INTERVIEWEE: R. G. CANNING

INTERVIEWER: Robina Mapstone

DATE: August 10, 1973

RM: The date is August 10, 1973 and this is Bobbi Mapstone and I'm talking with Dick Canning of Canning Publications, in Vista, California. This is an interview for the Smithsonian Computer History Project.

All right, let's start off by going back and just talking about where you went to school, your major, military service and your exposure there to electronics and radar.

RC: I took my college work at the University of Cincinnati, Bachelor of Arts degree in mathematics, graduating in 1940, and went into the Army in 1941. I ended up in the Army Air Corps, got my commission in 1942, and went through communications training in the Air Corps. Then I was sent through the Harvard and MIT program called ESMWT, whatever that stood for, on radar. The Harvard portion of it was not classified and was taught by professors from various parts of the country; a fairly good electrical engineering course. The MIT portion was very specifically radar and this was a classified course. Following that, I went to Boca Raton, Florida, for additional training, and then to Bell Labs for more training on the APQ 13, radar bombing set that

was going on in the B29s. I was sent to India with the first group of B29s, first of all as a squadron radar officer and later as radar maintenance officer for the 20th Bomber Command. I came back to the U. S. in 1945 after the Bomber Command was pulled out of India, in preparation for going to Okinawa, and came back for additional training on a new bombing set. The war ended right then.

I went looking for a job and I ended up at IBM in late 1945 with their Radiotype division, and went to IBM Endicott to work on this project. They were producing Radiotypes there, but they decided that Radiotype was not a big division and it was in competition with a very big customer of theirs, namely AT&T, so they sold it to Globe Wireless. I stayed at the IBM Endicott plant for a year as the Globe Wireless representative at Endicott. Finally, when all production was completed on the Radiotypes that they were making, I had my choice of staying with Globe Wireless or switching back to IBM. I chose the latter. I stayed at Endicott for another two years and then in 1949 was moved down to Poughkeepsie where I was stationed at the Tube Laboratory working on solving a problem of reliable vacuum tubes for the IBM 604. I left IBM in May, 1950, and came to the West Coast to take a job with the Electronic Engineering Company at the U. S. Naval Air Missile Test Center at Point

CANNING

Mugu.

RM: What was the name?

DC: Electronic Engineering Company. They were a major contractor for the Range Instrumentation Department at that time.

RM: Can you talk a little about the kind of work you were doing at IBM to make the tubes reliable?

RC: We had two main methods of attack for the 604. The main problem was that the 604 used 6J6 television tubes that had very tight spacing between the grid and the cathode of the tube. Vibration would cause little specks of cathode coating to come loose and become lodged between the cathode and the grid, causing a short circuit. Some of these short circuits would be annoying in a television set; they might show up as something like that on the face of the tube. In a computer they were deadly.

IBM set up a tube laboratory at Poughkeepsie to see how they could come up with a reliable tube for computers, and the 604 was the best testing ground at that point in time. The Tube Laboratory, of which I was a member, did two things. It worked with the manufacturers--this was RCA primarily at that time--to see what they could do to give us more reliable tubes. It turned out that the 6J6 tubes that were good for the 604 were not so good for television sets, and vice versa. RCA instituted 100% inspection, which

they usually didn't do, and really this comes out wrong, but the rejects for the television industry were just what IBM wanted. This improved the situation substantially, I would say, but at the same time IBM wasn't quite sure if this was where they wanted to put all their hopes and faith in the future. They hired a very good engineer, who by the way is working here in Oceanside.

RM: Who's he?

RC: "Red" Corson, B. R. Corson. He was the first professional vacuum tube engineer hired by IBM. He's the one who set up that tube laboratory. He now works for Hughes in Oceanside. Red Corson adopted the approach that the tube should be designed specifically for computer-type use, so he was working on a variety of tube designs. Most of these were small amplifier-type tubes as opposed to display cathode-ray tubes.

The upshot of it was that IBM was able to get the tubes they wanted from the manufacturers. I don't believe IBM ever went into production with tubes designed by the Tube Laboratory. But in the early days, let's say the 1947-48 period, they just weren't sure whether they were going to be able to get satisfactory vacuum tubes, so they used these two methods of attack.

CANNING

RM: If they had not been able to get them through another company, then they might have set up their own operation?

RC: In all probability they would, because they had a lot riding on the 604. It turned out that it was pretty effective procedure that they ended up with. I think there were something like twelve hundred 6J6s in the 604. They were reasonably critical tubes, due to the tight spacings, but even so IBM ended up with pretty effective tubes.

I remember I was on a travelling business show that IBM put on in 1949-1950. We went from New York to Detroit to Chicago to Kansas City to St. Louis to Atlanta to Washington to Boston, and put on the show in each city for several days. There was a Card Programmed Calculator in the show, the computing part of which was the 604. In order to explain to customers and visitors how the dickens vacuum tubes could count and how you could use them for computation, we had a series of display panels that would show a counter in operation. It was built around 604 panels and used a bunch of 6J6s. We must have had about six hundred vacuum tubes, something like that, in this series of display panels, and most of them were 6J6s. I was responsible for this display--setting it up in each city and giving my share of the explanation talks. In all that travel--setting up the display, running it for three or four days, taking it down,

putting it in a truck, trucking it for several hundred miles to the next city, and doing this for all those cities I mentioned--I only had to replace two vacuum tubes.

RM: Oh, that's nice.

RC: So that was, I think, a pretty effective demonstration of how those tubes could hold up.

RM: Yes.

RC: But, still, no question about it, when the transistor arrived, it just put the vacuum tubes out of business.

RM: Yes.

RC: Now is that a sufficient discussion of reliability?

RM: That's good. Who were some of the top-echelon people in the Tube Lab?

RC: Red Corson, as I mentioned, was the key person. Abbie Feindel (Abbott, I think, was his first name) was second in command. He was the tube engineer that Red brought in from Tungsol, or one of the major tube companies. After Red left, Abbie became the manager of the Tube Lab for awhile. Ralph Palmer, the senior engineer on the 604, was Mr. Electronics in IBM at that time; Jerry Haddad was one of his key engineers. There were many others. This was all in the eastern U. S., and I'm sure you've got that information already.

CANNING

RM: All right, so you came out here and joined the E.E. Company. Can you tell me a little bit about that company, what their charter was, and what they were doing?

RC: Their charter was to help the U.S. NAMTC (Naval Air Missile Test Center) Range Instrumentation Department staff up and get going. Things were moving at a very rapid pace; Civil Service had its problems of getting people and the E.E. Company had a contract to help them acquire personnel and to actually perform work that would eventually be done by Civil Service people. I was brought in primarily to head a section that would operate the RAYDAC Computer being built by Raytheon. I joined them June 1, 1950, and I left in May 1952. The RAYDAC still was not there, so that part of my job never did really materialize. (laughter)

RM: (laughter) What did you do while you were there?

