INTERVIEWEES: Bernard and Ruthie Howard and Harold and Sally Skramstad

INTERVIEWER: Henry S. Tropp

DATE OF INTERVIEW: 2 March 1973

Tropp:

This is the evening of March the second. I'm sitting around in the marvelous company of Prof. Howard and his wife Ruthie and Prof. Skramstad and his wife Sally and we're sitting in Prof. Howard's home and we're going to have a bull session on the early days of computers. Skrams, why don't you say something so, I can check your voice level on this.

Harold Skramstad:

Well, let's see...

Tropp:

Fine. [Laughter and several people talking] Well, we'll begin with this simulation problem that we started to talk about earlier.

Harold Skramstad:

Washington Traffic Control.

Tropp:

Yeah.

Harold Skramstad:

That's fairly recent compared to the early work during World War II and you need to...

Tropp:

Let's go back to the early days, to World War II.

Bernard Howard:

Start at the beginning of course.

Harold Skramstad:

Well of course I was involved in guided missile development during World War II and you need to...

Tropp:

This was at National Bureau of Standards in Washington?

Harold Skramstad:

It was at the National Bureau of Standards in Washington and we had to simulate the guided missiles, simulate the motion of an object going through the air and all the details...??: Six degrees of freedom.

Harold Skramstad:

Six degrees of freedom, the whole business. Of course there were no digital computers at that time so we used the analog ____ and so we had to devise analogs to represent these things and we did these by electronic circuits, where the voltages simulated the motions of the missile.

Bernard Howard:

Were these passive networks?

Harold Skramstad:

The early ones were passive networks. You see, this was before the operational amplifier was invented, really.

Bernard Howard:

The Bush was eletromechanical, mostly mechanical.

Harold Skramstad:

Yeah, mostly mechanical, but the ones that I was involved with were electrical analogs but it was before

the operational amplifier which really later ______ the analog computer. It was done by electrical circuits and I think the first ones that we developed for simulating was in about, well, '43, '42 or '43 or '44, in that general period. They were a combination of electromechanical--in other words, what we would do would be to simulate--we'd use an actual servo that was used to move the control surfaces on the missile and connect the arms of this servo to potentiometers which would vary electrical voltages which would cable to passive networks into which we would represent the aerodynamic characteristics of the missile in its flight, which again would feed controls into the servo, so we had a closed loop involving electrical circuits and the piece of mechanical hardware that did the driving and sometimes the passive circuits that we had to use or just ordinary amplifiers.

Bernard Howard:

That was really the first simulation that you were involved in.

Tropp:

How successful, as you look back, how successful were these, as simulation--representation of the simulation problem you were concerned with?

Harold Skramstad:

Well, they were adequate at the time for the rather simple aerodynamics that we were trying to include in them. They were generally 5 degrees of freedom rather than 6 degrees of freedom. Then right after World War II when the first readings--I guess REAC was one of the first...

Tropp:

REAC, yes.

Harold Skramstad:

REAC--when the REACs became available why then we switched over from the passive networks that

were used during World War II to the operational amplifier.

Tropp:

And some of those are still running, I think.

Harold Skramstad:

Oh, I think they are.

Tropp:

One of the questions I...

Harold Skramstad:

Old Project Cyclone that was up there in the meetings in New York. That was one of the original facilities.

Tropp:

What impact did your work have on the origins of Project Whirlwind, because that started off to be an analog flight simulator in its original state?

Harold Skramstad:

The idea was to get a digital computer that would be fast enough to actually represent the frequencies involved in the 3 rotational degrees of freedom of the missile itself, which means that it had to be able to represent accurately within, oh...a few cycles per second really, of oscillation.

Tropp:

Well, as I remember Project Whirlwind, though, it started off to be analog. It was only later that they moved in the digital direction. Originally, I'm sure there was some connection between that and your work, your early work.

Harold Skramstad:

Well, the main...

Tropp:

It was at MIT while you were there.

Harold Skramstad:

Yes, I don't recall the analog Whirlwind, but the first contact I had with Whirlwind was they were attempting to do digitally what you or I were barely able to do with analog.

Tropp:

That came later though.

Bernard Howard:

But I believe...didn't this whole business start off as the servomechanism's laboratory?

Tropp:

Right, it started off at MIT as the servo laboratory.

Bernard Howard:

Well of course. Naturally the servomechanism was analog because all the servomechanisms were.

Harold Skramstad:

Oh yeah, yeah, that's when...yeah, Al Hall was working in the servomechanism's lab, right.

Bernard Howard:

Right, and they were...in a sense they were implementing some of Wiener's ideas of feedback.

Harold Skramstad:

Yes, and in fact, I was up there at the servomechanism's lab during World War II and they were also helping in the simulation at that time.

Bernard Howard:

Whirlwind was a later development of the servo lab. I mean, I think that's what you mean when you say they first decided analog and then went digital, because the servomechanisms laboratory was essentially to develop Wiener's effectors...they had his sensors, his computer, and his effectors.

Tropp:

And then they decided to go digital and moved off in a totally different direction, sometime after, I would guess, after 1946.

Bernard Howard:

I keep...

Harold Skramstad:

We got the original work using the passive networks for...I've got a copy of that report, I think, in my files somewhere.

Bernard Howard:

Yeah, those people...this paper, digital versus analog... [There are several voices at the same time here]

Ruthie Howard:

We have a copy of it too, Skram, somewhere.

Harold Skramstad:

It was an MIT report.

Tropp:

Yes? About when, Ruthie?

Ruthie Howard:

Well, I remember it so well because, that's why I took Bernie in the study to ask him about it, because I

remember it so well...I guess it was better that I didn't finish it...[Not clear]

Harold Skramstad:

I can remember. Al Hull invited me to come up there and Admiral Gallery. Remember Admiral Gallery? He was interested in computers.

Tropp: I've heard of Admiral Gallery but not in terms of computers.

Harold Skramstad:

Oh, he's a character, I tell you!

Bernard Howard:

You know, it's interesting that there are a number of key administrators who really didn't contribute anything technically but who were essential in providing the support for the technical (improvements?).

Harold Skramstad:

Admiral Gallery was one of those.

Tropp:

I think Professor Condon was one of those too, in terms of knowing what to do with the money when it became available after the war and setting up the Bureau as a National Computational Center.

Harold Skramstad:

Your're right, uh-huh.

Tropp:

And there were a number of people like that, but Admiral Gallery is a new name. How would you define his role?

Bernard Howard:

Well, I think that there have been many developments in the course of mankind that have either been

made possible or been delayed for several generations--one generation or more, due to the infight of an administrator, somebody who has the money and he's got to decide which horse he's going to bet on, you see, and sometimes I think an administrator thinks he's smart and tries to figure it out himself, but then the good recover administrator, the one who picks his man, you see, and gives him free rein, often some very remarkable results come out.

Tropp:

Would you classify Ridenour as one of those, or how would you put it--where would you classify him as an administrator?

Bernard Howard:

Yes, I think Ridenour was the kind who picked his man.

Tropp:

I got that feeling as I looked at the development.

Harold Skramstad:

At that time there was this...that was in the simulation field--was his name Crawford? Do you remember him?

Tropp and Harold Skramstad:

Perry Crawford.

Tropp:

He did a very important thesis at MIT about 1942, that triggered a lot of work and...

Harold Skramstad:

Yeah, because I remember at that time, also in this field--what's his name? More, who's in North American.

Sally Skramstad:

Oh, yes, something...keeps coming back in the news...

Tropp:

Is that Calvin? Or is that somebody else?

Harold Skramstad:

No, John Moore, isn't it? I think so.

Tropp:

Moore?

Harold Skramstad:

John Moore. Isn't he the big head man there now?

Tropp:

I don't know, but that's another name I haven't run into. At North American?

Harold Skramstad:

He was at that time now. Gee, there's a lot of these other names that I just can't think of right now.

Bernard Howard:

But you know, all the time though that this analog-digital controversy was going and I kept--you know, right from the beginning I kept thinking of this lighter than air versus heavier than aircraft controversy and I kept thinking to myself that it's not going to be resolved on a rational basis--it's going to resolve itself.

Tropp:

That's exactly what happened.

Bernard Howard:

Sure, of course it's what happened. You know, looking back now we can see the rationale and so on, but at the time it did not seem reasonable to simulate since the discrete devices with some of the mechanical components, you know, were rather slow, with the time.

Harold Skramstad:

The thing that bothered most of our analog people was all the analog equipment works in parallel, so that if you have a big system you just need that much more equipment because everything works in parallel, whereas with the digital equipment everything was serial. How could you ever get a big system that's worked on a serial.

Tropp:

That's right, and also the early concepts were asynchronous so that the fastest number had to wait for the slow number...[Two voices speak at once here]

Harold Skramstad:

And we were almost wild when the first digital differential analyzer came out. That was when we thought, well now maybe this is going to be it! Because here we have a digital machine that can simulate--it's built especially for solving differential equations and so maybe this is the answer. But they were awful slow in those early ones.

Bernard Howard:

Of course, you know in solving partial differential equations--this was of course the equations of the airplane flight and so on we're dealing with--fluid mechanics means they're essentially partial differential equations--and we're coming just in the seventies. This is only the beginning of 1973 and it's only since 1970 that we're coming back around to the old idea of how to solve a partial differential

equation on an analog computer, in which time was the one intrinsic, you know, independent variable and so, if you had space you had to discrete-ize the space variable and develop, you know, solve these--change the partial differential equations to a system of ordinary differential equations. Well, the Russians call it method of lines and so on.

Harold Skramstad:

The analog had the advantage that you could reduce the dimensions by one that way, very much.

Bernard Howard:

And of course, the digital you had to discrete-ize everything. Now a whole...when the digital passed the analog by there has been--we've gone I think for ten to fifteen years ____ [Not clear]...we've gone with the finite difference schemes for solving partial differential equations. You simply replace every derivative at sight by finite difference approximations, thereby converting a partial differential equation into a finite difference equation approximation--approximating finite difference equation; and then thereby you convert the problem of solving the partial differential equation into two other problems, the problem of convergence and the problem of stability.

Now this has been a classic way of doing things and there've been, of course, some tremendous developments, in the New York University School and so forth, as you know, they've come up with some very significant fundamental theorems upon necessary sufficient conditions for stability and convergence and so forth, of infinite difference approximations to partial differential equations and this is particularly significant in terms of weather--everything to do with weather, whether it be weather forecasting or climatology or something, whatever, the fluid, the flow or air around the planet, and that was one of the reasons that Von Neumann, I believe, became interested in digital computers in the first place--the weather _____.

Tropp:

Yes, except the weather was his long, long time interest that stayed around for most of his life.

Bernard Howard:

And the stock market. I believe he came to the conclusion that the computer wouldn't help because the forces which caused the market to move were not evident in the past data and so...you know, how can you predict weather in real time. It was, you know, maybe the weather, tomorrow's weather you know by--in a week. Well, that was obviously not very useful.

