Oral History of Panel on the Development and Promotion of Fairchild Micrologic Integrated Circuits

A chronology of major events referred to in this session plus information provided in later correspondence with the participants is provided as an addendum to this transcript.

Participants:
Isy Haas
Jay Last
Lionel Kattner
Bob Norman

Moderator:
David Laws

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[Note: text in square brackets was added by the participants during the editing process]

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David Laws: We're here at the Computer History Museum in Mountain View, California. It's October 6th, 2007, 50 years after the founding of Fairchild Semiconductor, and we have put together a team of key contributors to the Micrologic integrated circuit development project, the first monolithic integrated circuit developed by the industry. We're going to spend a couple of hours trying to recall as much of that history as possible. To get it down on tape, so in a couple of hundred years researchers can come back and find out what really happened in those early days of the integrated circuit business. I'm going to ask each of you gentlemen, starting with Jay Last, to give us a brief summary of your background, education and how and when you came to join Fairchild. Jay?

Jay Last: I grew up in western Pennsylvania and went to the University of Rochester and got a bachelor's degree in optics, then went to MIT and got a doctorate in solid state physics, which was a relatively new field at the time. I then went to work for Shockley, and the story has been told many times. A group of eight of us left and started Fairchild in the fall of '57. Our first efforts, of course, were to build our first saleable product. After a couple of years, the technology and the timing seemed right to start working on integrated circuits. I set up a group and directed it for the next year and a half, at which time we had our first successful product.

Laws: Thank you. Lionel.

Lionel Kattner: Yes, I graduated from Southwestern University in Texas, with a degree in chemistry, physics and math. Shortly thereafter, upon graduation, I went to work for the Hanford Atomic Products Operation plant in the state of Washington on the production of plutonium for nuclear weapons. After four and a half years there, I joined the navy to get the military service over with. I was in charge of the nuclear components, as a nuclear officer there, for three years. Upon my release, I was looking for something else to do. I did not want to go back to Hanford, because the nuclear business then was kind of in a downward spiral. It was very unpopular. I accepted a position with Texas Instruments; I think that was in 1958, as a chemist on one of their product lines, for point contact germanium mesa type devices. Shortly after I solved some of the problems and then became a product engineer on that line. After almost two years there, Jay came to Dallas, interviewing some people, and I happened to luck onto him, I guess because at TI, in my opinion, I really wasn't learning anything. They were not very technologically advanced, and so I decided to join Fairchild. Then at Fairchild, I was part of the original microelectronics group under Jay.

Laws: Isy?

Isy Haas: My name's Isy Haas. I was born and raised in Istanbul, Turkey, and graduated from Robert College, an American college in Istanbul. I came to Princeton for graduate work in 1955. I got my master's in engineering and solid state physics in '57. I completed the class work for the PhD program, but quit without doing the required thesis, and went to work for a company in Philadelphia called Remington Rand Univac as a circuit engineer. Where I designed "tape read/write circuits". Univac was the company founded by Presper Eckert and John Mauchly who built the Univac computer for the University of Pennsylvania. I worked there for a year, and decided to leave the east coast, because our personalities didn't quite agree with each other. I wanted to get to the west coast. I applied for a job at Fairchild, and interviewed them in Columbus, Ohio. Bob Noyce asked me if I would be interested in miniaturization, and I said yes, even though I didn't know what it was. I started working for Fairchild in

July '58 for Vic Grinich in device evaluation. Vic hired Bob [Norman] to head device evaluation; I was told to report to him but transferred to Jay's group in 1959 to work on the development of the integrated circuit. I left Fairchild in '61.

Laws: Bob.

Bob Norman: My name is Bob Norman. I graduated from Oklahoma A&M with a BS in Electronics Engineering and Math. I went to work in my undergraduate year for Sperry Gyroscope Company in the Advanced Weapons System Development Department. They had set up a section to study and build a digital computer and give them knowledge in that area. I was the transistor person in my undergraduate time there. Thereafter, I rejoined them and the net result was, in '57, we shipped four different computers to the military, two Polaris computers and a Sergeant coordinate conversion computer, and a Puffs underwater sound system computer, all using transistor-resistor AND logic, which was a Sperry patent, and some DCTL for high performance stuff. One used core memory, and the others used drum memory. We started using Fairchild transistors, the 2N696, for write amplifiers on both the core and drum memory. That was an excellent transistor but it had the most excellent attribute, which was, when we ordered them, they were delivered. That was very unusual for the industry at that time. Howard Bobb was the salesman. He recruited Don Farina who was working for me at the time. He brought him out to Fairchild, and then Don talked Vic Grinich into going after me for the microelectronics. So they came and asked me to come out, [for an interview] which I did. I spent time with Bob Noyce and Vic [Grinich and Ed Baldwin] and many people, talking about what they were doing with planar technology, and putting metal interconnects on the surface of the device, which all were very attractive to me. I talked about my prejudices on what kind of logic would get the most reliable computers and be the most compatible with their technology. I came out here [to work] in August of '59, and Vic asked me to head up the device evaluation section, and as part of that, to do the engineering side of the development of Fairchild's integrated circuits.

Laws: Thank you, Bob. As I understand it, there were two key groups involved. Jay, you ran the product development group, and Vic Grinich, another founder, headed an applications group. Would that be right?

[**Note from Bob Norman:** Vic Grinich was Vice-president of Engineering and ran Device Evaluation, Integrated Circuit Engineering, Applications Engineering and Test Engineering]

Last: That's right, yes.

Laws: Members from both teams worked together?

Last: Yes.

Laws: Could you describe a little more about your team? What were the skills of the people? How many did you have? How did you relate to Vic Grinich's group?

Last: The problems we had to solve were diffusion problems, and work with steadily improving the masking processes and the photo resist. So we had people working in all these areas. Lionel was the key person working on device fabrication, doing the diffusion work. Isy was working both from the applications point of view, and the fabrication point of view. There were other people, including Sam Fok who was doing chemistry work and working on photo resist. Gary Tripp was working on device fabrication. Jim Nall joined us and was working on making mockup devices, individual devices, and working on mask making and photo resist. Later on, near the end of the program, Jim Campbell, who was from MIT was doing work on long time diffusions. There were a number of other people who came and went in the group. Essentially, it was under a dozen people.

Laws: Lionel, did you get a lot of direction in the task you were supposed to accomplish?

Kattner: Well yes, in the beginning, I think so. I spent a lot of time talking to Jay, because I knew absolutely zero about silicon devices at the time, since my past was all in the germanium mesa work, so I had a lot to learn. But then I spent a lot of time just with people in the lab, learning the processes. I started in July of 1959, and probably for the next three months, basically, and almost the fourth month, just spent most of the time learning diffusion, photo masking, all of which was previously very foreign to me. During that time was when Jay started directing some of the efforts to what he wanted to see accomplished. One of the first things was to try to put together a crude but early integrated circuit using the available technology that they had at the time, planar technology. At the time, the pure monolithic type technology was not available to us, so we kind of did some work of doing physical isolation of devices, rather than the junction type isolation. So that was that period of time.

Laws: Isy, you worked in Vic Grinich's group, and then you transferred to Jay's group. What was your role in those tasks?

Haas: When I joined FSC, I was hired to work for Vic Grinich in device evaluation. [Initially I worked on avalanche switching and *pnpn* four layer devices; my work consisted of understanding and defining how the devices operated, the physical phenomena involved and conceiving applications and studying their performance. I also designed and built test equipment to test the devices. I published and presented several papers on these subjects. In the fall of 1960 I taught a Transistor Physics course for the UC Berkeley Graduate extension program.] I started working with Lionel and Jay, under Vic Grinich, on the electronics of integrated circuits, testing and evaluating, but primarily doing different studies relating to how transistors work in the integrated circuit environment. [The challenge was to study how one might expect a multitude of transistors (more than one) to perform as a circuit to a given set of electrical parameters when they are picked at random and/or when they are neighbors on a silicon wafer!]. As Jerry Sanders was saying yesterday, we were all young, and “full of piss and vinegar”. [During the initial phases of the Micrologic Program, Lionel was building the physically isolated flip-flops. In this program the components were separated from each other by chemically etching them apart] I was testing them, [We were getting good results with operating flip-flops but the life expectancy of these units was very poor; this was due to thermal expansion and contraction. In the fall of 1960] I got this bug: “Why don't we just make the integrated circuits with diffused isolation?” I recorded this concept on page 127 of my patent engineering notebook, on September 29, describing the process. I note this, "This sounds to me like somebody else talked about it before, but I don’t know who." I think I was not familiar with Bob Noyce's patent, but somehow, it rang a bell. [Somehow I was under the impression that the concept was not my original creation but I was determined to seriously explore the possibility] Lionel and Dr. Sam Fok witnessed this patent note book entry, and that was September 29. [This event is the beginning of diffused isolated integrated circuit and I immediately spearheaded the effort to build a flip-flop with Jay's knowledge. Lionel was very helpful and we] started working very close. I did the diffusion calculations which came out to 45 microns in 18 hours [to isolate a 75 micron wafer within our existing furnace setups.] It took about three weeks to get a working flip-flop. The first entry in my notebook was, "We have transistor action." And the next entry was, "We metallized the transistors and all we got was shorts." The next entry, October 10, I was testing the first integrated circuit. After that, Lionel would build the circuits and I would figure out what was wrong, tell him what I thought was wrong with it and he'd fix the process. A couple of months or so later, things started coming out again. [It was a key factor that I was there and able to correlate device physics, electronics and process. Otherwise we would have had the blind leading the blind]

Laws: So there was a lot of iteration backwards and forwards to get there.

