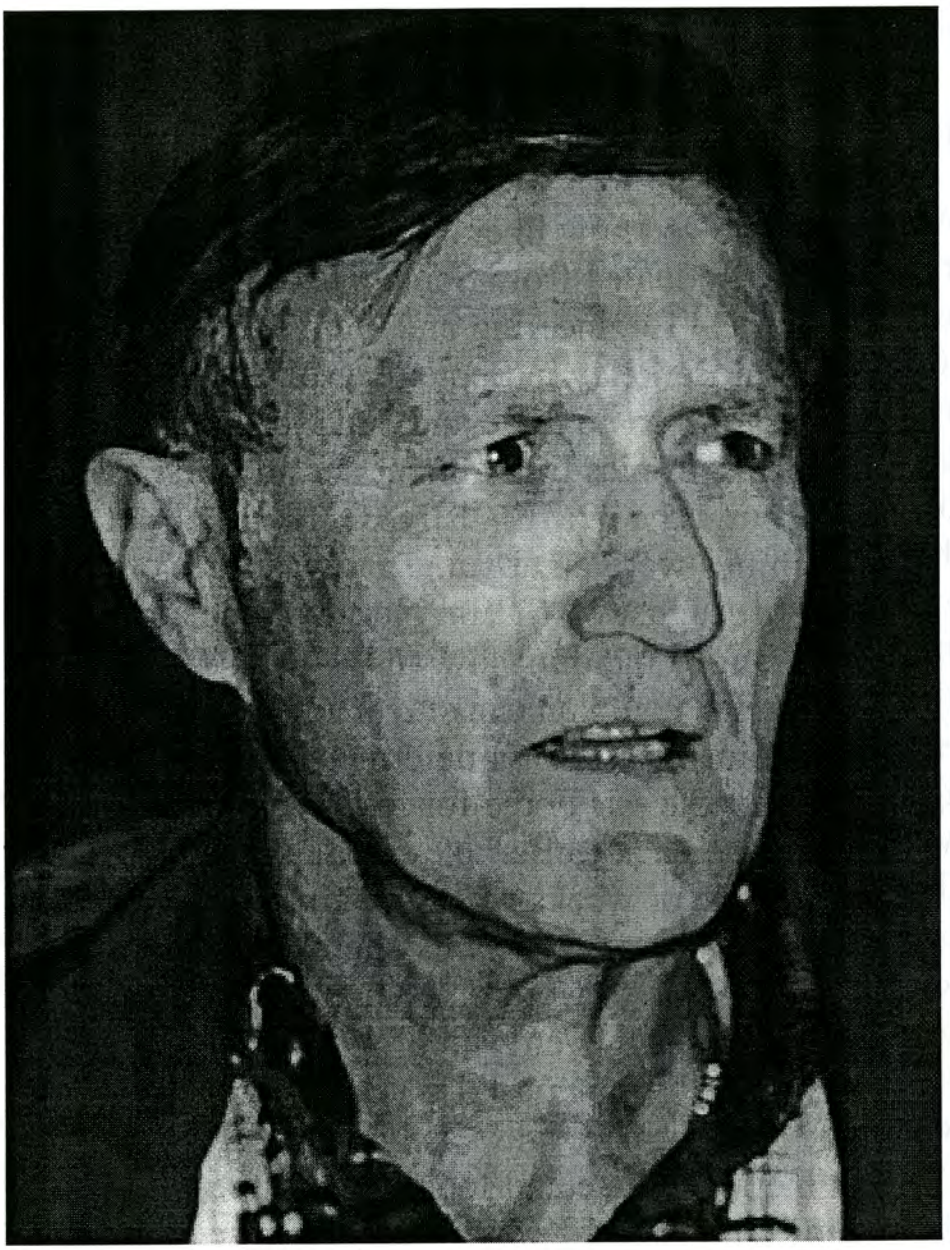
An Interview with Ed Lafranchi



EL = Ed Lafranchi
GAM = George Michael

GAM: Well, it's March 3,1997, and we are talking with Ed Lafranchi, who got to the Lab fairly early. Ed, you were one of the first computer engineers at the Lab, even before the first

computer arrived? How did that happen?

EL: OK, first of all, I'm a native Californian.

GAM: Wow, a truly rare bird!

EL: That's right; there weren't many of us at that time.

GAM: Thank heavens?