Anyway, in terms of those partial differential equations of atmospheric motion, various schemes--oh, an enormous number of schemes appeared every month in the Monthly Weather Review, you know, new idea you know, this method and that method and somebody's new method; but it was only in 1970 that Robert and something, Robert et al. even suggested the concept of a general theory of stability of numerical integration partial differential equations, and they had of course in mind the complete finite difference method where you discretize full space and time variables. Well, it turns out I think--I believe within ten years that those, all that work, brilliant as it was, will become history and that we will come back to the original scheme on the analog computers where you discretize just the space variables, convert the partial differential equation into a system of ordinary equations in, you know, on the space grid and then you still have, you have a system of ordinary differential equations on the time continuum That's the method of lines and then you proceed to integrate those by quadrature rather than by finite difference methods, and of course the quadrature methods are more stable.

Tropp:

That's interesting because as I look back historically to the first Bush differential analyzer in the twenties up to the evolution to the Rockefeller, I guess, which was the really big differential analyzer that was

copied so many places, I suddenly realized as I look through it that while this was solving some nice problems of ordinary differential equations, we were now faced with problems that required partial differential equations. Analog computers couldn't do them. Something else had to come, therefore whether Aiken did it, or Mauchly and Eckert did it--somebody was eventually going to come up with a fast computer, digital computer to solve these problems and now what you're essentially saying is that we're coming full cycle and going to come back to the original device to solve the problems that in many ways nurtured the origin of those machines, because those were the problems that were around and they began to show up only in the thirties. I mean up until then most of the problems that we were concerned with as scientists could be handled with ordinary differential equations.

Bernard Howard:

Yes...[Several voices here]

Tropp:

We hadn't done any supersonic flow _______.

Bernard Howard:

Yeah. I remember at Chicago this was one of our projects at the Advisory Board on Simulation. We had this aerodynamicist, Frank Marshall, from NYU and who's now at Purdue, incidentally, Professor of Aeronautical Engineering: and we got together, and we had the idea that you could produce--you could generate the forces on an air vehicle not just by means of the old stability coefficients, you know, C alpha, C beta, what not, you know--just the first terms in the linear expansions and then you'd get those--the values of the coefficients in the wind tunnel and so on. We had the idea of actually solving the equations of flow around the air foil on a computer and then feeding them in to the simulation, see, as the input. You know, in other words generating the forces instead of by means of some stability

coefficients and so on and I think during the fifties at the University of Chicago--I think we made some reasonable steps forward in that direction.

Tropp:

Because that problem, you know, just started to come around. I know Dick Clippinger talks about working on it and spending something like seven years.

Bernard Howard:

That's the Clippinger at Hinsdale?

Tropp:

Yes.

Harold Skramstad:

This area of partial differential equations is the area that really promoted this so-called hybrid computer, a combination of the analog and the digital working together, because using a hybrid computer you had a tool that was rather effective in doing this partial differential equation analysis. Quite often you would solve it and one section, if you'd take and discretize for space and use an analog, a separate analog for each segment or each little increment in the space. This required an awful lot of analog equipment but it would do the problem and it would do it in real time.

Bernard Howard:

Yes, the real time was one of the keys to the...I think as a matter of fact in the early days, wasn't that what gave the heavy weight toward the analog field? People sat down and made computations.

Harold Skramstad:

Take the case of the flight of a missile or an aircraft. You've got frequencies that are very high, on the order of a fraction of a cycle per second or several cycles per second for the motions of the...around the

axis, the rotation (matrices?), while the frequencies that you're involved in in following the path of the center of gravity in this thing are very low, and so the idea here was you represent the high frequencies of motion in the analog and then you convert the...you use a digital integrator to integrate the position of the center of gravity and so you actually follow the flight path over a long period of time in a digital computer.

Bernard Howard:

(Comment about degrees of freedom.)

Harold Skramstad:

That's right, because if you tried to do this on an analog, for instance if the missile is going to go 20 miles you let that represent 100 minutes or something...you can't tell whether it's got to its target to any precision at all, so this worked out fine. The what you might call the very low frequency, things that you needed to know with precision, you could do with the digital and the rapid oscillations of the high frequency components you could do with the analog.

Bernard Howard:

That's what you did in the simulation of the Washington Traffic Control problem, isn't it?

Harold Skramstad:

Yes, we did that. We used--well, this was a little different thing because in that one we weren't actually simulating the motions of the aircraft about rotation forces. We were only following centers of gravity...

Bernard Howard:

You used what we came to call physical simulation for that one. Didn't you have to use some link trainers hooked up...

Harold Skramstad:

Oh yeah, we had...

Bernard Howard:

Sitting there flying their wings in Link trainers?

Harold Skramstad:

We had some of that going and then we...______ we had to ______ with the instruments and everything, so that the man was in the lead, he was part of the whole thing.

Bernard Howard:

Of course, now you see this as a commercial on television then. It shows this as a training device.

Harold Skramstad:

Yes, oh it's used now, right. All of the early flight trainers around Washington, all of them are digital now.

Bernard Howard:

Yeah, the ones out at Eastern...The digital can make you sick just as fast as the analogs. [Laughter]

Tropp:

Well, did you get involved--out of the original work, the wartime work, did you get involved at all in computers for guidance purposes aboard the missiles? This was going on at, say, Northrup, the Snark Project--their problem of getting instrumentation...(Not clear)

Harold Skramstad:

Yes, we were developing various gyroscopic devices and this type of thing as, you might say, the autopilot of the missile, this type of thing, and then we would use that in our simulation. We would make a simulated model or actually use the autopilot itself and then feed that into an analog computer which would take care of the aerodynamics of the airplane and then eventually feed back into this

autopilot so we had the whole loop simulated that way.

Tropp:

Well then, this Washington simulation problem is really exciting. Bernie told me about it yesterday and how would that evolve and what would it eventually result in?

Harold Skramstad:

Well, this is the project for Air Traffic Control...

Tropp:

Right.

Harold Skramstad:

How it evolved. Well we would...

Bernard Howard:

Washington National. That was before Dulles was conceived.

Harold Skramstad:

Yes, this was where we used the old SEAC as the computer that we worked with, with the old acoustic delay-line memories and everything and we actually--of course that was a serial type memory and we would take these digital numbers that come out of the memory and connect them to digital-to-analog converters, so that in that way then the analog voltages would be used to drive the instruments and the scope and present quantities on the scope, showing the positions of the aircraft; and then we would be able to calculate in the computer imminent collisions, this type of thing, when two aircrafts were coming we could predict what their flight was going to be a certain distance ahead and be able to set up warning devices, this type of thing.

Bernard Howard:

Say, I sort of forgot, what was the ...can you recall what was the status in those days of D to A and A to D converters? Which was the problem?

Harold Skramstad:

There were all kinds of them there of various types.

Bernard Howard:

But in general which was the...

Harold Skramstad:

Well actually, the analog to digital is the easiest--I mean the digital to analog is the easiest and the analog to digital was usually by digital to analog in a feedback loop essentially is the way it was done. You got a little error voltage and you had a counter that would count either up or down in the early ones at a very rapid rate for anything and then you just take this digital voltage, convert it to analog and compare it with your analog input, and if there's a little difference then the digital counter would count up and down fast until you got equilibrium, and in this way the analog voltage would be converted to digital. This is fine if you're following a continuous voltage that's varying continuously with the time, because then you don't have to at each instant of time start over again and do a complete conversion from scratch and find the more significant the next guess.

Bernard Howard:

[Not clear]

Harold Skramstad:

You just had to be able to slew as fast as the analog voltage and digital had to move that fast, so that when it only moved a little tiny bit from the digital computer then you had to count a little bit up or a

little bit down in order to file. This worked fine but if your analog voltage--if you're going to shuffle a large number of analog voltages one after another, why then this is not the proper attack because you couldn't slew fast enough to switch immediately from one voltage up to another and so this is the reason that they went over to the other kind which essentially compared the most significant digit and then compared--made the next comparison and then switches and so forth.

Tropp:

Although it's discrete it looks like a continuum?

Harold Skramstad:

Yes, it looks like--it actually takes a fairly decent amount of time to reach conversion, but it doesn't matter between successive conversions whether the analog voltage was very close to what it was before or whether it's miles away. It does it at the same speed.

Tropp:

'Cause Whirlwind I think was the first large-scale computer to be able to operate in real time.

Harold Skramstad:

Well, it did the best of those that were there. It seemed to me there was another one developed at--wasn't it at the Moore School, that was trying to do the same thing?

Tropp:

EDSAC?

Harold Skramstad:

No...

Bernard Howard:

But you can't--I think that's too sweeping a statement to say--it depends upon the problem.

Tropp:

Well, I was thinking in terms of the thing that Whirlwind eventually evolved with and that's the ___... [Several voices at once]

Harold Skramstad:

That eventually did. I've seen how this whole simulation field gradually transferred from the analog step by step until it's practically pure digital these days.

Tropp:

And if what Bernie says is right, it's going to sweep back again.

Bernard Howard:

Not in terms of the device that's used but in terms of the concept. See, how do you solve ordinary differential equations on a digital computer? Well, you do it by some form of quadrature. You don't replace the derivative by finite difference approximations. That's not--that's a noisy process, you see, differentiation is a noisy process and instead you do it by quadrature where you--which is a smoothing process. Right integration is a smoothing process and I think that almost any of the standard schemes for integrating an ordinary differential equation in a digital computer can achieve greater accuracy than an analog, with very little trouble. See this...let's go back a little bit. The original advantage that the analog computer had was in terms of frequency response and the frequencies in command--it was in terms of speed. Intrinsically, the analog was intrinsically a real time device, whereas in the early days the digital, using the components that were then available, simply couldn't, you know, it had to chop up time in big enough chunks, you see, so that it just couldn't catch up. It couldn't do its calculation of the feed and the time in time peak and consequently the analog had this advantage, but then Von Neumann made this obvious comparison, you know, when he started comparing he said, "Well, hell, in terms of

accuracy you see, the analog is a measurement device and it is intrinsically limited by the one part in a thousand or perhaps one part in ten thousand at the most; whereas the digital computer, anytime you want to get one more order of accuracy you just start tacking it on and putting one more place in your computer and as a matter of fact you can make a little calculation and the cost is only going up logarithmically, see, which isn't very fast."

So, he said that there just is no question at all that the analog in the long race just doesn't have a chance. And of course, he was right and so just as soon as the digital technology speeded up a little bit we now have a situation, see, that the analog is still with its one part in a thousand or ten thousand, 10^3 or 10^4, whereas any old computer with seven or eight decimals, you know, the IBM 32-bit word or the UNIVAC 36-bit word or whatever - 36-bit word, eight significant figures (if you want to cut a scheme) --well that means that the digital _________ for instance, you don't have to take much of a step size where before your--your truncation error has been reduced to the level of a round-off error and you're working around an order of accuracy of 10^8 as opposed to 10^3 or 10^4 in the analog.