Haas: Yeah, a lot.

Laws: Bob, in the meantime, you were in Vic Grinich's group. Your role was to help define the circuit that would be implemented in the IC.

Norman: Yes. I had recommended the logic form and type of circuitry to use while I was being interviewed. Like Isy, I was unaware of any of the patent stuff that I really learned about over the last year, I think. At that time, we were not focused on patents, unfortunately, Somebody was, people like Roger {Borovoy] kept everybody honest. We were just focused on getting things done. Again, Vic kind of supported what I had proposed doing from a circuit standpoint, and it then became -- to me, it was simple. What did I know about three dimensional devices? So I did derive what resistors to use and so forth from a circuit standpoint. There turned out to be problems with things like overlap diodes and so forth. But the amazing thing was that everything was so quick. We'd find the problem and one side or the other, or both, would solve it, instantly. It was amazing to me how quickly we were able to get things done. One of the fundamentally important attributes from an electrical point of view of the planar process is that the transistors that we made fit very well with Ebers and Moll equations for transistors. The earlier alloy junction [devices] and so forth had enough problems with them from a mathematical standpoint, that it was an approximate fit. But Don Farina did all this testing that showed over many decades of current, [planar transistors] fit the Ebers and Moll model, which made doing the circuit design a lot easier. [While diffused transistors had a wide variation, their temperature coefficients were much more consistent than composition resistors] Up till then at Sperry, all the design I did was more statistical design, but in this case, we were able to start doing actual circuit design and could actually expect the parts coming out to look something like the circuits we were designing for. That was a remarkable part of what we were doing at Fairchild. Incidentally, part of my respect for Fairchild was that I was using Fairchild four layer devices. I'd been given a job at Sperry cutting the power down in the Marine Corps PPS portable radar. The radar was portable. You had to carry all these storage batteries around to run it. I was using the Fairchild devices as the modulator on the transmitter, and also as a switch on the klystron which cut the power substantially. So I'd had experience with the kind of work these people did when I wound up talking to them. [When I arrived in August '59, we quickly established the circuit form (DCTL) and the logic form (NOR) and the concept of a compatible family of devices to build military (including airborne) digital computers. Our half-shift register, "S" Element, emulated the Philco Master-Slave approach to implementing logic absent lumped constant delays to avoid race conditions. The early TI offering still required many discrete components to implement ordinary logic functions. Our principal departure from Philco was to avoid the use of series-transistor implementations of gates.]

Laws: Thank you, Bob. So Jay, you were there, and the other gentlemen were pretty much hired to help on the IC program. Could you give us a little insight into how the program got started at Fairchild? Were you aware of what was going on at TI with Kilby? How did the whole project get kicked off?

Last: Well, this was in the 1959 era. There was an enormous effort, all over the country, an enormous interest in micro-miniaturization for airborne devices, and every company that was involved in this had some kind of a program. It usually involved taking individual devices and making them smaller and smaller. It ran from that to the other end of the spectrum, where Westinghouse was going to have Molecular Electronics. or some name like that. They were going to fabricate everything, every sort of a device by building it up. None of these programs worked. In the summer of '59, I think in July, we had started seeing what TI had been doing, some of the work of Kilby, and they were talking it up at great length. Bob Noyce came to me just before WESCON [Western Electronic Show and Convention], which was in August, and said, "We have to show the flag there. We have to have something there that shows we're working on this stuff." We were very nervous about TI, because they had a record of getting patents and establishing a patent pool, and then beating everybody else over the head with this patent pool. We didn't want that to happen to us. In about three days, I put together just a simple flip-flop, four transistors and resistors. I used a pencil to make the carbon resistors

Laws: A hybrid?

Last: The Ticonderoga process. So this got quite a bit of publicity.

Laws: You demonstrated this at WESCON.

Last: We demonstrated it. We didn't say much what was in it. We just said we had these four transistors and this little device. Bob and I talked, and he said, "The time is right now. I think we have the technology to go ahead and make a formal program on this, start doing this. Why don't you put together a group and get it going?" As has been said, the technology for isolation [did not exist], [Note from Bob Norman – Noyce's patent contemplated diffusing collector tubs into an intrinsic silicon substrate] We knew if we were doing it by diffusion, we were going to have to use a boron diffusion. We were having trouble then with a 15 minute diffusion. This was going to need a 20 hour diffusion. So we said the technology's not ready for that. But the real point we wanted to demonstrate, which was the heart of the Noyce patent, was that you can interconnect devices on the surface of the silicon. We decided we'd make a device where we would provide the isolation by turning the device over, etching away all the silicon until we got down to the oxide on the other side, and then back fill with some appropriate plastic or chemical. It sounds now like, why would you ever do something like that? But our aim was to demonstrate that we could indeed make a monolithic device this way.

Laws: So the original objective was really just to show the world that you could build something, and then productize it later.

Last: Yeah, and see where it would lead to that. This was not a big effort ever, not a very big high on the Fairchild list, what we were doing. It was a minor R&D thing. Fairchild was having extreme success making transistors and diodes at the time. All the focus of the company, rightly so, was on that. This was just a little side project. The way the company looked at it can be seen in Gordon's remark a couple of years later. "We made the integrated circuit. Now what do we do?" So we just started, and it was going to be a brute force device. Later on, I had talked to Charlie Sporck and told him about this device. "How would you like to have this physical isolation device in production?" He came out with a very typical, profane Sporckism. He told me what he thought of that process.

Laws: I can just imagine.

Last: Yeah. So we just started, and Lionel was instrumental in starting and making the thing.

Laws: Who set the schedule? Was it, "We've got to get it done in six months?"

Last: No, there was no schedule. It was an open ended thing. I was about the only one at Fairchild who was working on things that were not related to our present product line. I worked for several months before this device on parametric amplifier devices, where you etch a pit and make a little dot down the center, which never went any place, but that was the sort of things I was working on.

Laws: I was looking through a paper that Bob Noyce wrote about that time. He identified what he thought the most important product developments going on in the company were. He said integrated circuits, parametric amplifiers, and tunnel diodes might be important. In spite of it being a relatively low priority project in the company at the time, you were allowed to hire some very talented individuals.

Last: Yes.

Laws: Did you drive for this?

Last: Fairchild, at that time, was starting to make a lot of money, and money was not the object, or the constraint. When we had a matched NPN and PNP device, we had the world. By that time, we had the planar PNP device, which nobody else could figure out how to make. So that was a very big source of income. I never felt under any financial pressure. Nobody said, "You have to do this with less people." It was just, "Go ahead and do it." It was sort of benign neglect. The money was there, but nobody cared much about it.

[**Note from Bob Norman:** The problems Jean had making planar PNP devices led to two major markets for Fairchild. As part of our Device Evaluation work we observed anomalies in the collector characteristics of these devices which turned out to be due to the emission of light from the junction. We use this phenomenon to build silicon light emitting arrays which were incorporated into Fairchild satellite cameras to add digital data to the film they were shooting. Gordon put Irv Solt in charge of electro-optical R&D. Meanwhile, C. T. "Tom" Sah, investigating channeling on these transistors developed the Surface Controlled Tetrode, which we evaluated. The device Tom developed to investigate drift in the gate input voltage over time we now know as the MOS transistor.]

Laws: Fascinating. I've never been in that situation.

Last: I never have since.

Laws: There are other people who either showed great interest, or contributed, or were responsible in some way. What were the roles of Gordon Moore and Bob Noyce during this time? Bob wrote the patent and went off to run the company, basically?

Last: Bob, to my recollection, never set foot in the lab where we were working. He had nothing at all to do with it. He was busy running the company. Bob's role was always the outside man. He was there, as he was at Intel, and he wasn't always around that much. He'd be in Italy or someplace, and he was a very effective spokesman for the company, obviously. He was very good at identifying customers and bringing them to the fold.

[**Note from Bob Norman:** Bob Noyce was heavily involved in what we were doing. He ran R&D for some time after Ed Baldwin left. He was interested in what we were doing with "his baby". They had a further interest because we were spending a growing amount of money in development and particularly test with no offsetting income. Vic, who reported to Bob was heavily involved in the project.]

Laws: And Gordon Moore?

Last: Again, Gordon was running R&D was focusing completely on other projects. I mean, he was aware what we were doing, but made very little comment on it.

Laws: Were you reporting to Gordon at this time -?

Last: When we started the program, Bob was running R&D, but when we left, he was running the company.

Laws: How about Jean Hoerni? What was his role?