EL: Well, we showed how liberal we could be by letting all you Easterners in. I graduated from Santa Clara in 1950 with a degree in Electrical Engineering and went to work at the Radiation Lab in Berkeley in September of that year. A friend of mine suggested I go up there and Jim Norton hired me. I worked in the Electronics Department and my first assignment was to help rebuild the 32MEV LINAC. I kind of got to see what big machines were all about. That was early on in the Korean War. In August of '51 Jim Norton came around and said they've changed the draft law and we aren't going to ask for a deferment for you, you can stay here as long as you want but we aren't going to ask for a deferment. I decided I'd leave and signed up for the Air Force for four years, I managed to cut it to two and came back to Berkeley in September of '53. I was only there a couple of weeks when the guy I was working for, Hilmer Larson, said "we've got this site at Livermore and they are looking for people to work on computers out there." I had no idea what computers even were. "Would you be interested?" Being single I said "Why not, I'll go talk with them." At that time security at Livermore was vastly different than today. Lou Nofrey met me at the gate and took me into the UNIVAC room. It was in and running and I was pretty impressed with this massive machine with all those tapes spinning and the fish line tensioning scheme and everything else. I talked with Lou and Chet Kenrich and probably Dick Karpen, I'm not certain of him. We kind of left it there and then I got a call a couple of days later from Hilmer and he said if you'd really like to go they'd like to have you in Livermore. So, that's how I got here. I started in, I believe it was November 17th of 1953, commuting from Oakland with John Efstathiou.

After a couple weeks of commuting I decided enough's enough, I'm moving to Livermore. Well, trying to find a place in Livermore for a single person, probably even for a married one, was not easy. But I found a room at 381 South L Street where I think the United Brokers or some other Realty office is now. It was a room that these people had put up basically to rent to folks from the Laboratory. They actually had a garage I could put my car in and I stayed there until I got married the following year in July. So that's how I got out here.

I was assigned to work with Larry Harrison on one of the shifts. There were Larry Harrison, Ray Telesky, Ralph Thompson, Bob Crew, Cedric Eastburn and Dave Rogers all working on UNIVAC things. Of course, I didn't know diddily squat about computers or what they were but I remember the first time I walked inside the UNIVAC. Literally walked inside the machine and saw the Mercury Delay Tanks and tried to figure out what was going on. Which was fascinating stuff, it really was. I can remember talking to my other friends, who were not from this area, about digital computers and they were most impressed by these machines, just as we were. Things went on and it wasn't too long after that we were working rotating shifts of course and it was, I think, ten days on and four days off or something like that. Basically we were doing operations and maintenance and a little bit of modification and fooling around with stuff and trying to learn. I clearly remember one night I was now in charge of one of the shifts, probably about a year after I started there, and something happened and the machine just quit. It just stopped, as it did when the dual checkers didn't match; you know, it just quit. I said to myself "Oh, my God, I hope I can fix this thing." It was late at night and I didn't want to call anybody. I don't remember who was on with me but we were fooling around and we got the print outs and looked at them and sure enough we found a good old 25L6 that had gone bad. We replaced it and restarted and away it went. That was it, it was marvelous feeling: we'd actually figured it out.

I can also remember on some really cold nights, opening the bay doors and standing in front of the bay of tubes and

warming your back with all the heat.

In 1956 or '57 Lou Nofrey left and went to Marchant Calculators, Bob Crew went to Remmington Rand to work on the LARC and Chet Kenrich left to go to GE.

By this time Dick Karpen had left and gone to ERA to work on the 1103. A bunch of people left at that time to go with Lou. Telesky left, Harrison left, Thompson left. Jim Moore and I were kind of some of the older crew. I think John Efstathiou also left.

Marchant Calculators were building a computer for Spiegel Catalogue and that's where Lou went originally, and John Efstethiou went with him.

GAM: I didn't know that, I thought that John stayed and became our vacuum tube maker. He'd made a raster CRT with something like 39 cathodes.

EL: Well, he stayed for awhile, but he didn't stay too long because he went in there with Nofrey and worked with him and then Nofrey left and went from there and John stayed on. I'd forgotten about that, because I remember going in and talking with them about going to work for them and decided not to for whatever reason at that time. But that must have been in '56 or '57.

What really happened about that time was that Jim Moore and I were basically put in charge of this computer work because most of the people had gone and the LARC was coming along. I remember writing specifications for the LARC. In fact, Lou Nofrey was still here when we began that process writing specs for the LARC and reviewing all that stuff. It was probably in 1955. I remember seeing a phrase in one of the early specifications that implied something like it's supposed to be able to take all sorts of input and I'm a smart-ass and I said, "Do you mean even voice input"? Nofrey kind of looked at me and said "Smart ass kid." We didn't get voice input then, but I kind of remember that incident.