Tropp:

I think your description earlier, about the battle was just going to naturally resolve itself, turned out to be exactly what did happen. How did you get into the computer field, Skram?

Harold Skramstad:

[Laughter] By necessity, I guess. During World War II we had to develop these missiles and the only way we could really develop guidance systems for missiles was to simulate the

aerodynamics of the missile in the laboratory with equipment, and by closing the loop between the actual hardware through the analog computer which represented the aerodynamic characteristics, could we study whether the thing was going to be stable or not.

Tropp:

Well you know it's interesting when you think of our limited computational ability in that time period. I'm impressed that anybody would even try it.

Bernard Howard:

There was a war to be won, you know and it's amazing--who was it, Patton...?

Sally Skramstad:

Well wasn't that the first guided missle ever used in combat?

Harold Skramstad:

Yeah, that...we worked on...

Tropp:

Which one was this?

Harold Skramstad:

This was the (FAT?)... There is a ____ at the Smithsonian...

Bernard Howard:

You mean this preceded the German V-1, buzz bombs, and so on?

Harold Skramstad:

Well...

Tropp:

The V-1 wasn't...

Bernard Howard:

You're talking about... [Not clear]

Harold Skramstad:

This is--I guess you'd say it's the first homing missile that actually homed in on the...

Tropp:

The V-1 wasn't a guided missile.

Harold Skramstad:

No. The V-1 was just...

Bernard Howard:

Inertial.

Harold Skramstad:

Yeah. The V-2 used open loop rather than closed. This was actually closed loop. Open loop and closed loop.

Bernard Howard:

You know, the other day that you were saying that somebody had presented a diagram connecting various contributions and they had Wiener and his prediction theory and all off in the corner not pointing to anything--but listen, all through this conversation has been that idea of closing the loop and that was Wiener.

Tropp:

Wiener's prediction theory turns out to be a very important thing. Even though no one knows precisely where it fits in, everybody knew about it. You know, it was an important part of the environment.

Harold Skramstad:

Mm-hm. All of us working...

Tropp:

I was talking about a diagram that George Stibitz did for me and what he was saying when I asked him for the important intellectual occurrences. He said, "Well, I can't show you on these Bell relay machines where we ever use these concepts, but here is an event that's very, very important which should be (developed?) as that's why it's there. The reason it doesn't come down, as you say, closing the loop is because it had, as he could see, no direct impact on the relay machines that were later delivered by Bell Laboratories to the various government installations. But it's in the literature and it affected all kinds of thinking in a lot of work that was going on.

Bernard Howard:

There have been a number of dichotomies, you know, analog versus digital, open...

Harold Skramstad:

I think some of the work I did when we first--right after World War II when we first moved our laboratories, we actually did the original work on which the DDA was built at that time. We actually--you know, the DDA has essentially a separate digital integrator to represent every integrator in the analog. There was a little digital unit called an integrator and they passed pulses between these and they all operated parallel. That's the way the parallel DDA works. So the very first parallel DDA was actually, built at the Bureau of Standards when we first moved out to California and I remember Max Palevsky, who's one of the early men in building the DDA, visited me in Washington when we were doing this work and he was interested in the parallel DDA and then that was the very thing he worked on when he went to Packard-Bell and at Packard-Bell Computer he came out with the TRICE, which

was one of the very first parallel DDA's.

Tropp:

I know about MADDIDA...[both voices here]...which grew out of the DDA.

Harold Skramstad:

The MADDIDA is a serial DDA because it had a separate track on the drum for each integrator, but it--and they were serviced in series, one after another, but the TRICE is the parallel DAA where you actually, have a little electronic package which is a digital package which would represent an integrator, a multiplier, assembler--there would be a one-to-one correspondence between the digital devices and an analog computer. But instead of passing electrical voltages between components they'd pass simple pulse circuits to represent transfers of bits between them and the TRICE actually used quite a bit. One of the problems was that in order to make this thing economically feasible you had to use in these digital integrators of which you had to have as many as there were integrators in an analog computer--you had to use a simple integration formula like the rectangular rule or the trapezoidal rule or something that wouldn't...had considerable error; and if you wanted to use something that was a lot more sophisticated integration formula, then the equipment got extremely complex and expensive and this was one of the real difficulties in this parallel DDA. But actually, that was one of the first things that we worked on at the Bureau of Standards.

Tropp:

What's the time frame on that?

Harold Skramstad:

The time frame was right after we...that's '51, '52.

Tropp:

'51. Yeah, MADDIDA was about--MADDIDA was demonstrated, I think, at one of the early ACM meetings, I think in '49 at Rutgers.

Harold Skramstad:

Yeah, right, and I was one of the proponents of this parallel DDA for a long time because it was the one that--it seemed to me if we could only get the cost of the hardware down so we could have a separate piece of hardware to represent every integrator and summer in the analog computer, this would be the way to do it. You get the speed and we got the speed. The TRICE that...

Tropp:

How do spell the TRICE? T-R...

Harold Skramstad:

T-R-I-C-E.

Tropp:

Do you remember what acronym it stood for or was it just...?

Harold Skramstad:

Gee, I don't remember the acronym now.

Tropp:

The reason I asked you is because I got a very excited call last week from this research associated who's out on the West Coast who she had just interviewed Polaski. Very excited...

Harold Skramstad:

Polaski is the one that built the TRICE and he's the man who could give you all the details.

Tropp:

She missed that. I'll have to make sure that she goes back and asks him about it now, because that's a brand new machine in the early period. I hadn't heard about it before.

Harold Skramstad:

Oh yeah, that's one of the--then there were other companies that tried to make parallel DDA's too, but the economics of the thing never quite worked out and then of course the other circuitry got fast enough. I still think there might be a possibility now that we have these, you know, integrated circuits.

Tropp:

Chips.

Harold Skramstad:

Ships that you could get together--a chip that would be an analog integrator and...

Tropp:

Maybe this will be realizable in the ______.

Bernard Howard:

I think in a way this is another dichotomy that may--could become swinging around, you know. There was the special purpose versus general purpose computer concept and it was not well--I mean after all if you build a computer to do a certain job, it's going to do it better than a computer which is built to do any old job, you see, and of course there are places for both but for a specific job a special purpose has got to be better. Now of course we have all kinds of general purpose--almost everything is general purpose. But you--this scheme we're seeing will come back around to the special purpose.

Tropp:

Somebody defined ILLIAC IV to me as a special purpose computer. Now I don't know ____ that well but I understood that it was sort of a super stretch or something, you know, if it had been running would handle...

Harold Skramstad:

Oh, you mean this latest--you mean this latest one? The latest one.

Bernard Howard:

It could handle all by itself all the computing that is presently being done in the world.

Harold Skramstad:

This is a parallel--this comes back to the same thing I've been talking before. The way to get speed is by parallelism and it seemed to me the digital computer field through all the early days never could seem to quite comprehend this idea of parallelism.

Tropp:

The thing that [these three words not clear] Von Neuman did at the Institute was to build a parallel machine, a pilot project at the University of Toronto that never went to a production model, in '49, was an attempt to do a parallel machine.

Harold Skramstad:

Well, this new ILLIAC is a parallel machine because it's doing--you see, when you're going through a program there are a lot of things that don't require branches when you're programming a thing, so that if you're just doing the same operations or a whole larger number of things without a branch, there is no reason why all that can't be done all at once in separate things, because there's no feedback from one to the other. It's only when you come to a decision that it slows you down.

Tropp:

I'd like to just throw an interesting question at you. Now in the early period one of the first computational devices that introduced a lot of people to computing was the CPC, especially on the West Coast, and when IBM...

Harold Skramstad:

D.C. I…

Tropp:

When IBM was contemplating building its first machine, the Defense Calculator that became the 701, there were computers--18 of them that were built in that environment. Apparently, there was a big hassle within the company as to what kind of a machine they would build and one fraction which lost out was a group that wanted to build a large CPC-type computer, which they didn't do and one of the reasons they didn't do it was because they wanted something that ordinary people could handle, that didn't take brilliant people which apparently, they felt the CPC-type machine required. Well, one of the men on that who advocated the CPC of course was one its originators, Rex Rice. Rex complains about the computing environment that has ensued and still isn't happy about the original decision and there's a machine that was delivered at Iowa State that you might be familiar with, called SYMBOL, and he claims that that is a contemporary realization of some of the ideas that he had back in the--that we're talking about, 1951, where a lot of what we now call software has been moved back into the hardware of the machine, which is a very different thing than what happened for better than 20 years.

Harold Skramstad:

Oh, yes.

Tropp:

Now whether there's ever going to be more than one of those I don't know. Whether that's a wave of things to come...

Harold Skramstad:

Hardware is getting more economical with these integrated circuits in that I think we will see a lot of things done in hardware that were previously done in software.

Tropp:

Of course, part of the reason that he has for this shift is the fact that many people don't really understand in the problems they're trying to solve, what's happening in the machine because they just spend so much time moving information back and forth from primary to secondary memory and shifting it around, whereas if they had to understand the way the machine operates, he thinks they could do a more comprehensive job of solving a particular problem. Of course, another approach has been the one I think I mentioned the other day and that's this idea of software engineering, the (Bauer concept?), which is an attempt to get people to think more about what it is in terms of the problem they're solving. Although I don't really understand too much of that development; I haven't seen much, but a lot of the early questions or problems are still around. That's what we were saying in the very beginning.

Bernard Howard:

They are indeed. Well, I mean one of the most primitive ones in terms of analog computation is, how do you multiply? Nobody's ever come up with a satisfactory multiplier. What is even more important is no one has come up with a function generator in two dimensions.

Harold Skramstad:

They've come up with them but...

Bernard Howard:

[Not clear]

Harold Skramstad:

They do it by approximating functions by a series of straight lines.

Bernard Howard:

You pick yourself up by the seat of the pants and, you know, all the things--well, you know what those series of straight lines is, that's really a linear spline and there are quite a few of the basic problems that have as a hypothesis that the particular functions involved are (fa-si-do?), right. I mean they are not only continuous but have continuous first and second derivatives which the linear spline certainly doesn't have and if we're going to do anything at all we can at least go to the cubic spline. Going to have some kind of a function generator in that. In one variable a cubic spline or in two variables a bicubic spline. There is a surface, either involving a curve--the cubic spline is a curve and the bicubic spline is a surface which is readily generated, and which is a fa-si-do. I mean it will satisfy the requirements of all kinds of problems. For instance, you want to know the Grange equations and calculate some variations, you see, and assume that particular function applies to the ____, you know, if the objective interval you're trying to minimize is of C^2. So, the linear splines--there are some fundamental difficulties because if you start putting those things in you get...you get reflections, for instance, at the joints. All kinds of mysterious things happen. You know, what the devil is going on?

Tropp:

Well, you're suggesting another thing that hasn't happened yet and that's the fact that up until today a lot of mathematicians have not paid any attention to the existence of computers. When you start talking about... [general uproar and laughter]

Bernard Howard:

And you know we're getting into some really fundamental ideas; it seems to me.