RC: Well, they also had a very urgent data reduction need. It was originally conceived that the RAYDAC would be an on-line, real-time, data reduction computer for missile firings. That data would come back via radar and telemetry from the missile, be fed directly into RAYDAC, be reduced in real time and displayed to the engineers. This turned out to be an invalid assumption, and it caused the RAYDAC to be quite late in delivery, because at that time the most reliable memory mechanism that they had was mercury delay lines. Raytheon

determined that for real time operation they had to have their pulse trains in the mercury delay line operating at eight megacycles, or some gosh-awful quantity. It was five or eight, something substantially beyond the existing state of the art. All of the other circuitry, therefore, had to operate appropriately fast. It was because of this, picking an operating point beyond the state of the art, that Raytheon had an awful time making it work. I think the logical design of RAYDAC was quite a bit in advance of its time. What they conceived in the late 1940s really was modern in the middle 1950s. It was quite an impressive machine from the logical standpoint.

When the RAYDAC didn't appear, obviously Point Mugu needed some means of reducing data. Reducing the data means that non-linearities had to be removed. The engineers would know that a transducer in the missile was non-linear; it had been measured and determined to be non-linear before the firing. When the data come back, with the readings that this transducer had given, some means were needed to remove that non-linearity. What the missile engineers finally received had to be a true picture of what the acceleration was, or what the temperature was, or what have you.

CANNING

Those of us who were brought there to work on the RAYDAC, and there was no RAYDAC yet, were put to work on designing and constructing what ended up to be an analog data reduction system. We used analog techniques because they were, for our purposes, more economical and more readily available. I think at that time it probably was a good design choice. Today, however, I think the digital techniques would far outweigh analog, but at that point in time our choice was analog, and we got a data reduction system of sorts designed and built. So that's what we worked on.

RM: Did E.E. Company design and build it themselves, or did they contract it out?

RC: We contracted it.

RM: Who built it?

RC: Oh! We specified the characteristics of the different components of the system, put them out for bid where we could, and then would take the lowest bidder. Where we couldn't, where it didn't seem feasible to have it bid, or where we were having trouble specifying it in enough detail, then we'd have to design and build it right there at Point Mugu. So it was a combination of events. Raytheon did deliver some equipment of a data reduction nature that was under Project Hurricane; that was the name of the RAYDAC

project. Our analog data reduction system was a pretty low-level hurricane, so we called it Project Breeze. (laughter) We were able to use some of the data reduction equipment that Raytheon built. Also, I think Hycon was the company that ended up as the contractor on one of our main components. What in the world was it? Well, I've forgotten the name of the device,but it was the main linearizing component. As I say, the system was a mixture of equipment that was put together from various sources.

RM: So actually the Electronic Engineering Company was set up more as a service bureau type of organization. Is that a fair statement?

RC. No. E.E. Company had gotten an early contract with the Range Instrumentation Department and performed so well and did all they could to serve range instrumentation that I think the government decided to expand the use of them for services that normally would not be done. There was such an informal arrangement it was hard to tell whether a person was working for the E.E. Company or Civil Service. Many, including myself, at some later point in time when they had the details worked out, transferred over from E.E. Company to Civil Service, which was the original intent of getting us there. So E.E. Company was a contractor that was doing all it could to serve the customer and get the job done. They were under a very urgent time schedule. It was recog-

nized on both sides that this was a temporary sort of arrangement, and that as soon as they could they wanted to get on the more formal type of arrangement between the contractor and the Navy. Sure enough, by 1951, I think it was, the Range Instrumentation Department was well-enough staffed that its jobs were then well-defined, as were the E.E. Company's jobs, and no longer did we see this mixture of people working on the same job, half of them contractor and half of them Navy.

RM: Did they ever get into building general purpose computers?

RC: E.E. Company?

RM: Yes.

RC: Not while I was with them. One of the men that joined within a few days of when I joined was Roger Sisson. They'd recruited him from MIT where he'd just gotten his Master's degree and had worked on the Whirlwind computer. Roger stayed with E. E. Company and they moved him down to Los Angeles about the time that I switched over to Civil Service. Roger is back in Philadelphia and he would be somebody who could answer the question better than I could. To attempt to answer your question, though, I don't think that E. E. Company ever got into general purpose computer design.

I know Roger was advocating the use of digital techniques,

and I think some of the other members of E.E. Company were pushing digital techniques at a time when analog was still sort of dominant. I mention Roger because in October 1954, he and I got together in a partnership, Canning, Sisson and Associates.

RM: Right. Which I'd like to get to. Just one more on the E.E. Company. Do you know what happened to them? Did they merge with somebody else, or fade out, or are they still extant?

RC: I think they're still in business in Santa Ana. Let's see, the key names were Burgess Dempster and Bob Bonney, Dempster was the president, Bob Bonney was the vice president in charge of engineering. In the late 1950s, they decided that trying to run a company in downtown Los Angeles was not the way to live. They looked for some location that was within thirty to thirty-five minutes driving time from downtown L.A. They picked Santa Ana, and the last I know they are still there.

RM: Okay, I'll check it out, because they'd be good to talk to.

RC: Yes. They were very closely associated with Point Mugu in the early days. I haven't talked to anybody at E. E. Company for ten years, so I'm out of touch. Bob Bonney would be the one to contact if he is still there.

RM: Okay.

CANNING

RC: A couple of other names: Bill McQuiston, and Ted Jarvis. I don't know whether either of them is still there, but both were associated when I was.

RM: Project Hurricane. Was Hurricane the name of a missile?

RC: No, it was the name of the project for which Raytheon was prime contractor to provide the RAYDAC computer and other related data reduction equipment to support the real-time reduction of missile test data.

RM: When you were considering the data reduction work and you went analog, did you consider DDAs?

RC: Ah! Yes. Let's see. We certainly talked to the people at Northrop. I knew Dick Sprague, Harold Sarkissian, and Don Eckdahl.

RM: About 1950 they had their MADDIDA?

RC: That's right. Yes. They had MADDIDA operating. We considered this as a way of linearizing non-linear data. I can't remember why we didn't follow that out. Well, I think perhaps we might be getting into personalities right here. The man at point Mugu who was really in the key position on this data reduction system was Bud Sorvaag, and Bud's background was radio. When Roger and I proposed the concept of Project Breeze, an analog data reduction system, I think this appealed to Bud very much. It was the concept that he knew and liked. He didn't have much background in digital circuitry.

