Interviewee: Maurice Halstead
Interviewer: Henry S. Tropp
Date: June 28, 1973
Repository: Archives Center, National Museum of American History

TROPP:

This is a discussion with Professor Maurice Wilkes –

HALSTEAD:

No.

TROPP:

I'm so--Wilkes. Wow! Halstead -- we were just talking about him -- at Purdue University and the date is the 28th of June 1973. Why don't we start off with your Aiken stories. You might start with the story you told me about when you first met him at Lockheed.

HALSTEAD:

OK. He had been retained as a consultant for Lockheed early in -- I don't know when he was retained, but I arrived there early in '67, and there was a review he was going to hold

over research projects that were underway at Lockheed. And he, this was held in my boss's office, and the first thing he was looking at was the Lomas project that Dr. Hutchinson had. I was going to be the second and last one, for some of the work that I was responsible for. So, I watched and listened as he went after the Lomas project, and it

was amazing. He was really boring in, and I had the feeling that George Hutchison was giving him the respect, he was due because of his contributions and that sort of thing, but

Aiken ended up making hash out of him, and it was just a terrible sort of ordeal.

So, I decided that all it really amounted to was a PhD oral, and he was just giving the normal PhD oral-type questions, and they weren't getting answered. So, when it was my turn, why, I didn't give him quite the full story perhaps on the research stuff we were after, and he immediately jumped at the fact that it was a total impossibility, it couldn't be done. You couldn't get from here to there. And he was really going to make hash of this one, too. But, of course, I knew what the connection was, he wasn't going to find a counter-example in a couple of minutes, for something I had thought about for a year or so, so I went to the board and showed him what the equations were and what the basis for it was. And good Lord, the guy immediately jumped over onto my side. He saw that yeah, that was right. He had been wrong. That didn't matter a bit. He was up there at the board himself trying to extend it into directions that were past where we were thinking of. In five minutes, he turned completely around. The fact that he had been wrong once

didn't stop him for an instant. Most people, in my experience will take a couple of weeks, before they realize that they were wrong when they explode the way he had at the impossibility of going that route.

So, from then on my view was, as I told some of my colleagues then, if Lockheed continued to bring Howard Aiken in, well, that would make it worthwhile to work there. Because he was a really tremendous intellect.

TROPP:

Who did he work most closely with? Nash or Bill Main or who were some of the people besides yourself that he worked closely with at Lockheed?

HALSTEAD:

Not Bill Main, but Nash, of course. But then later Nash became a -- I mean Nash was a Vice President, and later was moved over to another area. Now my boss was Dr. Garrett, who was head of Information Processing and reported to Nash. Nash had been head of Information Processing for, oh, I think only about a year. Or even less, before he was promoted to Vice President in charge of Research.

So it was Nash who had brought him in, and Garrett was the one in Information Processing that had -- well, we had 1200 people in Information Processing. There was lots of other computer activity in the area, in the sense that engineers in another area might well be programmers or something of that sort. But Garrett's outfit did have 1200 or so, 1250 people. It reported to Jack Nash until, oh, I don't know--I think I've already made a serious error on your tape.

TROPP:

Oh? What?

HALSTEAD:

I said '67. I meant '63. The date at which I went to Lockheed was '63. The date I came back here was '67. For two years after that I made frequent consulting trips to Lockheed through ' 68, '9 that sort of thing.

TROPP:

That fits more with the period that I knew that he was connected closely with Lockheed.

Another story that I don't want to forget is the marvelous story you told me about his French honor, his award.

HALSTEAD:

Oh, yeah. This was at a party at my place, why, he showed up wearing a little red thread which I didn't know what it was; but I asked him, and he said it was the French Legion of Honour, and he thought that De Gaulle sort of needed a little support that day and so he had worn it. I asked him how he had gotten it. Now I was aware of the fact that he had just about every national honor from many, many countries, but I assumed they were all for his computer work, but this one turned out to be different. So, he told us the story.

He -- there had been something that the Germans had during the German occupation of France, some equipment that the Germans had in Paris that they needed somebody of his background to evaluate, and so they had landed him by submarine in France, and he had had to pose as a Basque because his French wouldn't do. And he said he was barefoot except that he wore these straw sandals, and I think a tam or something or other, but I'm sure of the straw sandals. And he said that the main problem for him was what to do in the daytime when they didn't have a good place to hide or anything. So he was out on this, apparently a street which had a balustrade at the end of the street and another street down below it with a continuation. So that the balustrade was up a floor or so. And he was leaning against the balustrade, and he noticed this fellow on the other end was leaning against it, too, on the other side of the street, the other end of the balustrade. He looked a little later and the guy was quite a bit closer, but he hadn't noticed him move. And a little later, why, the guy was quite a bit closer again. And he said he was thinking to himself, that guy's after me, and about all he could think to do, and he said he had just come up with the conclusion that the next time that guy moved any closer he was going to grab him around the neck with his arm and quickly trip him over that balustrade onto the road below and run in the other direction.

And about that time the guy did move again, but he came up real quick and he said, "Professor Aiken, you're the hardest man to protect that I know of " or something or other. At this point it turned out that it was someone that had been assigned to protect him and he didn't know it.

TROPP:

[Chuckle].

HALSTEAD:

Now the equipment that he examined he didn't tell us about.

TROPP:

But that award is in the literature, and when he got it. He showed me the medal when I visited him. But that's a marvelous story, and if my memory is not faulty, I think that was one of a number of such landings in enemy territory.

HALSTEAD:

He was quite a man.

TROPP:

As far as the records show he was at Norfolk or at Harvard. If he was at Norfolk he was working on degaussing. [Laugh].

HALSTEAD:

Well, Grace Hopper said that he frequently left for periods of time and didn't tell anyone where he was going or where he had been when he got back. So she said that checked with the story, but I know the nature of the man, that was a true story.

TROPP:

Yeah. What are some of the other Aiken stories that either occurred in your own personal contacts or that he told you? For example, the Walk, the various problems when he was doing his PhD and before he built the Mark I.

HALSTEAD:

Oh, yeah. Well, you've already heard his story of his PhD oral, and how the final question they asked him was what he planned to do now that he had the degree. And the answer was that he wanted to make it unnecessary for anybody else ever to have to go through that much hand calculation. And he told me that during the following year he devoted his time to designing a machine that would handle one of three classes of mathematical problems. I've unfortunately forgotten his three classes, but one of them was transcendental functions. And another was -- was -- I'm sorry. I'm not sure what type of equations, but some other types of equations.