Tropp:

Right. This was true of the early days of computers.

Bernard Howard:

Where do we start? Right down at the most fundamental hypothesis, the entire logical structure of mathematics. What is the most primitive? Why, we start with the concept of a...the undefined concept of a set, you know, ______ and so on, the first semigroup, semigroups, semigroups, always semigroups...[Laughter] and, you know, start building up from there...

Tropp:

Generate larger and larger structures.

Bernard Howard:

Yeah, but some of the most primitive, some of the most basic, some the most elementary concepts underlying all of our whole structure of mathematics--the associative, the commutative laws and all of our lavish and--whatever, you know, whatever our binary operations are--associative commutative and, what do we do? We build some computers that are supposed to do arithmetic, you see, to solve problems in the realm of that type of mathematical structure and what do we have? Right at the foundation of the whole damn thing is the computational arithmetic. Arithmetic on a computer is neither commutative nor associative nor anything else! [Laughter] Not even closed! So really what we should do is our whole generation of--I mean, the whole mathematical team ought to work on developing--on putting a proper logical foundation under the things that computers are actually doing today.

Tropp:

[Several voices at once] As Ed Berkeley showed with that little machine of his back in 1949--here's a machine that does nothing. [Several voices]

Harold Skramstad:

The only logic that a computer really knows is to simulate an and or an or game. That's all it really does. It just hooks them up.

Tropp:

It took a while to realize that. [Laughter] Surprising how long it took.

Bernard Howard:

It comes as a tremendous shock, you know, here we learned from the first grade on up that the way to check the grocery list, you know, is when you add it down is to add it up and you got the same answer both ways. On a computer you don't, you know, it's, you know, you use it is order to obtain maximum accuracy and you add from the smallest to the largest and that's it and it's not good, you know. Addition is simply not reversible and that's a terrible disadvantage. You know--how--I don't know how we can use computers at all in any rational way. Of course, we do in a practical way but still the most fundamental concepts, postulates, or axioms are violated.

Harold Skramstad:

_____ invented a special type of DDA combining analog and digital, presented at the Eastern Joint Computer Conference in Boston in '58, which represented quantities as a combination of a digital number plus an analog vernier, as you might call it.

Bernard Howard:

Yes, I remember that.

Harold Skramstad:

Well, for instance if you--between each increment in the digital you would divide that increment into an analog voltage, it would run the full scale of the analog so that... For instance, if you were going to have something that had a part in a million, say, it would require--a digital computer to do this would require, say, twenty binary bits to represent a million, something like this, but if you used this other type of thing, this combined analog-digital, you would divide this million only into a thousand parts and each of these thousand increments would be represented by an analog voltage that would go from zero to a hundred, so the analog is a vernier--you might think of it this way: if you're going to count to a million, when you count the first thousand on the digital all you would be doings is running the voltage from zero up to a hundred, say, and then when you start on the next thousand, all you do is just make a little mark on the digital and start the analog voltage up on that.

Bernard Howard:

All the digital is doing is handling the exponent and the analog is handling the fractional part.

Harold Skramstad:

The fractional part, right. This is the idea and it--I worked out the theory of the thing and presented it at the Computer Conference and this was picked up by one of the students at the University of Arizona working for Dr. Korn and he made a Ph.D. thesis out of it by building one of these and it works and it's never been used as being economically feasible, type of thing, but the theory of the thing is perfectly good and then this fellow--I forget his name now--at Arizona took my paper and built a piece of hardware in order to demonstrate it and got a Ph.D. thesis out of it.

Tropp:

I'm going to be there in a couple of weeks to give a talk and I'll ask who he was. [Laughter]

Harold Skramstad:

Wait--Wait is the man's name. He's teaching there now. John Wait, W-a-i-t. He worked under Granino Korn and you know him--know of him, yes.

Tropp:

[Not clear]

Harold Skramstad:

Yes.

Tropp:

Well, it's interesting how the same arguments keep coming back and back and back, because the arguments you are articulating were there at the beginning. At one of the SHER meetings that I mentioned earlier a big battle developed over the feasibility of hybrid computers and the main proponent was a gentleman from General Motors, which has a great need for this kind of thing, but has yet to demonstrate that the cost is worthwhile--the cost of building a large hybrid computer for their needs and yet--a combination of the two.

Harold Skramstad:

The field in which the hybrid--it seems to me there are two areas in which the hybrid is really useful. One is in the case that we mentioned a little earlier where you could represent the very slowly varying quantities on the analog; and this is a place where you can include the simulation of the whole system including the high frequencies and the low frequencies on one device, where if you go to a purely digital computer to do this, that's fast enough to do all of it on the digital, it would be just too big a job. You wouldn't be able to run it fast enough to actually do it. And the other field for the hybrid, I think, is really the, well, the partial differential equation field that we talked about, you know, where you split up

and cut down the dimensions of your program by one by having an analog computer which has time as the independent variable, or just reduce the whole thing by one dimension.

Tropp:

Of course, again the technology may be the determining factor of the next decade. If magnetic bubbles I mentioned become feasible it may turn out that you can pack so much storage in so small a space and do the same with the hardware--then it won't make any difference. [Several voices at once]

Harold Skramstad:

You'll find Dr. Korn is originally the expert on analog. Then he got into digital and now he's of the opinion essentially that there isn't anything hardly left for the analog at this stage. The digital has just about overtaken it.

Tropp:

Well, at this point in time I don't think that's true. Five years from now one of us will be proved right or wrong, but at this point in time I know there are still places for the analog.

Harold Skramstad:

Yeah, but I mean their field of application has been shrinking. The days of the great big analog facilities or the hybrid--that's been gone for...

Bernard Howard:

You know, we keep coming around again, you know, as technology changes while it will bring one thing to the fore and then another thing to the fore, the thing that keeps coming back to my mind as this discussion went forward was, what is light? Do we consider light as a discrete phenomenon or do we consider it as a continuous phenomenon? Now there were, at various times in history, there have been serious arguments about what it really is, you see, but what is more important is what is the most

convenient way of handling it in order to understand and to use, to predict, and to apply. For some applications it's more convenient to consider light discretely (or bounce). For some applications it's more convenient to consider it continuously, right?

Tropp:

I think as you look at most physicists the ordinary practicing physicist, he probably spends more time in the Newtonian universe than he does in the (Aristinain?) because, as you said, it's more convenient. You know, it works in so many cases and it doesn't make any difference if that particular concept has been supplanted theoretically or not. It does work; and you live in the universe that works most conveniently for you, whether its abstractly or practically and you can make all the major breakthroughs you want to but people do not shift their paradigm until it's convenient to do so.

Bernard Howard:

The fundamental reality of the things is, is the universe discrete or continuous. Who knows--who cares?

Tropp:

It doesn't make any difference.

Bernard Howard:

As we progress from idea to idea, as our understanding develops, at one time it may be more convenient to handle it one way and another time, another way. I think the same thing has been true of the past with computation and will be true in the future. It will be more convenient at one time to handle things discretely and at another time continuously.

Tropp:

Well, back to this idea of the mathematician in the computer environment. Here in the Mathematics Department which has its share, I'm sure, of bright young people--how many of them have any concept

of the environment, the potential of the computer for other than straight computational purposes?

Bernard Howard:

Well, it's most amazing but I hardly ever see a math student. But you see, you think of the computer as being basically a mathematical machine. Frank Murray's early book on the development--he was another man on the advisory board, Francis Murray, and his book on mathematical machines--but the surprising things is that mathematicians are by and large rather remote from computers. They don't care for them, they don't have anything to do with them.

Tropp:

That's been true from the very beginning. Harvard and Princeton were most unlikely places in the world for computer development. The only mathematician at Harvard who had any interest in the problems was Berkhoff; and nobody at Princeton other than Von Neumann.

Bernard Howard:

Well, that's enough. The computers, I think, have been the result of a marriage between basically the mathematicians and electrical engineers, in spite of what we've been saying. It's not the bulk of mathematics. It's only a few mathematicians here and there. Every time you see a worthwhile computer development you see one good mathematician and one good electrical engineer.

Tropp:

O.K. let me throw another area of controversy at you. Talking to Morris (Welts?) a few months ago about the early days at Cambridge, development of EDSAC there. He commented, as he, in England and in the United States which he was visiting fairly frequently then, about what he called the tension between mathematicians and engineers on these computer projects. He said although it's not as strong today as it was then, it's still around. Now what he was referring to, I think, was essentially the very

bright mathematician who came up with a concept and said, now I've told you how this will work--now make it work!--and couldn't understand why it took the engineer so long to transfer this concept to a reality. Do you want to take off on that; either reject what he said or...?

Bernard Howard:

I'd like to come up with a non sequitur because I don't know why, but something you said kicked me back to something that was said a little bit earlier in terms of parallel computation. Did Aiken talk to you at all about modular arithmetic?

Tropp:

Yeah, he showed me some examples of the use of modular arithmetic in various computational--in designing computational elements.

Bernard Howard:

Well, I think in a sense this veers on your question of mathematics versus engineering.

Tropp:

Well, Aiken was really a physicist, although he had had a lot of training in electrical engineering and lot of experience in electrical engineering in a power plant, engineering before he came back to Harvard to get his degree. But did you sense this in the early days? Did you run into this al all?

Bernard Howard:

Controversy between mathematicians and electrical engineers?

Tropp:

No, I mean tension and so on.

Bernard Howard:

Tension.

Harold Skramstad:

The main thing I ran into in the early days is the fact that the digital people and the analog people really had no dialogue, no...

Bernard Howard:

Oh, that was a...

Harold Skramstad:

That was a real thing, I'll tell you. The people that worked in the analog field, most of them didn't understand digital computers and the people that worked in the digital field just didn't want to learn anything about analog computers. This was a real bad thing, the communication.

Bernard Howard:

In looking at one of the very early--noise. There were a lot of arguments.

Harold Skramstad:

A lot of arguments but no real communication between them.

Tropp:

Except the digital people with no machines and the analog people with lots of machines, whether they were in conflict or not, whether they spoke to each other or not--the digital people seemed to have control, because report after report in the early period when they were suggesting various alternatives, analog is out. Digital is the only way to go. Relay machines are out because they're too slow, therefore electronic is the only way to go. So, both of these are being rejected instantly. On example is a report that I have in the car right now, from John Curtiss to the Census Bureau (or to some group) on the proposed mathematical computational laboratory at the Bureau of Standards. This is the only way to go.

As I said, analog is out; relay is out. Digital; electronic. This is 1946.

Bernard Howard:

When you're talking about the Census Bureau and so on, they're counting things.

Tropp:

This was a general report, so this was a prospectus. I was thinking of something elese. This was just general prospectus on the formation--the proposed formation of a national computational laboratory at the National Bureau of Standards. The census was only one of the things they were going to serve.