GAM: Well, we're getting close to that now.

EL: Yes, only forty years later. That process went on, but also during that same period of time, of course, the IBM machines began to come into the Laboratory. The 701's, 704's, 709's and then the transistorized versions, the 7090 and 7094. So the activities of our group were focused primarily on the LARC and the STAR after that. We also got into the business of trying to do something about automating some of the data stuff at the Laboratory, more broadly than that. Jim Moore was in charge of the LARC and he went back to Philadelphia.

GAM: That's where he met his wife, MaryLou?

EL: That's where he met his wife, right. There was a whole crew that went back. He and Glen Strahl, Jack Noonan, Ken Kinney, Dave Nielson, Bill Jones and I don't remember who else. But that was the crew that was back there and the folks that were left here, were primarily doing operations stuff on the then existing machines. I got into the business of trying to work with some of the other folks around the Laboratory and automating their experiments. I did some automation work for Chemistry because they were running all those samples. I wish I could remember the guy's name now. But anyhow he was doing that, and there was kind of a split in the two groups, even though they were the same, their functions turned out to be different.

GAM: Fred Strange, maybe?

EL: Fred Strange? No he came along later. Probably in the middle to late 60's, well after the Chemistry counting rooms were fairly established. What really got the instrumentation business going were the small computers, the PDP 3, 5 and 8.

GAM: Not the PDP 3, the 5?

EL: The PDP 5 was the first one I guess.

GAM: Yes, the PDP 3 was the 36-bit machine that they were

glad they'd never built.

EL: Yes, I guess the -5 was the first one, wasn't it? This was followed by the PDP-8 that became very popular around the Lab. And the -8 was followed by the PDP-11 which was the 12 bit version. The -8 was an eight bit machine.

GAM: I think all the PDPs you mentioned were 12 or 18-bit machines.

EL: 12-bits, yes. That's what got the experimenters around the Lab really interested. That was a really kind of controversial era inside of Computation you know, because Sid really wanted to control the growth of that activity outside of Computation. I think he was afraid that those folks were going to go off and do computing with small machines and he felt it was very inefficient to do that. He strongly believed they should be doing it in the center and so he really wanted to control any outside machines very carefully. And he did through us. He basically got us; us being myself, Glen Strahl, Ray DeSaussure and a few others; to be pretty hard nosed about letting those guys, whoever they were, buy small computers. It took a long time and of course the other engineers and scientific staff were getting educated in how to use them. Actually, once they got them in their laboratories they fell in love with them; they thought they were the best things going and they were right about that. So after awhile the job basically became "Let these guys do their own thing" and, at least from my perspective, because they knew what they wanted to do and they were real good at it and why shouldn't they?

GAM: Yes, I agree.

EL: I maintained my association with Computations because I was a supervisor of that group for a long time and I remember building a tape to tape converter. If you remember the tape to tape converter where we converted IBM to UNIVAC tapes because it was a different format and I guess it sort of worked and did the conversions but I don't think it lived very long. UNIVAC left and when the LARC came, Jim Moore and crew

went to great lengths to put IBM compatible tape on it. And I think it was a really smart move to do that.

GAM: Well, from the reliability point of view it certainly was.

EL: Well, also just from the compatibility of being able to transfer tapes from one machine to another. It would have been terrible to try to do some other kind of conversion process on that, at least I think it would have been a miserable task.

GAM: Well, there was an argument that they could do it with software but that really didn't work out.

EL: One of my recollections of that whole era was also of the beginning of CDC coming into the Laboratory. They came in with the 160 and then I guess the real big machines,

GAM: The 1604, then the 160, then the 3600's.

EL: They really became quite a dominant supplier and there was not much need inside the Laboratory for us as "Computer Builders" because CDC could do it a lot better and cheaper than inside the Lab. We just weren't geared to do that kind of stuff and so we should have stayed out of the business. Instead, we got into the business of building hardware for the OCTOPUS. And printers; the Radiation printer, you know and all of the peripheral stuff was kind of left to the inside to find out how to hook it all together and make it play. Challenging times, vacuum tubes were on their way out and transistors were on their way in. Lots of technical learning going on about how to use all of that stuff and put them together in a big system and make them play. I remember Don Rose and his fabrication facility in the basement of the building. He really cranked out a lot of stuff down there and it worked reasonably well.

