up the task of just specifying the things in your environment that you must specify or know about if you are going to deal with them in adequate ways. And it's very, very difficult to get this point of view across in the prevailing climate of the computer field.

MERTZ:

There is a possibility that an intellectual basis for this approach in [?] by Wilhelm Wittgenstein, who in his TRACTATUS LOGICAL[?] PHILOSOPHICUS came to [?] .

CRAWFORD:

Generally. He of course is known by anybody who knows Korzybski. No, I would count Wittgenstein's work as not bearing directly upon the matters of issue here.

We were mentioning some of the other efforts. I mentioned the language structure group. In 1963 there was quite a bit of publicity given to what was called implicit programming. General Terhine of the Air Force, was setting forth the proposition that you could in fact expect to be able to define problems, define results, and have the computer itself solve the problem, produce the results. Then Tom Steele affirmed that there was nothing contradictory about these proposals, but the actual work toward the realization of these possibilities is still very limited.

In 1967 the ISDAS project began at Western Reserve; now being carried on at the University of Michigan, and I would count that project as having targeted this problem again after all this time.

MERTZ:

Who at Western Reserve and Michigan ...

CRAWFORD:

Dan Teicherow in both cases, first at Western and now at the University of Michigan, is leading the project. He's got a gung-ho project going based on the proposition that you can devise a machine independent problem statement method, a non-procedural problem statement method. That the machine, which sets up when it's applied to a problem, can itself after validating the problem definition, generate the specification of the optimum system for solving the problem. And then, from the specification, generate the system itself.

MERTZ:

When you say the optimum, that means the procedural language?

CRAWFORD:

No. It means in an economic, engineering, economic sense, that here are the available storage and processing resources. Here is a definition of the inputs and outputs. What is t he best choice of resources, organization of resources to satisfy the requirements that have been specified? Where the customer and the designer between them establish what they mean by optimum.

MERTZ:

Does he have any colleagues that you feel have been working on this?

CRAWFORD:

He has three or four or five faculty members at the University of Michigan and a dozen plus graduate students working, and the project has the backing and support of a number of outside organizations, computer manufacturers and large users of data processing. Teicherow is getting a very respectful hearing for his proposals, and you can see from the reactions that he gets, that people recognize that that problem is the real problem in the next--it has to be solved in the period immediately ahead. This recognition has grown only very recently, because two or three years ago, Teicherow would not have gotten a respectful hearing.

MERTZ:

One question comes to mind. Are there implicit in the statement of this problem any technological problems? By this I mean hardware, components?

CRAWFORD:

No.

MERTZ:

this is not a problem of componenture?

CRAWFORD:

No, this a problem of problem-statement. It is ultimately a linguistic problem, but not a linguistic problem in the sense of the established linguistic disciplines.

MERTZ:

Logic perhaps.

CRAWFORD:

It's a linguistic problem in the Leibniz-Wiener sense. It is more of the symbolic logic. Its linguistic cousins are mathematical formulas as used in engineering, and its; linguistic cousins are accounting statements, account numbers, specification of relations among accounts.

MERTZ:

Aside from his group, are there any other groups in the world elsewhere that are working on it?

CRAWFORD:

there are a dozen other projects around the world pursuing the general possibilities that we're talking about. I would count the Teicherow project as having come to a sharper focus on the problem and approaches to its solution than others that I know about. IBM has an effort along these lines, fairly well known, but not as well known as it should be. It is a program known as TAG, Time Automated Grid, a technique by which a customer can define his problems and requirements in independent machine processable terms. Then he can get a machine assist first in validating his problem statement, the requirement statement, and then secondly in defining a system for satisfying its requirements. The Michigan project is in a sense building on TAG. There is one other effort at National Cash that is in the same ballpark, and it is Accurately Defined Systems, but it' lacking some of the features of the TAG-ISDAS approach which I would count as required features of a successful approach.