Harold Skramstad:

At the Bureau of Standards there was no communication between the people that I was with at that time, the analog people, and the people working with the...we worked at different parts of the grounds. We'd see each other and argue once in a while but never really found out in detail what the others were doing.

Tropp:

Did you have much contact with Bush during that early period?

Harold Skramstad:

No, not with Bush.

Tropp:

I just wondered if you knew some of his feelings on this and this area.

Harold Skramstad:

No, not w…

JWM:

Yes. One of the things that I had glossed over was that in 1930, I guess, 1929 or 1930, I had earned some money during the summer working at the Bureau of Standards, and the first year I worked at the

mechanical laboratories calibrating water current years and testing fire extinguishers. We'd set a fire in an old building, we had for the purpose, and then see whether this soda acid extinguisher would last for two minutes or whatever the standard time was supposed to be. If it did, it was okay, and if it didn't, it was no good, and things of that sort.

We also tested numbering machines such as the Post Office uses. Some of them will stamp the number changing each time, and some of them essentially have a binary counter in them, and stamps the same number twice and then changes, and then stamps the next number twice and then changes. They had tests of those going on for the Post Office Department.

I did mechanical miscellaneous type testing one year. The next year I worked with people in the wind tunnels. My boss was a fellow named Hugh Dryden, who just recently died. They had some of the most advanced equipment there for aerodynamics testing. They had a 3-foot wind tunnel, I think, that would get up to perhaps l00 mph. There was one of them they could run up to l80 mph, something of that sort. They had a little jet some place which worked from a compressed air tank, where just for a few seconds, you could momentarily get very small jets of air which were practically the speed of sound. I never used that, but all of these wind tunnel things were very interesting.

The work we were doing mainly was with, what was called a hot wire anemometer, which attempted to measure the rapid fluctuations in air velocity close to the surface, boundary layer effect, and you get periodic oscillations in this.

We measured these with a hot wire which would be cooled more when the air velocity passing through it was high. Then you can measure the resistance of that wire which had changed with its temperature, and you would infer then what the oscillations in air velocity were near the surface.

I forget whether I spent one summer or two summers in that wind tunnel, but at any rate I was what at

that time called a junior physicist or something or that sort, and I earned like $2400 a year. When I got my PhD, I went to take the Civil Service exam again, because I didn't know what the chances might be as to what jobs I'd get, and there were no offers at all for senior physicist or whatever I was called. Supposedly, you were able to earn $3200 a year or more as a senior physicist with a PhD

After some years at Ursinus, with the depression only easing gradually, I finally got a query from the Civil Service roles, asking if I would be interested in applying for a job if offered, all the ifs, at Ft. Monmouth or someplace like that, Signal Corps in New Jersey. It turned out that the times were such, you see that they could get PhD applicants who were eligible as senior physicists to take jobs as junior physicists at $2800 a year or something like that. It didn't seem very profitable to change from getting $2800 a year teaching 9 months a year, to getting $2800 a year in Civil Service, where I guess you got 30 days' vacation, working l1 months a year. So, I turned that one down.

UCM:

This was about the mid-thirties?

JWM:

That was somewhere, I don't know exactly when, but it was well after the time I had gotten my degree, the time when I was beginning to think about leaving Ursinus because the money wasn't too good, you know.

Finally, in l940, somewhere in that era, I began to think very seriously about the possibilities of building electronic computers. I was also thinking very seriously about how to make more money because of the question you asked a little while ago about the college supporting any of this, and the demands they were making on me. As I say, the only way they supported me was to pay me salary. I had no budget for any research, and no means for tools at all. I bought this $75 calculator out of my own pocket.

UCM:

So, there were two of you?

JWM:

There were two of us who were distinguished from the others by the fact that we had Ph.D.'s. I think some of the others had master's degrees in some subject, but there were two of us who were Ph.D.'s, which meant that we were further along in age and training and were accustomed to a higher starting salary maybe. In particular, I had a family. Dr. Burks, who was the other Ph.D., in mathematical logic, from the University of Michigan, was as yet unmarried, I believe, so maybe he didn't have as big a problem as I did. Wherever I would go for employment, I wanted to be sure I was making enough to take care of the family. I couldn't take any job just for the love of it. Obviously, it would be a little bit more difficult to take something in another city.

So I was very pleased to find that Dr. Chambers selected two Ph.D.'s to invite to be members of the Moore School staff, for teaching purposes primarily, on a temporary basis, that is, for the duration of the War, if nothing else, because at that time they were just then losing, I guess, two members of the staff to war efforts. One of the persons who either had left or was leaving, was Knox McIlwayne, who left to take charge of some manufacturing for the Napleton [?] Company in war work.

The other one, to my consternation, was Dr. Travis, who was that member of the faculty who was supposed to conduct the course in the design of computing devices which was offered in the catalogue. I had already learned, of course, that even though courses are in the catalogue, it was not necessarily given, and I guess I had tried to enter that and found out it wasn't offered at one time. At any rate it turned out that Dr. Travis was leaving for active duty with the Navy, and so the Moore School was temporarily short of teaching staff, and they looked to the two Ph.D.'s here as suitable candidates for

replacement during the war period.

Both Burks and I accepted the appointments as instructors on the Moore School staff, with the assurance by Dr. Chambers, that even though the pay was not great, it was always possible in an engineering school to supplement this by various other contracts on jobs which needed to be done. This indeed turned out to be the case, especially with the war developing. They began getting some contracts from the military agencies, such as the Signal Corps, and secondary contracts from the Radiation Laboratory at MIT, which was working on war contracts. It looked like there would be a buildup in their contract work in association with Ballistics Research Laboratory in Aberdeen. So, all in all, there would be opportunities to collect a small salary for the staff teaching work, but also some additional salary for working on contracts at the same time.

As I say, that turned out to be the case. Burks and I got employed on some contracts thereafter, so the salary that started out [loinary?] differential equations, how we solve them; and we set up some problems and they solve them on the analog, and they solve them on the digital. They go over and they make a face-space analysis on the analog computer because you get a rapid cut or approximation of the face-space __ology of the differential system on the analog and then get an idea what's going on; and then you decide, you see, what you want to run through the digital computer.

Tropp:

So, you do have them working in both environments?

Bernard Howard:

Absolutely.

Tropp:

I wonder how common this is. You talk to a lot of colleagues around the country.

Harold Skramstad:

I don't think that very many of the total student population, very much of a percentage of those that are exposed to computers are exposed to both. I think most of them are exposed to digital and a very small portion ever hears about analog.

Tropp:

I was going to say as I've looked through a variety of computer science programs, you know, various areas of exposure, I have not run across the phrase "analog computer" in any of the descriptions.

Harold Skramstad:

______ teaches a course in the Electrical Engineering Department...

Tropp:

I'm just saying, I just haven't...

Ruthie Howard:

Rizzio teaches...

Harold Skramstad:

Yes, Rizzio teaches a course in analog computing.

Bernard Howard:

All right, here's our '72-'73 bulletin. Look in the index under Computer Science, page 132, and here we find--here is the degree program in Computer Science, L. K. Skramstad, Ph.D., Director. Here is a list of courses that are involved. They include EE, Electrical Engineering 411-54, 513, 515; 515 is Analog Computer Theory.

Tropp:

Very good.

Bernard Howard:

But you see, now, that is--a person takes a 3-credit course from Professor H.C.O. in Electrical Engineering and that's the whole thing, but I talked about a math course that I give in solving differential equations in which I say, well, we spend a certain number of hours over in the analog laboratory and learn how to use that thing and solve some problems.

Tropp:

It really doesn't take very long just to learn how to...

Bernard Howard:

And they're extremely cooperative. They're delighted to have somebody--it's a university and if somebody comes in and wants to use the computer, it's fine.

Harold Skramstad:

Which course do you use it in?

Bernard Howard:

Numerical solution of ordinary differential equations.

Tropp:

When did your paths cross?

Bernard Howard:

1951, I should say.

Harold Skramstad:

Before that.

Ruthie Howard:

Before that, it was in Chicago.

Bernard Howard:

'51 was when I moved to Chicago. Come to think of it, I did encounter you during World War II when I went down to Washington to give a talk at the National Academy of Sciences at one those yearly meetings we had. I know about Curtiss and Skramstad in the National Bureau of Standards, you know...

Harold Skramstad:

Yeah, I think it was '50 or '51. That was when the Advisory Board...Have you heard of this Air Force Advisory Board on simulation?

Tropp:

No, I haven’t, and he started to talk a little bit about it the other day but didn't go into much detail.

Harold Skramstad:

This was set up by the Air Force with the headquarters at Wright Field. The idea was to get several people who had been in the simulation field and like to work in the field, and form a board to advise the Air Force in this general field of simulation.

Bernard Howard:

If you haven't heard of the Advisory Board you don't realize the role it played in the whole field of computer science. [Several voices and laughter] On the other hand, a story comes to mind about--what did our graduation address from the State of Vermont say--doing something or other is like painting a picture, not of Jell-O, but with it.

Tropp:

The first I heard of that Advisory Board was when you mentioned it the other day. You gave me one

those reports about fifty years old that turned out to be reproduced by Steiffel at the last ____ meeting.

Bernard Howard:

I just grabbed a few at random. That's only about two inches of the five-foot shelf.

Tropp:

The first one you gave me is "Dynamic System Studies - Analog Computation - Design of a Facility." WADC.

Harold Skramstad:

Wright Air Development Center.

Tropp:

About that time this Naval Ordnance Laboratory at Corona was part of the Burea of Standards.

Harold Skramstad:

It actually became--let's see, this was...

Tropp:

This was '56.

Harold Skramstad:

It was part of the Navy at that time.

Tropp:

These are all dynamic system studies from your simulation work?

Harold Skramstad:

Yeah.

Tropp:

That must bring back memories.

Harold Skramstad:

You bet you! Right. What's this last one--it's called "Conclusions and Recommendations". Well, well, well, I haven't seen that for a long time.

Tropp:

It's rather interesting that these all bear the same date. You don't have to worry about those conclusions and recommendations when I'm glancing through it. Those are all the lists of publications.

Harold Skramstad:

Here are the people that were on the Board: Dean Rodkey, Dean Draper from MIT, McDonald from Cook Research, Murray from Columbia, J.B. Rea, Siemens from MIT, Shank who was then Hughes....

Tropp:

And you form the Bureau of Standard.

Howard Skramstad:

___ from Naval Ordnance, ___ from RCA labs, and Oscar was the Project Engineer from Wright Field, and Bernie Howard was the secretary.

Tropp:

He told me he was the secretary of that group. Were you at the Naval Ordnance Laboratory when Atanasoff was trying to build a computer there?

Harold Skramstad:

Let's see, he was at the Naval Ordnance at White Oak, wasn't he, in Maryland? This was the California one and there was really no connection between the two.

Tropp:

Did you know anything about his work?

Harold Skramstad:

No, I really didn't.