And anyhow, he took this first type of problem and designed on paper a machine that would solve that whole class of problems. And this took him about a year, he said. Then, he said, that he had started in then on a second rather different class of mathematical problems. And this took, I assume, about another year. But when he had finished that, why, he realized that the two machines were really the same. And he said he took himself a walk and started to think about the fact that they really were, that he could do it with the same machine. His two designs didn't really have to be two machines. Therefore maybe he didn't really have to design an almost infinite number of machines, as he had originally started out: "OK, I'll design the thing that's most important and another one that's less often used, and so on, keep on going." He was going to design them all. But now he realized that, no, those two were so similar that he could make them the same, that he really only had one machine.

And so, as he said, he took himself a walk and thought all about this. Then he went back

and designed a third machine, and it turned out also to be the same. So at that point, why, he was ready to get the machines built, but there really only needed to be one kind of machine. And that was his story.

TROPP:

Did he talk to you much about the situation at Harvard during that pre-Mark I period, while he was a graduate student and shortly thereafter?

HALSTEAD:

No. He may have and I don't know the names of people he was talking about, and therefore I don't remember them well enough. He told me about being ordered to teach a particular course because that was exactly the course, he didn't know anything about. I think it was antennas or something of this sort. And his major prof said, well, that was exactly the reason why he ought to teach it. It was time he learned something about that. He says it worked. It was good for him. But that's about all that I know.

TROPP:

Do you remember some of the comments he made about the Mark I era? The first machine, or any impressions that you have about that machine and its role in computation?

HALSTEAD:

No, I'm not the one to answer that type of question. I do know that he was .. rather -- he pointed out where the funds that IBM put into the development came from. He said that he had definitely seen, and I'm not sure but what he had a copy of, the budget sheet that showed that it really came out of a customer service and public relations account rather than from their research and development accounts, where presumably it should have come from.

TROPP:

I'm not sure –

HALSTEAD:

Which almost indicated, I mean, he was using this, I think, as an indication to me that they were almost doing it to keep him quiet rather than -- I mean to satisfy Harvard or something or other, rather than feeling that they really had -- that this was their research and development goal. But I don't know for sure.

TROPP:

That doesn't surprise me. Because looking back at other things that Watson personally got involved in, like Ben Wood's work at Columbia, and later Wallace Eckert's astronomical calculating, computational laboratory. I think there was a side of Watson that was partially altruistic that was totally divorced from the business. This is my feeling. And I don't think IBM had even looked at it prior to 1950, really did much in the way of research and development. I don't think that was a company interest, nor Mr. Watson's attitude about the role of IBM. I think if you look at the company organization you won't find research and development. You won't even find an engineering department. It was sales-oriented. ... So, this was more of a public relations thing, in terms of something separate from IBM. It was Mr. Watson's thing. Which kind of explains why the funds would come out of that part of the budget as opposed to one of the more technically-oriented portions. I don't think he considered it a part of IBM. Totally divorced. And I don't think he had the vision to see that this was a coming area of IBM involvement as a business.

[Knock on door. Recorder off]

HALSTEAD:

About IBM and Watson, not IBM, but Watson.

TROPP:

He was, again, outspoken in our discussion.

HALSTEAD:

Yeah. He was very outspoken. He told us the story of the NCR conviction for a felony, that sort of thing long before it was published later by someone in a book. I've forgotten which one, but anyhow.

TROPP:

The Lengthening Shadow, I think or

HALSTEAD:

Well, there was another one, IBM and the Watsons or Think or –

TROPP:

It was either Think or The Lengthening Shadow.

HALSTEAD:

Aha. Well, that must have been it.

TROPP:

Apparently, it made Mr. Watson, Senior paranoiac. He was paranoid about this. Again a lot of the stories are hard to verify, but one of the early groups working on computers tried to interest IBM in taking them over, and he was really worried and turned them down because he was afraid if they got into it they would be the dominant company and would get involved in this anti-trust problem. And this would be in the late forties or early fifties period. And he resisted that for quite a time. And, I think, you know, once having been burned, he was paranoid about ever getting into that position.

HALSTEAD:

Howard Aiken's view was that at that time of the suit, why he, I mean Watson, knew that they would call him as the first witness and the first question is have you ever been convicted of a felony? And he didn't intend to go down there and handle that, so he had his attorneys settle the suit just on any terms that the government required. The consent decree was one in which IBM hadn't put up the slightest bit of resistance. And Aiken's view was that he knew exactly why that was.

Now, what knowledge do you have of Aiken Industries?

TROPP:

None, and I was going to ask you about -- He talked just generally when I was at Fort Lauderdale about his different careers, each of them independent essentially, after Harvard.

HALSTEAD:

Mhm.

TROPP:

Of course, Aiken Industries is a part of that and one we never got to, because we were more concerned with the earlier period.

HALSTEAD:

Well, I don't know too much about it. I do know a little about it. He told me probably in '65 or '66, I imagine it was about '66, that he had made more money since he had left Harvard than he had made in his entire productive life. And he used the word his entire productive life as though that was the time when he was doing something useful. But

that since then he had been making money.

He also told me once that he had tried to buy the UNIVAC division of Sperry Rand at a time when I recall their stock was selling at about 14. And he thought this was a good time to buy the stock because either they'd sell off UNIVAC and the stock would go up or UNIVAC would start making a profit and the stock would go up. But it had been nothing but a drain on the company. Well, it was at about that time that he had, he said, that he had the financing arranged with the Chemical Bank of New York to support the purchase, and that when the negotiations got under way, why Sperry Rand found out who was really behind the thing. And when they discovered that Howard Aiken himself was behind it, wanting to buy it, they decided that the Division must really be worth something, if that guy thought it was. Therefore, they would keep it and make something of it. But if it was just a bunch of businessmen that wanted to buy it, they wanted to sell.

TROPP:

[Laugh]. So, they had a good deal of respect for Aiken's judgment.

HALSTEAD:

Right. Exactly. But Howard Aiken Industries was not a computer conglomerate. But he went around, and he'd find small companies that were poorly managed, which he could buy and fix up the management. Now his blunt methods were just right for the kinds of technological companies. I mean, they were all highly technological areas in which the guys that were running them knew their technology but didn't understand how to handle the company itself. So, this was his idea.

