Oral History of Paolo Gargini

Interviewed by:
Harry Sello, Session One
Daryl Hatano, Session Two

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SESSION ONE, conducted by Harry Sello

Harry Sello: Are you ready?

Paolo Gargini: I'm ready, go ahead and fire away.

Sello: Well, Paolo, it's a great pleasure to meet with you and talk with you. I see it as a-- quite apart from an historical interview, I want to take advantage, to make a friend of someone that I felt was always a friend, but never really got a chance to talk to.

Gargini: And now I have to tell the truth, nothing but the truth, right?

Sello: Exactly, exactly.

Gargini: Okay.

Sello: That's exactly what we want. So please, as I raise an issue or you feel that it is improper in time, or something like this, don't hesitate.

Gargini: Okay.

Sello: Now what's of interest to me is that you got your education not very far from where I spent a few years working for Fairchild. Perhaps it's about, maybe 15 or 10 years before you. You're not gray haired, but I preceded you in this sense, when I went to work at Fairchild, at SGS. You may know a lot of the history, even better than I do. But you got your degree, in two very, very prominent universities, Bologna, certainly, which I was aware of, in fact, I had a chance to even give one small seminar on semiconductor technology in those days at Bologna, but please, would you explain for the benefit of people, a question which I always wondered about. In the education of technical people in Italy, in the semiconductor business, what is the relative training of a person who comes with a degree, Dottore, compared to a person in the United States who goes on to form a PhD? Can you give us just a little few words about that level of training?

Gargini: Okay, so first of all, let me start. You know, my name is Paolo Gargini, and I was born in Florence, and I did my undergraduate studies in Florence. And I was always fascinated with science. You have to understand, Sputnik, 1957, impressed everybody. The attention on science became very high. And actually, in 1958, with a friend of mine, we built a vacuum tube radio. There was really an accomplishment and then in 1963, then I built, again, with a friend of mine, a transistor radio. So in a very short amount of time, things changed from vacuum tubes to transistors. At that time, transistors were not very good; in fact, it was very difficult to even match two transistors, especially for the push-pull of the output at the very end.

Sello: These were silicon transistors at that time?

Gargini: Silicon transistors, germanium diodes. And the best shops would have a curve tracer, where you had the opportunity of testing different transistors if you wanted the output characteristics to be matched. So that was considered for the high class shops, where you had the opportunity to do this. In addition, I had what was called a galena, that is, essentially simply an inductor with a capacitor, and then there is a zinc sulfide, that is called galena, you know--

Sello: Galena, it's a crystal.

Gargini: And when you have a cat whisker, and you have to adjust it, and then you can listen to the radio, AM, with absolutely no energy (necessary). The only thing you need is an antenna and the coil, the capacitor, a diode, and the headphone. That's all you need. It cost you nothing. And actually, at night, it was very clear to listen to this. So with this in mind, by the time I had to select my high school, I selected one that had a strong emphasis on technology and science. Typically, you know, many people want to go into liberal arts, because they can study for the rest of their lives, and that's, you know, whatever they like. Then there is another type that is more like science and philosophy, and then the one I selected was a mix of theory and also practical applications. I had shop work, with metals and wood and all the other things. And at the end of this, then I enrolled in engineering in Florence, and at that time, there was only the first, you know, the undergraduate, because Florence was primarily in architecture and Pisa and Bologna were primarily in engineering. And so when it came time to I finish, my graduate study, then I selected Bologna, because it had a good reputation for science and engineering, they had the Marconi Foundation and many other elements. I thought being in Europe, that I was going to do telecommunications. And then something strange happened. In 1965, a professor had the intuition that, indeed, semiconductors were going to be very important, and also laser and telecommunications. So he sent two people to Bell Labs to work on this, and sent another couple of people to study for--

Sello: Now this was in Bologna?

Gargini: From Bologna.

Sello: From Bologna.

Gargini: So this professor, Professor Ercole De Castro, sent two people to Stanford, and Stanford usually is very liberal, so they got all the blueprints on how to lay out the lab, and they bought precisely the same equipment that was at Stanford. So this was in 1965. So by the time, in 1967, that I had to select my major and so forth, I became aware that actually, this kind of possibilities existed, and they were quite unique in Bologna. And by 1968, after a couple of finals, the professor called me up and wanted to know if I was interested in this, because in May, 1968, this lab was completed. In fact, last time I went there, they reminded me that Intel only started in July, 1968, so they beat Intel by two months.

Sello: You preceded Intel.

