INTERVIEWEE: Stanley Rogers

INTERVIEWER: Robina Mapstone

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RM: The date is August 9, 1973. This is Bobbi Mapstone and I'm talking to Mr. Stanley Rogers at Simulation Councils [Inc.] in La Jolla, California. This is an interview for the Smithsonian Computer History Project.

Let's start with you going back to the university that you graduated from, your major, and go through the story about how you got to join Convair.

SR: I went to Occidental College majoring in physics with a math minor and a minor in modern languages. I graduated in 1932. After that I was offered, what was in the time of depression, a fabulous starting salary [$44/week] for a rather peculiar job which involved applications of number theory and a little other mathematics, but didn't make too much sense to me otherwise. It did allow me though to continue going to graduate school. I went to USC, and took electrical engineering, sociology, and economics and, in fact, completed almost all the requirements except the thesis for a Masters degree in sociology with an econ. minor.

Then I had an opportunity to enter the electronics business, which I was really interested in and became a development engineer for a company [General Communication Products Co., Hollywood, Ca.] manufacturing high fidelity equipment. After a year or so that led to a job teaching low math, not high, and radio-television theory at a school called American Television Laboratories.

RM: What was that?

SR: The American Television Laboratories.

RM: Yes.

SR: It was an organization which Dr. De Forest [had founded] in Chicago, and [which] had opened up a branch in Hollywood, where I was living. They were running war manpower training programs. I was engaged as an instructor in that program, both for lower math, starting with arithmetic, and for [radio and TV] theory and lab courses. One of the other instructors was working for Dr. De Forest as his one and only development engineer, and when he was offered a job on the design staff at Convair in San Diego, Dr. De Forest, on my fellow faculty member's kind recommendation, asked me if I would become his assistant on the radar altimeter [he was developing for use by the

Air Force and commercial aircraft.] So I did that, in addition to my half-time job teaching.

After I had worked for Dr. De Forest for a year, Dr. [C.J. Breitwieser] who had arranged the job for me with Dr. De Forest, asked if I would like to come to work for Convair, [which was later to become a division of General Dynamics.] If I did, he was in a position to send me off to MIT for four months of graduate work. I took that offer with alacrity and moved to San Diego. A couple of weeks after I joined Convair I did go off to MIT. I stayed there about [3 1/2] months, earning approximately half of a Masters degree in electrical engineering. The courses I was attending were those of the Radar School, but since I was a civilian, I got both the Army and Navy portions of that program. That is why it took [over 3 instead of just 3 months.] On returning to Convair, I worked in their Radio and Electrical Laboratories and became the built-in expert on radar, airborne [navigation systems], and other systems that I had studied.

After I had been back a while, I was given an assignment to develop a reasonable set of standards for measuring the overload capacity of [electrical] generators on bombers being manufactured or in design [at] Convair. The first of these was the B-24, [followed by] the B-32, and finally the B-36. The problem with heating [of] the aircraft generators was that some loads came on only for a short length of time and perhaps only existed during certain operations such as landing or taking off or some [other maneuver. The problem was to prevent damaging overloads.]

RM: By overload are you referring to electrical overload?

SR: Electrical overload. The generators could carry overloads for limited amounts of time but, of course, [the generators could overheat.] The thing that limited the amount of overload they could carry was how hot they got. The failure of a generator could be very serious in an aircraft by depriving it of essential power so that many of the [electrically controlled and operated] systems would have to be taken out of operation.

The rating system in use at that time seemed rather illogical to me, so I tried to develop a new scheme. I took the worst possible combination of generator loads that the people in other parts of the Engineering Department who knew about these things could come up with, and calculated the amount of heat this would release and the resultant rise in temperature. This involved a great deal of addition, multiplying, summing, and so on, and seemed like a very tedious thing to be doing with a desk calculator. My boss, Dr. Breitwieser said, "Why don't you try using the Accounting Department's machines? It might make it easier for you." [Laughter] I thought that was a pretty good idea; so I went over to the Accounting Department and asked if I could do that. They said, "Well, if you don't get in our way you can." [Laughter] I tried using their machines and I found that indeed they could add rather well, subtract well, take subtotals, totals and grand totals, but when it came to multiplying they were a headache. Their reliability was very poor. Nevertheless, in spite of all their faults, it seemed like an easier way of doing [the calculations] than [using the desk calculator.]

I spent about a year working on the electrical load problem on these three aircraft using the Accounting Department's machines. I complained bitterly all the time tc the IBM representative about how unsuited their machines were for doing engineering calculations, and why didn't they do something to make it easier. I also became sufficiently sold on using these machines so that I kept telling the [Stress] Group, which had a great deal of routine calculating to do, to use the machines instead of deak calculators. Finally they said they would try it out on one particular kind of problem. They did, and found that, despite all the handicaps with the unreliable multipliers, indeed it was a labor saver. They too became staunch users of these machines and hounded IBM to please develop something that would be more suitable. IBM did. They came out with machines [whose names] I have forgotten now. There was one we called an ice box [because it was the size of an ice box] (it had a memory for thirty words or something like that). [Even that small memory] was really quite a help.

RM: I think the ice box was on the CPC.

RS: CPC, that is what it was. Yes.

RM: Which was already about 1949.

SR: They also came out with a better multiplier, but I don't remember the [type] number [of that machine].

RM: The first electronic one which looked like a suit-case with tubes sticking out of it was the 603.

SR: Yes. That is a familiar number.

RM: That was their first electronic one. Before that I think it was the 405. Those were the big clunking calculator machines with the printing mechanism which made that God-awful noise.

SR: I did get pretty familiar with IBM [accounting] machines and how to make them do things that the manufacturer never really intended them to do. It was a lot of fun and it was very challenging. I found myself working one or two hours overtime practically every day. I just hated to quit.

Anyway, to skip a little history, from that time on, after I completed this project, I did not use the machines much more. Incidentally, I did write a paper for the American Institute of Electrical Engineers on this particular project and it was published in the December issue of _Electrical Engineering_. If I remember correctly, that was the last issue of _Electrical Engineering_ that was ever put out; my paper undoubtedly caused its demise. [Laughter]

RM: This would have been approximately 1946?

SR: It was after the war was over; it would have been 1945 or 1946.

RM: Was it an _Engineering Magazine_ or _Engineering_?

SR: The title of the magazine was Electrical Engineering; [it was published for the AIEE.] One of the nice parts about that was it gave me a chance to have a trip at company expense to Seattle to present the paper. My first adventure in that sort of thing. The company kindly gave me a couple of weeks vacation in Canada, and also allowed me to drive back on company time. I really felt that I had hit the jackpot.

RM: I'd say.

SR: The next contact I had with the computing part of things was in 1948 when the Navy offered our company [a] LARK simulator which was a kind of analog computer. The equipment was built by Reeves Instrument Company.

RM: I have a confusion here. Did the LARK simulator have anything to do with the LARK project of Remington Rand?

SR: Yes, [but there is some confusion here. RAND Corp. of Santa Monica also got a LARK simulator -- I don't know about Rem Rand's involvement.] The LARK simulator was furnished to the contractors who were working on some aspect of design or construction of the LARK missile. Rand [Corp. (Santa Monica)] was, I'm sure, involved in this, because they were one of the companies that got [one of these simulators.] We set up our particular simulator. It [had] arrived damaged, and we were not allowed to put it into operation until the engineers from Reeves Instrument came out and checked it out. This caused roughly a month's delay in our getting to use it. I well remember [the day] they came. [They] checked it out, and found that it was all right. They had it set up on the floor, explained to me how you use it for about three hours, shook my hand, and wished me luck.