MERTZ:

Are there any others that seem to have some of the features that you feel are necessary if not sufficient conditions for the proper pursuit of this ...

CRAWFORD:

No, I'm not very excited about any of these projects that I know about.

MERTZ:

Where would you say is the main focus internationally? Is there any?

CRAWFORD:

There is no international focus. The ISDAS people have been at some pains to learn about every related project around the world, and their documentation includes the documentation of other projects. There are, as I say, a dozen of fifteen, and the number is growing right now every day. There is some number like that of projects taking up this problem.

MERTZ:

Do they get a respectful hearing in the progessional literature?

CRAWFORD:

Not very much appears in the literature. It's sporadic, and I could propose an explanation that the results are so provisional, so tentative, that nobody feels like publishing it. We had a rash of publications many, many years ago: Mathematical Approaches to Information System Design, but that faded away for lack of tangible results.

MERTZ:

About when was this rash?

CRAWFORD:

The late fifties. Work on decision tables was an early attempt at this kind of thing. In decision table approaches you had approached that people visualized giving forth to the designer, the systems and procedures man, and to the computer a specification of the logic governing design, governing processes of design calculation. There was important progress in the development of deciding table approaches, but beyond a certain point, the going got sticky and the efforts tended to fade away.

There has been a resurgence of interest because people are now recognizing that the

answers must lie in that general area, and are to be found in that general area, but people are a little bit more cautious now, in making claims.

MERTZ:

They rush with all due prudence into print.

CRAWFORD:

And setting schedules.

MERTZ:

Has your work with IBM been largely in this area, or did it more or less wind up in it?

CRAWFORD:

It wound up in this area. We recognized that you had to solve the problem of problem-definition to take the next significant step beyond where we stand today and have stood for a long time past in computers. I've been engaged in it very nearly exclusively for the last twelve years.

MERTZ:

Prior to that, correct me if I'm mistaken, were you not interested in and involved in IBM random access storage problems when you first came to IBM?

CRAWFORD:

Yes. I think I mentioned in my last outing that one of the reasons why I was drawn to IBM was that I rated IBM as having the lowest, smallest likelihood of pursuing magnetic tape approaches.

MERTZ:

That's right.

CRAWFORD:

It was partly right at any rate. IBM did pioneer random access. Jack Potter of Potter Instrument of course, kind of kicked it off with an assist from Aberdeen Proving Ground in the late forties. But it wasn't until '53-'54 that IBM launched a major effort that gave the first, I should be calling direct access storage equipment, in 1955.

MERTZ:

Did you go directly in New York from your involvement with the [?] Intellectron [?] ?

CRAWFORD:

Yes, I just moved to 30th Street, uptown.

MERTZ:

And with whom were you associated when you came into IBM? Was it with Dr. Hurd?

CRAWFORD:

No, this was in the organization known as Future Demands, under Gordon Roberts. This was a gung-ho outfit pushing the first business use of computers, and the machine became the 702 and then the 705. It was a separate group. Cuthbert Hurd's group was pushing the 701, and the 704 similar line advance through the fifties. I had had extensive dealings with IBM prior to joining. The de Flores Company was trying to team up with IBM in their venture for Newsweek, and we had some IBM equipment as part of the system that was under development. So, when the Intellectron venture ended in 1952, my choice was really a three-way choice: IBM, RCA, or UNIVAC, for we had dealt extensively with them as well. The decision was finally IBM, because I think the random access, their responses to my proposals even as an applicant in random access, were very supportive in contrast really to UNIVAC and RCA. At that time the typical engineer's attitude was, people don't need the information that fast. They can wait till the tapes get wound, unwound more.