CANNING

MADDIDA wasn't digital in the sense that we think of it now. It was and it wasn't. It was sort of hybrid, in-between. By the time we came across MADDIDA and considered that type of technique, I think we were deeply committed to the analog approach. Thinking back, I feel that we would have had a hard time trying to sell the concept: "Okay, let's scrap all of this analog work we have done up to now; it's wrong. Let us switch over to the MADDIDA concept." I just don't think we'd have had a chance on that. I do remember we did investigate it and consider: how could we use a digital differential analyzer to linearize non-linear signals? We would have had to convert signals from the missile that came back in analog form to a puluse train where the rate of the pulses was proportional to the measurement. This was just a little bit of a problem, as I remember, the techniques we were using could use those analog signals directly and did not require a conversion; the MADDIDA technique required a conversion and that was a problem. There we were. We just didn't choose to go that road.

RM: If I were to talk to somebody there who was fairly high up in the hierarchy, can you think of who I should try and contact?

RC: At Point Mugu?

RM: Yes.

RC: How high do you want to go? Like Chief Scientist?

RM: No. I would just like somebody who was there when they started to realize that they needed a machine, and the events that led up to their contracting for the Raytheon; that aspect of it.

RC: Fred Arndt. I think he was head of the Range Instrumentation Department. He later went to work for Hughes in Culver City, and I sort of lost track of him after that. He would be one. Then the Office of Naval Research is the one that funded Project Hurricane--I'm sure it was ONR. What was his name? Charlie Doersam. I have a feeling that those two leads might uncover something. And Bob Bonney, of course, from E. E. Company standpoint.

RM: That's good. All right, you left there in 1952.

RC: Yes.

RM: What did you do then?

RC: I went to UCLA. A new project was just being set up by Melvin Salveson, who was in the process of getting his Ph.D. His thesis was the basis for getting this project funded by ONR. All of a sudden I'm beginning to wonder whether I'm confusing Doersam's role on Project Hurricane and this UCLA project. I have a feeling that ONR was involved in both but I cannot remember now whether Doersam was on one project or whether he was on the other. I'm vague on this.

CANNING

Okay. Salveson was able to get money from the Navy to set up a research project at UCLA on the use of operations research techniques and computers for solving production scheduling problems. I would qualify this further, as production scheduling in a job shop environment. This involves extremely difficult combinatorial problems. His doctoral thesis had proposed a method of attack on this type of problem. That's how he was able to get the money. I heard about it and applied to UCLA for a job to work on the use of computers for production scheduling.

To go back to IBM, I took an evening course when I was at Endicott--IBM, had an excellent educational program for employees--on using punched cards methods for production control, and was fascinated by it. They didn't have a textbook for the course, so I kept pretty good notes. After the course was over, I was really charged up; this subject really appealed to me. I wanted to see if I could get into the part of IBM that worked on production control using punch card methods. I was in electronic engineering at that point in time. I wrote a textbook based on the notes I had taken. It was a couple of hundred pages in length, pretty much based on what they had taught. I'm sure it was not the world's greatest textbook, it was pretty hammy as I

look back on it now, but anyway it was something. It was a beginning point. I turned it in to the production control people, and said, "Say, I'd like to work for you. This electronic engineering is fun, but production control really interests me." I got nowhere. It was like swimming upstream. You couldn't get out of electronic engineering at that time; IBM knew that it was heart of their future business. There was no way I could get myself transferred. I tried, and I kept trying for several years, and just never made it.

When I heard about this project at UCLA using computer methods for essentially the same thing, I said, "Hey, this is for me. This is what I want to get into." So I joined in the early summer, I guess it was 1952, and worked on it even beyond the time that Roger Sisson and I set up our partnership. After we set up the partnership he also joined the production control project. However, at that time we were part-time on the project working whatever hours we could spare. Our first obligation, of course, was to our consulting.

While I was at Point Mugu I had joined the Institute of Radio Engineers, IRE, so as to participate in the professional group in electronic computers, I think it was. They had monthly meetings in Los Angeles and a bunch of us

would go down most every month to the UCLA campus. Harry Huskey was very active in IRE then, and I think it was in late 1951 that the idea of a symposium first came up. For some reason, Harry asked me if I would be general chairman of that symposium, and I agreed. We had our first symposium in May 1952--April 30, May 1 and 2, 1952. I believe this was the first symposium on the West Coast on electronic computers. We had a pretty good program. You might want to borrow this.

RM: I think I might, yes.

RC: This is my only copy, so I'd like it back. (laughter)

RM: Yes.

RC: You will find there really were some pretty impressive papers in there. Norman Gibbs of Raytheon talked about problems involved in magnetic tape recording. This was the first exposure that some of us had to dropouts in magnetic tape recording, dirt embedded in the tape, splicing of tape and all the different problems it had.

Those of us involved with the symposium made an awful mistake; we didn't really know too much about what we were doing. We had it on the UCLA campus, and therefore it was not possible to charge admission. We had no money, but we felt that there should be a proceedings. We tape recorded parts where the people hadn't given us any papers.

CANNING

We took orders from the attendees on the basis that they would pay five dollars, I think it was, for the proceedings when they were published. After it was all over we recognized that we had to get this darn thing produced. Well, what a mess it was! I remember spending evenings at UCLA where somebody had a tape recorder I could use to do some of the transcribing. We got all the help we could for the transcribing.

I figured the way we were going to finance the proceedings was to get the printer to delay billing for thirty days after he printed the things. We'd mail them out, the people would send in their five dollars, and then we could pay the printer. It turned out that the printer was just as much on a shoestring as we were. He printed the things and submitted his bill simultaneously, and had written some checks which he figured would be covered by our check. When we found that out, oh, my God, we were in trouble! Luckily, one of the fellows was able to put his hands on enough money so we could pay the printer. But it took us a long time to collect all our money. This was our first exposure to putting out the proceedings.

RM: Great. We had started to talk about Salveson's doctoral thesis.

RC: Right.

CANNING

RM: Would you say the thesis is one of the key original documents on this work?

RC: Yes. Yes, it certainly would be on the use of operations research methodology. He didn't get much into computers, because Mel hadn't studied them. He wanted quantitative methods applied to the production scheduling problem, and he recognized computers would play a role. That's why he got people such as myself on the project. So it would be a key document for getting people's attention directed to this subject. The model that he presented in this thesis was a fairly conceptual one. It wasn't something that could be programmed; all that type of work was yet to be done. But he said, "Here is what the problem is like, and we should dig into it more." From that standpoint it served a very useful purpose.

RM: So the project was set up to do that digging in?

RC: That's right.

RM: Where did it lead? Where did it go? Did you get into using computers? Did you use the SWAC, for instance?

RC: Yes. Yes, I tried to use the SWAC early on for a very small model which I wanted to explore, a queueing model. That's just at the time when--what was his name? Dr. C. B. Tomkins, I think. This was after Huskey had left, and Tomkins was the head of INA, where the SWAC was located. He decided to

institute a new policy about the time he received my request. Under the new policy, I had to make a thorough study of the literature of all of the work that had been done on queueing theory, to see if it would apply. I knew darned well it didn't; also my friends on the project agreed that it didn't; but I still had to do it before I could get any time on SWAC. I tried and made a few feeble efforts. That was about the point, I think, when we'd set up Canning, Sisson and Associates, and I had relatively little time to make this study. I finally said, "Well, the heck with it!"