My boss had come by previously and said to me, "Well, it was your suggestion that we accept this equipment; now it is your responsibility to see that we can use it." I really had no idea what I was going to do the first time something went wrong with that collection of hardware. And it didn't take very long for it to happen. I found that by just going after the troubles one by one [and] tracking them down, I very soon learned a lot about the insides of the machine. After getting a few wrong answers from problems that were put on it, like a pendulum that swung in ever widening arcs until finally the bob of the pendulum took off for infinity, we finally figured out why they were occurring and how we could modify our circuitry so as to eliminate them.

The [problems were] wired up with telephone-type patchcords on fixed panels made out of telephone jacks, and anytime we wanted to change problems, we had to pull [out all the patchcords] and put in another set [--a] long operation. The machine stayed in balance for twenty minutes or so at a time; [consequently,] you had to stop your problem solutions and rebalance [the] machine quite often or you would get wrong answers. We became very exasperated with the machine and its shortcomings once we had learned how to use it, because we saw that [potentially] it could do a great many things for us. The Dynamics Group, in particular, was a heavy user of it. We developed our own removable patchboard adjunct to the machine; [it] was a monstrosity. With four jack screws, you could engage the panel and the subpanel and you could remove it and put another one on with [a] set of patchcords already in place with [a] pre-wired problem. This enabled us sometimes to work two or three shifts using this equipment. Everybody was dissatisfied with the many difficulties we had. Reeves Instrument people came back after some months, possibly a year or more, and installed a tremendous improvement: an automatic balancing system [for the operational amplifiers.] After that the equipment was much more usable than it had been before, but we still could see tremendous [potential] uses and no practical way of [realizing] them because of the awkwardness of using the machinery [we had].

We started talking amongst ourselves as to what we could do to get a more usable computer. At the urging of several of the technical groups that were using the equipment, I sat down to [specify (with help)] the ideal analog computer for that day. I considered [the] task [a waste of time because I thought that all the improvements we wanted would cost more money than] we could ever get. [Nevertheless,] we [did] design [a system for one] machine for use by our Pomona division, [and] another machine for the use of the San Diego division. These machines looked like nothing that had ever been seen before in the way of analog computers. We decided to buy basic computing parts from various manufacturers; we would design and build the entire control system ourselves; it would have easily removable patchboards [with] very low leakage [between jacks, independent mode control for each integrator, semiautomatic testing of all computing components, and many other innovations.

We became increasingly enthusiastic about [the project] and, in fact, prepared four separate proposals for various government agencies for this analog computing equipment. We submitted all four with the feeling that, with luck, we might get one of them. I think the total bill came to about three million dollars. To my utter amazement all four contracts were approved, and I suddenly found myself as the engineer in charge of a group designing all this equipment that we had dreamed up. It was really exciting. We enjoyed it thoroughly. The problems were endless, but it was a tremendous experience working with a team of competent designers and [developing the detail] designs for these four machines.

Three of them were to be installed side-by-side as an integrated unit in San Diego; the fourth one was to go to Pomona. The San Diego system consisted of two large C-shaped consoles with four operating stations on each. The Pomona system also had the same general design, but there was less equipment [behind the panels.] The patchboards for the two systems turned out to be different, but fundamentally the design and concepts used in both systems were the same.

It took us a couple of years, probably a little longer, to build the systems. As I recall, the one in San Diego wound up with some ten thousand miles of wire in it and there were quite a number of other impressive statistics. I did write a report describing the San Diego system, and I presented a paper on it at a conference held at the University of Pennsylvania around 1952 or 1953.

[One of the innovations in the new machine was the semiautomatic test system. It connected the inputs of individual computing elements (amplifiers, multipliers, etc.) to calibrated voltage sources and their outputs to bridge-type measuring circuits that balanced out the nominal correct output voltages and simply read the error. The test technician got a "go/no go" indication, depending on whether the element was operating within its specified tolerance. For example, amplifiers were tested for offset with zero volts input and for accuracy at + and - 100 volts output. The special patch panels used for these tests served to test the whole computer or any section of it that a user thought might be out of calibration. Amplifiers were switched one after another into the test circuits by means of telephone-type stepping switches. Repaired computing elements of all types were kept on hand to replace any element that flunked its test.

Another innovation made sure that a user could not forget to mark the scales on the 8-channel strip-chart recorders or fail to check them for drift. Whenever a simulation run was initiated, the strip-chart recorder first recorded on all channels a zero, then voltages that should produce exactly full-scale deflections, + and -, a coded indication of the sensitivity (voltage for full-scale deflection) of each channel, and the initial-condition value of the variable being recorded on each channel. Then the computer switched itself to the operate mode, and the solution of the problem began. This feature (or a variation of it) was later incorporated in many commercial analog computers.

The patchboards themselves were unique. One of the problems that plagued early patching systems was electrical leakage between the jacks, which allowed small amounts of signal energy to reach inputs that should have been completely isolated from such leakage. Such leakage was rather bad on patchboards made of the black plastic that IBM found satisfactory for its accounting machines. Our solution was to substitute a glass-filled alkyd plastic for IBM's regular plastic and we spaced the holes farther apart, largely to provide more room for labeling the jacks. Another thing we did to minimize critical lead lengths was to place gain-determining resistors and capacitors directly behind the patchboards in ovens kept at constant temperature.

As a further aid to the user, the patchboards were color coded and laid out so that the most common connections could be made with standard IBM "bottle plugs". This greatly reduced the clutter of patchcords on fully wired boards, which for large problems was still pretty bad. For maximum versatility of the computer, we brought everything out to the patchboards and provided no "default" connections for the most commonly used connections. While novices did not like the extra wiring this entailed, the expert operators (mostly from the Dynamics Group) made good use of the extra capability of the machine. The patchboards fitted standard IBM hardware and met every requirement we had in mind in designing them.

For the San Diego installation we designed a two-story computer room to shorten the cables connecting the operators' consoles to the racks of computing equipment on the floor below the consoles. The computer room itself was in a corner of the experimental factory, and a pilot's "cockpit" with instruments controlled by the computer was set up on various occasions to "fly" airplanes that had not yet been built. When the control system for a new airplane was designed, a rather literal mockup of it was built on the experimental factory floor and connected to the computer. Control-surface positions were picked up by simple devices like potentiometers and fed to the computer. The cockpit was beside a window so that the pilot could see the movement of the surfaces.

As everyone who has ever designed a large analog computer knows, much trouble can come from spurious signals caused by very small voltages in the signal or "reference" ground system. We attacked this problem by separating the various electrical systems (signal, relay, shielding, and power). Each was designed as a radial system around a grounding point. We drove a heavy copper rod into the soil under the ground floor of the computer room and connected the center point of all four systems to it. (The computer used no niggardly amount of power since it was built before the days of the transistor.) The problem of whether to ground only one end of shields (which is theoretically perfect for electrostatic shielding) was solved case by case, empirically. We did what worked best and ended up with negligible 60-cycle disturbance and overall excellent performance.]

RM: If you could find anything on that, it would be very useful. How about the proposals themselves? Are they extant?

SR: I doubt very much that the proposals still exist.