I remember one of the first extensions of OCTOPUS was to put clocks, remember the central clock system that Bob Wyman built and hooked up to all of the machines?

GAM: Yes, The Wyman clock, I remember that.

EL: I'm not sure I know what that was used for but I guess probably for the operating system to keep track of who was on.

GAM: Right. Well it gave us a common basis for timing.

EL: I also remember in the early '60's when the PDP 1 came around.

GAM: There were a lot of interfaces on that thing.

EL: Yes that turned out to be a really interesting machine to have. It did a lot of work; putting the eyeball on; and the Voght camera.

GAM: Yes, the Voght camera, with Frank Armbreuster.

EL: And the Admiral, I remember the Admiral. Who's the guy that came after Armbreuster? Jack Newman?

GAM: No, Lane. Richard Lane.

EL: Was that who it was?

GAM: Down there, yes.

EL: They finally turned the camera into something that I guess was useful and Otto Krause putting the eyeball on it. Actually, he put the eyeball on a different machine too.

GAM: He put the eyeball on a 704 first.

EL: But, you also put in on there didn't you?

GAM: We put one on the PDP-1, yes.

EL: How did that turn out?

GAM: Well, it did all of the diagnostics and reaction history analysis for L-Division coming out of Nevada and their own Lab's in building 121. It was a good thing, but it had some difficulties, and it was your boss, Alex Stripeika who found that

we had, for instance, not used the linear part of the photo-multiplier operating range correctly. Our technician had set its operating parameters to be down on the knee of the sensitivity curve instead of moving up to the linear portion where it was much more stable and would have given us much better signal of noise ratio and stuff like that. But, with the cessation of testing, the Eyeball just didn't have any place in the test program anymore. Actually, by that time we had transferred such jobs to EG&G in Las Vegas. We helped them procure a much better Eyeball, the PFR-2 from Ed Fredkin's company, Information International, Incorporated. After that, all such digitizing was done in Las Vegas

EL: But, wasn't it the basis for the Data Processing Center that L-Division finally wound up building over in 121?

GAM: Yes, that also was one of the things, but first it was the basis for having that ultra high resolution eyeball, the PFR-2 down in Las Vegas,

EL: That's right, I forgot about that.

GAM: And we transferred everything over to EG&G to do the work there. I wanted to go on with the eyeball to get it into a more flexible state. To use the one we had, we had to have the data on film and I would like to have been able to work on paper. I wanted a different kind of sensor, but we could never get everything together. We could have gotten it if the management had said, "Ok, it's worthwhile doing." But I think in a sense they were right that it was not in the main line of our charter for the AEC. We developed the thing and it was interesting, and we did help get the big PFR-2 built for EG&G use in Las Vegas. This machine was super accurate, and was used for a long time. A few years later I met a guy, Phil Peterson, from the TX 2. Computer at MIT's Lincoln Laboratory. He had done the same thing on the TX 2. One of the splashy things he did was to scan a slide of the Mona Lisa, and then plot it out on a 30-inch Cal Comp plotter. He had built an eyeball almost exactly the same way we did.

EL: Totally independent?

GAM: Yes, really interesting. Actually, I believe he was slightly earlier than we were.

EL: Yes, how those things all evolve.

GAM: Well, there's this certain kind of simultaneity that spreads over many areas of technology

EL: Let's see, what happens after that? I guess about that time Wyman comes along in '62 or thereabouts and becomes a pretty strong influence in Computation. Wyman's a strong influence no matter where he goes.

GAM: Yes, he's a good guy.

EL: Yes, he is a good guy. A very good Engineer, he really understands it well, knows how to get things done, a good guy. Pehrson arrives in '65 and was very instrumental in a PDP-6. I think it was being prepared as the main OCTOPUS controller. Putting in a memory pagination and segmentation system.

GAM: You guys were designing the Pag-Seg (Pagination & Segmentation) unit and that was supposed to be added to the PDP-6. Wyman said no one ever used it.

EL: I don't remember, I really don't recall.

GAM: It was nurtured by Bill Mansfield from the programming point of view but I think the rest of the timesharing system was then so flaky that this part of it just didn't get much attention.

EL: That may be right, although there was an awful lot of effort that went into that.

GAM: It was a big, big device.

EL: Oh, I know, it was huge, just huge.

GAM: Well, that was more a commentary on the state of the integration and printed circuit boards and stuff like that, right?

EL: Yes, it was, the early effort at putting large quantities of IC's and transistors on single systems and making that all play and keeping the noise down. It was a big engineering job that none of us had ever tackled before.