Tropp:

Bernie. when you get around to xeroxing things, would xerox these early pages of the _____ including the recommendations, for me? In fact, maybe the whole report.

Bernard Howard:

I think it's extremely interesting. Of course, I haven't read it for years.

Tropp:

The bibliography, the variance reports, memoranda, and I guess it's a really central reference item for this whole period up through '56.

Bernard Howard:

You know the story of Riemann's doctoral dissertation, you know, two pages and that sort of thing--revolutionary period, complex variables.

Tropp:

Under Gauss, you mean?

Bernard Howard:

That's right. Well, that report, that Volume I, The Conclusions and Recommendations of the Advisory Board, I really worked like the devil on that thing and it's--you know--the sort of a situation where the fellow apologized that he didn't have time to write a shorter letter. Well, that one I tried to boil the whole thing down into a few enough pages so the General could read it. I know that the Air Force was accustomed to weighing reports, you see--as a matter of fact we hoped--Don (Dibutti?)--I think he's at Marquette now--he came up with a new scheme, Eigen value or something, problem--I've forgotten but

he's made some contributions. He invented a test, a two-part test. The Air Weapons Institute, you see, had a little aptitude test for prospective candidates and we throught that was king of ridiculous, so we countered it by inventing a little two-part test. Now part 1 of the test was, we would give the candidate one of these inch-thick one-pound Air Force reports, you see, and have him boil it down to one page and that was about 30% of the test. Now the other part of the test, which constituted about 70%, we would give him one page and he would expand it to a one-pound report. [Laughter]

Tropp:

This whole list of personnel I guess includes not only the Board but all the people who were involved in writing reports. It's a very complete list. There's a complete bibliography too, of all the different reports and progress reports are in here for a bi-institution. Not only Chicago but the Naval Ordnance Laboratory and MIT and Cook Research Laboratories and the ____.

Harold Skramstad:

Well here is this--we mentioned the parallel, the original parallel work--McCormick and ____ here, I think that would have to do with that.

Tropp:

January of '53. Evaluation of analog and digital computers for flight simulation. E. M. McCormick and L. P. Meisner. This a great thing to have. These are all broken down so the ones that you would have been involved in should be up here.

Harold Skramstad:

Two up there, yeah. I don't know that I wrote any of the report myself.

Tropp:

They must all ring bells.

Bernard Howard:

I can't tell you how excited I am to have somebody actually read that!

Tropp:

You know what I'm going to do? I decided I'm not going to wait for you to copy it--I think I'm going to steal it.

[END OF TAPE 1]

Bernard Howard:

Von Neumann and the security clearance. Different people test you for this and that, so the end of the game this psychologist was asking him a number of questions, you know, to make sure he was well-balanced or something. So, Von Neumann said, what's the matter with this interviewer? I have a theory, by the way, of the I.Q., measured I.Q. versus actual. So, the guy kept asking Von Neumann questions and Von Neumann kept answering. He realized what it was that the guy wanted and he'd answer it in some irrational--but it was rational, you see, except not what the guy was expecting and this fellow was just--I mean it was getting on his--so finally he said, "Professor Von Neumann, would you show me your hands?" And Von Neumann says, "Certainly." He was holding one hand and the hand down. [Laughter]

Measured I.Q. is, you see, assuming there is such a thing as an actual I.Q. as an independent variable, and now you go to work and measure it and so that's a dependent variable and you now want to see what kind of correlation there is. I assert that you start out with a 45^{o} line that measured equals actual for a while. Then you come to a point where there's a turn; the curve turns and it dips down, and it drops, and then it comes to a bottom and it bottoms out and it rises again, and then it comes down again. So, there's a series of oscillations, you know, as the measured I.Q. versus the actual I.Q. are both increased. Now

what is the cause of that first dip in the measured I.Q. versus the actual? What is that point then? Well, I assert that is the I.Q. of the examiner. See, the old Chinese thing, you know, you put your hands behind your back and you come out with even or odd and so on. Well, the first round of course the examiner is smarter than the guy being measured and so the guy being measured tries to figure out, now what is the answer to this thing, and puts it down and sometimes he gets it right and sometimes not, you know, in percentage. But then it comes to the point where the guy is smarter than the fellow who made up the questions, so he realizes the questions and then he thinks of certain answers that the guy that made up the question didn't think of; and so, he answers them really more intelligently, but they're marked wrong. Well, that goes for a while and then the guy gets smarter and then you take a person a little smarter yet, you see, and then he says, "Oh, well now, this is an interesting question. The answer is obviously so-and-so. Oh, wait a minute. Now that's not what the guy who made up the test had in mind. What he had in mind was this incorrect answer, so I'll put down the answer that the examiner had in mind so I'll get it marked right even though I know much better than that so then his score starts to rise again.

Tropp:

Do you want to react to that?

Bernard Howard:

Minsky says artificial intelligence. The computer is artificial intelligence, you know the idea that's behind this.

Tropp:

No, I'm not sure I know what you're talking about.

Bernard Howard:

Well, can a computer think? That's a subjective--it calls up subjective emotions and so on and we

emotionally react to it. Gosh, we're the only people that can think, because you know, by definition. But suppose you do the following little experiment. Sit yourself down at a teletype terminal. Now if you know that it's a Western Union terminal and you're connected to the next guy down the line, 20 miles away down the railroad, and you start to tap, and he taps back at you--you know there's a human being at the other end of the line and he's responding. If you sit yourself down at a teletype terminal in our ready room which you know is hired line connected to the computer, well you know that the computer is responding. Now suppose you were put down at a terminal and you say, "Well, there it is, now figure out who's at the other end. Is it a man or a computer?"

If it come to the point--you see right now there are certain tests that we could make and find out--but computers are getting more and more human all the time, you know; they're making more and more mistakes anyway, where you can't decide by the experiment whether it's a person or a machine at the other end. You know, it's typing back at you. Well, then how can you say that the computer isn't thinking? And Minsky has the idea that, well, whatever your view of the matter is, that you will agree--most reasonable people agree that computers are getting smarter, so to speak, and he says it's not reasonable to suppose that man can invent a machine which is almost as smart as he is and then stop. And that the last invention man will ever have to make is when he first puts a model of the computer into the computer, because then the computer will put its capabilities to work to improve itself and it will take off and we will have to resign ourselves to being the second most intelligent species on the planet. There you have it. And quite possibly the planet may be better off. We certainly haven't been doing very well with it. Our stewardship leaves much to be desired.

Tropp:

Yes, except I'd like to think that there will always be things that I can do better than a computer, no

matter how intelligent.

Bernard Howard:

I believe that that is true. Now for example, there are things that a little puppy dog can do that a flea can't do. I mean he can reach around back, you know, and nip a flea at the base of his tail and I can't do that.

Tropp:

That cat can do things that I'll never be able to do.

Bernard Howard:

It's not clear whether these abilities are worth anything or not.

Tropp:

Well, it's just a question of shifting values. That puppy dog has his set of values for that pretty important characteristic. Did you have much contact with Von Neumann?

Harold Skramstad:

No.

Tropp:

How about Wiener?

Harold Skramstad:

Not really, no.

Ruthie Howard:

Well, we did. We rode to school with him every morning.

Tropp:

With Norbert Wiener?

Ruthie Howard:

Sure, down Mass. Avenue

Tropp:

You were at MIT during that...

Ruthie Howard:

Yes. I was working at MIT at the time and going to school at night; and Bernie was working at MIT and working on the...well, I guess I don't know what you were doing.

Bernard Howard:

Classified. Now we can talk about it but then it was classified.

Ruthie Howard:

Bernie had the Radiation Lab and supporting me--trying to--but anyway Norbert Wiener used to--we all got on the Mass. Avenue trolley...

Bernard Howard:

At Harvard Square and it rolled our...

Ruthie Howard:

We know him very, very well because he like to have everyone know that he was on the trolley. Especially he loved to talk to anyone who spoke Chinese or--is this right?

Bernard Howard:

Well, yeah, the best thing he liked to do was to speak to people who only knew one or two words of Chinese, you know, to whom he knew a few more words than they did. He didn't want to speak to anyone who know Chinese well because...I can remember Chu came into tea once and Wiener was there holding forth and he was talking Chinese with Chu. Then Wiener left and Chu remained behind,

so, we asked Chu, "How was Professor Wiener's Chinese?" Chu was a very polite fellow and he said, "Yes." Well, we bored in, you know, and he said, "Well, it's not very good."

Tropp:

Which Chu is this?

Bernard Howard:

Lon Chen Chu. We called him Long John.

Ruthie Howard:

I know you have many Wiener stories.

Tropp:

I love to collect more of these.

Ruthie Howard:

You probably never get enough Wiener stories.

Tropp:

I never get enough Wiener stories, so if you have some, be my guest. We've got hours of tape.

Ruthie Howard:

Bernie has--did you tell them about your thought for the day?

Bernard Howard:

God, I forgot all about that.

Ruthie Howard:

Everyday Frank and Bernie put on the blackboard a thought for the day.

Bernard Howard:

This was our office right across the hall from ____ and we used to leave the door open--an air

conditioning system was something we didn't have--so in summer we'd leave the door open for ventilation and so on, you know, a window so that we'd get a cross draft; and by leaving the door open a bit to get the ventilation, there was this blackboard that was right there as you walked down the hall you could see the blackboard. It was not a very large one but consequently we couldn't use it for any of our classified calculations in many things and so there was a useless blackboard. And so just to make life a little less dull we started coming up with a thought for the day. In the morning we'd put on a thought for the day and all the little V-12 students would come by and they'd read it in between classes and so on, you know, going from one class to another.

We discovered that--Wiener never realized this, but the light was such that we could see his shadow. The door was opaque, you know, it had a big window in it with opaque glass, you see, so that you couldn't actually see, but you could see shadows. And so, Wiener's profile was unmistakable. We opened up shop at 8 o'clock in the morning with our thought for the day and about 8:15 or 8:30 or thereabouts, why Professor Wiener would come by very quietly and he'd stop outside the door and he'd read the thought for the day and to on his way very quietly. And then about 10 o'clock (this was, you see, in between classes, about 8:30 or something like that when nobody was around)--then about 10 o'clock he'd come by again, you know, right in between changes of classes when there'd be a great big group of V-12 students standing outside the door reading the thought for the day. He would come huffing and puffing, you know, and he'd say, "Well, let me see the thought for the day. Let me see--what is that? Oh, that's very good. Let me think now." And then he'd have a flash of inspiration which he'd thought about for an hour and he'd step in and he'd add his thought for the day below it and then he'd step back and he'd "Ho-ho-ho," and he'd look for approbation and everybody would... [Laughter] I think he really enjoyed it because here he was putting on this great act and we really

missed it days when he didn't come by, we really missed it.

Ruthie Howard:

Did you tell him about lambada?

Bernard Howard:

Yes, but I'm sure I quoted correctly. I was thinking I ought to write it down but I think it was something like this: "Professor Wiener had a lambda. From whence it came he did not know. For not everywhere that Wiener went Was the lambda was sure to go.