The Convair San Diego machine remained in active use for twelve years after it was built, which was quite a longevity record for a piece of computing equipment. The general philosophy back of it, one which I abandoned later, [was] by what the Rand Corporation had done with its equipment. They opted for maximum flexibility so that you could solve the greatest variety of problems, and also for having [standard IBM] removable problem boards [(with special marking of the jacks)] that [made it possible to] change problems fairly quickly. The difficulty with this philosophy is that it took a great deal of patching to set up a problem. Engineers who did not understand how they could make use of the fine points of having an entire control system brought out through a patchboard, were frustrated by having to do so much wiring in order to get their problem working. The few engineers, particularly some of the men in the Dynamics Group, who did understand the machine thoroughly, were able to do things with it that they were unable to do with much later model computers, and hence they preferred to use it even when they had computers that were manufactured eight or ten years later.

My task, once this machine was developed and in use, was simply to maintain it, to expand it as needs arose for additional capability, and in many cases to design digital auxiliary equipment for it to do things that could not be done really well by analog means. So, within the Analog Computer Laboratory, a group of engineers developed who were designing and building digital hardware. This group submitted proposals to a number of government agencies, including [the Bureau of Standards and] the Atomic Energy Commission, for special-purpose digital computing equipment, often as adjuncts to existing digital equipment that they had. We had several contracts to do this sort of thing and I found this extremely fascinating.

The reorganization of the company in the mid-1950s sometime, required me to make a choice between remaining in charge of the Analog Laboratory - which was mostly maintenance [and] assistance for operators because [we] ran on the open-shop method - or staying with design of the digital equipment and, in fact, picking up another group which was concerned with analytical work on electronic systems more than vaguely related to computing systems. I became a Senior [Research]Group Engineer with three groups reporting to me under this new organized scheme.

Our concentration now was almost entirely in the design of ultra-reliable high-speed digital computing equipment. The principal task that we had in this period was one for which we won a contract [from the Navy,] against [competition from] the entire industry. As near unknowns in the field, we felt that we really had achieved something when we beat out IBM, Ramo-Wooldridge, Remington Rand, virtually all of the large digital computer companies in business in the mid-1950s, and some smaller operations as well.

The contract was [for] six-month [study]. Two study [contracts] were awarded; one was to RCA, and one was to us [in association with Electronic Associates, Inc.]. In discussing the reasons for these two awards with project people at Point Mugu, we found that their reasoning was [that] they were sure RCA would be able to deliver the sort of gear they wanted but they felt that the design might be a little pedestrian. We [(with EAI)] had submitted the most imaginative and innovative approach to the problem. They felt that if we could do what we said we could -- and [the] six month study [was] to determine that -- it would be a far superior system. Three months later when the two [competitors] submitted their imterim, half-way-point [reports], the government cancelled the RCA [study] and continued ours. We were tremendously bucked up and the company put quite a lot of its own money into the project.

We started building a fourteen-bit word length machine embodying the design principles that we had developed for the large systems Point Mugu said they wanted. We called this, I think, the X-14 computer. We were proving out on it the concepts for our very fast parallel multiplier, the reliable circuitry which we had developed with quadruple redundancy, and a series-parallel arrangement for all of the essential computing components. The circuits had the brute force approach to reliability, but it did enable us to meet and exceed the specifications we had for hours of unattended operation with no fault. I no longer remember what the specifications were, but three of the four computers were to be installed at remote locations, where they would get no service for long periods of time. They were intended for the automatic processing of incoming radar information.

RM: So that was the problem.

SR: Yes. They were [to be] installed at remote sites where the [computers at the radar sites] were to digest the [radar] information and transmit the]residue] over a communications link to Point Mugu. The equipment was primarily intended for [range] safety. This meant that the computers had to be fast enough to do everything in real time during [a missile] launch, and the extreme reliability was demanded not only because of the inaccessibility of the computers for repairs, but because the [Navy] could not tolerate a failure during a launch.

RM: Are we talking now about missile launching?

SR: [Yes. As I said,] our associate contractor on this particular job was Electronic Associates. They had come up with some very novel computing techniques for the super-high-speed computer. It would take the incoming data from the three peripheral sites, plus additional data from Point Mugu itself, [where there would be a] fourth computer identical to the peripheral ones, process all of this data in real time, and give the range-safety officer all the information he needed as well as prepare certain digested information for later use. At every point along the way we kept in touch with the technical contact people at Point Mugu so that we were doing exactly what was wanted -- [we thought]. Finally the project organization was moved to the Stromberg Carlson Division of General Dynamics, still in San Diego.* The company

*What's the relationship here between Convair and the Stromberg Carlson Division of General Dynamics? [Both are divisions of General Dynamics.]

continued to finance better than half of what we were spending because we thought we really had a good solid inside track and that we [could provide] exactly what [the Navy said it] wanted.

However, when we went up to Point Mugu to make our final presentation, we were informed that there had been a change of command and philosophy, and that the [Navy was] going to reconsider the whole [project]. We never did get to build the system since their final decision was to cut back their specifications until they could buy off-the-shelf hardware and settle for what it could do for them.

RM: How long a time was this development work going on?

SR: About six months into the contract itself, and I think we continued working at company expense without being told the government was changing its mind. After that I simply don't recall the details of the timing.

RM: Do you remember the approximate year?

SR: It was [early sixties]. It was probably about the end of [1962] when we were told that the government would not [buy] the system on which we had been working. I remember that painfully well, because it meant in effect, that I had to find jobs for everybody in the group. I had what I considered to be an outstanding team of excellent technical people, and I wanted to keep them together if I possibly could. I found this impossible, so I had to settle for finding jobs for everybody scattered amongst the divisions of General Dynamics and a couple of outside companies. At least when the ax fell, everybody had a job to go to.

RM: Was that the end of the digial laboratory?

SR: That ended that phase of my experience. I moved to the Astronautics Division of General Dynamics and became an assistant to an assistant chief engineer. I found myself, for a period, doing administrative type work, analyzing company systems and operations, and so on. This did not get me much into the computer field. Let's see now, I'll rack my brain a little bit.

RM: While you are racking, let me just go back a little bit. With reference to this digital device that you were designing for Point Mugu you mentioned the communications link. Did you get into the design of that aspect?

SR: We did not design the communications link itself. We simply had to have the computer put out data in a format which would be acceptable to the communications link. I think, but I am not at all sure, that RCA equipment had been adopted for that link.

RM: That ties in with something else. RCA had developed a communications link for Lockheed based on their specs and I was wondering if that was related.

SR: Probably, although I wouldn't know much about that.

RM: It is about the right time frame.

What about the guts of this computer? Would it have been a core memory?

SR: Yes. It would have had a core memory. Besides the redundant computing circuitry, the most interesting thing about it was a combination of a fast-carry system, which we developed, and a matrix multiplier in which the entire multiplication process would be carried out essentially in either one or two word [times]. It was a square matrix and there were as many [rows] in the matrix as there were bits in the word. I don't recall the details of how it worked now, but I am sure that other things more or less similar have been done since. The multiplier and multiplicand were set simultaneously [in]to the matrix and the fast carry scheme enabled the multiplication to [move quickly] through the matrix.

When it came out the bottom it had a ["double-precision"] product. The matrix wasn't [mathematically] square; it was sort of a [rhomboidal] matrix. It was the secret of being able to [obtain] a great many trigonometric functions required for multiplication, [for] evaluation of power[series], and so on, at the high speed desired for the project. This particular part of the machine was developed by Paul C. Sherertz, who is still working for the Convair Division of General Dynamics. In my estimation he is an extremely brilliant designer and engineer. The logical design was done, if I recall correctly, to a large extent by Robert C. Crabb, who was also outstandingly capable.