Last: Jean had played his role in developing the planar device. So we were aware of improvements in the planar process. We were all in one room, essentially, in one bay, so ideas from one section would go to another one very quickly. He was never focused directly on the integrated circuit, but improvements in planar transistors were also spread along.

[**Note from Bob Norman:** John Hoerni was heavily involved in the isolation issue, looking for ways to improve the low manufacturing yield. When Motorola announced the epitaxial process, John was immediately interested for his power transistors. Unfortunately the epi produced look like orange peel. We promoted the use of epi for isolation since unlike power transistors we only needed a low-voltage epi. For power transistors, John tried unsuccessfully to use backside diffusion to improve thermal conductivity of the collector.]

Laws: Now you were all in the 844 Charleston building at this time?

Last: Yes.

Laws: All in the same building, on the same floor, and were bumping into each other all the time.

Last: Bumping is putting it mildly.

Laws: How many people were in that area at that time, and how well equipped were the labs?

Last: It's hard to say how many people were there. We had side buildings. The big contribution Ed Baldwin had made is that, before we'd made our first device, he went to Fairchild management and said, "You have to put up a big building, fast." That was the Whisman Road building, production had moved down there. This was all R&D stuff.

Laws: I think we've set the scene here in terms of who did what, and what the roles were, and we've touched on some of the challenges, but I'd like to tunnel in a little bit more on some of those now. There were many major technical challenges you had to resolve find work around, climb over, or whatever. Electrical isolation was obviously a major issue. You all mentioned it in one context or another. Let's take that one first. You mentioned the first approach was physical isolation.

Last: Yes.

Laws: And then Lionel and Isy both worked on an approach to doing a junction isolation?

Last: That was considerably later.

Laws: What was the timeframe of these? The first was the physical isolation.

Last: The first was the physical isolation. We'd started on that, really formalizing on a device about December. We had the first one--

Laws: This was December, '59?

Last: Fifty-nine. By May, we had made the first device. Then in the fall, Isy and Lionel made the first diffused [isolation] device.

Laws: So that was fall of '60.

Last: Yes, so it was about a year. Lionel can talk about some of the problems with the physical isolation.

Kattner: That was really the first approach. I did want to point out one thing that hasn't been mentioned either, is that although TI had made a lot of noise about having what at the time they called Solid Circuits, I think one of the key differences was the fact that Fairchild had the planar patent and the planar technology. TI did not. So all of their devices had wired interconnects. But what made the physical isolation feasible was the fact that, as Jay mentioned, we etched all the way down to the oxide layer on top, and then back filled. We tried many different epoxies, materials called Sauereisen, which people don't even know what it is any more. I'm not sure I do either. And then polycast with some of Corning's materials that they'd come out with. The effort was to find something that had a coefficient of expansion similar to silicon, because you didn't want, under different temperatures, stressing itself. In the beginning, that did occur, because of coefficient of expansion characteristics, the oxide layer was broken, and therefore the leads, the interconnects, that were evaporated on the surface of the oxide, were broken as well. Ultimately I became very discouraged with it, because I thought, "This is never going to really work." But for demonstration purposes, we forged ahead, and ultimately, I think it was around March of 1960, we had some semblance of a device that did work. It started with flip-flops, since it was the simplest. By May of that year timeframe, then we had a sufficient number of devices to say that we had a product that could be produced, but nobody ever felt it was going to be ultimately a production device, or method. I know that in August, I think it was July and August of that same year was when I spent a lot of time with Dave Allison, trying to learn more about the diffusion processes, and even spoke to him about doing a collector type diffusion rather than this isolation even. He said, well, he'd thought about that, it had been batted around a lot in the past, and that it was part of Noyce's patent. Then the other thing he said had been talked about was this sort of electrical isolation of some form. I think even Jay had mentioned that Gordon Moore or somebody had talked about that in the past, but never really considered it on a serious level. So I think from that conversation with Dave Allison is when I first took it kind of serious, like maybe this can be done. I know an entry I had in the notebook was August of that same year, August 27th, I actually sat down and ran calculations trying to figure out how long it would take, but we never did anything with it at that point in time. So in the meantime, we were making more more of the physical isolation devices, but then most of my efforts then started focusing on the ultimate of doing some sort of a junction type isolation of devices. That was when, in September, didn't really try any effort of trying to diffuse it, mainly because there was no diffusion capacity available in the lab. Most of it was consumed by other work. The furnaces were limited, and also, due to the fact the calculations showed it would be 18, 20, 24 hour type diffusion, that would have to be done at night and weekends. So that was the effort that finally took place, and that was done largely in September. Then I think the end of September is when some of the first workable devices -- or it looked like they were workable. And then later in October and on into November is when we made a few more. Most of them weren't any good, because we still had a lot of problems. A long diffusion like that created a lot of surface destruction and pitting and so forth.

Last: And we were also limited by our masking ability. Things would be misaligned regularly.

Kattner: Almost daily.

Norman: The long diffusion made the wafers very brittle, so if you looked at them cross-eyed, they'd shatter.

Laws: Somebody last night described them as potato chips, I think.

Kattner: You had to make the wafers very thin, and they were so thin, you could hardly handle them. So that was the other problem, but we overcame those problems ultimately.

Laws: Isy, you mentioned you made notes in your notebook about junction isolation processes. Were you aware of the work of Lehovec at Sprague at all during this time?

Haas: No, no.

Laws: He did get the first patent on that technology.

Haas: On what?

Laws: Junction isolation.

Haas: Oh, he did? [I was not aware of Lehovec's work, but as I said above, I was under the impression that I had heard of the concept. My position at the time was: this concept is a good idea; it my not be a "slam dunk" but I want to give it a try]

Kattner: Yeah.

Last: Yes. It's very interesting that the three key things were:, How do you put two devices on one substrate? That was the Kilby patent. How do you interconnect the devices? That was the Noyce patent. How do you isolate the devices? That was the Lehovec patent. And these three guys had never met or had any contact with each other.

Kattner: Never heard of him [meaning Lehovec. I heard that a patent had been issued but did not know who] [Laws input - Lehovec, Kurt. "Multiple Semiconductor Assembly" *U. S. Patent 3029366* (Filed April 22, 1959. Issued April 10, 1962)].

[**Note from Bob Norman**: People don't appreciate that workers in the field are busy working and are usually unaware of work going on elsewhere in the industry. We were unaware, which led to overlapping inventions. It should be remembered that Kilby's patent described a transistor and the resistor on one substrate, not towo transistors. Noyce' patent described more than one transistors diffused into an intrinsic substrate as Lionel described. These were self-isolating.]

Last: Incidentally, Noyce had filed a patent for the diffused isolation. It ran interference with the Lehovec patent and was thrown out. So Lehovec got the patent. And Sprague had so little interest in this that Lehovec had to pay for the patent with his own money.

Laws: He had to pay for that. I believe he got a dollar commendation from the board of directors or something.

Last: Yeah. He's still a little miffed at the way that they had treated him. But this is just an example. The time was right. That's the history of most inventions, I think. The time is right and everybody puts their little bit into it. The one that's looked on, the inventor, is the one that realized the time is right and the world wants it.

[**Note from Bob Norman:** Inventions are solutions to problems. Disclosures usually include prior art. Patents recognize and protect inventions.]

Laws: And/or has the best PR outfit.

Last: Yes. It's the sort of thing, if we hadn't done it, it certainly would have been done sometime within the next couple of years, or five years or something.

Laws: Isy, there's a question that came up last night and someone answered it anyway, but do you remember the kind of tolerances you were working with?

Haas: One mil.

Laws: In microns, what is that?

Haas: Twenty-five.

Last: Now you put half the world in 25 microns.

Laws: Absolutely. And the challenges of masking these kinds of tolerances, who would like to address that?

Haas: Are you aware of how we used to mask things?

Laws: Please tell us.

Haas: Well, the glass mask was clamped between two square frames with a square hole in the middle; the wafer [had one flat side and] was wedged up against the corner of the frame. That was the reference. So one mil was just about the best you could do in realigning the subsequent masks.

Last: If you look at the early wafers, they always had a big, flat section on one side.

Laws: I have some upstairs. I didn't realize that's why it was -- I understand Sam Fok had a major role in some of the lithography, and there were some struggles with photo resists.

Last: Yeah, and Jim Nall was involved. And of course, we had to line up the front with the back, and the silicon, you couldn't see through it. But silicon became transparent in the near infrared, so we developed an infrared alignment device, where we could use the infrared to look through the wafer and see what the pattern was on the other side. That was the brute force extent we were going to, to try and make these things. But Sam and Jim Nall -- Jim Nall was mainly working on that. But we developed a pretty sophisticated way. But with these tolerances we're talking here, and a fairly large device, it needed an enormous amount of work. Developing the more sophisticated photo resist was a major activity in the company, throughout the company.

Laws: Was there somebody leading work on the photo resist problem?

Last: That was in my area. Of course, they were working in pre-production trying to improve the process, but for any new things, it was the group that I'd mentioned: Sam, and Art Engvall was involved in that also. But it was a steady problem. Like all the other problems, we just sort of-- brute force. And the photo resists gave problems too, starting with a resist that was used for making printed circuit boards, and we had a lot of cooperation from Kodak. We'd feed information back and forth, and we finally got much finer structure to them, so we could start making these tighter tolerance devices.