SWAC was used on one other part of our study. It was a fairly limited machine, so there wasn't a lot we could do. I remember we had help from Rosalyn Lipkis, who had been one of the best programmers that they'd had at INA. Are you familiar with INA.

RM: Yes.

RC: Let's see, she got married and had a baby, so she couldn't work full-time, but we got her on a consulting basis to do some programming and running some job that we needed done in short order. That, I remember, was done on SWAC.

While I was there, the available machines did not really have the capacity for tackling fairly large scheduling problems, which leads me to a related subject.

CANNING

I can't remember how it came about--whether it was the Dean or Assistant Dean or somebody in the School of Business where we were physically located--but somebody said that IBM had approached UCLA about the idea of providing a computer for academic uses and research, not for university administration functions. My immediate reaction was, "This is tremendous. This is just great." Oh, I remember. When I heard about it, IBM had approached several universities: Stanford, UCLA, and maybe USC--I don't know who all--and the reaction at UCLA was, "Oh, we don't have any place to put it." I thought to myself, "Dear God, this is awful. We can't just give up on something important like this; we'd better do something."

I started searching around to see what spaces we could provide, and I got some of the details on what the floor loading would be, what the floor would have to bear, and talked to some of the people in the Dean's office at the Business School. I found that there were two rooms down in the basement of the Business School building that could be used for the center. I called the people at IBM and asked them to come on out and talk this over. I've forgotten who it was that came out, but he was fairly well up in the sales staff for IBM Western Region. I showed him this spot and, I think one other, and it was clear that this just wasn't

what he was after, that it was completely unsuitable for the computer to be hidden down in a basement. I said, "Well, the only other thing I've been able to think of is that maybe we could build a building out here in the parking lot of the Business School." "Ah, now that is a possibility!" He smiled and said, "Follow it up." Fine. I now had the information I needed, that this was the way he would like to see us go.

The next person I saw was Dick Hill. I can't remember exactly the sequence. Have you gotten Dick Hill's name? Do you know Dick Hill?

RM: No.

RC: Dick Hill is a very important person in West Coast data processing. At that time he was assistant to the chancellor at UCLA, and I went to see him. He felt very much the same way as I did about the IBM offer. "Let's do something. Let's move." Whether it was my idea for the separate building, or his idea, or jointly our idea, I don't know. But the idea came up: let's have a separate building, we don't have much of a budget, but let's see what we can do to put a building in the parking lot behind the School of Business for this computer center. That's about the time that Rog and I set up the consulting business and therefore I had to pull out. I think Dick Hill carried the ball and out of that grew the Western Data Processing Center. Dick was its

first director. He then went on to be one of the founders of Informatics Inc.

RM: Oh, I didn't know that.

RC: He's played a key role in the computer field on the West Coast.

RM: Yes. Where is he now, do you know?

RC: He's at Honeywell, Waltham, Massachusetts. Honeywell Information Systems. If you have any trouble getting through to him, I can dig out his address.

I was not close to that project from that point on. My understanding is that IBM helped pay for the building so that it turned out to be a very attractive building. I don't know if you've seen it there.

RM: I don't think so.

RC: It was a first-class building and it met all their requirements for a showplace. It was a tremendous asset to UCLA.

RM: What was the machine?

RC: 709 was the first machine installed there. That lasted for a couple of years, then they put in a 7090 and it went on from there.

RM: In other words, then, the work that started with Salveson's thesis was the thing that indirectly triggered the Western Data Processing Center.

RC: Yes, yes.

CANNING

RC: Then the 709 was used for this study on operations research? Is that correct?

RC: By the time Western Data Processing Center had been built and the machine installed, I was putting no time in on the project, so I can't answer that directly. What I can answer is that one of the other members of our project was Dr. Allen Rowe, who is now At USC, I'm pretty sure. Most of the people on the project of the early 1950s have now gone to other places. Al Rowe went back to General Electric for awhile, and while there designed and programmed one of the first job-shop scheduling programs and ran it for GE. That was a direct outgrowth of this project. The term is now called simulation. In those days, we didn't call it simulation. I would say that it was probably one of the first simulation models for production scheduling.

RM: I hadn't realized that simulation covered job scheduling. But oviously it makes sense, doesn't it?

RC: Let's say that one approach to job shop scheduling is to simulate the flow of work through the shop. You run a number of alternative schedules and find which one comes closest to meeting your criteria, but you recognize that you can make no claim about finding an optimum schedule. The mathematicians are searching for ways to come up with the optimum schedule. This simulation approach was hopefully

just a better way of scheduling, but certainly made no claims to optimization. So a partial solution is about all it was.

RM: This was sometime in the mid-fifties?

RC: Yes. Alan Rowe did this. The work is still going on. I recently got a letter from the project at UCLA; there still is a project and one of the original members is still a professor there, and he's still working on this job shop scheduling problem.

RM: Who's that?

RC: Rosser T. Nelson. He's at the School of Business at UCLA, and he's still working on it.

RM: Salveson is still there too, isn't he?

RC: No. No, Mel is long gone.

RM: Oh, really?

RC: Mel left before I did. You might be saying that he's back there now and it conceivably might be true, but it surprises me. Mel felt that quantitiative methods represented the wave of the future, and there's nothing more hidebound than a university faculty. Nothing! I still come across it. Maybe programmers are more hidebound, but between programmers and university faculties nobody is less willing to change than these two groups. Therefore Mel was not welcomed with open arms by other members of the faculty. It was a problem area, and he recognized this, and therefore when it became

clear what the politics of the situation were, Mel chose to leave. He went back to the Univac installation at Louisville, Kentucky, that first GE installation. He was one of the early people there. Whether he did production scheduling there or not, I don't know. He could have.

RM: The chronology of it is that his thesis was one of the major beginnings in this field.

RC: Yes, it was.

RM: GE now has got computers and is working in this field. Which other computer companies are involved in . . .

RC: Production scheduling?

RM: Yes.

RC: I think that this has expanded all over. As I say, simulation came out of this. Harry Markowitz at RAND corporation also went to work for the GE production control consulting group headquartered in New York, and did some of the early work on simulation. Then he returned to the West Coast and developed Simscript. I think that's the sequence of events. This whole simulation question has permeated the field, and I would say this production scheduling work was some of the earliest simulation work. There is something else that Salveson's thesis really triggered off. Since it created the project, and since Mel worked on the project, he saw a need for a professional society. He felt that the

Operations Research Society of America was not addressing the problems he was interested in, or not appealing to the audience that he felt should be appealed to. I would say he, of all people, is probably responsible for The Institute of Management Sciences, TIMS. I remember the early discussions on the project about the need for such a society. He contacted a lot of other people, and other people may get the credit, but I think really Mel was the instigator of it.