Gargini: Preceded Intel by two months, right. So I was quite surprised, but intrigued by the opportunity, because typical electrical engineering degree at that time, was primarily design, because you buy components, typically made by a very small percentage of the engineers, and then a majority of the people are circuit designers, application and so forth. And then, you can imagine '67, '68, the transition was occurring where the circuit design and making the components were one and the same. And so that caught us by surprise, and was very confusing. And in fact, I must say that '69 was a very confused year, until finally, I bought this book [A.S. Grove, “Physics and Technology of Semiconductor Devices”].

Sello: Bravo.

Gargini: Okay, and it cost me, it says here, imagine, 11,400 lire.

Sello: So what was that in dollars then?

Gargini: It means, 15 dollars, 10, 15 dollars, okay? But I bought this book, you can still see it has all my notes, and finally, I began to understand how making a device and device and so forth, fits together.

Sello: So did you have a chance, then, to jump right into silicon? You didn't have to proceed with germanium.

Gargini: No, the lab was set up for silicon.

Sello: It was set up for silicon.

Gargini: We had germanium wafers, We made a few diodes and so forth, but '69, '70, when I was working in the lab, it was silicon, okay, and of course, it was like three eighths of an inch wafers. And the other thing that was very advantageous, since it was only three or four of us that had been selected for this and we had a whole lab to ourselves. I ended up doing everything. So I cut the mylar, I did manually the step and repeat, it was not automated. It was really like, you know, like a cannon, right, you had to step x and y, then flush and everything else, develop the masks and everything, but that was very good training, because I think I would say there were probably less than ten people in Italy that really had an opportunity to access this technology at the college level.

Sello: Paolo, excuse me for interrupting. This is strange to me in another sense. Here you are at the level of advanced teaching and learning in a good university. And not far from you is a company called SGS, and they're working in that very same area on the very same material. Was there any connection, could you go to visit them?

Gargini: Yes, so what happened, by 1971, we began organizing some seminars, and then a few people from SGS would come to listen to the seminar. People more from Castelletto, they were the people in research, not the people in manufacturing. And then, you know, one thing became clear to me, as I was reading more and more papers, so that, to be honest, the people around me were, I would say, confused. It was very difficult by reading articles, to understand which direction the industry was going. And I don't think that's surprising, and that time was confusing, okay? So I took advantage of one of these international Fulbright competitions, and I won a Fulbright for a three-year training, in the United States. And then I knew where to go, so I applied to Stanford, Berkley, UCLA and USC.

Sello: Direct connection, right away.

Gargini: Direct connection. Stanford replied, you know, a month ahead of Berkley, so I ended up in the Stanford lab, and I looked around. I knew all the equipment, because it was precisely the same equipment, and so that was advantageous because I think in Italy, most of the studies are primarily theoretical. And actually, you are almost discouraged from doing too many detailed calculations; you're more led toward theory, in fact, if you can develop a set of equations, and reduce the calculation to a very small number, that is considered a very high accomplishment. In addition, what I did, since I realized that I was very ignorant of some of this stuff in physics. As soon as I finished engineering in 1970, I finished in September; I was just in time, in October, to sign up for solid state physics. And so by the end of '72, I had finished all the classes except the thesis, you know, for physics.

Sello: And what level were you in at the university at this stage, compared to the United States, compared to Stanford, let's say.

Gargini: When I came, I had by then, seven years (of experience) after high school. That is two years beyond the US high school (plus five years of college). So the only difference is that the universities in the United States had much more equipment than we ever had. I think the main adjustment was to be a little more practical, but fortunately I had done shop and a few other things, so it took me a year, really, to take, to adjust from theoretical view of the world, in which, Fermi equation was the highest thing I could think about, to become a practical guy.

Sello: Oh, Paolo, I must ask you, at this stage, forgive me, why didn't you come to Milano, and come see us at SGS. We needed you.

Gargini: So wait, come-- well, so what happened, to be honest, the concept in the university was really to get the Nobel Prize, and not really to work with the industry. To some extent, this (concept) still exists now. In fact, in Europe, the concept is still there, if you are in the university, the highest accomplishment is to get the honors, and the Nobel Prize is the highest, okay?

Sello: It is the biggest…..