Laws: All these variances in what the device is going to look like, Bob, did that put a lot of constraints on the kind of circuit that you were trying to design and optimize for?

Norman: I'd learned long before to try to use what I'm handed to work with. Actually, as I said earlier, the physical consistency of the electric parameters to the device structure was actually better than I was used to working with in the past. That consistency made it easier to do circuit design. It was a lot more straightforward, and again, because of the consistency with the Ebers and Moll equations, that helped that. To me, it was really a novel thing, part of it because of my ignorance. I didn't know what these guys had to go through to make these things happen, but they were a lot more consistent than stuff I had dealt with, with discrete components before that. I wouldn't let on that.

Haas: I can add a little bit to that. Much of the work that I did before and after the diffused isolation related to understanding whether or not you could get [a circuit to operate to a given set of specifications, namely a given fan-I and fan-out) when a number of] devices picked at random were connected. Up to that point, the customer would specify input voltage, output voltage, gain and all that. If the device was within those parameters, the circuit would work. [The question was:] when we use all the devices on the same wafer next to each other, would they or wouldn't they fall within a desirable distribution of gain and VBEs and V-sats etc. So most of my work, except when I was troubleshooting the process was doing statistical analysis on randomly picked devices, adjacent devices, performance over temperature, distributions, the effect of different power levels on speed. [Compliance with military temperature ranges was a must. Jean was interested in the same statistics because of his interest in producing “matched pairs” from adjacent transistors on the same wafer.] We packaged adjacent devices and tested them. Those with matching VBEs were packaged as matched pairs. As it turned out adjacent devices on a silicon wafer would indeed be very similar. So we started spending a lot of time analyzing that to see if, indeed, we would have the advantage of better distribution on a chip, on a small chip; a and that proved to be, fortunately correct.

Last: There was the tyranny of numbers remark that was made by somebody at Bell [Labs]. These small yields and you multiply them, and nothing works. So Bell had proved mathematically that you could never make integrated circuits. And the work Isy was doing was saying that adjacent devices and areas of what we wanted -- and the key thing, we weren't making a universal device. We knew the exact circuit environment of this device. So it was discouraging. Nobody would even conceive of making a device that had 100 transistors in it, but we thought four would be possible.

Laws: Isy, you've talked about all this statistical analysis work you had to do. Did you have any tools, a slide rule? Did you have a calculator in the company?

Haas: Oh God, I wish I had a calculator. I've thought about this a lot really; if I had only had a Radio Shack TRS-80 at my disposal in those days, I could have done a hell of a lot more calculations [and saved a lot of time]. It was frustrating. It's in some of those notes. To make one calculation at one temperature range took a whole day, and then you had to do the next temperature range and the next temperature range. Essentially, I think what was encouraging was the fact that using DCTL as we were, adding a resistor in the base solved a few problems. At first we built the resistor into the transistor base, but this accentuated the overlapping diode effect, which made me an expert in how transistors work and don't work. Eventually, the distribution was close enough that it made it a saleable product, in spite of all the customers' initial cynicism and disbelief.

Laws: That's certainly something I want to get to a little later on, because it's a very important part of selling any new product. Are there other major things that come to mind about challenges you had to overcome, equipment limitations, or testing devices? You mentioned the very long diffusions and the lack of equipment to do that.

Last: I think the key thing was the fact that a boron diffusion of this length of time, was just inconceivable at the start. The boron, one breakthrough in the boron, was some work in pre-production, I believe on the methyl triborite.

Kattner: Right. [All diffusion calculations were made using a slide rule or by hand]

Last: It was a pre-production idea that was brought back to R&D. That offered some hope.

Kattner: It was a big help, because most people that I talked to at the time said, "You just can't do it; 20 hours is not going to work." [I did oxidize the wafers longer to achieve a thicker oxide layer since the long diffusions caused severe pitting of the surface. The thicker oxide layer provided better protection of the silicon surface.]

Haas: The experts said it couldn’t be done.

Kattner: Yeah, they said it couldn't be done.

Last: Moving a 15 minute, difficult diffusion to a 20 hour diffusion was what we were facing.

Laws: And presumably all these diffusion systems were hand-built by Fairchild?

Norman: Yes, most of them.

Laws: There was nothing commercial.

Last: Yeah, most of them. Art Lasch was at Fairchild and he went on and started a successful company [Electroglas] building this kind of stuff. But we had to build essentially everything in those early days.

Laws: What would a typical piece of equipment cost you to build? Under $10,000 do you think?

Last: Oh probably, yeah. I mean, we got the whole company in production for under $2 million, so we certainly didn't have many million dollar devices or pieces of equipment.

Laws: How about reliability testing? How did you approach that?

Norman: I wanted to add something to what Isy had said. I had had the advantage, before coming in, that I was working with that circuit form for a couple of years before I got to Fairchild. The kinds of testing and data reduction that he was having trouble with, I'd already been doing. I had programmed our little Sperry computer. I had three girls working rotary calculators to do the data reduction on the tests we were running on the Philco and other transistors. I ran the first successful program on the Sperry computer and it was to do that data reduction. Of course, as Isy said, we didn't have anything like that, but I had this background of knowledge that helped me plunge forward when he was trying to get some data. That was very helpful in getting the job done. [In Device Evaluation we used a simple transistor (1210 small geometry) and resistors from the pilot line. These were the prototypes of the devices we were going to incorporate into Micrologic. We had an Olivetti printing calculator for data reduction.]

Laws: Were there any computers on the Peninsula at that time?

Norman: Philco Western Development Labs had a computer.

Laws: Which was just across the road from you in those days.

Norman: Walter LaBerge [was General Manager]. I guess we tried, a little later, to get computer programmers, and all they knew was business programming. When I would say something to one of them about using machine language, their eyes would kind of cross and that would be the end of the conversation.

Laws: How did you test the devices?

Haas: I used to test them, and first problem was, you had to build a jig and test equipment to test them. Well, that was too complicated. Furthermore we didn't know what we wanted to define. I had tested so many transistors and so many ICs [with a two and three point probe and a Tektronix 575 curve-tracer] that I had learned how to tell a good circuit from a bad one [by observing the current/voltage curve between two points on the circuit (usually between the power supply point and ground)] with a Tektronix 575 curve tracer. The good ones had a very characteristic signature. [This was the technique I used to separate the good devices from the bad before packaging] I think I was the designated tester. When testing transistors, we used to test them on the wafer and ink-mark the bad ones. With the ICs, we'd sawed them apart and I would test all the chips and pick out the good ones. We would get a couple of good chips per wafer sometimes. That went on even with the more complicated circuits. It was a very practical, effective way to test them.

Laws: How many wafers would you make in a run?

Haas: Ten.

Laws: Ten at a time. And how many potential devices were there on a wafer?

Haas: Zero.

Laws: Potential, not actual.

Haas: The first run, I think we got one, and we got so excited.

Laws: How many sites on the wafer?

Haas: About 70.

Laws: Out of which you might get one occasionally, as I'm understanding.

Last: Two percent yield was beyond our wildest dreams.

Kattner: The first one was a flip-flop. Okay, it did. It flipped and it flopped and that was a success. Isy and I immediately ran up and told Jay, "We got a flip-flop."

Last: Instead of just flops.

Laws: That was when?

Last: October 10.

Kattner: Yeah, October 1960.

Laws: Did you get a flop or a flip out of the physical isolation ones then?

Last: Oh yeah. They were all over the place.

Laws: How many is all over the place, Jay? Hundreds?

Kattner: May and June of 1960 is when they really started coming out on a regular basis. Then it ramped up from there till the end of the year. At the end of the year, that was called Phase Two. Then at the end of that year, it was decided, let's phase out Phase Two and concentrate on Phase Three, which was the isolated junction -- I'd rather call them the junction type.

Laws: So let me clarify this. Phase One was the Ticonderoga. Phase Two was the physical isolation. Phase Three was the junction isolation.

Last: And we made a lot of these Phase One devices. A whole family. Jim Nall was working on that. Just to put them into a system environment and start to see how they worked. We were actually distributing some of these to potential customers. One thing about a memo that has always intrigued me was one that Gordon Moore wrote in December of '60. He said, we have to make a decision which one to push the most, the physical isolation or the diffused isolation. So in his mind, it was still not clear which approach we were going to take. So none of these things were obvious at the time.

Laws: Looking back, it's clear, isn't it.

[**Note from Bob Norman:** Using the definitions above, to my knowledge, the only devices to go to outside customers were Phase Three. I shudder to think what the impact might have been of quick failure of a part delivered to the customer. Gordon's comment would make more sense in December '59 rather than '60 and and would have taken into account the universal failures of the chemically isolated circuits. I only know of a single Phase One (Ticonderoga) device being fabricated. Only Flip-Flops were fabricated then Gates until the process was nailed down. Then (mid 60) the other devices in the family were designed and fabricated.]