He also had the view that yeah, you can make money fairly easily. That's not a very difficult thing to do. But the other step was the productive step. But, well, how about making money easily? Well, you're on a plane coming out here, for example, and you're sitting next to a guy, and you ask him what business he's in, and he'll tell you. And it turns out he's one of the top guys in the company, why, you ask him, "well, are you making any money?" And he'll tell you. He says, occasionally he'll tell you things that probably he shouldn't, but it doesn't matter. He'll tell you. And when you get off the plane, why, you call your broker and either buy or sell, depending on what you have just learned. And it works.

TROPP:

That was an interesting shift, too, because he had never apparently been concerned with making money when he was at Harvard. He didn't patent or claim things or get involved in that whole aspect of computers during his Harvard period.

HALSTEAD:

Yes. He really wasn't interested. He thought that, well, at that age it was reasonable to

see how you did that, and to him this was no problem either.

TROPP:

Well, it's interesting. I mentioned his views on education. He had first taken his degree in, his bachelor's degree, I guess, electrical engineering. When he did go back to school it was primarily to get an education, and even talking to him recently he had, you know, strong feelings about the role of university training in terms of its educational capacities as opposed to specific training. And he had very strong feelings on this subject.

HALSTEAD:

He had strong feelings on teaching in computer science at the American universities, too, and I agree heartily with his views. But he was very clear in the way he expressed them. He said that the university that teaches how a particular machine works, that's wrong. He said that, that, somebody had asked him a question of, "well, how about the 360/20?" or something of this sort, and he'd look at them, "what does that mean?" as though he didn't know what the numbers meant. Of course, he did know, but his view was that that's no fit subject. And he pointed this out by saying that he felt sure that here at Purdue we had a department, that somewhere in the department of Electrical Engineering there would be some course on induction motors. He said "Now, you wouldn't teach a course in induction motors -- I mean," he said: "that's a fine subject, there's a theoretical background, there are principles involved and that's what they teach. But you wouldn't teach a course on induction motors and say first we'll start with how Westinghouse built an induction motor, and when we get through with that, later in the semester we'll consider how GE builds an induction motor." He says, "that's the way an awful lot of the computer science outfits are doing. They're just anxious to know what the latest machine put out by any of the major manufacturers is like, and then they start teaching how that works."

Of course, Aiken was in an enviable position -- he could always have been ahead of them. He thinks the universities should be ahead. It's true he started them ahead. But they didn't manage to keep that lead, and because of that, why, frequently the top research may be done in a manufacturer's outfit, so we are interested. But his view is that we shouldn't teach it that way. And of course, this is my view too.

TROPP:

What would his position have been on the current thinking in what's called software engineering? Propounded by people like Fritz Bauer. Are you familiar with some of his writings?

HALSTEAD:

I guess I'm not.

TROPP:

Then we'll not get into that subject. That's another thing I didn't get a chance to talk to him about. I think he would have prescribed to the ideas in terms of the need to know something in terms of general principles of this subject as opposed to just learning languages per se.

HALSTEAD:

Oh yeah. But his view on, well, let's see, should we discuss at all his contributions to Aiken dynamic algebra?

TROPP:

Yea.

HALSTEAD:

He set up a series of operators. We were working together there at Lockheed, either while I was an employee at Lockheed or while I was consulting, we'd frequently arrange our consulting trips. After -- I left Lockheed and came here in '67. And I was on leave from Lockheed for a couple of years and they had me back as a consultant every so often and he was going in as a consultant about once a month for four days, or five days a month. And we would try frequently by phone to arrange that our trips would be at the same time so that we'd have a chance to work together out there at Sunnyvale. And he, he figured that we should design a much better language than any that existed for computer programming and he laid out the, oh, 26 to 36 operators that he felt were essential. And we were ... there was going to be a book on this subject, and he wrote the first chapter, and I was going to write the chapter on reducing it to an actual language; here were the operators we'd use. And then after he had written the first chapter, he decided that it wasn't a good beginning point, so he wrote chapter minus one. And then after he had written that, he felt that wasn't really a good beginning point, so he wrote chapter minus two. And this was about the last chapter, I am sure, that he wrote, but we were going in the wrong direction,

TROPP:

[Laugh].

HALSTEAD:

as far as I was concerned, to get to the part that I was concerned with. So he'd laid out this 26 to 30 operators, and they were a really neat set of just the sorts of things we ought to have. And so, Dr. Noonan and I started working in this area. Well, this was when Noonan was working on his thesis.

TROPP:

What's his first name again?

HALSTEAD:

Robert Rownan. He's at the University of Maryland now, and Bob and I had worked all one summer in this area. And we realized we didn't have any -- I mean the book was moving in the wrong direction, so how could we have a reference from which Bob could start, because obviously these were Aiken's operators, and we had to reference them somewhere and then build a language around them. They were just the operators and not the language. And so, we had to reduce it to something that you could work with, and that was a big job, but where do we cite these things? We didn't have any publication except this book that was going back to earlier chapters all the time. So, I explained the problem to Howard Aiken and twisted his arm a little bit, and he came up and gave us a symposium lecture here at Purdue. And in this lecture, why he was, he laid out the operators. But, of course, we had had the written material anyhow. But that lecture was the only thing that Noonan could cite in his work.

Well, that was all right, and Aiken's view was that this thesis would be an excellent thesis, and that sort of thing, but that Noonan should not be required to also do the hardware side of the same thing, because the operators fit into the hardware very well and it looked as though they would, but you'd have to demonstrate this so that his view, and mine, was that there was another thesis in the same area that needed to be done by a different man. His view was that we can't make Noonan spend a lifetime on his thesis. And so, Larry Schutte is about to finish up.

TROPP:

Is that S-C-H-U-E-?

HALSTEAD:

S-C-H-U-T-T-E. Larry Schutte is with Bell Labs, but here at Purdue getting his Ph.D. And so he is demonstrating how useful this approach would be and how efficient. Yes, it is. We are very happy at the moment with the prospects of this sort of thing. Boyce has extended Noonan's work to show that the parallel machines or pipeline machines are likely to have very real advantages. Now this isn't entirely, I mean, this is not, you understand, just the 26 operators, it's also the goto-less approach that Noonan had to add and the other various things that had to be done. But it's based on those particular operators.

TROPP:

In a sense now, you have the operators, you have a workable language, and you have the prospect of a .. of the hardware to implement things.