RM: It was set up about when, Dick, do you know?

RC: 1953 would be my guess.

RM: I'm not familiar with that organization.

RC: Okay, TIMS is concerned with the use of quantitative methods for solving management problems, and is similar in many ways to the Operations Research Society of America. TIMS and ORSA put on joint meetings very frequently.

RM: Do you know when they might have held their first meeting with proceedings? Would it have been around that 1953-1954 period?

RC: I would think so. There's the address.*

RM: Volume 19, nineteen years, which would put it at 1954.

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*Prof. Martin K Start, The Institute of Management Science, Graduate School of Business, Columbia University, 401 Uris Hall, New York 10027.

CANNING

RC: Yes, right.

RM: Now while all of this is happening you and Roger Sisson got together and formed Canning, Sisson and Associates?

RC: Roger had stayed with E.E. Company when I switched over to the Civil Service, and he stayed with E.E.Company, I guess, until 1952. (I can give you Roger's address in Philadelphia if there's any need to follow this up.) He then left E.E.. Company to join Computer Research Corporation, which, in 1954 I guess, was acquired by NCR. He and I had stayed in pretty close contact throughout this period. I had been teaching some evening classes at UCLA on the use of computers for business data processing. I started that in 1953. Then in the summer of 1954, the University of Chicago asked me to put on a one-week seminar at the University, which I did. A forty-hour seminar on, I suspect I called it, Electronic Data Processing in Business and Industry. I had given it a number of times on campus and also a number of companies around the Los Angeles area had contracted with UCLA for me to give it on their premises in the evening.

By the summer of 1954 I must have given it seven or eight times on campus and at companies such as AiResearch, Lockheed, and North American, mostly aerospace companies. Out of this had grown some consulting work. In the course of the seminar they'd start to get interested and they'd

ask me to come in and work with some of their people who were studying how they might use computers primarily in production control, inventory control, and so on. By the fall of 1954 I had some consulting work going at a very low rate per day--I didn't know any better--and Roger and I got talking and came up with an idea. He felt he wanted to get into consulting, so we formed a consulting partnership in October 1954. We called it Canning, Sisson and Associates.

Earlier in 1954 I had made two proposals to John Wiley & Son to write books, and they accepted them both and offered me contracts. I signed two contracts before I knew how hard it was to write a book. At the time we set up this consulting partnership, I was deeply involved in writing my first book, which was based on that seminar that I had given a number of times. It's Electronic Data Processing for Business and Industry, by R. G. Canning, and it was published by John Wiley in 1956. It was fairly widely sold at that time. It was aimed at the systems analyst and management level. Then, when it went fairly well, Wiley pushed me for the second one, and that's Installing Electronic Data Processing Systems, published by John Wiley in 1957. This was not based on the seminar; it is additional material that I had picked up. The two books were companion books: the first one led

up through the design of the new system using a computer, and the second book picked up at the point where design had been completed. It dealt with what a company goes through to actually program and get an application system running.

After Roger Sisson and I set up the partnership, I continued with the seminars, which turned out to be a very useful way of getting in contact with propective clients. We desired to do consulting in California, and we had a set of rules that we followed. One of them was that we would not work for competitive companies at the same point in time.

RM: In other words, you wouldn't work for Northrop and North American.

RC: That's right. At the same point in time. There'd have to be a gap of some length of time before we would work for the second company. It also turned out, although we didn't know it at that time, that our business came from companies that had gross sales of somewhere from fifty million to a hundred and fifty million dollars per year. Much below fifty million they just weren't accustomed to hiring consultants at any price. It may still be true now, I don't know. Above a hundred and fifty million, they felt they had their own staff that knew as much as the consultants did. And in truth, they often did, so why did they need consultants?

CANNING

Also, our work always dealt with the use of computers. We didn't do general consulting, or systems and procedures or forms control or things like that. It was always related to the use of computers in business, and to a great extent on production control related jobs. When you put these three criteria together, it limited the market on the West Coast for sure! There was a very small number of companies that we could be working for. The obvious answer was that we had to look at other places in the country for this type work. I think we were one of the early consulting firms specializing in the use of computers, I don't think there were very many ahead of us. John Diebold set up his consulting firm in New York at about the same time. Certainly some of the accounting firms were getting in. Arthur Andersen, the CPA firm, had gotten in deeply on the Louisville, Kentucky, GE Appliance Park installation. There were other firms that were getting into consulting, but I don't think there was anybody else on the West Coast that was ahead of us on setting up a computer consulting firm.

"Firm" may be the wrong term, because I was the one who really didn't want to grow. Roger would have liked to see us expand, but I preferred not to. I thoroughly enjoyed consulting, and I knew if we grew to a staff of five or ten or more consultants that my job would be primarily administrative, supervision, and sales. If there isn't any business

in the house then it's the bosses who are responsible for getting it in. This has always turned me off, I just don't want any part of it. As I say, "firm" is probably the wrong name, because it was never my hope to see it build up to a larger consulting firm.

RM: Did you keep it as a two-man concern?

RC: We had one other consultant, Bruno Chippanelli, join us later on when we needed more help, and perhaps this was a key event in the future of the firm. He was on an assignment in Phoenix and came down with meningitis and died while he was there. This shook us up pretty badly. Bruno was just a tremendous person. If I had needed any convincing previously that I didn't really like the idea of building up a firm, this was a convincer for me. Fortunately, the doctor who attended him was a specialist in this disease, and had just attended some national seminar on meningitis. He knew all the latest information, but it still happened. This shook us up badly.

It was not too long after that that I became very discouraged with travelling. I'm now jumping clear to the end and we'll go back to the beginning. It was in the fall of 1958 when we decided that this didn't seem to be working out the way we wanted. Roger had other desires and I wanted to stay as small as possible. He went to Ford's Aeroneutronic Division in Newport Beach, and I stayed on as an individual

consultant. That's the birth and death, so to speak, of our consulting business.

RM: All right. Now let's go back and get into some detail.

RC: Okay.

RM: One thing which occurred to me was: were you recommending specific machines to your customers for the work that they wanted you to consult on?

RC: I guess you'd say it evolved. Generally we would help in the preparation of a request for bid that the client would send out to get bids from different computer companies. Then we would help in the analysis of those bids and end up with a recommendation to the client on which one we thought best fitted the needs. In the early days, I think we took more on ourselves; we made the evaluation and the recommendation. We soon learned that it would be better to actually get bids coming in, set up a specific work load, and get the manufacturers to bid what equipment would be required. Then we'd go through and check their figures pretty carefully.

RM: Basically you were helping write the specs. I suppose in a way it's systems analysis.