[**Note from Laws**; As the first diffused isolation devices were not produced until October 1960 it is most likely that is that Moore's note was written in late 1960, per Jay Last's statement above.]

Last: Yes, it is.

Laws: This is the second tape of the recording of the Fairchild Micrologic development panel. Just one item I would like to try to clarify a little more. It seems to be a very important aspect of the whole program in making it successful; the different people who came up with the ideas for the diffusion or the junction isolation. It was essentially expressed, I guess, in Noyce's original work, but was not allowed as part of the patent. We mentioned that Dave Allison had a major involvement in helping develop it. Isy, you had some thoughts on it. Who made it happen?

Haas: Well, I started the ball rolling, but up to that stage, I had never done a diffusion calculation. Lionel and I had a wafer etching girl in the lab who told us, "The limit is 75 microns." She can't etch anything thinner than 75 microns. So okay, 75 microns became the norm. Jean taught me how to do diffusion calculations, and everybody except Lionel and Jay said it's impossible. I'm young, inexperienced, naïve. "It's not impossible. The calculations say it's possible."

Kattner: I've always said too that technically, if you do not know enough, then you do it anyway.

Laws: You don't know it can't be done.

Kattner: Yeah, that's right. That's kind of how all this went.

Norman: That was very important to the whole program.

Haas: I think what made it happen was the temperature cycling failures of the others [the physically isolated devices].

[**Note from Bob Norman:** Every device made on the pilot line was temperature cycled before going on environmental and life test. All chemically isolated devices failed. All the environmental and life test results were from Phase 3 devices made on the pilot line.]

Laws: You were able to accomplish it with physical isolation, but [they were not] reliable.

Haas: That was frustrating. That was very discouraging.

Last: Obviously Fairchild was building a massive amount of production capacity and improvements in diffusion. So things that were impossible in '59 started to become feasible in '60.

Laws: Time solves many problems in development.

Last: And Fairchild was still very focused on--

Norman: The motivation.

Last: There was a very strong motivation. It was turning into a very successful company.

Laws: It was the Google of its era, there's no doubt about it, in terms of both the impact it had on the world, and the excitement and the youth and everything that went with it.

Last: The first product came out in August [of 1958], and at that time, there were 60 people at Fairchild. A year later, there were 600, so we grew. And after that, it just kept growing and growing.

Laws: At the time you were doing this work, Jay, do you remember about how big the company was, how many, let's say starting in late '59? Was it around 600 at that point?

Last: It was 600, about the time we started to program. It was turning into a big company then.

Laws: Sure was, 600's a good size.

Norman: I was 592, I think, and that was August of '59.

Haas: You were 592 in August of '59?

Norman: Yep.

Haas: Oh, I'm thinking '58. Yeah, you're right.

Laws: So we've gone through a year or more development project. We've made hundreds, if not thousands of devices now. Jay, you mentioned that even some of these Phase One devices were sampled to customers. How did you select the customers? How did you decide who should get them, and what kind of feedback were you expecting from them?

Last: That's more in Bob's area to discuss, I think. As I said, there was not a tremendous interest inside the company for integrated circuits. Bob was developing an interest outside of the company. He gave a talk in February, discussing this in detail, and gave a succession of talks in the spring.

Laws: So that was February, 1960.

Last: February of '60. Bob was talking down some of the practical problems we were having making the devices and pointing out, eventually these things were going to take over the world. He illustrated, I think--

Kattner: Great vision, Bob.

Norman: I had the great advantage of ignorance.

Laws: Tell us about your first discussions with customers, and how they felt and responded.

Norman: Remember, I came from military background, as far as use, applications and stuff. So I worked with Howard Bobb, who was military marketing at Fairchild. So he and I spent a fair amount of time together. I was telling Jay earlier -- in fact, he probably remembered, that we delivered the first two flip-flops to Fort Monmouth, to a guy who promptly put them in his drawer, and never looked at them.

Laws: When was that, Bob? Do you remember?

Norman: Well, I'm a little confused, because if you like, I'm in a time warp. Vic Grinich -- see, my background in computers was to start shake, rattle and roll on everything I built as soon as I built it. That was the environment I came from, and that's what I was doing with these things. We shortly moved out of 844 Charleston into a little building behind it, on was it Fabian Way?

Haas: Fabian Way.

Norman: And set up shop in there, and did a lot of temperature stuff and things like that. As Isy pointed out, a lot of those plastic back devices, just wouldn't stand up to the temperature environment, and that was a requirement. And the other thing was, there was a paper in the proceedings in the fall of '59, an IBM paper on test to failure. Vic said, "I want us to do that on Micrologic." Certainly I was in favor of that. It involved quite a bit, but we learned a lot more from that, not just about the integrated circuit stuff, but also transistors. We learned exactly where to set the centrifuge, and what to call failures, and what tests were important and not important, of the mechanical shake, rattle and roll type. So that turned out to be important, not just to the integrated circuit program, but also to the transistor programs at Fairchild. We also took every integrated circuit we got, and put it on 125 degrees C operating life test. So when we finally gave the paper on Micrologic, we had data in hand from the tests we had been running, as to the reliability of the parts. I was impressed then, and impressed now with what we did on that test to failure program. We ran temperatures down to liquid carbon dioxide. We ran temperatures up until the devices just got turned into resistors, but we did all these centrifuge tests. In fact, early into the Micrologic program, we got a request from Ames laboratory to know if we could put together some devices for them to shoot from a 20 millimeter cannon. We had in hand the information that said that would work. We actually built hybrid devices for them, which they fired into the supersonic wind tunnel, over here at Ames, to test what happened to things in hypersonic flight. So it was a lot of spillover from that. Vic's integrity, in saying that we should do this, and how we did it and what we learned from doing it, at a time when we were still a small company, I think was incredible. It made it very easy when I went into MIT Instrumentation Lab to talk about Micrologic. I had all the data. They chose to go our way, instead of with welded Cordwood modules on the Apollo command module. Except they'd only use gates. They were afraid something might happen and the other devices wouldn't be available. So they built the whole computer with gates, but it worked for everything they did.

Laws: Jay, you had a question about the design of the DCTL.

Last: We certainly picked DCTL because it was the simplest to fabricate, and didn't have terribly difficult components to make. I wanted to ask Bob what other circuits were discussed. Were you discussing things with Vic about other things we should be doing?

Norman: About that time, a request came in from IBM for us to do DTL circuits, because all their work was in DTL. I went through it some with Vic from a circuit standpoint and the [numbers and]kinds of components [including capacitors] that would have to be built in order to do it. [It is worth mentioning that DTL capacitors were yield problem for some years. Meanwhile we developed Transistor Transistor Logic, TTL. When we filed for a patent we learned of a coincidental invention by Pacific Semiconductor and withdrew.] But fundamentally, as I expressed it to Vic, I didn't want to dilute what we were doing with the DCTL. I'd had a lot of experience with that, and in a sense, we had embarked on it. That's where I wanted to take it. He agreed with that. [We did put in place it backup plan that if we couldn't produce an integrated circuit we would produce the transistor and integral base resistor as a DCTL transistor, the silicon analog of the 2N240. That would have been the subject 1960 ISSCC paper. We should have proceeded with that anyhow since there would be many available sockets for a silicon "surface barrier transistor".]

Laws: When you went out to the customers with your first devices, did they think that this was a good choice, or would they have preferred to have something different?

Norman: They all preferred something different. At WESCON, by then there was a group of us from around the country who I called "paneleers." They started having panels at all these conferences and I was one of the paneleers. That kind of question would come up. I would stand up -- and it was in the first Westcon.

Laws: This was WESCON in 1960?

Norman: Yes. I said, "How many of you are ready to quit designing flip-flops for every job you do?" And I think about three people stood up. Out of hundreds. We're in this big, huge Cow Palace [near San Francisco]. The following year, I asked that same question, and only three people didn't stand up.

Laws: Fascinating change in that short a time.

Norman: It is a fascinating change. But I, of course, had done a lot of work, learning how to build DCTL that worked, and had the advantage that, resistors were per square, so many ohms per square, so the size of resistors was important, and certainly capacitors were a big deal from a chip area standpoint. The disadvantage of DCTL to the equipment manufacturer was that the transistors were by far-- by maybe two orders of magnitude-- the most expensive component in the computer. So the idea that you're going to forgo resistors at 15 or 20 cents apiece, for a transistor at $15 or $20 a piece just didn't make sense. But in terms of the overall ability to build a rugged computer, with a few consistent components, it was the first machine to a control computer, was a DCTL computer. When you're business is transistors, and in fact, the easiest thing for you to fabricate more than one of on a chip, and the least area on a chip, is transistors, then DCTL makes a hell of a lot of sense.

Laws: If you took these first two flip-flops to this gentleman at Fort Monmouth, who obviously didn't understand what a jewel he had in his desk drawer all those years, was it just a shotgun approach to the market, or did you know exactly where you wanted to take these?

Norman: I think it was July; I gave a talk at the NATO AGARD [Advisory Group on Aeronautical Research and Development] conference in Oslo, Norway.