Do you have any other questions about what I was saying?

RM: I think you have pretty much covered it.

I do want to jump back even further and ask you a couple of questions on the LARK simulator and the analog device that you built. I guess I am still a trifle confused on the LARK. This was a Navy Project?

SR: It was a Navy missile project on which Rand was a "Think Tank" contractor, probably dealing with design. Several companies in southern California had one part or another of the hardware connected with it. The Pomona Division of General Dynamics wound up with the principal manufacturing responsibility, and they carried on a lot of the design investigation. A certain amount of the theoretical work was conducted at Convair, and that was what we were given the equipment to do.

RM: Okay. So this is what you were simulating.

SR: Yes. Point Mugu was to do the testing of these missiles and therefore they had a set of the equipment. They did a very interesting thing with their equipment, which, as far as I know, was original with them. They used the equipment not only as the rest of us [did], but also when they fired a missile they telemetered it and compared the actual performance of the missile with what the computer said it ought to do. If there was a difference, they then found how they had to modify their LARK simulator hook-up so as to duplicate the missile's performance. When they were able to do that, they felt they understood why the missile had done what it had done. Since what it had done in these cases was usually what it shouldn't have done, it formed the basis [for] fixing the trouble. So they used it after [the] flight test [to analyze] what went wrong during flight test.

RM: Do you know when the LARK actually flew?

SR: No, I don't. We got these simulators in 1948; so it was probably a matter of two or three years after that before it actually flew. It probably wasn't too long because Point Mugu wouldn't have needed it for evaluating flight tests, but they still would have needed it for confirming work that was done by other contractors. They could set up simulations if they were skeptical about whether contractors were [getting] the [same] results that they got.

A person who knows a great deal about this phase of the Point Mugu operations is John McLeod, who is currently editor of our monthly technical journal Simulation. His home address is 8484 La Jolla Shores Drive in La Jolla. His phone number is 454-0966.

RM: He is the same man who set up the Simulation Council.

SR: That is right. It was his idea to set up an organization called The Simulation Council when the ACM and the IRE weren't interested in establishing some sort of group in the field of simulation.

RM: Okay. I would like to come back to this aspect of his work. He is the person to talk to about the

LARK operation as it was happening at Point Mugu?

SR: Yes. Well, to get back to me and Convair.

RM: Before we do that I have another question. You refer to the term "balancing" in the simulator. I'm not sure what that means.

SR: Balancing an amplifier is the process of making its output zero when its input is zero. The electron-tube amplifiers of that day were subject to a certain amount of random drift. In the first model of amplifiers that we had there was no built-in correction. This meant it was necessary to correct the drift very often, like every twenty minutes. Since it took five or ten minutes to do the whole thing, it was really miserable. Once the kits were added which provided the electronic [compensation for] drift, you didn't have to do it nearly as often. However, it was still necessary from time to time to check the balance of amplifiers, and if they were out to rebalance them, that is, to bring the output back to zero [with] the input to zero.

With the very large computing system that we built which I described to you, it was necessary to go through a very elaborate check of practically everything in the system every morning before we turned the machine loose for people from other groups to come and use, mostly on an open-shop basis. We had to be able to guarantee that everything was working within specifications. After standing for a few hours, with power on, but no use during the night, we just could not guarantee it would work without going through the system completely. We had a special early crew that came in and checked out the machines before the dynamics engineers came to work.

RM: The other thing I wanted to get back to is the analog computer that you built. Was this an electronic machine?

SR: It had thousands of vacuum tubes, it consumed nearly a hundred kilowatts of power, and required a really potent refrigeration system to keep the temperatures in the machine room livable [for both man and machine]. High temperatures without that [air conditioning] equipment would have caused the equipment to fail. Even so, we did have a problem, because no matter what I said, the plant Engineering Department really didn't believe that we had all that equipment on all the time, which we did. The refrigeration system was, in fact, underdesigned for the system, and temperatures of ninety degrees were not uncommon in the machine room. Another novel thing about the design of that machine was that we had all of the computing equipment on one floor, the control equipment and operating consoles were on a floor immediately above it. The wires ran vertically from the computing equipment up to the computing consoles enabling us to use much shorter wires and get capacities and inductances down and, therefore, not impair the operation of the system to the extent that the more conventional one-floor design would have.

RM: So it was a two-story analog computer.

SR: Yes. [Laughter]

RM: Am I clear that you designed four of these?

SR: They were built under four separate contracts, but actually we built two different designs of machine; one for Pomona and one for San Diego. The hardware hooked up to the San Diego machines was not the same at every station; the patchboards were the same at every station.

RM: We were talking about the analog computer and I was just wondering whether you considered buying a REAC, for instance?

SR: The machine the Navy sent us [(a LARK simulator)] was very early model REAC equipment. Then later they sent us more of it and also put in this modification kit [I referred to] so that the amplifiers would stay in balance for much longer. That was basically Reeves Instrument Company computing equipment.

RM: But you needed something with more power and more capability.

SR: The big problem with it was that it did not have [re-] movable patchboards. It was primarily to overcome that problem that we decided we had to have some kind of removable patchboard system so that we could change problems. Without that, it meant only one problem could be on the machine and, until that problem was solved, no one else could put on [another] problem. The dilemma was: what [to do] if there were four or five people who wanted to use it all in the same day.

RM: Were in contact with Rand, because they too had done that?

SR: Yes. I went up and I talked to Bill Gunning.

RM: Yes.

SR: I liked his ideas [very] much. Our philosophy about bringing all the controls out on the patchboard was suggested by Bill Gunning and brought with enthusiasm by me. And we [learned] from them their general approach to the removable patchboard using IBM equipment. However, they had a problem with theirs which Bill told me about and I resolved [to avoid] it on our equipment. Their problem was that the patchboard material had a certain amount of electrical leakage between the various jacks [(holes)]. When we did ours we used an [alkyd-glass] plastic material which has extremely low electrical leakage properties and, in fact, proved to be an almost ideal material for dimensional stability and imperviousness to electrical troubles. Rand's modification of their computer influenced us more strongly by far than anything else we saw. In fact, our installation and the Rand installation were the two installations which caused Mr. Dewey -- he was Vice President of Reeves -- to comment in a lecture he was giving that, "The further West he went, the less a REAC looked like a REAC." [Laughter]

We had lots of Reeves gear downstairs below our equipment, but we just bought the basic amplifiers and their power supplies. We did the same thing with

Electronic Associates. We also bought some [Goodyear and some] minor company equipment.

RM: Then you put it together as you saw fit. Those were the questions on the parts that we have already discussed. Let me see, we had you where?

SR: I think I just joined the Astronautics Division and got hooked into administrative work for a year or so. Then I started getting involved in proposal preparations for electronic systems, many of which involved computers one way or another. During this time I argued with my superiors in the company that space simulation was certainly going to be a requirement of the future, that we should be prepared to play in this field, and that if we were going to do so it was really not practical to wait until after we got a job to prepare ourselves to be able to simulate things. I felt that we should decide what we wanted to simulate and set about building a laboratory that would enable us to do it.

This proposal was finally bought by the company management and we were given about two million dollars to build a spaceflight simulator which was designed around requirements for [the] man-machine interface given to us by the life sciences people and by pilots.

It was decided that we would build a simulator to simulate orbital flight, with particular reference to winning the manned space station contract. We started down this road and got quite far along when the contract was awarded to another company.