RC: We helped with the system analysis and system design. Let's say this is the design the first time through, to the point where they (the customer) could define what they wanted the new system to do, identify each

of the computer runs, the files, the contents of the files, and what the reports would look like. Then this would be in the specs that the computer manufacturers would bid on. When the customer finally got into the implementation phase, some parts of this design would change. In some cases, quite a bit of the design would change depending upon the specific computer; the characteristics of the computer could impact the design of the system.

The first big job that we got into where there were many manufacturers involved was Associated Merchandising Corporation's study. They had a fairly impressive program. They'd set up a pilot installation at Higbee's Department Store in Cleveland, Ohio, and had been running it. Stores from all over the country are part of AMC: Bullock's in Los Angeles; Dayton's up in Minneapolis; Lazarus in Columbus, Ohio; Shillito's in Cincinnati; major stores in major cities. They had selected the RCA Bizmac as their computer and had set it up in Cleveland. They had programmed many of the applications on this; accounts receivable, accounts payable, sales audit, merchandise control, etc. They had the first really good point-of-sale recorder. It was an on-line point of sale recorder, and it had its own special computer that controlled it. The point-of-sale recorders on the market today aren't a heck of a lot different or better than that

one they had back there in the 1950s at Higbee's.

RM: That was an RCA machine?

RC: That was an RCA machine.

This was all set up as a pilot study. The retailers had high hopes that by having computers do all their back-office work and also a better job of merchandising control, that this would be a big cost saving solution for them. They set up their system specifications, and they could give very good specifications because they had these applications running on the Bizmac and they knew what to be done. They set up the specs and sent them out for bid to all the major computer manufacturers, and we were called in to evaluate the bids. We found what all other consultants who dug into these questions found at that point in time: That manufacturers were trimming things just as thin as they possibly could. They were squeezing down, and if you really got into the analysis you'd find that the programs were going to take longer to run, were going to take more memory, more peripheral units, more tape units and all that, than were generally bid so that the costs would be higher. Generally, the manufacturers did not include all the loss factors that had to be included, for setup, rerun, idle time, preventive maintenance, unscheduled maintenance and all this other stuff that has to be added on. What originally looked like a one-

shift job was now going to take two shifts, plus a staff of operators for two shifts instead of one, and all the rest of it.

The upshot was that unfortunately computers didn't look like they were going to save the department stores any money at all. The economics looked pretty bleak.

Some of the stores were thoroughly unhappy with the results after spending all the money on the pilot project, Some of them were unhappy, but recognized that there was still potential there, they just had to figure out how to do it. Some of them, such as Lazarus in Columbus, Ohio, and Dayton's in Minneapolis, to name two of them, just kept moving ahead and did put in computers in the early sixties. They still had problems of getting economic payback, but I think they eventually reached it. It was not an easy job in those days to make computers pay off the way estimates originally said they would.

I think that was our first big job of evaluating many bids. A slight digression: one of the jobs in the retail field that I worked on turned out to be the first use of SCERT. Have you heard of SCERT?

RM: No, I haven't.

CANNING

RC: Comress Incorporated is the name of the company, just outside of Washington, D.C. They developed a package called SCERT, Systems and Computer Evaluation Review Technique.

RM: Which means what?

RC: It really is a form of simulation for doing what we had done manually, namely estimating what the computer times would be for running a system of programs. You would define the work load to SCERT, define each of the runs, the characteristics of the files, the inputs, the outputs, and all. SCERT had a library of all the characteristics of the computers and software used. It would prepare time estimates for each computer that was specified, for each configuration specified, for the different jobs. I could probably dig up more information on SCERT; this was in the early sixties. I was asked to come in and evaluate SCERT for my client, and this was the first commercial contract that Comress had. Instead of going out to the manufacturers, having them submit bids, and then having somebody review the bids, the theory was: let us set up our system, define it to SCERT and have it tell us what the costs are for each configuration. We'll specify the configurations for each manufacturer, run this series of jobs, and then see how much computer time the workload would require.

CANNING

If a configuration takes forty-eight hours per day to do the workload, it's obviously not the right choice, so let's find a configuration that will do the job in one shift or two shifts. I remember working on this and evaluating SCERT and being very impressed. It used a degree of detail well beyond any manual estimating that I had encountered.

Comress was successful on that job and the client was well satisfied. Comress went on to build up a pretty substantial business based on SCERT. There have been some other simulation packages, competitive ones, that have come out since then, but that was the first big simulation package that I know of.

The founders of Comress had come out of RCA. RCA had come up with a mechanized timing system and these fellows tried to get them to improve on it. When RCA wouldn't, they left RCA, set up their own business, came out with this enhanced package, and made a commercial business out of it. Really, as far as I was concerned, it made life for the client and the consultant an awful lot easier.

You've got a very difficult adversary type environment when you get bids from manufacturers and you start picking holes in these bids. There's one major manufacturer,

whose name we will not include here, who doesn't like to lose. If somehow a consultant recommends some other manufacturer, he just literally gets his frame climbed by this major manufacturer and life is pretty unhappy for awhile. After you go through this a few times, you sure enjoy a solution where there is a much more mechanized method of evaluation. This analysis may show two different competitors as being very competitive; if so, the client can negotiate with those two companies. Great. That's fine. But this business of sending out requests for bids and getting bids back and analyzing them has its shortcomings.

Let's say that people are still debating this question of how best to do the selection process. I know there's still people now who think the only solution is to actually bench mark, actually program and run jobs on the competitive computers. I think the government does a certain amount of this, but most commercial enterprises don't want to go through the time and the cost of bench marking. Something like SCERT turned out to be a more feasible way of doing it. That's a little background of life in the consulting business at that point in time.

In March 1955, Roger and I, as a part of Canning,

Sisson and Associates, started the Data Processing Digest. It was one of the first periodicals in the computer field; not the first. Computers and Automation, I'm sure, was ahead of it. On our consulting jobs, we would find that we were just not keeping up in our field, that we weren't able to read all the literature. We knew we were missing something and we figured other people felt the same way. So we started Data Processing Digest to survey the periodical literature and books. In DPD, we picked twenty-five or thirty of the best articles and books per month to digest, review, abstract or whatever was appropriate. It's still going, although we sold our interest in 1961.

RM: Did you abstract some of the proceedings materials?

RC: I would think so. Anything of value that wasn't too technical. I think we recognized that data processing managers were our prime audience, so we reviewed, abstracted and digested things that were more of interest to them.

RM: As consultants you were seeing what your customers needed. Were these customers or were you able to influence the manufacturers on things that they should be designing into their computers? The second question is: did the manufacturers listen to their customers?

RC: I have a hard time pointing to a specific case. Let's say we tried. I know we tried. This would be in all aspects of

the hardware and some of the software. To go back to this point-of-sale recorder question, transaction recorders, yes, I remember trying, writing, arguing and so forth about what was required. It just didn't seem to happen. We didn't seem to get what was being asked for. Whether the users I contacted had any real influence on the manufacturers, I don't know; I can't think of any case that stands out in my mind that they did.