HALSTEAD:

Right. Now this hardware will not be the sort of hardware "here is a machine that has the Aiken operators." It's a sort of demonstration that if you were to put the Aiken operators into any machine and wire them, why, you would have an improvement in that machine of virtually zero faults--not quite zero, but darn near.

The thing is that all of the machines that we've had tend to make a comparison on the basis of the comparison to jump somewhere else or not to, depending on that sort of thing.

TROPP:

The various branchings.

HALSTEAD:

Right, the branchings. Now, Aiken showed that it was very straightforward to, if you wanted the larger of two numbers, you didn't have to put one of them into one register and subtract the other and then if the quantity was less than zero you'd jump to a place where you'd load the first number and if it was greater than zero you'd jump to a place where you'd load the second. Instead you change the wiring so that you get the larger or the smaller of the two immediately with one instruction instead of with a number of instructions that are in there jumping.

This sort of thing is the same -- Noonans's work was one of the earliest goto less languages because it works out that if you try to get rid of most of the jumps in the machine you also can go to the higher level language and get rid of most of the jumps there. And one is nice for people, and the other is nice for the machine. And so, yes, this sort of thing works fine.

Now, there's nobody out beating the drums about how good this is or anything of that sort. It's a new research area.

TROPP:

I like your analogy to the Kronecker delta. As opposed to the true-false, or the branching.

HALSTEAD:

Yes. It makes so much difference. And it isn't precisely a Kronecker delta, but it's darn near it.

TROPP:

It's the same kind of role, though. You either get a 1 or a 0.

HALSTEAD:

And you multiply that 1 by something or the 0 by something. If you're in a particular part of a program in which, if you're in this branch you'll increase an index i by 1, but if you're in this other branch you won't increase the thing, I mean, you'll just leave it what it is, then you can combine both branches by saying that i plus the Kronecker delta, which is -- I mean, it might be a less than b, i plus (a less than b) times 1, and then of course you leave out the times 1, so it's just that, replaces i. At this point if you're in the right side of the loop, why, nothing happens. If you're in the other side, it's increased by 1. It works beautifully. It works fine. And it gets us out of the "if-then-else" situation. You don't even need that.

And Noonan proved that you can translate FORTRAN into Aiken dynamic algebra and therefore -- any program that requires Gotos can be translated, can be written in FORTRAN -- if you can translate that into Aiken dynamic algebra without any Gotos, well obviously you didn't have to have them in the FORTRAN. So that was his method of showing that this would work for anything. It was general.

TROPP:

Is the name "Aiken dynamic algebra" something that you've given to characterize the whole system - the operators, the language and the hardware - or just the operators?

HALSTEAD:

The Aiken operators, and one instance of a dynamic algebra is the Aiken dynamic algebra.

TROPP:

Oh, ok.

HALSTEAD:

Now, no other instances of the dynamic algebra have been implemented.

TROPP:

[Laugh]. OK. But the Aiken dynamic algebra is essentially the language in Noonan's thesis.

HALSTEAD:

Yes, right. So, Noonan characterizes the thing in general. And then when you have to get specific and implement something, why, OK, if you use the Aiken operators in the dynamic algebra you've got Aiken dynamic algebra that has all of these properties. And his view is that this is one instance of a dynamic algebra.

TROPP:

Of which there are no others [laugh].

HALSTEAD:

Of which there are no others, right. TROPP:

Did you have any contact with Howard Aiken recently, during the work I mentioned he was doing for Monsanto?

HALSTEAD:

No. He was concerned, I mean he was consulting for Monsanto and for Lockheed at the same time, and he'd spend time at both places. Now I knew that he was trying, but claimed that it wasn't going over very well, to interest the Monsanto people in this new language sort of thing. And he demonstrated how it would work on process control-type programs. But when it comes to the hardware facet of things, I didn't even know what he was doing. I didn't know what he was doing at Monsanto. I did know that, yes, he was –

TROPP:

Do you know some of the people who were involved with him that I might talk to? Was Minik, for example involved on that?

HALSTEAD:

I don't know any of them. If I know them, I don't know they were.

TROPP:

Oh.

HALSTEAD:

Now I, but I understood from Joe Donagher of Lockheed whom I ran into back at NCC about the 6th of June that Aiken had been making his consultant trips to Lockheed as late as this spring, but I don't really know.

TROPP:

There was a period when he was in the hospital, at the end of 1972, I am sure he was not doing anything for anybody.

HALSTEAD:

Correct.

TROPP:

And only in the early part of the year was he able to get back to his consulting work. When I talked to him in early January he mentioned how heavily this load had built up during the period he was in the hospital. He was just getting his strength back and just getting back with it.

HALSTEAD:

What was the hospitalization?

TROPP:

Disc removals.

HALSTEAD:

Oh, so. Not his heart?

TROPP:

No.

HALSTEAD:

OK. What was his final..sickness.

TROPP:

Nobody knows.

HALSTEAD:

They don't know if it was his heart? He had angina; you know. He told me when he found that I also had angina, why, he gave me the suggestion, OK, you pop the nitro pills under your tongue not when you get a pain, but when you encounter what would probably produce one. He says, "For example, you get into Chicago airport and know

you're going to have to trot your bags way down long corridors. Before you pick up the bags you pop the nitro and then you go dancing off." Yeah. Smart man.

TROPP:

Unfortunately, there was no autopsy done in St. Louis and none was done back in Ft. Lauderdale. So, there is really no way of knowing. It could have been as a result of a disc removal, as well.

HALSTEAD:

That's a shame. Well, I just automatically assumed since I knew that he had the angina that –

TROPP:

Well, I think that's probably a fairly safe assumption. But again we have no way of knowing. What are some other Aiken stories? Because when we get through with those, I want to come back to your 1940 exposure.

TROPP AND HALSTEAD:

[Laugh].

TROPP:

I want to save some time for that.

HALSTEAD:

I don't really know what, which things would be the most interesting.

TROPP:

Almost anything that gives me clues, because I, we have limited our discussion to this very early period. And I'm more interested in getting a total overall picture from people he was associated with. And I've talked to a good many of the people he was associated with in the early period and now I'm kind of moving ahead.