The project was not really completed according to the initial design at all; instead it was adapted as best we could to simulate landing on carriers and various things which the company needed to simulate. In other words, my enthusiasm for simulating orbital spaceflight had actually steered the company down the wrong road and it turned out that we had the wrong kind of simulator for doing what was [needed]. It was sufficiently flexible so that the company did find quite a few uses for it, but it was too specialized to really realize its full potential.

I have to own up to having run into a couple of technical problems that were not solvable by me, my group, and the state of the technology at the time. One of these problems centered on our plan to enable the simulator to simulate flight around the earth in any orbit whatsoever. Not just equatorial, or thirty degrees off equatorial plus or minus, or anything like that, but any orbit whatever. We had to have a globe that could be moved [relative to a TV camera] according to any orbit. [The simulator] had to have rotation of the earth built into it, and also the orbiting of the satellite. We decided that we wanted to simulate orbits from roughly a hundred miles altitude up to the distance of the moon. We decided we would present a three-window view from a spacecraft so that you could see the earth when you were pointed directly toward it or when you were pointed at various angles from it. We were quite successful in simulating that part of the system. The problem with the all-orbit globe was that it had to be completely sealed, and then you had to be able to illuminate it from the outside with fairly high intensity lights in order to get enough light for a television camera, or you had to illuminate it from the inside with some internal lighting system. [We decided on inside lighting with 1200-cycle power supplied by means of an air-core transformer.]

The difficulty we ran into was that plexi-glass or lucite was the material available at the time for making the globe. We realized there was a heating problem because we needed a certain number of watts of energy to [get enough light from] the cold-tube illumination which we were using [inside the plastic globe]. Before we ever started on this project, [we] set up a laboratory test in which [we] dissipated the amount of power which we had calculated would yield the amount of light we were told would be needed for TV cameras. We had verified incidentally, that this amount of heat could be dissipated without problems to the plexi-glass. When it got down the line to doing it, the problem was [that in fact] there wasn't enough light. The TV engineer said, "We need twice as much light." This meant more watts and raised the temperature so high that the plexi-glass would soften. A new material, polycarbarnate, was becoming available, but it was extremely costly and we did not make the switch because of our other technical problem with that globe.

Our Manufacturing Research Department had assured me that we would be able to get an adhesive which would enable us to cement aerial representations of the surface of the globe to the plastic globe, and do it in such a way that the cementing would not be seen and would not in any way interfere with the operation of the thing as intended. We were assured that it would present the right sort of view if we had the right sort of material to put on the globe. Because of our desire to simulate 100 miles to 250,000 miles altitudes with our camera system, the hundred-mile [altitude] had to be about one inch from the globe. At this point, the camera was seeing a space roughly one-half inch by four-tenths of an inch on the globe [surface]. Blowing it up to full window space in the spacecraft, which is putting it under a sort of microscope, [showed] that most adhesives [were] composed of little globules, dots. Manufacturing Research could find nothing that didn't exhibit this property, so we could only present views which appeared to be suffering from smallpox. [Laughter] We were defeated at two points [by] technology.

Whether that same problem would have existed with the polycarbarnate globe system, I don't know. I think it would have withstood the extra wattage that had to be dissipated without any trouble, but whether a suitable adhesive for joining a film to polycarbarnate exists, I don't know. Maybe. Maybe not.

At any rate, with our loss of the contract for a manned space station, interest in spending further money on trying to solve these problems disappeared.

The television art also advanced so that there are now television camera tubes that do not require so much light; so we wouldn't have needed additional light. We had to push ourselves ahead of the state-of-the-art and didn't succeed.

RM: Who did get that contract?

SR: I think it was McDonell Douglas, but I am not sure. At any rate, it was a very challenging and interesting project providing complete interface between man, flight equipment, and a simulation laboratory [whose] computers [did] all of the computation in real-time and [ran] the instruments.

RM: When did you start on the project?

SR: We must have started that project about 1964 or 1965. We had to give up on it about 1967, somewhere around there.

The last three years at Convair, I was working in wide-band communications switching systems, which I also found very fascinating. It didn't have very much to do with computers, except that now I became a user of computers as simulators [to solve] design problems. We did work out a great many rather novel approaches to solving the problems of matrix switches for switching signals [which ranged] all the way from direct current up to fifty [or] a hundred megahertz in frequency.

I don't believe other people, at that time at any rate, had been able to achieve that. The way our designs were developed was by using the digital computer facility as the simulator for the circuits that we worked out. I am happy to say that when we built the circuits, they worked the way the simulator said they would.

RM: Fabulous.

SR: As a result of that, another engineer [and I] obtained some seven patents on advances in circuitry. The main tool we used was simulation.

RM: Going back to the digital work that didn't get off the ground, [did the Navy]have a machine built?

SR: I don't know. They said they were going to procure off-the-shelf equipment and cut back on their specs to the point where they could buy off-the-shelf equipment to do the job. In other words, it was a complete change of philosophy. With the change in command, the personnel was shifted and the whole philosophy of the operation was restructured; it was completely different. Under the new philosophy they adopted, there was no need whatever for the sort of system that we were proposing.

RM: That [was] pretty dramatic isn't it?

SR: Yes. It was terribly disappointing. That was probably one of the most disappointing days of my life.

RM: I imagine. That is a lot of work. How about within Convair during the period of time you were there? Were you familiar with the work and the kinds of computers they were bringing in? I am thinking now about 701s, UNIVACs and things like that.

SR: Yes. I was one of the Engineering Department's representatives in the selection of the digital computers that the company leased. We sincerely believed that IBM was not the manufacturer with the most suitable equipment, but it was very difficult to convince company management of it because, let's face it, IBM has an extremely effective sales force. However, when we outgrew our 701, we were successful in convincing management that the thing to get to replace it was not a 704, but instead Remington Rand's UNIVAC.

RM: 1103?

SR: We bought an 1103. We were not dissatisfied with that choice at all. A lot of things happened subsequently. They merged the Astronautics Division back into the Convair Division, and the personnel involved in making computer decisions changed. We found ourselves using the Astronautics [IBM] 7090/7044, a combination which proved inadequate for many engineering applications. Once again IBM waged a very aggressive and very effective campaign. Finally, if memory serves me correctly, they told the company that they would put a 360/67 on the floor for us for a year's time at no charge to prove to us that, with the 7094/7074 emulator [they would supply] it would handle the Engineering Department's workload. The Engineering Department's analysis [showed that] it would not. Well, it's awfully hard to refuse a free computer, so the engineering management rationally decided to take IBM's offer. At the time, it was during my last years at Convair, I was in the role of user of the machinery as a simulator, [(I was working on the switching systems)]. We could do lots of things and it was wonderful having that free computer time available, but in the end, the machine proved, as the Engineering Department computer executives had said in the first place, that it could not do the job. The machine was removed and [a] Control Data 6400 was put in to replace it. It was [the] machine the Engineering Department had previously determined that they wanted, and it did do the job.

There was another factor very heavily in IBM's favor. At least so seemed. They claimed that the emulator for the 7094 would enable the department to use all of its 7094 programs without reprogramming. There was more than a million dollars worth of programming effort there, and that is big money. The programs would run on the emulator, but the throughput was so slow that it would not do the quantity of work that was needed in the time available and, therefore, it wasn't practical. [So] whether we kept the 360/67 or put in a new machine, all of the 7094 programs had to be redone anyway. There was no way out of it. So [the reprogramming] was done for the 6400. This [is] a problem that I think a great many companies ran into.