Another one of our ground rules was that we didn't do much consulting for the manufacturers; we were primarily on the user's side. We did some for the manufacturers, a limited amount, but we ourselves did not want to get into product specifications, we did not want to do that kind of consulting. Because then if we ever recommended it to a client later on, right there we'd have lost our credibility because we were recommending something that we had conceived.

RM: Conflict of interest type of thing?

RC: Yes. So we were very limited in the amount of consulting that we did directly for manufacturers.

RM: You didn't want your own Watergate. (laughter)

RC: It made life a lot simpler, yes.

RM: Did you get into programming much?

RC: No. Just about that time, a friend of ours, Mark Shiowitz, got into dome contract programming. The company he worked

for was going to make him a European representative. He got on the plane and flew as far as Chicago, if I remember right, and said to himself, "What am I doing? I don't want to do this!" So he got off the plane, got another one coming back to L.A., resigned, and went out on his own to do contract programming. Almost before you knew it, he had built up a staff of programmers. He'd just hit it right, and it grew very rapidly.* We were pretty close to Mark. We knew what he was doing, and when we saw this happening Roger and I asked a question: "Do we want to get into this business? Do we want to get into the software business?" Again, I didn't want to. I can't remember what Roger's position on it was. This meant getting a group of programmers in there who we'd have to supervise and all of a sudden I would be tied to the office. I still resist it to this day. (laughter)

RM: We talked about this first really large job, which was for the Associated Merchandising Corporation.

RC: That was the first large job involving lots of bids. I think we were on other projects, other large projects, before then. But that was the first one that involved a lot of bids.

RM: Is this Shiowitz Associates?

RC: I think so--later named Mesa, for Mark E. Shiowitz Associates, which still later was acquired by Planning Research Corp.

RM: Okay. I was going to say, what others would you feel were really major projects that you were involved in or that were sort of breaking through into new areas? I think I'm going to turn the tape before we get into that.

RC: Okay.

(Tape 2, Side 1)

RM: All right, we were talking about some of your other customers, and without being too specific, you were going to give me some data.

RC: All right. There was one major corporation we consulted for that was looking for advanced uses of computers for production control purposes. We worked as consultants to the project. This was a pretty substantial project. One of the outputs of this project was, I won't say the invention of decision tables, but it certainly was one of the first applications of decisions tables. I don't think there were really any earlier users than this particular project. Another output of it was a reasonably formal methodology for conducting systems studies that has been documented in the public literature. These were consulting projects where something significant was done, I guess. Let's postpone that for a second and let me go on, and I'll come back to that. It's hard to discuss it without revealing the client and I'm having a hard time.

RM: Okay, We'll leave that untouched.

In the production control work, when were the computers far enough along that it was really feasible and useful to use them; the point at which they made the whole process workable? For instance, could one do anything really productive with a 702, or was it much later on in the computer chronology?

RC: I think the Al Rowe model that I mentioned was done on the 702.

RM: Oh, really?

RC: I think so. The 705, maybe. Anyway, it was very early in the process. By the middle fifties, it was certainly theoretically possible to do much more than was done, it's just that management wasn't willing, and we were all stubbing our toes a little more than we wish we had. The machines were able to do some portions of the production control process. The scheduling of a large job shop, I think, would have been fairly difficult by way of a simulation model. I don't know much about how Al Rowe's model actually ran, perhaps it was more of an R and D effort by General Electric than it was production effort. I don't know whether it was put into productive use, I think he proved that it could be done. When you get a substantial machine shop or job shop where you've got thousands of jobs going through hundreds of work

centers, to come up with very many alternative schedules is just a tremendous computing load. I don't think that portion of it has been solved yet, the way people would like it to be, but inventory control, order release, order tracking, things like this, were certainly done in the late fifties and early sixties. These were economically feasible and were being done. But even now they're still fighting with job shop production scheduling.

In October 1958, we stopped the consulting partnership per se. I continued on as an individual consultant. Roger went to the Aeroneutronic Division of Ford Motor Company. We incorporated into Canning, Sisson and Associates, Inc., to carry on publishing Data Processing Digest. As long as Roger stayed here on the West Coast we continued that operation. In 1961, he decided to move back to the Auerbach Corporation in Philadelphia, and it no longer became feasible for us to try to run Data Processing Digest operation with one on one side of the country and one on the other. We had to make up our minds what to do. After much consideration and talking to people, we ended up selling Data Processing Digest to Margaret Milligan, who was the editor. She had worked with NCR when Roger was there, and joined us a few months after we started our consulting firm

in order to be the editor of the Digest. She still is the owner.

RM: Although you had this thing about not getting into administrative and management type work, to some degree didn't you have to do this with the Digest?

RC: Well, it was running pretty smoothly, and Margaret and the office staff were able to run it. I think they liked life a lot better with Roger and me not in the office; they'd just call us when some problem came up. In general they ran it themselves, and they did it very well. They liked this arrangement. I don't blame them.

RM: No, I don't either. It was a technical staff who were tech writers?

RC: Yes, they had done it for years and they knew how to do it. If questions came up, they could contact us; otherwise we let them alone. When we sold out to Margaret, that's the way it continued. In one sense, that's the way I still operate here. I don't want to get too much involved in administration, so I'm still the editorial side, gathering the data and writing the issue. Mrs. Canning is really in charge of the subscription end of it. If I'm out of town somewhere gathering information, everything goes on.

RM: In the early days of consulting, were the requests for you to consult coming from middle-level management, and did they, in turn, have to sell to their high-level management? In other

words, was there a generation gap?

RC: Yes, there was. I would say that most of our clients were the equivalent of the manager of data processing. Either they already had sold the idea of bringing in a consultant to higher levels of management before they contacted us, or they had to do it later. If they had settled on us, or whatever consultant, they would have to approach top management and say, "We think we need a consultant for the following reasons." Some of it was to help their staff on some aspect in which they weren't yet well enough trained; for instance, the staff might not have performed a complete systems study. Another thing was the objective review of bids from competitive manufacturers. The retail field stands out in my mind here. It is very shrewd about buying; these people always have been. They look to where the overall deal is best and least expensive for them. Other companies I have consulted for were willing to pay more money per month for a brand of equipment with which they were familiar, or something of that nature.

RM: Another aspect of this I'm questioning is in many cases when you were dealing with old-line companies, I suspect the top management was fairly rigid. I wonder how they felt about electronic computers coming into their organizations. Was this a lot of hassle, and were you involved in that aspect of selling?