Laws: This is July, '60. [

Norman: Yes. It was an R&D conference. We got a lot of response, and of course Howard [Bobb] was out shaking trees everywhere there were trees. We had a lot of interest from a lot of people. Then AC Spark Plug division in General Motors started looking at this for flight control, which they actually built. Martin Denver was looking at these for test equipment. There were two kinds of customers in the digital realm at that time. There were the people like Sperry, who were building the fairly small, military computers to do a variety of jobs, and people like Univac and IBM and so forth, were building large data processing machines. We were selling, if you will, to the small, military computer, where reliability was terribly important. It's interesting that about that time, there was a paper, I think from Remington Rand about introducing faults into a computer, so that the technicians would stay capable of quickly finding and fixing faults. Perhaps a year earlier, they didn't have that problem, but as the computers got more reliable, they did have the problem that the technicians were losing their ability to keep these big computers online. So we had two different worlds going on out there, and we were aiming for the high integrity world.

Laws: People who would pay for small size, high reliability, low power. Typically, what were the devices priced at initially? Say in 100 piece quantities. Do you remember?

Norman: I'd figured out some time before that it was costing us $60 a transistor to build a computer, not including the cost of the transistors. So when we talked about pricing on the first flip-flop, which had six transistors, I said, "Let's price it at $360." I think they actually did price it that, must have been for at least an hour or so. Then, of course, it began coming down. But basically, I would guess people were paying $100 apiece for integrated circuits at that time. I think that's about where it was.

Last: One point I'd like to make, at this same time, the reaction of the sales department was not extremely favorable toward integrated circuits. I was at a meeting. I don’t remember, but it was all of the top people in the company, including Tom Bay, the marketing manager. Tom said to this group, saying all the money I had wasted on integrated circuits, and we should shut down the group. There was just a silence. Nobody came to my defense or anything.

Laws: Because they were having trouble selling it, Jay?

Last: No, because they had such a successful business selling transistors to circuit designers and here they were seeing that they were going to lose their main customers with integrated circuits. They were finding a big resistance to selling a fabricated circuit to somebody. The circuit designers all basically didn't want to. So Tom Bay just looked on it as a big nuisance and an impediment to his main role, which was selling transistors and diodes. But it shocked me, of course.

[**Note from Bob Norman:** Tom didn't like his salesmen pushing ICs they didn't have instead of transistors they did have. Transistors were selling in the $5-$10 range.]

Laws: About what time was this, Jay?

Last: This would be in the fall of '60. It was just before we had made the first diffused device.

Laws: But it hadn't been formally announced to the public at that time. That came in '61.

Last: No, we hadn't announced the device. But Tom expressed in fairly graphic language what he thought .we should do with that program. As I said, nobody commented on it, or said, "Tom, where are you coming from?" They just sat there and let me stew in this.

Laws: You felt that it showed a lack of understanding of the potential.

Last: It showed why the integrated circuit got off to such a slow start. It was a completely different world. It was as big a change as from the vacuum tube to the transistor, from design points of view and the approach to it.

Laws: We hear the same stories about the microprocessors going through exactly the same cycle, trying to get people to understand what it was for.

Last: That's right.

Laws: People don't seem to learn from history, do they?

Kattner: No, they certainly don't.

Norman: He was upset, because too many salesmen were talking about integrated circuits, which they didn't have -- if they had orders, they couldn't ship them-- instead of talking about transistors, which they did have. It was a fairly common thing for the salesman to talk about what he didn't have. Tom was really angry about that.

Laws: So what was the first real design win that you saw, Bob, that said, "Boy, I think this thing is going to be important?"

Norman: It never occurred to me. I don't know.

Laws: The MIT visit?

Norman: I was very happy about that. Fundamentally, I couldn't think of a better place to put all the work we had done on building a reliable solution. Probably the reason for MIT is Walt Andrews, the only guy in Fairchild who got list price on everything he sold. Part of the reason for our visit to MIT is that we were working with them. They were getting ready to use welded Cordwood modules, on the Command Module. Fairchild built the transistors for that. In fact, part of my job at Fairchild was evaluating this. The problem was lead consistency. If they didn't have high consistency in the leads of the transistors we were shipping them, then that would throw off the weld schedule on the interconnecting wires in the module. So one of the problems solved by the integrated circuits was that they no longer had to worry about that. In fact, the fundamental attribute of the integrated circuits is a consistency of the technologies we were using throughout. By then, well known, reliable transistor technology. I think Howard [Bobb] probably took advantage of my knowledge of all the problems these guys were having using transistors [in the cordwood modules].

Laws: Do you remember when you made that first call on MIT, which year?

Norman: Oh, it was in '60.

Laws: And I think it was in 1962 before they made the commitment to actually use the circuit.

Norman: [This would be after they spent a lot of money validating the reliability of Micrologic]. Those things take a while. Well, they used to at Sperry, so that kind of thing didn't bother me. But it was part of Tom Bay's problem.

Laws: So Jay, in late '60, you were somewhat shocked that the sales department wasn't going to spend a lot of time selling your baby. You left the company early in '61?

Last: Yes, I did.

Laws: Was that as a result of that [comment by Tom Bay]?

Last: It was one of a number of things. I went from an environment where integrated circuits were very minor thing to join Henry Singleton. He said, "I want to build a systems company [Teledyne] based on advanced integrated circuits." The company was changing its nature then too. The eight of us started it, and we worked together collegially as a team. By this time the company was getting stratified and I was just another employee in a back room in an R&D lab. I loved the excitement of starting Fairchild. I said, "That would be fun to do again." So I was ready to go. Isy left with me and Jean left with me. Lionel later in that year started his own company. You can see that the believers left the company to do something else.

Laws: Sure. You and Isy went to start an IC company {Amelco], and Lionel, you went to found Signetics, I believe. Again, to focus on integrated circuits.

Kattner: Primarily, yes. It was the only thing we focused on.

Laws: Meanwhile, Bob, you stayed with Fairchild until you founded General Microelectronics. What year was that?

Norman: In '63.

Laws: So you did stay the first couple of years after the introduction of the Micrologic device.

Norman: Yes.

Laws: And the first public introduction of that, I believe was at the IRE [Institute of Radio Engineers Conference in New York] in March, 1961 when the *Life* magazine picture came out that we're familiar with.

Last: At that time, we made the thing too small. The *Life* magazine picture shows the device in a real small, TO-18 can, which the customer couldn't handle easily. So we quickly backed off and put it in a larger package.

Laws: And what style package did you put it in?

Kattner: The TO-5 can.

Last: The standard TO-5 package. So we miniaturized too much.

Laws: "Smaller than the D on the dime" was the caption in the magazine.

Last: Yes. A memo I ran across was [written] in December of '61. There was a sales department document that said, "The integrated circuit has arrived. We've now sold half a million dollars' worth of them, and we've got the complete family to sell." It went on to discuss the good response that it was getting. When we left, we had made just the first flip-flop. Lionel, in the months he was there, filled out the family and got them into some kind of production.

Laws: What were the other members of the family, Lionel? There was a gate.

Kattner: We had a gate, a half shift register, and the adder, I think.

Haas: Half adder.

Kattner: Half adder, yes.

Haas: Counter adapter [used to build a fast carry counter, dual three input gate, and the buffer.

Kattner: Yeah, there we go. They were all previously designed. The layouts were completed, so it was just then a matter of producing them. So before I left, we had--

Laws: Was it like a master slice, single design you just masked differently, or was each one uniquely designed?

Last: The others had more transistors. [Each was uniquely designed]

Kattner: Yeah, more complex. But the transistor was the same transistor. And then, of course, resistors, depending on what value you wanted determined the size of the layout. So most of the other devices were all combinations of different components. They all followed the same processing. The flip-flop was the basic one, and whatever we accomplished there applied to all the rest of them. Not to say there weren't some difficulties. You still had alignment problems even then, in the photo masking, but diffusion was fairly well under control.

Laws: And you were still working with the 25 micron tolerance at that point, or had you tightened up anything?

Kattner: We had tightened it up quite a bit.

Laws: To what, do you remember?

Kattner: No, I don't really recall any more. I know the wafers were like 80 microns thick.

Laws: What size wafers were you working on?

Kattner: Roughly an inch in diameter.

Laws: You started out at an inch?

Last: Three-quarters was the first one, then an inch. Then we had to cut a bevel on one side, so that took away some of the area. There wasn't a whole lot. One other thing we haven't mentioned, the fact that we were trying to get the optimum transistor for this particular environment, so there was a lot of work going on, developing a good transistor. It was a dumbbell transistor. Isy was involved with that.

Laws: Dumbbell, meaning the shape. How did you come up with that shape, Isy?