But you were speaking of inquiring about anecdotes, and anecdotal treatment of this, and there was kind of an interesting anecdote here. In connection with settling the affairs of Intellectron we had a visitation from a number of IBM people, including Red Dunwell and Jim Dreeve and two or three others. I might say that de Flores and Special Devices had extensive associations with IBM going back into the War. We had lunch at an establishment where we and de Flores ate with some regularity, and they featured fine draft beer. Most of the IBMers involved had a stein of beer. Well, they couldn't be quite as they portrayed the company. Between that and finding Jum Dreeve and Steve Dunwell very, very able, very fine people I was drawn, attracted, that way too.

MERTZ:

Was you work pretty much exclusively associated with direct and random access and to that problem, prior to the last twelve years?

CRAWFORD:

It was random access approaches, but it turned into what would now be called telepassing

and data-based data communications systems. I was a member of the team that took up the study of what became the Saber System late in 1953 and was associated with that actively until the end of 1956. I wrote the original functional specifications, and the original proposals, made the original proposals to American Airlines and helped get the project going in 1956. Then I turned my attention to similar systems approaches in other parts of the market, primarily in manufacturing. We conducted quite extensive studies of the application of data-based data communications systems in manufacturing, but we targeted the problem-definition problem, the requirement-specification problem, as the key to the whole progress in 1957-'58. We've been hacking away at the problem since that time.

Had we known how long it would take to get within hailing distance of a solution, I don’t think we would have targeted it and stayed with it the way we did. But throughout that period the problem has had the appearance of the pot of gold at the end of the rainbow. It's just right there, just another day and you're going to have your hands on it. But the ramifications of the problem are immense. When the problem is eventually solved it’s going to turn out to be childishly simple in a quite literal sense, in the sense that we will have children dealing with it in school the way they deal with their arithmetic and letters today. But to ferret it out is quite a project.

MERTZ:

If I might, I'd like to recapitulate in terms of your own career and what you feel to be the highlights.

CRAWFORD:

This is another one of these areas when I feel that I'd like to do a little reflecting, but I will just submit a few remarks.

You inquired about going to MIT, and I think in the earlier tape I mentioned that my father was an engineer and I had been attracted toward engineering. In the high school at New Trier I had heard that MIT was tops. As a Sea Scout I got fired up about naval architecture and marine engineering, and that then had me settled on MIT. Largely as a result of the influence of my father I changed to the business program, that being his choice. I didn't regret it because it did give me ample opportunity to pursue elective studies and pursue them very energetically in many areas.

As a freshman I first got the taste of computers through R.D. Douglas, my calculus professor as a freshman, who was a bug on mathematical instruments and graphical methods of computation. Then as a sophomore we had an opportunity--Ed Schrenk, who was doing a doctoral dissertation on cosmic rays making use of the differential analyzer, and I followed his homework that was being done over by [?]. It was a quite exciting piece of work, so that kind of confirmed my interest, and then by the end of my sophomore year I had laid out a course of study that took up subjects in mathematics,

physics, instruments, and electrical engineering that would qualify as a study of computers and related matters.

Then I took Caldwell's course in what was called 'Mathematical Analysis by Mechanical Methods,' which was the first course dealing with automatic computers, and it was in that course that we first heard about electronic digital computing proposals of Bush that kind of cast the die and I've been fully engaged ever since that time.

It's a very interesting fact concerning the whole development of computers, that right at that time at MIT, in '38, '39 and '40, the future course of computers was perhaps as clear as it has been at any time since, with the random access telecommunications approach already projected at that time. The use of the computers in automatic control, numerical control of tools, process control, had all been placed on the table at that time.

MERTZ:

This still anticipates somewhat pulse frequency technology and the development that evolved during the War, radar and the like.

CRAWFORD:

To some extent, but it didn't really. Given five digits, five bit [?] codes, telegraph, teletypewriter codes, you had the starting point for the telecommunications. And of course Stibitz was using them and demonstrating them in 1940 and '41.

MERTZ:

Well, we're about to run out. Thank you, Mr. Crawford.

CRAWFORD:

You're welcome.

END OF TAPE II, SIDE II