Ruthie Howard:

He was a wonderful person to have around because everyone at MIT appreciated him and he wanted to be appreciated.

Bernard Howard:

Oh, he needed it very much. He was, you know, brilliant as he was, a child prodigy or something, he just needed appreciation.

Tropp:

That shows in that two-volume autobiography.

Ruthie Howard:

[Not clear]

Tropp:

Have you read any of it?

Ruthie Howard:

No.

Tropp:

Very interesting pair of books. Was he as absent-minded as some of the apocryphal stories?

Bernard Howard:

Yes, he really was. I myself remember when he went down to a math meeting down at Providence. He drove down, you know, and he was at the math meeting and then afterwards he was talking mathematics with some colleagues and they all went to the station and got on the train and came back to Boston. The next morning, he went to his garage to get his automobile and it wasn’t there! So, he reported it stolen to the police and so on. They checked back on his activities for the last day or two and they discovered he had been in Providence. So, they contacted the Providence police and they discovered the car. I don't know whether they hot-wired him or whatever, but anyway they drove the car back, you know, and this all happened within a day, so in the evening Wiener came home and here was his car! They didn't tell him, so he just thought, well, the Cambridge police are just wonderful. In the course of a single day; it wasn't damaged or anything! [Laughter]

Ruthie Howard:

I can see him now. He used to walk--he had a very straight back and he walked very straight.

Bernard Howard:

His fee sort of pointed out this way.

Tropp:

Like a penguin?

Bernard Howard:

Yeah.

Ruthie Howard:

He was real (leader?). You know, when I would meet him in the corridor, I always spoke to him because we saw him in the streetcar and everything. Sometimes he would see you; sometimes he wouldn't. He had a very...

Bernard Howard:

Tunnel vision. He had poor eyesight and his corrective lenses were very thick so that his field of view was rather narrow.

Ruthie Howard:

But did you tell him what he did to Professor Baer?

Bernard Howard:

Oh, my goodness, that was the most unbelievable thing. This was in the late '40s and there was this math meeting--it was an international math meeting being held at the University of Illinois for the first time in 50 years or whatever. You know, there are that many places to hold math meetings that it never gets around but very seldom and when it does, why it's an important occasion. Before I get to the Wiener story I must tell you another one that was rather delightful. [Awkell?] Hall was one of the original buildings on the campus. It had been the original library so when they build a new library it went through several transformations and so there it was. One of the things was an elevator that went from the ground floor up to the lecture hall on the third floor. It went up past various remodeling and so on and there was this little cul-de-sac that was sort of in between floors, and I don't suppose it was more than 10 square feet. You know, just a little triangle thing and the elevator went past it and, you know, just a little sort of a shelf in between floors. It was sort of an open elevator and moved slowly so the graduate students thought, well, that doesn't look very good for all these visiting mathematicians to go

by and see this piece of space that's not being utilized effectively. They thought, well, we must do something about this. So, they got this rickety old table and smuggled it in and put it in there some way or another. How I don't know because the elevator was only if the faculty had teas, you know, or disabled people or something. I mean ordinary people had to just walk up and down the stairs.

They put in a rickety old table and a rickety old chair and a Chianti bottle with the wax dripping and an old ledger and an old quill pen and a rickety old ink bottle...

Tropp:

Real production!

Bernard Howard:

And what did they see as good, you know, I mean you see on every door the professor's name and everything, so they put Professor Beaumarquis. [Laughter] I tell you--absolutely priceless and every mathematician at the meeting would have just gone wild about it, but the sad thing is that the night before the meeting, members of the board were around, checking on last minute arrangements and making sure that all was well, and the University was presenting a proper image and so on. They went up and saw this thing and they said, "Well now, this is making fun..." and they had the janitor remove the whole damn mess.

Tropp:

That is so beautiful!

Bernard Howard:

Of course, it was absolutely priceless. It was right in the spirit. I mean it would have been one of the hits of the meeting.

Tropp:

Like the math meeting at Berkeley where they put on the marquee of one of the restaurants near the campus, "Algebra Spoken Here" [Laughter]

Bernard Howard:

But you see a lot of mathematicians did understand who Beaumarquis was thought [not clear]...

Tropp:

Does any intellectual area have an immortal person?

Bernard Howard:

I know, I know, it was just unbelievable. So at this meeting of course--gee, I'm not even sure whether it was that meeting or not. Maybe Wiener had come out to give a talk. Anyway he gave a talk and after the talk there was the usual introductions around and so on and Professor Reinhold Baer, algebraist, from Germany was very--is very--I think he's still alive--I have to be careful if he's still alive...

Tropp:

I'm sure he's still alive.

Bernard Howard:

Well, anyway, he gave the most beautiful lecture of anyone I've ever seen, absolutely precise--excellent algebraist and so on--but they were introduced by Professor ______ you see. "Professor B Wiener, Professor Baer," and so on. Baer had just as much ego as Wiener, but Wiener said, "Oh yes, Professor Baer. And what field are you in". There was another occasion--I can't remember whether it was Wiener again, or somebody else, but A.B. Coble, who was a rather famous mathematician, gave a party. The evening got well along and he was really enjoying his own party. So here he comes weaving up to whoever it was--let's say it was Wiener--I don't remember--and he said, "Professor Wiener, certainly

looking forward to your talk." [Laughter]

Tropp:

What are some anecdotes around the early Bureau days?

Bernard Howard:

I've heard you talk about so many of the different people in the Bureau--past and present.

Harold Skramstad:

I don't...

Tropp:

Start with Condon. What kind of a man was he?

Harold Skramstad:

Condon? He was often referred to as sort of a bull-in-the-china closet type. The Bureau of Standards was like (I've heard this expression)--was sort of like a delicate instrument, a lot of good research going on..

Tropp:

Precision watch.

Harold Skramstad:

Precision watch, the Bureau of Standards could be compared to a precision watch, but when Condon came in he just reorganized and changed things. A lot of people had this reference to sort of a bull in the china shop.

Bernard Howard:

You mean he used a hammer and a saw on the precision watch?

Tropp:

A marvelous talk he gave on "Research and Security," I think was the title of it, back in the late '40s, that I would love to reprint today. It's really a very exciting talk that Ernest Ryabec gave me a copy of. Do you remember him? Ryabec? R-y-a-b-e-c.

Harold Skramstad:

No, I don't remember him. No, I…he made a lot of changes there and I think a lot of them were pretty good, but he did stir things up quite a bit when he was there. He brought in a lot of people from the outside and he got a whole lot of new activities and decided to get rid of a lot of activities that had been going on there for years and years.

Tropp:

The big thing was bringing in setting up a National Computational Center.

Harold Skramstad:

Yes, that's right. He brought in John, didn't he?

Tropp:

He brought John in, that's right, and the whole Mathematical Tables Group. Did you have much contact with them?

Harold Skramstad:

Not very much, no, because at that time when Condon came, I was still in war work and we were sort of off by ourselves, so we didn't have too much contact.

Tropp:

Did you have much contact with Sam Alexander and Ralph Slutz?

Harold Skramstad:

Oh, yes, Ralph Slutz, sure. I worked for Sam; he was my boss for so many years.

Tropp:

Tell me about Sam. He's not around to talk to himself anymore. He's one of the important figures that I wish was still around.

Harold Skramstad:

He was a difficult man to work for.

Sally Skramstad:

He didn't get along with anyone.

Harold Skramstad:

Well, one of the reasons that I left the Bureau and went to California--to the Naval Ordnance Lab.

Sally Skramstad:

One of the reasons lots of people left the Bureau was because of Sam.

Tropp:

Were you involved in _______?

Sally Skramstad:

Never.

Tropp:

NCR project?

Harold Skramstad:

No, I wasn't involved in that. You see we were in two separate groups, but Margaret can give you the best story on Sam, I think, having worked with him for so long. Margaret Fox is your best source on that. If she ever lets her hair down, she usually tells you.

Tropp:

I can't imagine what it would be like if Margaret let her hair down anymore. She's so totally relaxed.

Sally Skramstad:

Is she still at the Bureau?

Tropp:

Yes. She's talking about retiring in about another year, but she's off on a round-the-world cruise at the moment, or a trip around the world.

Sally Skramstad:

We never see her anymore because we're so seldom in Washington.

Harold Skramstad:

Sally, don't you go to Washington any more in the winter, at least, because, God, I'll never forget how sick [Not clear]...

Sally Skramstad:

But it always seemed to me that Margaret was on tour...

Tropp:

I talked to her just before she left and that's been 3 or 4 weeks now. She was going around the world; she has a 30-day leave.

Sally Skramstad:

Have you talked to her at all?

Tropp:

Well, very informally. I can't seem to tie her down for a long enough period.

Sally Skramstad:

Have you seen Sam's wife?

Tropp:

No. You just started to tell me about her. Is she married to...?

Sally Skramstad:

She married Bob Huntoon and they're living in either Levittown near Kensington or Wheaton or someplace. You see, Bob had a house and his wife died of cancer and of course Sam died of cancer and so Bob called Ellie and asked about funeral arrangements, because I guess the Huntoon's were Unitarian so he didn't want to get into a lot of religious ceremony for the funeral and I think Ellie--I don't know, I guess she had a Jewish undertaker but somehow or other...but anyhow Bob started dating Ellie and they got together and it was just great.

Tropp:

Somebody told me, and it was again one of these places where I didn't know the speaker and I was on the periphery of a group, about--does he have a son?

Harold Skramstad:

Yes.

Tropp:

Somebody had mentioned in passing that they heard that his son was thinking of doing a book on Sam's life and accomplishments, and what have you, but do you know where his son in located? Because he'd be a good--if that's true--I don't know if that's--my contact would be Mrs. Huntoon--because if he's doing that then he would have probably papers and documents.

Harold Skramstad:

You mean, he's their son?

Sally Skramstad:

No, it was Sam and Ellie's son. No because Ellie and Bob have just been married, you see, in the last two or three years.

Harold Skramstad:

Oh well, I mean if it was Sam's son then it would be Ellie's son too--the present Mrs. Huntoon's. She might know where her son--oh sure, sure.

Sally Skramstad:

There is somebody, and I believe it's her daughter, who is in Fort Lauderdale, Ellie's daughter, and then they spend the winter in St. Pete--or at least Ellie and Bob...

Tropp:

He should be easy to locate.

Harold Skramstad:

Did you run into Al Leiner who worked for Sam?

Tropp:

No. How do you spell that?

Harold Skramstad:

L-e-i-n-e-r, Leiner, Allan Leiner. He and Sam couldn't hit it off together and he and his whole group, which really were the--they were the logical designers of the SEAC, his group really. Al Leiner and--who were the others in that group--Smith and Weinberger.

Tropp:

Was it C.V.L.?

Harold Skramstad:

No, not C.V.L. Smith. This is a differnet smith. He and his group left the Bureau of Standards as a group and went to IBM, with a considerable increase in salary, up to their research lab in New York someplace. I'm not sure that Al is there...