RM: As they changed generations?

SR: Yes. In a way you can't affort not to have as much more efficient computer, but you can hardly afford to reprogram everything to use it efficiently. If you can't do that, then it is not worth having.

RM: It is a bit of a vicious circle then.

SR: Yes.

RM: Going back to the Convair 701, are you familiar with the kind of work that it was used for?

SR: Not at close range. That was set up practically as a subfacility for the stress crew -- the group that I finally persuaded in the early days to try to use the accounting machines and had become sold on the merits of computing. They used it intensively and found they could do practically all of their work with computers when the 701s came in. It was, in essence, operated under the structure group.

RM: As I presume were the CPCs.

SR: Yes. They finally got the Accounting Department to agree to lease some extra machines from IBM and they put them over in the low-speed wind tunnel with some auxiliary gear. [It was] mainly used by stress people in structures. [It was] effectively a structures group digital computer facility which was great for their work. The man in charge of that was named Ben Ferber.

RM: A name I seem to have run across somewhere.

SR: You probably have. He was at Convair for years. Most recently I heard about [his] running a school for [medical technicians] in San Diego. I haven't seen him for a long time.

This is really strictly not related to computers, but speaking of Ben Ferber I can't help thinking of it. He was in charge of this digital facility for the structures group and, in fact, for anybody else that wanted to use it. He was involved in negotiations to get a better computer and at the same time he had discovered a growth on his back. There was considerable reason to believe it might be malignant so he had set up an appointment to go to the Mayo Clinic for surgery and biopsis to determine what it was. I had occasion to see a good deal of Ben in some of the meetings he was having on his last day at work before he went. Nobody who was in those meetings who did not know what I knew could have had any idea there was anything on that man's mind but business. I thought it was really extraordinary. Few people would have that much ability to thrust something so immediate and threatening out of their minds and attend to the company's business with such concentrated attention as he did.

RM: That is incredible. It really is.

I would like to go on to the Simulation Council, but before we do, are there any other areas of key Convair contributions to the computing industry that you can think of?

SR: Well, I guess I was the fellow who started us down the road of digital computing with that [early] project that I did. Perhaps the paper I wrote gave the whole concept a little bit of a push, so there was [a] little contribution there, I think. It was really a matter that the state-of-the-art was getting to the point where this was feasible and there were going to be hundreds of people, quite independently, scattered everywhere, who would be [having] the same sort of thoughts and doing the same sorts of things.

It makes me think of the Williamson amplifier, which hi-fi addicts from yesteryear all know about. I know at least six people who invented it; I am one of them. I built one for myself and I built one for the girl I later married. It never occurred to me to patent the thing, and this was a couple of years before Williamson became famous for it. I have talked to a number of other people who did the same thing. The technology was there. The obvious benefits of going down the road to this design were there for anybody who was interested in hi-fi music reproduction, so all of us who were familiar with the need and with the technology just put it together and did it without thinking we had done anything.

I didn't think I had done anything noteworthy when using accounting machines to do [engineering] calculations. I am sure the others who did the same sort of thing about the same time thought the same way. Collectively, of course, it did start a trend. Collectively, our complaints to IBM about how ill suited their equipment was for this sort of thing did have an effect. So that is area one.

Area two was, I think the very radical improvements in analog computing equipment that we made. Both in scale -- it was much the largest computing system that had been put together at that time -- and in flexibility and general capability. Of course, it was later surpassed by other systems. Milt Warshawsky at Wright Field, not too many years ago, had a machine installed [that put] the one we had built in those days to shame. [His] system was much better and much more capable. It was a hybrid system, which had tremendous advantages. I think the company did advance the art somewhat by knowing very well what was needed to develop the state-of-the-art to make computing equipment more useful. We did originate a number of features for analog computers by sitting down with engineers from manufacturing companies and telling them what we didn't like about the equipment they currently had, what features we thought it ought to have, and even suggesting how they could go about achieving them.

RM: Can you give me some specifics on the companies and also the changes?

SR: Yes. [In] the early machine, the big one that we built, we provided individual control relays that control the state of each amplifier. The [amplifiers (integrators) can individually be put in] RESET, HOLD or OPERATE. There was a theoretical fourth state which you can get out of two relays which we didn't find any use for in those days. Later we found the use for it [in a "problem [check" mode]. At this time the commercial companies were not providing anything but control of the whole machine; the whole machine was in HOLD, the whole machine was in OPERATE, the whole machine was in RESET. For solving [partial] differential equations and some [other] problems, it is very important to be able to have some amplifiers in HOLD and some in RESET at the same time. [With] sample-hold circuits, for example, particularly if you have a hybrid system [and are] feeding [analog] information to a digital computer, you may want to sample everything at the same time, hold those samples, and then feed the results serially to digital computer. But the [samples must] all refer to the same time. This sort of thing required that you have individual control. We were able to twist Electronic Associates' arm, I know, and I think probably other company's arms. If they[didn't give] us individual control, amplifier by amplifier, at least they created blocks of control so that there were amplifiers in different parts of the patchboard that could be switched as a block to one of these conditions or another. This provided a good deal of flexibility in solving some kinds of problems [that] simply [could not be solved] without it. As far as I know we were the people who precipitated the inclusion of that feature in commercial hardware.

At one time I had a list of about fifteen features for analog computers that we had pioneered at Convair, which later various companies included in their literature sounding as though they had invented them. That used to irritate me a little bit. Regretably, I can't think of too many of these things. One of them was a means of checking whether or not the time constants for the integrator gains were what they were supposed to be. This requires checking the capacitor and the resistor to see that they supply the right time constant. We rigged up some equipment that would do that and variations on this were later incorporated in some commercial machines, though that was not a particularly popular feature. It became possible to buy very accurate capacitors and very accurate resistors, so people began to have great faith in this time constant without measuring it. I think we did extend the concept of problem checking. Reeves was the first company to put a problem check in their commercial computers and it really was a tremendous idea. It was a static problem check and it didn't really do anything that people couldn't do before. What it did was mechanize it in a very handy way for the person who used the computer. I think we thought up some embellishments on it that may or may not have been incorporated in other people's gear.

We were always modifying our computer, updating it as we had better ideas or if we thought that somebody had come up with something that was worth copying. We were very early, if not the first, with digital readouts for analog quantities. I'm sure we were the first people to provide [on] eight[-channel] strip-chart recorders, [coded] digital information which positively identified the scale that was being used on that chart. [Each channel gave] a zero reading [not only] before you started your [simulation, but also] after the [run] was finished, [to show] that the zero hadn't drifted. All this was recorded automatically right on [each channel of the] strip chart. [The] calibration [data I mentioned earlier was all recorded automatically before each simulation run started. When the run ended, the machine] gave you a final check that [it] hadn't drifted during the run. This was copied by Reeves Instrument Company on some of their later equipment. I know that was one of the things we originated. There are many more, but I can't think of them now.

RM: Okay, maybe some of them will come to you when you get the transcript. If they do, feel free to expand on them. That would be good.

SR: We had a very inventive group of engineers who were charged with maintaining and modifying the computer. Because of that, and because we worked closely with the people who were using the machine open-shop, we knew what their problems were and what their gripes were. If we could figure out a way of making things work better for them and it didn't cost too much money, we would do it. We always had money to make modifications and improvements as long as they didn't run into hundreds of thousands of dollars.