HALSTEAD:

Well, his views there at Lockheed. He was a consultant in a whole number of different ways for Lockheed. I mean, on organizational structure of computers, his view was that a computer operation could get so large that it was totally unmanageable. And he would point out that it was virtually never that the head of the computer center got promoted to Vice President of the Company unless he started that way, because the thing would be so

unmanageable that he'd do nothing but make enemies around the place. It wasn't really a type of job that could be handled. So he was one of the - I mean he was even then seeing that, despite the fact that Grosch's law says you should centralize there are certain other reasons why you should decentralize, and that sort of thing. I don't know whether he believed in Grosch's law or not, but anyhow he believed in decentralizing quite a bit.

TROPP:

I guess I don't know about -- you're talking about Herb Grosch? I've talked to Herb and I don't know that I'm familiar with that phrase.

HALSTEAD:

Well, well this is - the whole industry knows Grosch's law, really. But we put law in quotes. So does Herb Grosch. But the strange thing was -- well: What Grosch said was that the computing power goes up as the square of the cost. Therefore, if you can pay three times as much for your computer, you'll get nine times as much out of it. That means then that you should put all of your computing dollars into the same machine so that it will be so big that it can do more for you than if you bought two or three of the smaller ones. Now there is some thought that they used Grosch's law in pricing policy to det -- and it was a self-fulfilling prophecy, but the darned thing worked. I can remember sitting with Herb Grosch one evening and telling him about the Ph.D. thesis done by Kenneth Knight at Carnegie, which had proved that Grosch's law was phenomenally close. He had made a study of the first 200 computers developed in the country, starting with the Harvard Mark I and going all the way through till about, oh, I guess '67 or something of this sort. And then he had gone back and worked out the coefficient that you should get for power versus cost.

TROPP:

By power, the speed of addition or multiplication or something like that?

HALSTEAD:

Well, it was much more complicated than that. You have to consider the effect of memory and the effect of different types of functions and so on.

TROPP:

I'm used to a more simplistic definition.

HALSTEAD:

Right. Well, this was, this was apparently - it won't work at all if you merely use add speed, because a machine with no memory and a machine with a huge memory will do much different things and they cost differently. But anyhow, Knight's thesis, and later a

couple of pages in Datamation, showed that this was indeed so close that -- well, he did it the opposite way so .5 was his exponent if Grosch's law was perfect, and for commercial and scientific he had a slightly different, very lengthy equation, and came out about .46 and .53 or something of this sort. Anyhow.

TROPP:

Phenomenally close.

HALSTEAD:

Yeah, it was right on the nose, really. And Grosch told me that he hadn't imagined it would be anywhere near that close. Anyhow, we don't know that it's continuing. It's not a law of nature, as far as we know. But - anyhow.

TROPP:

It neither fulfills nor violates any basic laws of physics either.

HALSTEAD:

Right.

HALSTEAD AND TROPP:

[Laugh].

TROPP:

While you're stretching your memory on Aiken let me take you back to this period you told me about of your early involvement with computation. And maybe you might even go into a lead of about how you got there.

HALSTEAD:

Well, I'm not sure about which thing you're mentioning.

TROPP:

I'm talking about the 1940 computational...

HALSTEAD:

Yes. We had a -- While I was working in the Department of Ag in Washington, and, let's see, this would really be '41. And the The Soil Conservation Service was where I was working, and we had a lot of computation to do. And we had access to a computer center

out in New Philadelphia, Ohio which was made up of 50 or 100 WPA workers, who were the white-collar workers. And they couldn't very well be put on raking leaves, which was the WPA strongpoint in those days, but anyhow these guys sat at desk calculators and worked all day. And we'd prepare the programs in Washington in which we'd say, "add column 1 to column 2 and put it in column 3," and that sort of thing.

TROPP:

Take half of that or square it, and then do something to column 4.

HALSTEAD:

Right. But we had to be very careful on the square root things, which we frequently needed in statistical work because there were only two of the WPA guys that knew how to take square roots on a desk calculator. And this meant that the turn-around time would drop off terribly if you had too many square roots in your program.

TROPP:

That was pretty tricky on a desk calculator.

HALSTEAD:

Oh, it was.

TROPP:

I remember how long it took me to learn how to do that on an old Marchant, I think. But I'm really curious about that group of people in New Philadelphia, because I told you I knew about the Math Tables project in New York which was also WPA.

HALSTEAD:

Right. It was WPA and I assumed they were the same, but I don't know that for a fact.

TROPP:

I don't think so. And I guess I'm curious, in terms of any information you can give me that will enable me to chase down what that group was, how they got started, and what their role was other than doing problems for a government agency like yours. I assume that then they took problems from other agencies. Unless they were strictly in the Department of Agriculture.

HALSTEAD:

This is what I suspect. It might have been part of the Muskingum Conservancy District

in Ohio. But anyhow it was set up in the fairgrounds at New Philadelphia. And occasionally we would take the overnight train from New York to go out and get closer to the operation.

Now, we also had some other things going on out there that were part of soil conservation itself. Then there was another place we could drive to that was about 30 or 40 miles away, where NYA, the National Youth Administration, had another group, one could get certain equipment built there. So I don't know the specific structure.

TROPP:

I guess, the, the

HALSTEAD:

Mr. Corwin, I think was in charge of the people who ran the calculations. They'd actually, in those days when they used graph paper they'd design it and reproduce it themselves.

TROPP:

His name was Corwin? Do you remember his first name?

HALSTEAD:

Oh, yeah. I don't know whether he was in charge or whether -- I think it was J. C. Corwin -- I don't know whether he was in charge of that or merely our contact with that. I'm not sure.

TROPP:

Are there any other names associated with that group that might help me chase it down?

HALSTEAD:

No, none that I would know. He lived in New Philadelphia and he worked for Sansway [?] in the Soil Conservation Service. But I guess he wasn't really in charge, but he was our liaison man with this group.

TROPP:

The problem with most of the WPA projects of this period is that their records have been destroyed.

HALSTEAD:

Right.

TROPP:

and practically nothing remains unless they turned out a publication. Now in the case of a group like this it's highly unlikely they would have done any publications, because they were apparently, from what you described,

[END OF SIDE I]

TROPP:

at least your use of them, they were a service group. HALSTEAD:

Oh, absolutely.

TROPP:

They weren't like the Math Tables project turning out tables of reciprocal functions or exponential functions or ...

HALSTEAD:

No. We just assumed, I mean I just assumed, with no knowledge, that they were the same sort of thing. Oh, I know a man that you could make a quick phone call to in Washington. He's the Vice President of Hopkins, George Benton.

TROPP:

B-E-N-T-O-N?

HALSTEAD:

Yeah. He's the Vice President of Hopkins now. He's on my committee. He was the one that made the best use of that operation. Of course, he is fantastic himself.