Sally Skramstad:

No, he's in Philadelphia now.

Tropp:

If he's in Philadelphia he shouldn't be hard to find either.

Sally Skramstad:

I have the address.

Tropp:

Al Leiner sort of headed up the original group that did most of the logical design under Sam, of the SEAC in the beginning.

Tropp:

I had the impression in talking to John this afternoon that Ralph Slutz had the heavy responsibility.

Harold Skramstad:

He did, very heavy responsibility.

Tropp:

In the whole design and rationale [not clear] of the SEAC, and that Sam was more the administrator.

Harold Skramstad:

I think that's right. Slutz was really one of the best men there; and then of course Al Leiner and Smith and Weinberger--I guess Bob Elmore was also involved and he's still there, I guess.

Tropp:

Yes, yes, he's still there. My only contact with them has been mail. I really can't tell you anymore. The project I'm on has been going about five years. I inherited some tapes. One of them was a tape of Al Leiner and I correspond with him, but I haven't had any direct contact with him. I almost want to do some of these things over again. I'm happy with the original, but as I was telling you...I can erase that ...[not clear]...

I want to ask you probably a very unfair question, Skram. As you look back, because you were involved early in the field and still are, what do you see as some major milestones, really important key advances or publications or developments, implementation of ideas? What do you see as some of the really major milestones that got us where we are today?

Harold Skramstad:

Just one little thing tacked on to another.

Tropp:

Well, you know there are some standard milestones and, I mean, just think of a few. Of course, the first machine, you know, was the MARK I at Harvard and ENIAC at the Moore School. I think of the Moore School Summer Session for example as being a major milestone, introducing people to what was going on there, in 1946. I think of the Harvard Symposium in much the same way. Things like Far Memory.

Harold Skramstad:

[Not clear] Then of course the development of these higher-level languages. It's just hard for me to try

to pick out any particular item.

Tropp:

In your own experience, who have been some of the giants that you've been associated with? People that you felt pushed the art in various areas.

Harold Skramstad:

Well, I think we've talked about a lot of them already.

Tropp:

A number of the, yes.

Harold Skramstad:

A very large number of them. Well, I think the original Bureau of Standards group did an awful lot, the group working with Sam and (visibly progressed?) in getting that SEAC going. That was a tremendous thing.

Tropp:

In terms of future research, in looking at the history of computation, if you will, at the Bureau from say 1938 to SEAC and then SEAC to the next generation, what do you think are some of the prime areas I ought to be focusing on? Or that whoever does this research ought to be focusing on, the things we ought to be looking for? Can you think of any help or assistance you can give me in that way?

Harold Skramstad:

[Not Clear]

Tropp:

That's an isolated project that I think is very important and nobody's bothered to do it yet, because this was one of the centers of computation for the United States and the idea of a mathematical central

computing laboratory made it a focal point for so much that was going on, even if it was only as a consultant to various other people, as well as a problem solver. What are some of the areas that you think that I ought to be looking at that I might otherwise overlook? I know that's very unfair, hard question.

Harold Skramstad:

Yes. I was just trying to think of things here. On the hardware side the revolution produced by the transistor replacing the vacuum tube is obviously...

Tropp:

I was thinking of really before the transistor. You were around at the Bureau during the period before SEAC and you were involved in computations and then you were involved later as well.

Harold Skramstad:

Hm-hm. But of course, in that period I was sort of forced into this by the war and having to simulate, so that really brought me into this development of analog computers and how they could be effectively used, this type of thing. So, at that period I really had very little contact with the digital field except to hear about it and talk about it. I know what was going on and especially going on at the Moore School but over-all I really wasn't close enough to it at that time.

Tropp:

Was SEAC you first introduction to digital?

Harold Skramstad:

My first introduction to the digital side, yes.

Tropp:

You hadn't been introduced through work at Harvard or the Bell

Lab work?

Harold Skramstad:

No, not at that time, you see, because when World War II came along, I was put over all this war work.

Sally Skramstad:

He stranded in aerodynamics.

Harold Skramstad:

I was in aerodynamics, you see, so that...

Tropp:

Because one of the big problems that the early digital machines had to cope with was the reduction of wind tunnel data and there was, you know, pressure for doing this was, I think, again one of the big influences for...

Harold Skramstad: It was obvious that Government money had an awful lot to do with pushing the art--it's wartime--because the various agencies--the Army, the Navy, and the Air Force--were all putting money into it during that period. I remember when--well, firing tables and things like this, that Aberdeen had big rooms full of clerks pounding on calculators.

Bernard Howard:

Just like the WPA tables.

Tropp:

Well, this was going on at the Moore School before ENIAC. The girls who eventually became the first programmers on ENIAC for the most part started off with desk calculators, doing ballistics tables.

Harold Skramstad:

Government money to do ballistics table job really financed an awful lot of development.

Tropp:

The Bell machines were developed for that same purpose, to do ballistics tables. There was, you know, a tremendous need.

Bernard Howard:

You know, something you said earlier, Skram, made you think of another dichotomy. You said you were involved in simulation and so on. That was what we described during the fifties as physical simulation versus mathematical simulation.

Harold Skramstad:

In the simulation field have you run into the early works of the New England Simulation Councils Incorporated?

Tropp:

No, not at all.

Harold Skramstad:

This is really the technical organization of the people in the simulation field right now. They're a member of ___, Simulation Councils Incorporated, a technical organization, and I think I've got the proceedings--the publications ever since it was begun way back in the early fifties.

Tropp:

It does go back that far?

Harold Skramstad:

It does go back that far. John McLeod was really the driving force behind getting [not clear]...

Tropp:

When they first started in the early fifties, what type of simulation problems were they interested in and

what kinds of equipment were thy using?

Harold Skramstad:

They were using almost entirely analog equipment.

Tropp:

So, it was primarily physical simulation?

Harold Skramstad:

Pretty much an analog computer group, really. It was a technical society for analog computers and it was the only and the main technical society for analog computers because there was the ACM, of course, which sprung up and that was all digital really, and this was the only organization for analog people. But Simulation Councils have grown with the times and now they're still in simulation but what they're doing now...

Bernard Howard:

Right now, of course, John McLeod and so on, is talking about world simulation, simulating the world. He was developing along his own ideas something that Forester has actually been doing, you know, at MIT with world dynamics and so forth, in the problems of thinking, why not get a group started interested in world simulation. Different term but saying, what the devil is going with the future of the world let's try to put a model and so on. And I'd like to correct a statement that you made a little bit earlier when Skram said something about analog computers and you said physical simulation. Now there's a great deal of difference between analog computers and what we call physical simulation. Physical simulation is Link trainers, for example, meaning you've got the physical device to behave, to simulate the real thing, which is rather different than having a mathematical model in the middle. See, if you have physical system A and you have physical system B and the two have a common mathematical

model, then you say system B is simulating system A and so on, because of the common mathematical model. They're both governed by the same set of ____. In a sense that's what we do when, well, when we were simulating a rocket to the moon and that sort of thing. We created a mathematical model, put it on a digital computer, and let the thing crank away and so on. One of the difficulties--the difficulty is particularly apparent when analog computers, when you've got a guy who is designing an airplane or whatever, something in the airplane business. He comes over to the analog laboratory and, well, simulate my deign and let's see what happens, and of course in general, the first time around disaster happens, and then the agony starts. The guy with his design says, "Well, gee, your hookup wasn't right. You didn't wire it up right," to the analog people. And the analog people say, "Of course we did. Your design is wrong. And then they have a knock-down drag-off battle to figure out which one is right and eventually it'll turn out they both have made mistakes and they sort of evolved. But the general idea is that when you simulate on either an analog or digital computer, mathematical simulation, you tend to think of the output in terms of the device that you have in your mind, that you're interested in; whereas what you're really doing is conducting an experiment upon a piece of laboratory apparatus and that experiment that you're conducting really has no direct connection with the thing that you have in mind at all. I mean it's only through your mind that the connection exists and that's where the mistake-makes often comes is that you fail to analyze the output of this apparatus, you see, as the result of an experiment upon it and only later do you infer by analogy what might happen to the other physical device that presumably has the same mathematical model and then of course the basic question always is whether the mathematical model is correct or not. Jay Stratton says in his book on electromagnetic theory in a beautiful phrase, he says that, "It is only the consistent agreement between prediction and observation that constitutes the true test of any theory."

Tropp:

That's an elegant phrase.

Bernard Howard:

Isn't that beautiful?

Tropp:

Yes, that's beautiful. You know, language gives us some interesting clues to developments. I mentioned to John Curtiss today that in a 1945 or '47 report (I think it's this 1946 report that I was referring to earlier--prospectus) that he uses the word "computer" still to mean a person; and that's about the time the shift in our usage goes to "computer" being a thing, because then what we now call computers were automatic calculators or calculating engines. The same thing is true with the phrases "digital" and "analog computers". As it were, you have differential analyzers and other kinds of computational devices and it wasn't until the advent of the digital computer that somebody had to think, "We've got something different here and we've got to have some words that describe it." It's interesting how those words were picked, because the idea of an analogous machine was a very descriptive kind of phrase and I don't know how the phrase was coined and it just seems to have happened naturally. But I think if you look hard to the MARK I and ENIAC you will not find the phrase "analog computer" anywhere. So, there are some interesting clues. When the Oxford English Dictionary catches up with all of these shifts I think we'll learn something about when these changes occurred in our own operational usage.

I can find early verbalization of these differences back in the thirties but they're (uncommon?), trying to describe what you mean by an analogy. It would be interesting to look through some of the early textbooks on electrical engineering of this era and see what they have to say about it.

Harold Skramstad:

As far as the history of simulation itself by John McLeod would be a good way to start. They're going to be visiting us a week from this weekend.

Tropp:

I'm going to be on the West Coast for the next two weeks.

Sally Skramstad:

Well then, they're going to London. They sent us their itinerary.

Tropp:

If they're going to be in Washington after the fifteenth, I'll give you my card and make sure that he gives me a call. I'd like to see him.

Sally Skramstad:

They're going to a lot of computing of Simulation Council meetings and he's going to be giving some talks, all the way.

Harold Skramstad:

He's giving one at the University of Georgia in Athens at a simulation meeting and...

Tropp:

I'll be sure to give you my card before you leave and if he's going to--I'll be back in Washington on the evening of the fifteenth, so if he's going to be there after that, if he has any free time, if his itinerary allows it, I would love to talk to him.

Sally Skramstad:

They're going to be on a real tight, you know, just a day or two here, there, and so...

Tropp:

[Comment about being on the West Coast]

[END OF SIDE ONE OF TAPE TWO]

NOTE TO TRANSCRIBER: SIDE 2 OF TAPE 2 CONSISTS OF PIANO PLAYING AND SINGING. THERE WAS SOME CONVERSATION WHICH WAS NOT CLEAR ENOUGH TO TRANSCRIBE.