RM: Electronic Associates was one of the large analog companies?

SR: Yes. The largest.

RM: Are they still extant?

SR: Oh yes, very much. They made digital, analog, and hybrid computers. They are listed on New York Stock Exchange, I think.

RM: They started back about when, do you know?

SR: Actually, there is another little bit of history we made. We were the first people to buy any of Electronic Associates computing operational amplifiers. They were designed by an engineer named Monroe Miller, who later left the company and, with a man by the name of Gordon, formed MILGO, which is now a flourishing company in Florida. The paper specifications of these amplifiers [were] superior to anything else that we [had] bought, but they had never been manufactured. We had our own specifications that we wanted to meet and these amplifiers were better than our specifications. We were in the beginning stages of building this [2-] million-dollar computer system, and we wanted those amplifiers badly. We agonized over them for some time, and then decided to buy a block, keeping our fingers crossed and hoping that it would turn out to be a good purchase. We gave them what I was later told was their first order for equipment.

All through the years [when] I was associated with computers, I was a glutton for punishment in buying the first production run of equipment that various companies put out [all] because [I couldn't resist the] enticing specifications [they published]. This is certainly the way to a great many headaches because there are all sorts of bugs in first production equipment. We wound up buying many more of the [Electronic Associates] amplifiers before we were finished with the project. They were good.

RM: When was it date-wise?

SR: This would have been between 1951 and 1954. Somewhere in there.

RM: So they got started [in the] early 1950s, you would say.

SR: Yes. With this equipment. They had made plotting boards before. Monroe Miller was [a] really brilliant engineering designer who understood the problems of operational amplifiers very well and he came up with a beautiful piece of equipment. Later they also came out with other pieces of equipment with advanced specifications. They [got] their start by being the Cadillac company in the analog computer field. They [also] had the lion's share of the market.

RM: Yes. Reeves fell by the wayside, didn't it?

SR: Yes, they finally did. They were a subsidiary of Dynamics [Corporation of America]. They built the first LARK [simulators] and were working under contract with the Navy for equipment to use in fire-control computers for the LARK program. Their charge [(Project Cyclone)] was to design and build, as fast as possible, analog computing equipment [that would work] for these particular purposes. At the same time, RCA was given a contract by the Navy -- I think both contracts came out of the Special Devices Center -- to build the best possible analog computing equipment, given the state of the art. The result [RCA's work] was Project Hurricane, which was an enormous analog computer, physically speaking. The pieces in it were very large, but it had much better accuracy than the Reeves equipment and [a] great many refinements. It really advanced the art a lot, although HURRICANE itself became an obsolete computer fairly early in the game because of the difficulties [of] using it. It was used for a few years. Since all-digital computers tend to become obsolete in about three or four years time, I guess one couldn't complain [that] HURRICANE did the same thing.

RM: That is right. Some of them were obsolete before they were produced, actually.

SR: Yes.

RM: A couple of names while you were talking came to mind. Did you have any dealings with the J.B. Rea Company?

SR: Yes. We certainly did. J.B. Rea, incidentally, was at the organization meeting of The Simulation Council. He suggested that the [word] analog not appear in the organization's name because that limited it to one kind of equipment, whereas really the interest was in simulation: a way of using computers of any kind. That was a very wise suggestion on his part. Of course, the society really did start off as an analog society, and was so thoroughly painted with the analog brush that it is hard for people even today to realize that only a small minority of what is printed in our publications deals with the analog world.

RM: I know, I am tarred by the same brush. Do you happen to know where Rea is?

SR: No. I don't know where he is now.

RM: I'm trying to track him down. I would like to talk to him because he is an interesting man and covered a lot of interesting work. I've talked to some of the people who were with his company when they built the digital computer, RAYDIX.

Another name that I just remembered is Larry Steinhart. He is supposed to be in La Jolla/San Diego area. Does that mean anything to you? He worked with Bush.

SR: I'm sorry to say I don't know him. Of course, Bush was a real pioneer.

RM: Yes. That is right. I believe he was [a] student of Bush's.

SR: Unfortunately I don't know him.

RM: Would you have any suggestions about how I might track him down?

SR: I think if you look in the phone book you will find some companies that have digital in their name. Without more information than that it may be very difficult to find him in the phone book.

RM: Yes.

SR: There have been several digital computer companies, small ones. I know who you might call: Roger Brown. He is president of Copley Computing Services; it's a subsidiary of Copley Newspapers. He runs their computer services, which also serve the public as a sort of computer utility. Roger is pretty well acquainted. He is a very competent programmer himself and knows lots of people in the field.

RM: Okay.

SR: Somebody else you ought to talk to, [one who] undoubtedly can help, is Donn C. Parker, whose current speciality appears to be how to assure the security of information in computer memory. He worked for the Astronautics Division of General Dynamics at one time. He has been an officer of some group within the ACM not too many years gone by, and is currently working, I think, [in] the Menlo Park area somewhere. He should know an awful lot of the people in the game. [He has been active on some AFIPS projects.]

RM: Can we get into the history of the Simulation Councils?

SR: It was organized at Oxnard.* The organization first

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*First letter on Simulation Council Meeting sent out by John McLeod, November, 1952. See Smithsonian files.

existed in Southern California. All of us who were responsible for analog laboratories were pretty faithful attenders at the meetings, because we could get help in solving our problems and pass on ideas we had to others. The organization was noised about a bit through the newsletter that John McLeod and his wife were putting out; so people in other parts of the country who also had this type of equipment began to think they too should have Simulation Councils. The first one was organized, I believe, in the mid-West, and then others started springing up in other parts of the [country]. I think it was about 1956, four years after the initial organization, that the various independent Simulation Councils thought they really should have some national organization through which they could exchange information, cooperate with each other better, and generally pull the thing together. An unincorporated association developed, in which each of the regional councils named directors. They met in conjunction with the Joint Computer Conferences, and at one or two other convenient times, and tried to plan how they could better serve the general purposes of the society in [its] field.

In 1957, three members of the society formed a corporation called Simulation Councils, Inc. They told the Board of Directors of Simulation Councils that if they would care to be named Directors of the new corporation that the three initial incorporators, would resign and then the [Simulation Councils would be] incorporated. This was done and [Simulation Councils (SC)] became SCi. For better or for worse it has been known by those initials pretty much since then. The "i" on the end of the name has been quite a disadvantage because people have tended to think it was some kind of business organization, which, of course, it isn't.

Ten years ago this fall, the society began to publish its own technical journal. It had started out for a few years with a newsletter that John McLeod and his wife put out. [Later] we [had] half a dozen pages or so of text [(with John as our editor) in] each issue of Instruments and Communications Systems, later called Instruments and Automation. That continued through the year 1963.

In fall of 1963, we decided to stick our toe into the publishing field. I had met a very talented graphic designer from Denmark named Hans Jorgensen, and he had got me all fired up about having our own publication. I had been thinking about a sixteen-page publication, which is a big improvement over six pages, but our enthusiasm built up and we decided finally to go for broke and have a full-fledged technical journal. We didn't have any money, though; so we decided to put out a Fall 1963 issue if we could get the money by selling ads in advance. We sold ads hard, and we found various manufacturers in our field who were very kind and generous. They took ads and paid for them before the magazine was printed. That way we collected over fourteen thousand dollars. It didn't begin to cost us that much to put out the first issue, and it was bigger than it has ever been since -- a full one hundred and twenty pages [Laughter]. The net surplus on that gave us the capital to publish the first issue of our regular monthly publication.