GAM: It was very tough and we sort of paid the penalty in reliability.

EL: Whether it actually ever worked or not I guess I just don't remember.

GAM: Well, Wyman said it worked, but that no one ever really used it. In other words, the timesharing system did not take advantage of it because it was busy trying to solve even more basic problems. The PDP-6 was a little bit flaky too, because we put a maverick memory on it. But by the time that we had moved to the replacement for the PDP-6, the PDP-10 and things settled down, the Pag-Seg unit was passÈ.

EL: Yes, but all in all, those were interesting and fun times because of all the new technologies and new systems that were coming along. There was a large array of ideas about what to do and how to do it. I think one of the frustrations of the thing was to keep all those systems up and running so the users wouldn't complain so they could get some real work down while you were doing all this development work simultaneously with trying to keep an operating system working.

GAM: Along about this time too you were going further up into the realms of management here. You were Sid's mainstay, almost, right?

EL: Yes, I had taken on some responsibilities that were outside of Computation. I still maintained close contact with Sid and, I guess in a way, was kind of a sounding board for him about what to do. I made sure that at least the engineering side of his business was not going to interfere or conflict with what it was

he wanted to do. I saw that he had a staff of technical engineering people who knew what the game was all about and were willing to work. And I maintained that contact with Sid all the time. Sid and I always seemed to understand one another; I never had a real confrontation with him. We got along well. I never thought Sid was a particularly good manager, but I thought Sid was a marvelous leader. I thought the way he ran the Computation Department per se, left an awful lot to be desired, but I certainly have a lot of respect for him as a leader in advancing the state of computing in the country. Not just at the Laboratory, but in the country. He really was a leader.

GAM: Well, that's true; you find his footprints all over the country.

EL: Yes, I'm not surprised at that. I always felt that the Laboratory did him a great disservice by putting him in the closet after he left Comp and some of that was maybe Sid's fault, but I always felt he was a resource the Lab just wasn't using.

GAM: He was very active on the outside after he was removed from being Director of Comp. He had his contacts in the National Security Agency and the Defense Department and the National Science Board. All these other places in Washington; he was there an awful lot. I think he kept his influence up there pretty well. A lot of people in the DoE management headquarters had the same feeling you just expressed; they felt he had been handled improperly by Lab management. But I don't know how to evaluate something like that. The problem was clearly related to the STAR and our, perhaps, too long attempt to get it to work. I don't know, but it seemed to need somebody to be the fall guy and he got elected.

EL: Well, I guess in some sense he has to share part of that blame, it did happen on his watch.

GAM: Well, yes, I've tried to say it this way; the STAR was not bad, neither was the LARC. In some sense the LARC did not turn out to be as influential as we thought it should. The

LARC arrived on Sid's watch, but no one blamed him for that and one STAR would not have been his blame either, but when it was two Stars it was harder to handle. Physics went to a standstill in some sense while these guys tried to figure out how to use the STAR's effectively. It was a very unbalanced machine and we were used to balanced machines, which meant that some of our own errors would be glossed over because the machine covered our mistakes if it was well balanced. That's largely because it was Seymour's policy of getting things in balance. But the STAR wasn't balanced, they didn't even know how to do it, I mean the scalar unit was much, much, too slow for the vector unit and I/O wasn't all that sensational. But it did introduce the era of vector processing into the world and it gave the laboratory an excellent start on compilers for vectorizing machines. That style went into the best of the compilers that Japan developed as well as the people at CONVEX in this country. So I don't think it was a total loss.

EL: No, it wasn't a total loss.

GAM: But that's where he got his, so to say come-up-ance.

EL: Well, I knew that the STAR was kind of the root of all the problems and then I think by that time he had probably developed a sort of adversarial relationship with the design divisions in the Lab. They wanted something that he couldn't deliver and I'm sure they felt he was too dammed autocratic about it. He was in charge of Computations and he was going to tell them what they needed and all that stuff. I suppose there's some truth on both sides of that issue. My real concern about him was that when he got basically removed from the head of Computation, the Lab just didn't seem to use him or his expertise for broader issues at the Laboratory in Computing. Maybe that's because he bent his pick with the Director.

GAM: I think that's a reasonable observation.

EL: And I always felt sorry about that because I thought he had a lot to contribute. Sid could be pretty caustic and cynical at times but in a way that was sort of his charm too. I always felt

that with Sid if you had a legitimate question, an honest question, he was more than willing to take the time and sit down and talk to you about it and try to arrive at a reasonable solution. I never found that you couldn't talk with him about something.