TROPP:

You mentioned the project, the computation he had them do in terms of weather. That's worth putting on.

HALSTEAD:

Yes. He had them crank out the statistics that would show when the very driest day of

the year would be for the longest period of time without rain, and that sort of thing. And then he had them crank through the stuff on Warsaw. And Hitler's invasion of Poland appeared to be timed perfectly with the drought that came out of that computer ...

TROPP:

We were talking about a period when the word computer means a person rather than a machine.

HALSTEAD:

Right, right.

TROPP:

It isn't until after the Mark I, ENIAC, and that era that it comes to mean something else.

HALSTEAD:

Right. Benton could do tricks with a Marchant. I can remember once when he was -- this was stuff that he wasn't sending out to New Philadelphia, and he was doing it himself on the Marchant, and he was adding two numbers--he was squaring two numbers and adding them together in his head and then taking the square root on the Marchant. And he was singing "the Music Goes Round and Round" because it didn't take his full brain power to do this.

TROPP:

Well, you, very early then, were involved in problems requiring computation. When did you first become exposed to something other than pencil and paper or desk calculator in computational...

HALSTEAD:

No, I'm no pioneer in this area. I designed slide rules when I was a senior at Berkeley, but it turned out that somebody else designed the same slide rule and that sort of thing. And I was concerned with monograms and this sort of thing early and -- over in the meteorological field, which is where the earliest work was done, on, well, numerically forecasting weather had been done in World War I by an ambulance driver in France, who had worked out the fact that yes, you could run this thing through if you could ever keep up with it. But, of course, it would take years to get tomorrow's forecast. But he showed that in theory you could do it. OK, so we were always concerned with this facet of things. But, no, I got into designing analog computers myself.

TROPP:

At what point?

HALSTEAD:

Oh, well, let's see. I designed and built slide rules as it were, but building a slide rule –

TROPP:

A slide rule is a computer.

HALSTEAD:

It is true, it's an analog computer, but I was doing that sort of thing as a senior at Berkeley. And this was made very easy in one sense by the fact that the outfit that I was with, was also, I mean frankly, was laying out funds for certain things, and he'd had some slide rules built out of wood that were large, but you could go ahead and put new scales on them. OK, so that meant that it was pretty easy to lay out the thing if you just knew how. And that sort of deal.

But back at Hopkins in '52 or so I was designing analog equipment to handle problems in thermodynamics of the earth/atmosphere interface. And later it turned out you could use the same techniques to handle grub on the well and things of this sort. to handle the things with analog circuits. So that was how I got into it. But then the software side's what I've been in for ever so long now. But I'm no pioneer in that area.

TROPP:

I was just building again more of a total picture. We talked a little bit at lunch about Snedecker and I'd like to get some of your reminiscences.

HALSTEAD

Oh, yes. He's a fascinating gentleman.

TROPP:

When did you first meet him?

HALSTEAD:

Oh, it must have been about 1960 or it might have been '59. He had retired from Ames and gone to the Triangle area and

TROPP:

Triangle area. You're talking about Duke and –

HALSTEAD:

Right. And I'm not sure which of the North Carolina sort of thing. And he'd gone there, and he was talked into coming to the Navy Electronics Lab, which became his third place to retire, well, no, second place to retire. So, he was too old to be put on as a permanent civil service employee, but we could put him on one year at a time. And he had to have some place to report, so it turned out to be me, but it was not because he reported to me, of course not. But Snedecker was a tremendous guy, and he got into learning oceanography -- well, I used him in a beautiful way. We had a situation in which physicists and oceanographers and so on were planning long cruises, which would really take a lot of expenditure of capital on a long cruise. And they'd come back, and they'd have computing to be done. So I would want them to use my stat section to design their experiment properly. But their own view was that they knew how to design an experiment, so I couldn't get them to use my statisticians for this until they got back, which time was the wrong time to do it.

So as soon as Snedecker showed up, why, we had a totally different deal. I'd just go around and point out to them that when they got back we'd have a lot of computing to do for them and it might help a little if they would go to the great Snedecker and have him make sure their experiment was designed correctly. Well, nobody can lose face by going to Snedecker on a stat problem, so they went. Well, he'd look it over and before long he'd send them to Van Peek or some other statistician and tell 'em, "go ahead and tell Van that he ought to do thus and so for you." And they'd go over, and they wouldn't lose face then, because they would say Snedecker sent me over and said you ought to do something or other for me, and again they weren't losing face, so it worked.

At the same time, he got into, well, he coined the term oceanometrics, which was a combination of computer science and oceanography and statistics. And in that time, why, he learned all about oceanography. Not all about computers, but he kept pointing out that they completely changed stat, because all his life he had been trying to make things so that you wouldn't have to calculate as much in order to get answers, and now you just went right ahead and calculated. You didn't need an estimate. You'd just go ahead and calculate it. Yes, he was a grand gentleman.

TROPP:

Did you ever have any conversations with him about his Ames period back in his early years there?

HALSTEAD:

Oh, I probably did, but I probably can't recall anything of great value. No, I can't. But,

he was a splendid gentleman.

TROPP:

Have you thought of any more Aikens stories?

HALSTEAD:

No, I guess not. His brusque manner was the delightful thing.

TROPP:

I take it the two of you got along quite well. A rather blunt open direct approach which always works.

HALSTEAD:

With him nothing else would have worked. So, yes, we got along just splendidly. We nearly always agreed, but there were times when we didn't until we mashed it down to the point where we may both have changed our minds slightly before we agreed. Well, I can remember one of his thoughts was when people think together in the same way it's good, and I can remember we were talking about this graduate student I had, that was going to be working in this area. And I made the comment that he thinks the same way I do, or something or other, and Howard's view well that's the right recommendation. He wasn't being flattering. He just meant if the two of you think in the same way you can work together and get things done. He didn't mean that's the only way to think or anything of the sort.

But when he and I were working together it turned out that yes, we could think in the same way. I don't know whether that meant that he just changed his way to match mine. He was the genius and he could do it if he wanted to. But anyhow he was easy to work with.

TROPP:

How did you find his general range of knowledge and grasp of information on a broad basis?

HALSTEAD:

I thought that it was excellent, although I suspect that there would be people who would say that Norbert Wiener was the broad type and that Aiken wasn't. I'd argue a little with this. Aiken was down to earth and brusque, and this seems somewhat anti-intellectual or something or other in certain ways of thinking. But I don't believe that was at all true. I think his basic knowledge and his basic interests were as widespread as anyone's.