CANNING

RC: Yes. In some of the aerospace firms that I consulted for, I remember proposing a form of work-in-process inventory control that I felt that the computer could do and was needed. It could supply information on how many parts actually were available, how many should be available on the factory floor, and how many of them were coming through the production pipeline. I remember very clearly some of the old-line production managers telling me, "You're wasting your time. It's impossible to maintain work-in-process inventory control here. There's too much 'moonlight requisitioning', stealing of parts." Not to take home; that was not it; it was stealing of parts from one project to use on another. When a supervisor has a production deadline facing him for which he needs some parts, and he happens to know the same part is used on another project, somebody goes over at night on second or third shift and scouts around until they find where the parts are stored. They would pick up what they needed. This is the way things got produced. I guess I felt, and still feel, that this isn't any way to run a railroad, but these guys were in the position where they had to get things produced. You could talk theory all you wanted to, they weren't about to buy it. This was fairly typical of those days. They would let computers come into areas that didn't vitally affect their production, but if you were proposing to put it in on something that was really

the guts of their operation, then there was resistance. I guess I don't blame them. This wasn't true just in aerospace. I remember I consulted for a drug manufacturing company, and the vice-president of manufacturing felt exactly the same way. "Just keep those computer types out of our operation. Let us produce. You can use the computers in sales or wherever else you want, but stay out of our hair." This was fairly typical.

RM: I guess what we should just do is continue on to when you were setting up this company.

RC: Right here?

RM: Yes.

RC: Okay. After Roger joined Ford there was nothing that really tied me to Los Angeles. We wanted to get out and we looked around. In late 1958 we selected Vista, and in early 1959 moved to Vista, where I set up my consulting office and operated here as an individual consultant. 1962 was a frantic year; I don't think I've ever been as busy in my life. I got concerned about the amount of travel that involved, and the sleeping problems I had developed. Some of the other consultants had the same problem. You're out of your office constantly, you're only working when you're in the client's place of business it seems like, and not working in your own office. Also it was fairly common practice all the way

through this period to be in the middle of an engagement and a client would suddenly turn to me and say, "Hey, what's going on in such-and-such an area? Optical scanning. What's going on in optical scanning? Should we be looking into it?" I'd think to myself, "Holy gosh, what do I know about this? I haven't studied this." Maybe I had read an article in Datamation, but it didn't seem to apply to this business. I just didn't know what to answer the client. So in September of 1962 I got the idea for a monthly publication that would take subjects that I thought most managers of data processing would be interested in, research them, and then write them up. This is the way it has worked out. Managers of data processing were interested, and consultants were interested. We're all in the same boat. We all keep wondering what the dickens is going on in this crazy field. So that's the way this got started.

RM: When did your first issue come out?

RC: February, 1963.

RM: The EDP ANALYZER.

RC: Do you need a copy of it for any reason?

RM: Do you have your first copy?

RC: Um-hmm.

RM: That would be good.

RC: Don't let me forget. I'll dig it up.

RM: I won't. I don't suppose you have the first copy of DP Digest, do you?

RC: Oh, boy! Yes, I have it.

RM: I could Xerox it. Is it a big thing?

RC: I've got it but away in a storeroom somewhere. Margaret Milligan might be able to provide it.

RM: (pause) In recapping I'd asked you the rather general ~~puestion:~~ What do you feel were some of the most significant developments that happened in the area of computing business management that you've been involved with?

RC: I guess you are asking what I'm proud to have been associated with. I think we have covered most of these. The ones I would single out are the following. First, the early work on computer reliability, primarily for the IBM 604 and Card Programmed Calculator. The early operational success paved the way for the acceptance of later machines. Next, I guess, would be my early work on simulation for job shop production control, in the 1952 to 54 period. I hope I had some influence on some of the models developed in the next few years. I would imagine that today it is the most widely used technique for corporate planning models. Thirdly would be contributions to the literature on commercial uses of computers. That includes the proceedings of the first west coast symposium, in 1952, my UCLA research project

papers from 1952 to 54, starting Data Processing Digest in 1955, the two books published by John Wiley and Sons in 1956 and 57, a number of other papers, and then EDP Analyzer in 1963. Next I think would be my participation as a consultant on a large project that ultimately resulted in the first major application of decision tables as well as the Study Organization Plan method of conducting application system studies. Fifth, I have been an advocate of business oriented programming languages, and participated in the May 1959 meeting at DOD, out of which grew COBOL.

RM: How about some of the business societies? Were you involved at all with GUIDE?

RC: No, GUIDE and SHARE still are fairly restrictive, although they claim not to be. I tried to subscribe to both the GUIDE and the SHARE secretary distributions, to get the proceedings, about a year ago and couldn't do it. You have to be, I guess, an IBM user. At least I got turned down. You were going to ask about other associations?

RM: Other associations that were related to the business data processing side, as opposed to scientific.

RC: Well, I'd single out ACM, DPMA, ASM, the IEEE Computer Society, SMIS and ACPA. ACM is primarily a scientific, research, and system software programmers society. However, its first special interest group, formed in the early 1960s

by John Postley, was for business data processing. (I was chairman of SIGBDP from 1971 to 73.) DPMA, as their name implies, aims at data processing managers. I have been active in DPMA and was on their Certification Council in 1966 and 67. ASM was formerly the Systems and Procedures Association; it is an association of business-oriented system analysts, many of them dealing with computer based systems. The IEEE Computer Society is the successor of the former IRE Professional Group on Electronic Computers. They are mostly computer engineers, but they hold some workshops on subjects of interest to business data processing. The Society for Management Information Systems is aimed mostly, I think, at directors of management services in larger companies. And the Association of Computer Programmers and Analysts is aimed at business data processing analysts and programmers.

The American Management Association has had a continuing seminar on management information systems since the mid 1950's. H. Warren White, who lives on Balboa Island, has been associated with that for much of the time.

Then there are the users groups, such as GUIDE and SHARE. They are supplier oriented. I would think they constitute the main source of technical information for the data processing staffs of user companies. But I have never been

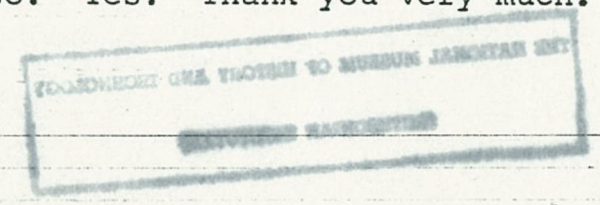
active in any user groups.

Then there are the EDP committees of the industrial and trade assiciations. These may be the second largest source of technical information for DP staffs. They include the bankers, savings and loan associations, retail grocers, retail merchants, and so on.

Finally, I would guess would be the informal groups that get together about once a month to talk over problems and exchange ideas. These are mostly managers and executives of data processing. I think they occur in most of the larger cities.

That do it?

RM: I think so. Yes. Thank you very much.



END OF INTERVIEW

Index - Richard G. Canning
10 August 1973