Our Board of Directors and Executive Committee were very worried about plunging into this expensive business with no experience back of us, and for reasons that aren't hard to think [of], there [was a lot] of [cautious opposition]. However, the enthusiasts -- Jack Sherman, John McLeod, and I --

prevailed, and we got a reluctant approval from the Executive Committee to go ahead and put out [the first] issue on [what we hoped would be] a monthly basis. That was January 1964. It was very well received. Members were joining our society at wholesale rates and if memory serves me correctly, [the membership] tripled within a year after we started publishing. At first we had trouble getting articles for the magazine, [and had to resort to reprinting some important paper to fill out our early issues.] But [our trouble] evaporated as we became better known. We were publishing original work so; we started being cited by authors writing in other journals, started being indexed and abstracted, and built up from there.

[Though] we had a real going concern, there were some rough spots on the road. One of them came when the Government cut back rather abruptly on the amount of aerospace business. That hit the market for computers, which hit the manufacturers of computing equipment, who were our principal advertisers. All of a sudden we found our advertising income, which had been up around eighty thousand dollars a year, dropping to a very small fraction of that, and no other sources of income building up fast [enough] to replace it. We had a financial crisis to make our way through, and [we] did. About the time that [crisis] was all settled, [a combination of] hybrid computers, a lot of switching around of activity within the computer industry, changes in product lines by computer companies, and a recession, led to a [second sharp] deterioration of advertising revenue and created another problem for us. However, it [did] not keep us from continuing our publication. We became very well established as a technical journal. The thing that really earned us [the journal's] reputation [in the early years] was the excellence of [our] graphics; they seemed to call everybody's attention to it. We received countless compliments on the high quality of the graphics which, of course, was very pleasing. We knew we had an excellent designer in Hans Jorgensen; so we were really not surprised. Interestingly enough the excellent graphical quality we were able to achieve was not expensive and our publishing costs were not out of line with those of our sister [societies'] publications. It was possible because we had the right combination of people to do it.

RM: Is the publication the prime function of the Council, or do you hold meetings?

SR: We hold meetings. The Regional Councils, of which there are nine, eight in the United States and major portions of Canada and one in the United Kingdom, plus a scattering of members in a total of more than twenty countries, hold meetings at varying intervals. Our major activity in terms of money certainly is publishing Simulation. It accounts for the lion's share of our budget.

RM: In the early days was it just users, or was it manufacturers of the equipment that was used for simulation work? Who were members?

SR: Primarily it was users, although people from manufacturers' engineering staffs and sales staffs were members. Many of [the latter] had pretty good technical backgrounds [and were helpful, energetic members].

RM: Basically it was to exchange ideas?

SR: Its prime purpose [of our publications (monthly Simulation and semiannual Simulation Councils Proceedings)]was to record advances in the art, [that] is recording original research [which is the main task] of all primary journals in the technical fields.

We also published articles about applications of simulation, and of particular ways in which difficult problems have been put on some kind of computing equipment and solved, providing simulation-type help in solving various problems. We always published applications as well as more fundamental stuff.

We've been interested in languages, we have been interested in fundamental mathematics, and [in] of the use of computers as simulators. We have published a fair amount of material in these areas.

RM: Has the Council developed its own languages?

SR: The closest approach we have come to developing a language was to lay down the fundamental specifications for continuous system simulation languages in digital computers. This was in our December 1967 issue. This set of standards has been observed by most people who have designed languages for continuous simulation since then. It was sort of [a] foundation the document in that field.

RM: This is something that I haven't even thought of. Of course, there is a whole different set of languages for this type of work than in regular digital computers.

SR: That is right.

RM: What were some of the most important ones?

SR: [One is] SIMSCRIPT; by its name you would know that it is a simulation language.

RM: Who developed that?

SR: You know, I don't remember. There are quite a few of them which I don't have at my fingertips now, but if you will remind me when I see this copy, I will give you a list of simulation languages.* We published surveys of them from time to time. Of course, many languages that are not simulation languages are used for simulation. You can do simulations in FORTRAN or ALGOL, and many have been done.

RM: Can you think of any other things you might want to add?

SR: I don't right now, but I am sure that when this comes back for rework, there will be a lot of things that will come to mind.

RM: Good. Thank you very much.

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*See list.

END OF INTERVIEW

Index - Stanley Rogers
9 Aug 1973

ALGOL
65
American Federation of Information Processing Societies (AFIPS)
57
American Institute of Electrical Engineers (AIEE)
7, 8
American Television Laboratories
2
Association for Computing Machinery (ACM)
28, 27
Breitwieser, C.J.
3, 5
Brown, Roger
57
Bush, Vannevar
56
CDC 6400
42, 43
CPC
6, 44
CYCLONE
54
Cadillac Company
54
Convair
1, 2, 3, 4, 17, 26, 29, 39, 41, 42, 44, 49
Convair Analog Computer Laboratory
11-19, 30
Convair Spaceflight Simulator
34-39, 47-51
Copley Computing Services
57
Crabb, Robert C.
26
DeForrest, Lee
2, 3
Dewey
33
Dynamics Corporation of America
54
Electrical Engineering
7, 8
Electronic Associates Inc.
20, 22, 34, 49, 52, 53
FORTRAN
65
Ferber, Ben
44, 45
General Communications Products Company
2
General Dynamics
3, 22, 24, 26, 27, 57
Goodyear
34
Gordon
52
Gunning, William
32, 33
HURRICANE
54, 55
Hi-Fi music
46
IBM 360/67
42, 43
IBM 405
6
IBM 603
6
IBM 701
41, 43, 44, 45
IBM 7090/7044
42
IBM 7094/7074
42, 43
Institute of Radio Engineers
28
Instrumentation and Computer Systems - 59
Instruments and Automation
59
International Business Machines
5, 6, 7, 15, 16, 18, 20, 33, 41, 42, 43, 44, 47
J.B. Rea Company
55
Joint Computer Conference
58
Jorgenson, Hans
59, 62
LARK
8, 26, 27, 28, 29, 32, 54
Lockheed Aircraft Company
25

MILGO
52
Massachusetts Institute of Technology
3
Mayo Clinic
45
McDonnell, Douglas
39
McLeod, John
28, 58, 60
Miller, Monroe
52, 54
Occidental College
1
Parker, Donn
57
Project Cyclone
54
RAND Corporation
8, 17, 26, 32, 33
RAYDIX
56
REAC
31, 32, 33
Radio Corporation of America (RCA)
20, 25, 54
Ramo-Wooldridge
20
Rea, J. B.
55, 56
Reeves Instrument Company
8, 9, 11, 32, 33, 51, 54
Remington Rand
8, 20
SIMSCRIPT
65
Sheretz, Paul C.
26
Sherman, Jack
60
Simulation
28, 63
Simulation Council Proceedings
63
Simulation Council, The
1, 28, 45, 55, 57, 58, 59
Steinhart, Larry
56
UNIVAC 1103
41
US Air Force
3
US Air Force, Wright Field
47
US Army
3
US Atomic Energy Commission
19
US National Bureau of Standards
19
US Naval Air Missile Test Center, Point Mugu (USNAMTC)
20, 21, 22, 23, 24, 28, 29
US Navy
8, 20, 22, 23, 26, 32, 40, 54
University of Pennsylvania
13
University of Southern California
1
Warshawsky, Milt
47
Williamson amplifier
46