TROPP:

I don't think I would classify him as anti-intellectual from just my short conversation in those few days plus looking at his library, which was obviously well read.

HALSTEAD:

Right, right. He had broad knowledge and, and - yeah, there is no doubt about it.

TROPP:

I found him thinking, on specific problems, of course, he could get down to that problem that was at hand and then grapple with it, but I got the feeling or the impression that in terms of the computational environment, the whole problem of information processing, he had very strong feelings of a broad nature into which all these individual problems fit in their proper perspective. And they were properly organized. There was no disorganization.

HALSTEAD:

Oh, no. He would sometimes, I think, put forth an idea as strongly held when he didn't really hold it strongly yet. But it was the sort of thing as "let's toss it up and shoot at it, and if we can't shoot it down, well, OK it's all right." But when he'd say it you wouldn't know which way he had in mind. And I think this could have made some people think that "well, he was quite opinionated," which he wasn't.

TROPP:

Another impression that I had, which you might react to, is, I found that he had a rather interesting sense of humor which doesn't come out in conversations with a lot of people who had run-ins with him, but that was my impression.

HALSTEAD:

Oh, yes, yes.

TROPP:

He really enjoyed himself.

HALSTEAD:

Oh, yes. Right. He enjoyed a joke. He enjoyed a drink. He enjoyed good food. Yes; he was complete. I admit that I was greatly shocked when I realized that he had been landed by submarine into occupied France. This was a bit beyond what I thought of be[ing] complete.

TROPP:

The whole environment of Mark I, I got the feeling was like being aboard ship. The whole project was run that way. I remember in talking to Grace Hopper about that period, she described various things they were doing, there were various obvious extensions: "Did you think about this, or did you think about doing that"; her reaction was "of course not, we had a job to do, it was wartime, and you didn't bother with those things. You went on with the job."

HALSTEAD:

Right.

TROPP:

And I think that characterizes much of his work, not just the wartime environment. Once you set a goal, you didn't allow all these other side issues, many of which may be very fascinating, to deter you from achieving that goal. I guess, in terms of your contact with him on a consulting job, I just wonder how you would characterize that aspect of it.

HALSTEAD:

I suspect that he became a bit more interested in exploring more avenues as he got older then, because I think he was willing to consider changes and things of this sort more. Now, he was no longer in charge of "let's get this job done," but, "which way should that gang of guys be pushed so that they will come up with the best possible." Now, he was also, what you might almost call ruthless in his view of "that guy will never succeed at that problem." He didn't mean the problem couldn't be handled. He just evaluated the man and there was never any chance that it would be successful. When he made that sort of an evaluation, why, I don't ever know of a case in which I thought he was wrong.

TROPP:

Well, I think the characteristic of, you know, the ... willed. He had definite opinions about the capabilities of the people who worked with him or whose work he had a chance to view.

HALSTEAD:

Right. He looked for excellence and if he saw excellence why OK you could go anywhere.

TROPP:

He had no tolerance for people who were not competent to his standard of competence.

HALSTEAD:

Right. Unfortunately, [chuckle] there aren't many of us that can

TROPP:

No. [Laugh]. I guess I would never have made it. [Laugh].

HALSTEAD:

I wouldn't make that either. I keep thinking in terms of the way he thinks that computer science should be taught. His view is yes, the manufacturers should be watching what's happening in universities, not the other way around. Well, I grant that he's right. But I also grant that it will be quite some time before this happens again. But he started it. It was that way in the first place.

TROPP: Well, but I guess one of the standard complaint is that the manufacturers don't even pay any attention to their own customers. [Laugh]. Which is, you know, pretty common.

HALSTEAD:

Yeah. But, no, that's not it. It's a matter of where is the interesting research being done. And, of course, a large amount of it is being done in industry.

TROPP:

Yea, I think once that he stopped building machines at Harvard, it was a deliberate cessation in terms of, the university really had to take on a new role, and that the universities weren't places to build machines. That could be done better elsewhere.

HALSTEAD:

Right.

TROPP:

Universities had a very different function.

HALSTEAD:

Well, his view of computers on a campus was, well, his view of running a computer center, for example, was that that was comparable to running a printing operation or something of that sort on a campus. He used the analogy. He said if you went over and found that one of the professors had this press that he had set up so that it would print old

English or medieval something or other just in a very neat way, that he had done this as a research development, you wouldn't expect to go in and use his press to get some work done for you. You would expect to go down to a printing press that the university may have and get your regular stuff done. You don't bother this prof that is trying to set up and develop this thing that is like an old one or something of that sort. And he said, “similarly, if you're working on a computer yourself, it's not a service computer if you're doing that as a research project. And you shouldn't mix the two." Well, that was his view.

TROPP:

I haven’t' spent any time looking at it, but apparently he got the opportunity to implement his views at the University of Miami when he retired and moved to that area, because I have the impression that their center and department was built around his advice. [Recorder off] I have the impression from talking to two members of that department that it is very flexible and can change as the needs change,

HALSTEAD:

Right.

TROPP:

which was another thing he felt very strongly about, not locking yourself in to something where you can't get out of it if

HALSTEAD:

Right.

TROPP:

the atmosphere and environment changes.

HALSTEAD:

Well, one of the things that he wanted to see accomplished there at Lockheed, and that didn't get accomplished in the days that I was related to it, was that he wanted a preprocessor, a front-end machine put in that would do all the compiling for the main machine that would do the work. And his view was that we could get some front-end machines that were small, that were as cheap as we could get nowadays, and then whenever we changed the main machine, as things got cheaper and changed, why, we wouldn't change the environment the user had at all. We'd just keep these old clogs around for many, many years because they only did the compiling. And we could change the machine for which they compiled anytime we wanted to just by changing the software on the front-end machine. Now this is a very reasonable approach and will allow you to

make the changes in the big hardware, it doesn't have to be large in size but large in capability, without damaging the effect to the user. This seemed to me to be an excellent idea and they were trying to implement this one, too. Our trade-off studies the last I saw said "Yeah, that would be a good idea," but I don't think that we ever got to the stage of really doing anything.

TROPP:

That makes sense even in the university environment where you want students to use a computer for instructional means. That makes a great deal of sense because then you won't have to change that approach, and they don't know what it is they're working with anyway. Just various on-line stations.