Haas: Well, the shape was obvious. The problems were not. The stumbling blocks with the RTL or DCTL was the low input impedance of the transistor. To get decent distribution, you had to have VBEs within a couple of millivolts. That was not possible, so we started adding an external resistor. Somebody had the bright idea of, "Why don't we just elongate the base to make the Rb prime, the input resistor, larger?" Well, in theory that works, but when the transistor goes into saturation, it looks like a collector base by-pass diode, that decreases the input impedance again, and also increases the collector saturation volt. So that was kind of a negative benefit, which really brought forth the fact that in integrated circuits understanding the solid state physics of the chip was key to making good working circuits. This became even more apparent later on, with TTL, DTL and all those saturating components on a single silicon area. Later on, this is after Fairchild, I figured out that if you put two collectors on a transistor, you would solve the problem. Fortunately, Fairchild did not have that patent. Teledyne did. Fortunately for me, I really enjoyed that part of the work. I happened to have the right background for this solid state physics and electronics combination, to understand what was going on. I think Lionel and I worked very well as a team. I would figure out what was wrong, and he would figure out how to fix it. And Dave Allison was an extremely key person.

Laws: He was the diffusion expert?

Kattner: He was a device developer. [He also had a lot of diffusion experience.]

Haas: He worked for Jean, I think. He pointed out a lot of the problems that we couldn't understand, like the overlapping diode, he pointed out to me.

Laws: Tell us more about the overlapping diode. It's come up a couple of times.

Haas: When a transistor goes into saturation, the base become more positive than the collector. So you have a “P” base and an “N” collector, giving you a forward biased “PN” diode. It looks like you have another diode with an anode on the base and a cathode on the collector, PN. Well, that clamps the collector to follow the base. [So when you build a resistor into the base in series with the base by elongating the base (which is what we did), the base voltage will rise when the base is driven positive and the transistor goes into saturation. But as the base goes more positive because of the base current flowing into the built-in base resistor, the base voltage cannot rise indefinitely because it is clamped to the collector voltage as if a forward biased “PN” diode clamps the base contact to the collector; and so the base is clamped to the collector earlier than it would otherwise.] So it was kind of a lose-lose situation, instead of a win-win situation. I think eventually, Fairchild took the dumbbell resistor out of the base and put an external base resistor, but I'm not sure. That eliminated the problem. It took more room on the chip but it helped to sell the device.

Kattner: It worked.

Laws: Bob, you referred to the circuit as DCTL, but later on, several years later, Fairchild started calling it RTL. Was anything different about the circuit, or was that easier to say?

Norman: It's kind of interesting, because the DCTL means that there's no intervening component between the collector on one transistor and the base of the next. As a logic form, the way the Philco 2N240 transistor and it's various derivatives worked very well in DCTL. The reason it worked is because there was an intrinsic resistor in the surface barrier. So when I was proposing DCTL at Fairchild, I was saying, "We have to build in that resistor." When somebody, I don't know who, decided we'd just extend the base and that will simplify that, and that's where the overlap diode problem [came from] -- but the consistent thing was to get that small amount of resistance [about 125 ohms] in series with the base, and that would prevent the various problems the DCTL had when you didn't have that intrinsic resistor. Somewhere, I guess somebody probably complained to marketing, it wasn't really DCTL because there was a resistor there. Then they came up with another way around it. And besides, that was another way to solve the problem that some people fundamentally didn't like DCTL.

Laws: So RTL was DCTL at Fairchild. There was no difference, no change as you moved forward.

Norman: No. Prior to that, as I said, because of the cost of using transistor-only type logic at Sperry, we had developed resistor-coupled transistor logic, where we did all the summing in resistors, instead of in the transistor. So a three-input NOR gate had three resistors and one transistor, instead of three transistors. We call that resistor-coupled transistor logic. But then you started getting all these acronyms. It kind of got confusing what you were talking about. We knew what we were making.

Last: One point on the diffusion, we keep referring to Dave Allison, who was really an unsung hero at Fairchild. He joined when the company started, and he was extremely important on the diffusion processes for our first device, and he continued, as has been discussed here, to be a very senior person in diffusion.

Norman: He was skinny as a rail, and seemed to lose weight every time there was a problem.

Laws: You talked about a number of unsung heroes today. You talked about Jim Nall, you talked about Dave Allison, you talked about Sam Fok. Are there others that you'd like people to know about who made a contribution here?

Last: I think those were the key ones. Robert Martin, who brought back, for some reason the idea of using a different boron compound, which eased the boron diffusion. He worked on that, and Lionel picked that up, which helped greatly to make the diffused isolation device possible. So he was an unsung person. Do you remember more about your interaction with him?

Kattner: Very little, because once that was suggested, we kind of took it and ran with it. It worked, so I didn't really have too much more interaction with him on that.

Laws: There's a couple of other names that turn up on some of the early app notes. There's Don Farina, Richard Andersen, they were involved in helping market the products to customers?

Norman: Don had worked for me at Sperry. Then he came when we set up the Micrologic program, he came to work for me in device evaluation at Fairchild. He was the one that did the measurements that showed that the Ebers and Moll equations would apply to planar devices, which was very important to coming up with a robust circuit design for the DCTL. And I did it over several decades of current and several temperatures and so forth. The guys, Don, Dic Anderson, Orville Baker, Howard Bogert, Helmut Wolf, Bill Hafner, Dick Crippen and others in the group, did a lot of the grunt work that's required any time you're taking something and getting ready to manufacture it and make sure that, the design will be robust over the temperature, over all the margins, and that it will be, to the extent that we can influence it, will be manufacturable. He was an important part of that, and Dick Anderson also was very important. We had Howard Bogert who was part of that group. He and Dick Anderson started together coming out of Stanford. Howard, took gate elements, the three input gates, and cut the collector leads apart, and used that to measure the transistor characteristics, which showed how closely matched the base emitter characteristics were, which then said that we had a leg up in using these processes on analog devices. He did a lot of that work. So we had an incredible team of people working on this.

Laws: You mentioned a number of salesmen: Howard Bobb, also the gentleman who never cut the price, I don’t remember his name.

Norman: Walter Andrews.

Laws: In my experience, there are two or three really champion salesmen out there that just take this thing and run. Are there any others that come to mind that could make it happen in the field?

Norman: Dick Eiler and Tom Murphy, both government-oriented people. One of the fairly important sales events that happened, although we didn't know it at the time, was when this guy came in from the Department of Defense. His name was John Orleman. He came down to see me in my office at Fabian Way. I'm describing all the stuff that we've done and we're doing, and this is pretty early in the program. He's got a little pad that must about an inch or an inch and a half square, and he's making little notes. I'm kind of, how much can you write on a little pad an inch and a half square. A month or so later, Gordon Moore calls me up to his office and say, "We have a RFP from NSA." What it was, was a verbatim [transcript] of what I had talked to him about in that meeting. It turned out John had been a stenographer in the Signal Corps before he went there. , So that's how we got the R-13 contract, which was a second generation of the DCTL integrated circuits, designed for lower power and with higher complexity devices. And at that time, of course, it was all communications, security stuff.

Laws: What's R-13? Does that mean anything to the uninitiated?

Norman: I don’t know, but it's also called Milliwatt logic.

Laws: And that came out in about '64 or so?

Norman: No, no. It came out, it must have been '62, before I left [Fairchild Semiconductor in early ’62 and Fairchild Space and Defense Systems] in July of '63, so it was out well before that. They'd asked us to do reliability and QA specs. We took all the stuff we had done under the [Micrologic] test to failure program. The only [residual] problem we were having at that time was that in die-attach, in manufacturing, sometimes the girl's tweezer, when she was scrubbing the die into the header, the tweezer would slip and scratch the metal. Enough metal would remain so that the device worked when we tested it, but very quickly, like due to metal migration or something, [the conductor] would separate and the chip would quit. So we had to do an optical inspection, which drove manufacturing insane, because they had to work with one device at a time on that, until R&D came up with a way to put a glass coating on the chip, and cut the incidence of that problem way down. The other one, the integrated circuit was a low impedance device. We normally did gross leak tests, by putting a device in a detergent. If it's a transistor, we put the device in the can into a detergent and if any got into the can, then of course we'd see that, and the characteristics of the transistor instantly, and throw the transistor away. But the integrated circuits were such low impedance devices, we wouldn't see it. It wasn't until one time, at an RRE conference, Joe Gattuso was a salesman. I'm standing there at the booth and he comes and says, "I just got these integrated circuits. I had left the uncapped circuits in the pocket of my shirt when it went to the laundry, and when it came back, there's no metal." Then we realized that if we did that gross leak test on integrated circuits, we could wipe them out without even knowing, or just trap some of that stuff inside cans. We had to go to a completely different gross leak test on these things.

Laws: We're running towards the end of this tape, so I'm going to ask a couple more questions. What I'd like for now is to get you to look back, and then ask each of you, what are your strongest memories of those times? What are the things that you're most proud of? What are the things that stick in your mind? Jay, could you start off!

Last: Well, it was just such an exciting time. We were working ahead of the technology that existed, so we had to make a new invention every day -- you could immediately see the contributions you were making to move the whole thing forward. It was certainly the most exciting and interesting time of my life.