GAM: I agree, very nice. Well, after Sid left you sort of had no more contact with Computation?

EL: I had very little contact with Computation after Sid left. I got back into contact with Computation on the PC business when they started to come into the Laboratories. The IBM PC kind of, quote, became the sort of standard that we were willing to support. Engineering and Computation got into this conflict about who was going to be in charge of it and all of that stuff. Hank McDonald and Gus Dorough had all these meetings and I think the issue really was that "yes, Computation should do it, but they haven't been able to do anything right around this place for a long time so let's just do it and make it happen." It was one of those overlap things, the engineers got involved in that and they could write code and the Computation people felt that because it was computers they ought to have them. So I got into the middle of that and tried to make some sense of it and keep it going. Finally in the end that whole process got turned back to Comp in about 1990 or 1991 just before I retired. But it took a long time to do that and it took some of us to get together and say why don't we do it right. Why don't we join forces, stop the fighting, stop the turf issues, lets do it right for the Laboratory. As it turns out, I think it probably turned out OK. I don't know what's happening now, I haven't kept track of it since I left, but, at least, it was worked out.

GAM: I think that everyone was always trying to do what's right for the Lab, maybe that's the trouble too much insisting what's right. It was somewhat of a question of who had the best vision, but I think everybody was trying to do the right thing. As it sits now, I see this sense of community as disappearing. It's no longer a Computation Department that is good for the Lab. Everybody has his own little machine; they're really not interested in any of the guidance that Comp might provide.

EL: Too bad, because there's just a wealth of knowledge and experience in Computation.

GAM: There was, I'm not so sure how much is left now.

EL: Well, I don't know what it's like today, but there was when I was still connected with them.

GAM: Well, you actually got to be head of the whole Electrical Engineering Department at one point didn't you?

EL: Yes, in the late '60's I had taken on this Division Leader role that included the support to Comp and the support to the Physics Department, and I don't remember who else at the time. Then in March of 1973 there was a big blowup in the Laser Program between EE and John Emmett and, of course, if you ever had the pleasure of seeing John Emmett in action it's a marvelous thing to see if you can view it from a distance. So they asked me to go out there and run the EE activities for the Laser Program.

GAM: Ah, yes, I remember that now.

EL: I wasn't there very long. I was only there about six months. I think we got some things straightened out. Some things may never get straightened out, but I think we at least developed a relationship with Emmett that was supportive of where he wanted to go. Part of that confrontation was that there were a bunch of PhD EE's who were quantum electronics' folks and they believed that they knew as much about building lasers as John Emmett did. But, John Emmett was in charge of the Laser Program and there was just this simple confrontation that occurred.

GAM: I think that's the right thing for Scientific progress, but it has to be done with the right emotional attitudes too. I agree.

EL: Anyhow, the up-shot of that was I went out there for about six months and smoothed it over and got some things going and, hopefully, Emmett was satisfied with a part of that. Then,

in August or September of 1973, the Lab decided to make two new Associate Directors. One for Chemistry and Computations, who turned out to be Gus Dorough, and one for Engineering, who turned out to be Hank McDonald. Up until that time Chemistry probably reported directly to the Director and Engineering reported to Duane Sewell. I think Wally Decker was instrumental in reorganizing this in some fashion. Ed Hulse was the head of EE for a long time. He was an interesting person himself, and he gave me a lot of opportunity as he pushed me along. I remember one time, he called me into his office on a Friday and said, "I want you to go over here and do this job. Think about it over the weekend." So I went back on Monday and said, "I'm not so sure I want to do that." He said, "Well, you're going anyway." That was the era when if your boss said, "you're going," you went. There wasn't any such thing as "I don't want to go." I don't know; I'd like to see some of that come back. It was in 1971 that Hank McDonald became the head of EE and then in '73 when he was named the Associate Director I took over the EE Department and was Department Head from there until December of 1986. Then I worked for the new AD, at that time Dennis Fischer, trying to coordinate some of the common activities of EE and ME because those two organizations were competing for resources and prestige and everything else and had for a long time and there were overlapping activities. Also, at that time, it was decided that the Engineering Research Program needed to be run out of the AD's office as opposed to being run out of those other two Department offices. So I took that over and tried to combine it. Even though the work went on in the two departments, it was managed from the AD's office. Basically, I stayed in that same role until I retired on October 1, 1991.

GAM: Well, I hope you're enjoying your new staus, and I want to thank you for a marvelous revisiting of our earlier years.

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