HALSTEAD:

No, I didn't ever find that his view were anything but looking toward the future sort of deal. His physical stamina was fantastic. As I told you he was the last man at the party. And he had been enjoying himself tremendously and we had been enjoying his presence tremendously. And we took him down to the Student Union about, oh I imagine, it was two in the morning. And then he had a plane to catch, so we had to meet him at seven in the morning. And my wife and I were sort of dragging at that stage. And he comes spryly down. He wasn't dragging, we were.

TROPP:

[Laugh]. Yeah, he did tell me another one of his stories where he had an award to get in Philadelphia and he also had to be in Europe within a short period of time. He arranged things in such a way that -- I guess this was during the Aiken Industries period -- where he appeared at this affair, not long enough to get the award, but long enough to have made a proper appearance. He dashed out to the airport, picked up a company plane, which got him to New York just in time to pick up a plane for Brussels or wherever else he was going. This was within the last decade, and he was beyond the age of 60, and I imagine he - just the thought of it tired everybody else.

HALSTEAD:

He had plenty of energy.

TROPP:

I was surprised during our visits, because this kind of thing, of taping discussions, we did from, we'd start it in the morning, and then we'd finish about five o'clock in the afternoon. And then he and I would go for a long, long walk, and then we'd come back and go to dinner, and dinner was two or three hours, and then we'd continue just conversing informally, and I'd be totally knocked out. Come in the next morning and he was brighter than anything. A hard man to keep up with.

HALSTEAD:

Oh, yes.

TROPP:

It's very tiring to do this kind of thing for long hours.

HALSTEAD:

He was tremendous. There aren't many like him.

TROPP:

Well, when we look at that whole group of the computer pioneers, if you look at George Stibitz at Bell Labs, and you look at Howard Aiken, and you look at Eckert and Mauchly, and the whole group of pioneers, and of course, Von Neumann, Vannevar Bush, every one of them is pretty much of a giant. And each one of them is a giant, and those who are still alive today, still in pioneering fields, and, for example, Stibitz apparently is very active in medical research, medically-oriented research, I'm not sure of the area. I found Aiken right out on the frontier, as I told you, in terms of magnetic bubbles; and from Bush's publications, at least until, you know, recent years, you get the impression of still a very active mind,

HALSTEAD:

Right.

TROPP:

and this is one of the most impressive things, that these aren't men who did that one thing and then quit.

HALSTEAD:

No.

TROPP:

It characterizes him, and he talked during our discussion about the fact that he stayed alive intellectually as well as physically by continually taking on new careers, in a sense, new intellectual challenges and when he stopped doing this, when he got himself mired in one, he figured he was dead.

TROPP:

Right. Well, we discussed this sort of thing, the two of us. I had my own personal views on this, and he agreed, he had them before I had, but there is this set of statistics which can show that a man's greatest contribution to science usually occurs when he's around 26 years old, 25 or 26.

TROPP:

Before the age of 30.

HALSTEAD:

Yeah, but I mean the curve actually peaks.

TROPP:

Peaks at about 25, 26.

HALSTEAD:

OK. Now I point out that this is the length of time it takes a young man to have learned e enough in a field to make a contribution if he's going to. And that this really takes beyond the bachelor's degree level, it usually takes about five years or so. So, you put him into a new field, and it'll take him about five years. And you keep shifting him from one field to another and it'll take about five years. And so if you will change from field to field, you'll make your contribution, I mean it'll show about five years after you got there. It has nothing to do with the mental age, I mean with this age process thing.

And, I think that, yes, his view was that you shift a little. He didn't have to shift too much, it didn't seem to me.

TROPP:

You have to shift it enough to avoid plowing the old pasture over and over again.

HALSTEAD:

Right.

TROPP:

And that's the critical factor, which, in terms of our university environment with which we are both familiar, characterizes so many of our people. Do work and the rest of it is just a continuation of the same thing.

HALSTEAD:

Right. Yes, I agree.

TROPP:

I am correct?

HALSTEAD:

Aha. I remember in a university or college I was in, somebody was pointing out that t these guys that talk research but that if you check, it's probably, it is probably something they did their thesis on that they are still talking about. Yea, ok. That's not a great, intellectual research university.

TROPP:

That's equivalent to dying on the vine.

HALSTEAD:

Yeah.

TROPP:

I laugh when Hardy makes this point in his Mathematician's Apology, and his own record his significant accomplishments begin at the age of forty.

HALSTEAD:

So, I just don't hold with this view, you see. Neither did he. And neither did any of these guys that I have known that were quite old.

TROPP:

I've had that drummed into me in mathematics with example after example, and I can think of counterexample after counterexample of people who are many years my senior who I still find are among the most intellectually exciting mathematicians. And they have shifted branches and areas and there are few things that they aren't involved in, or haven't been involved in.

HALSTEAD:

I'm convinced that he was right and that that's the way it works. You can take the statistics if you want to, but if you don't understand what they mean you're just stuck.

TROPP:

I think the statistics in terms of my field, in terms of mathematics, are illuminating when you look at something that was done during the I think it was the Eisenhower period when we recognized the shortage of scientists and the need for mathematicians and so on and so forth, and one of the mathematical societies did a study that showed that 90 percent of the publication was being done by something less than 10 percent of the mathematicians and most of the Ph.D.s in mathematics have done no publications beyond their dissertation. And so the statistics have to come out, [laugh] you're almost forced to that peak that you talk about, age roughly 26 or 27.

HALSTEAD:

After you turn this off, I'll show you some stuff that I think is interesting.

TROPP:

OK, let me turn it off now with many thanks.

TROPP:

William Geldner, he was at Chicago and I think he himself installed it...some informal assistance to Michelson.

HALSTEAD:

Oh, that could be. That was before my time.

TROPP:

...custom built them. The interesting thing was they still did that about 1930, I think they changed the design from the cone to the cylinder, and they made some modifications...

HALSTEAD:

Oh. No, this machine I'm speaking of at Geneva, its base was I guess a steel casting roughly the size the shape of a grand piano. And it had gearing underneath it so that it drove on top of it not 80 but maybe 20 or 30 discs, each of which had a resolver on it. And it drove them at speeds one, two, three, and so forth, and each of them had a resolver on it so that its motion at right angles to the lead screw was the motion of the ninth component. It didn't look at all like the Michelson-Stratton.

TROPP:

This sounds more like the Miller which was an entirely different...