Kattner: I'd sort of describe it the same way Jay has. For me at least, it was beyond my imagination at the time, because I had no idea what I was going to be doing when I came there to Fairchild. So I learned a lot within another couple of years that I was there. Ultimately, before I left, I realized that the world is changing. We're onto a whole new era of technology, and the future's out there. At that time, you couldn't even imagine how things would change and change so rapidly. It was almost like orders of magnitude every year that you saw more progress and new devices, new methods. The whole thing just improved by leaps and bounds. It was an exciting time to be involved with it. That's all I can say. It's the best part of your life, really.

Haas: I have to second everything that's been said. In addition, I can say that I'm convinced that the work we did has changed society.

Norman: It was an incredible experience for me, working with these guys. As I've said to them before, our ability to interact. They were beating their heads against the wall, and to me, it's simple, because every time I asked them for something, they did it. What more can you-- I'm a systems guy, what do I know? But I'm so incredibly happy at what we turned out. What we turned out was good stuff that worked. You didn't have to do any fiddling around with it. You didn't have to protect it from the environment or anything else. It just worked. To me, I'm an engineer. That's why I get the big bucks.

Laws: Lionel, you mentioned you had a sense of where this thing could go. Did you really understand what this was going to mean in ten or 20 years beyond?

Kattner: Specifically no, but I just had a pretty good insight that we were really onto something phenomenal. That's basically why I left, started another company, because I knew it was going to be an exciting time. I also realized it wasn't going to be easy. I knew enough then that I knew that the customer still thought in terms of discrete devices. What would happen to all the circuit engineers out there in the industry? Because we were going to replace them, so it was going to be a long drawn out battle, and it was for a few more years. I think everybody experienced that. .[At the time I felt we could put fifty to a hundred transistors and passive elements (capacitors and resistors) on a single chip but a diffused collector would be crucial. Subsequent to leaving Fairchild the diffused collector was accomplished at Signetics and that vision was realized.]

Laws: It took longer than expected then, to get the circuits accepted and into production, Jay?

Last: Yes. It certainly did. It was a military business and it took a couple of things happening. Gordon Moore has told me that he wrote his famous paper discussing Moore's Law because he was feeling so frustrated that the world wasn't accepting integrated circuits.

Laws: And this was in '65.

Last: Sixty-four. He wrote that in '64, and he was pointing out that the circuits were going to be accepted for economic reasons, and not for the small size or power or the other reasons that had been led up to that time. So it was a four or five year period. A couple of technical things happened in that time. Epitaxy made an enormous effect on the ability to fabricate these things. These long diffusions we had went away. Then eventually, the MOS circuitry came in. That's the one that has changed the world, going up to millions of devices, instead of the dozens we were talking. It's pretty hard to say, "This device is going to change the world, and there's going to be millions of devices on a wafer," when your yields are so low. If you made a device that had a dozen transistors on, you thought you'd conquered the world. I don’t think anybody could ever imagine where this was going to go, to the extent that it's gone now. You start looking at how much R&D has gone into the development of the products we have now. You just look at each company, spending upwards of 10 and 20 percent on R&D, and you multiply all these billions of dollars of all these companies, you have an unbelievable amount of development work that's taken place in the last half century. So none of this came easy. Every step forward is a difficult thing. I haven't seen any early remarks about how big these things were going to get. Anybody that made any estimate like that was low by a couple of orders of magnitude.

Laws: Before we sign off, does anybody want to add anything, before we finish?

[**Note from Bob Norman:** Howard Bobb and I left Fairchild Semiconductor in early '62 to join the newly formed Fairchild Space and Defense Systems on Porter Drive in Stanford Industrial Park. Our first contract was from the Air Force for a Triple Redundant Adaptive Flight Control System for the X-5 rocket plane. Gene Franklin and Bernie Widrow of Stanford Control Systems Lab consulted. This was fabricated using Micrologic and successfully flown on the X-5.]

Last: I think we covered it.

Kattner: Covered pretty well.

Haas: Thank you for your work.

Last: Yes, thank you.

Laws: I would like to thank you gentlemen for coming in at such short notice. This was another historic moment and I'm thrilled to have captured it with you.

Everyone: Thank you.

END OF INTERVIEW

<u>ADDENDUM</u>

Fairchild Micrologic Integrated Circuit Development Chronology

Compiled by David A. Laws and Michael Riordan

December 1957 - On December 1, 1957 theoretical physicist and co-founder of Fairchild Semiconductor Corporation, Jean Hoerni describes in his lab notebook an idea for a new method of fabricating semiconductor devices under the heading “Method of protecting exposed p-n junctions at the surface of silicon transistors by oxide masking techniques.” Due to other priorities Hoerni’s idea is not pursued at that time.

January 1959 **-** Seeking a solution to reliability problems with exposed p-n junctions on the company’s mesa transistors, Hoerni returns to his 1957 concept. On January 14 he prepares a patent disclosure based on his notes and proceeds to develop a prototype device that he demonstrates to his colleagues in March. U. S. Patent number 3,025,589 “Method of Manufacturing Semiconductor Devices” describing the “planar” process is filed on May 1, 1959 and issued March 20, 1962. Also in January, director of research Robert N. Noyce writes down his ideas on interconnecting multiple devices on a single piece of silicon by using the oxide layer as an insulator and *p-n* junctions for isolation between components.

March 1959 **-** Texas Instruments announces Jack Kilby's Solid Circuit concept at the Institute of Radio Engineers (IRE) conference in New York. Noyce calls a meeting to discuss how to respond to the TI device. He describes his interconnection ideas that in association with Hoerni's planar structure lead to the filing of U. S. Patent number 2981877 "Semiconductor device-and-lead structure" on July 30, 1959. This fundamental patent on the monolithic integrated circuit (IC) was issued on April 25, 1961.

April 1959 – Unknown to the Fairchild group, physicist Kurt Lehovec at Sprague Electric files for U. S. patent number 3029366 "Multiple Semiconductor Assembly" that describes electrical isolation of multiple semiconductor devices with reverse-biased *p-n* junctions. Lehovec's work takes precedence over Noyce's notes and was issued on April 10, 1962.

August 1959 **-** Following discussions with Noyce, Fairchild co-founder Jay Last builds a concept integrated circuit (made of individual silicon transistor chips on a ceramic substrate with pencil lines as load resistors) to "show the flag" at the WESCON trade show.

September 1959 **-** Last forms a team to develop a monolithic IC; members include Sam Fok, Isy Haas, Lionel Kattner, and James Nall. That fall, Robert Norman, Device Evaluation section head under Victor Grinich, uses electrical data developed by Donald Farina to design a direct-coupled transistor logic (DCTL) four-transistor flip-flop integrated circuit.

December 1959 **-** In his notebook entry of 12/22/59, Kattner records the metal interconnect pattern he designed for the circuit with an active area of 28 by 34 mils based on a "physical isolation" technique devised by Last, to be accomplished by etching the wafer from the back side through to the oxide layer and filling the cavity with epoxy or plastic material.

February 1960 **-** Norman, Last, and Haas present the first public paper on Micrologic, "Solid-State Micrologic Elements," at the 1960 IEEE International Solid State Circuits Conference (Session VII: Microelectronic Considerations) on Friday, February 12, 1960.

May 1960 **-** The first working flip-flop IC with four transistors with diffused base and load resistors fabricated with Last's physical isolation method is tested on May 26, 1960. (See "FF-105" in Kattner notebook, dated 5/26/60)

August 1960 **-** Kattner explores alternative isolation techniques (See Kattner, "Micrologic Electrical Isolation," notebook entry 8/27/60) such as the Noyce *p-n* junction approach after discussing diffusion techniques with diffusion expert David Allison. Haas proposes fabricating the deep junctions with boron diffusions across the back and in selected areas on the front of the wafer. (See Haas patent notebook 8/31/60). Requiring diffusion times of 18-24 hours, much of this work is "bootlegged" in the evenings and weekends due to limited furnace capacity.

September 1960 **-** The first successful devices made using diffused isolation are tested on September 27, 1960. This success is further borne out by Haas and Kattner comments in a Fairchild R&D progress report dated October 1, 1960 (for example, Kattner: "Rather marked success has been achieved in the development of electrically isolated DCTL flip-flops.")

November 1960 **-** Subsequent temperature and lifetime tests show that reliability of flip-flop ICs made with diffused isolation are far superior to those made with physical isolation. In the November 1, 1960, R&D progress report, Nall states: "The electrical isolation studies look by far to be the best and most economical method of making micrologic elements."

January 1961 **-** Tests by Norman's group indicate that the first Micrologic ICs are finally ready for marketing. The February 8, 1961, R&D Progress Report (for the period ending 1/31/61) summary by Grinich and Gordon Moore states: "The flip-flop runs indicate we are in adequate shape for a product announcement at the March IRE."

March 1961 **-** Fairchild begins formal marketing of the type "F" Micrologic integrated circuit flip-flop at a press conference at the March 1961 IRE Conference in New York. A photograph of an earlier, physically-isolated device is published in the March 10 issue of *Life* magazine.

December 1961 **-** During the summer Fairchild introduces five additional members of the Micrologic family and on December 1, 1961, an internal sales memo states "We are at a point now where we can discuss micrologic as a true product. Our shipments by the end of December will be in the $500,000 region and the current backlog is about 5K units"

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