Gargini: So even though there was some communication with the industry, but there was somewhat of a sense that these were practical people that just wanted to make money and so forth, and that the researchers were pure, “they only think about science”. So in the early '70s, this was the attitude, okay, so I came to Stanford in '72, and Professor Linvill, at that time, you know, was the Dean of engineering, and then there was this young aggressive guy (Professor), you know, Dr. Jim Meindl, that was really very busy, and I got into the lab after three or four months, and at that time, you know, Jim Plummer, had just finished his PhD, and T.J. Rodgers was working on his PhD, and Krishna Saraswat was completing his PhD and so forth. And so the first assignment we got was really to build a low power CMOS process that, at that time, was really considered somewhat academic, because everybody was still struggling between, “there's going to be P channel or N channel?, N channel is too expensive, are you going to ask me for more than five masks,?” you know, that CMOS was not appropriate for manufacturing. And this (CMOS process) was for a low power application, and so I began working on this process, and the group leader, which was Dick Blanchard, was also working at Fairchild.

Sello: Dick Blanchard.

Gargini: Yes.

Sello: Personal friend.

Gargini: Yes, so Dick was the group leader, and the three of us was Jerry May, [ph?] and Gulshar Greewal, [ph?] and myself, and the three of us were-- he was somewhat the leader of the group, to say, okay, that's how we do, and so. So it ended up that I made the devices, so essentially, again, I cut the Mylar, made the mask, did all this stuff, and then I did all the runs from the beginning to the end myself. And then-- go ahead.

Sello: One historical point from educational point of view, when you went to work at Stanford, what was the official degree level of people at your level? Were they a Masters level, were they a PhD, were they--

Gargini: It was mostly Masters, and people that were maybe one year or two years away from their PhD.

Sello: And now if you track that back to Bologna, would that also be equivalent Masters level at Bologna, also?

Gargini: Yes, as I said, the difference is-- it's that in Italy it was more theoretical, so there was the tendency, spend time developing a new equation. Whereas the thing that I had to get adjusted was, you know, the back of the envelope calculations. Those were considered degrading, to be honest, if you did it in Italy, the professor would look at you.

Sello: I like that, back of the envelope.

Gargini: Yes, the back of the envelope calculation, that was absolutely out, okay, but if you-- I remember, in fact, at one time, I was solving a problem, and actually, I demonstrated, this was like one of these two hour tests in the class. I demonstrated actually that I didn't need to do the second calculation, because the two equation were off by a factor two so I didn't need to recalculate, I just took the other result and I multiplied by two, and the professor thought it was outstanding, because everybody else, did two sets of calculations, and only multiplied by two the result, and he said I hadn't thought about it myself. Whereas, you know, when I came to Stanford, it was simply this would be 3.4. Ah, (about) 3, you know, this would be 5.6, (this means it is) between 1 and 10. You know, so there was a very different approach, but you know, it's understandable because there was no-- first of all, the ability to even measure some of these elements with accuracy, they were only real on paper, but not in reality, so it was an interesting adjustment, you know, the things that, for me were more at the theoretical level, made the connection with reality, and after a while, you know, the two things could coexist in a natural way.

Sello: So germanium was gone from your thinking.

Gargini: Germanium was gone, it was just silicon.

Sello: Finito.(Finished, over)

Gargini: Finito, and the only thing was--

Sello: Silicon now full tilt was in your hands.

Gargini: Yes, and actually, you know, Professor Gibbons put together the first ion implantation machine, and then Krishna Saraswat trained me on how to deposit doped oxides. And to do those, you had to attach phosphine and diborene, and I said, "How do I know if they're attached?" Well, if you drop down, means that you didn't do it well, so I held my breath, carefully, before I opened the phosphine, you know it's not really good for you, okay? And I think I made the first doped oxide depositions, and I developed, actually, a technique for co-depositing first the phosphorus and then diborene, since it was a CMOS process in a single step, I could do source, and drain for both the P channel and the N channel.

Sello: So did Stanford consider you qualified to get a Masters degree, or a Ph-- how does that work?

Gargini: Krishna was-- Saraswat was getting his PhD, and we worked together, you know.

Sello: So you got your PhD--

Gargini: I didn't feel any different, okay, in what he knew and what I knew, okay? But you know, as I say, I had the advantage of having been exposed to this lab in Bologna. Probably for another person from Italy, would have taken him a couple of years to get a job, and catch up, because, you know, due to lack of instrumentation, that's the main problem I see. But other people from-- that I knew from Bologna, other places that had similar experience, in a couple of years, were adjusted.

Sello: So who do you credit for pushing you over, or letting you know about Stanford?

Gargini: It was really this professor in Bologna, that since his connection, you know, and actually, if I fast forward, when I-- after the three years when I went back and Professor Linvill came to visit, he came to visit the lab. Of course the only buildings you can get in Italy are official buildings, and Professor Linvill came, and he looked and we have frescos on the ceiling, because that was the only kind of rooms we could get. So we had put on our coats and walked though the lab__ but there were frescoes on the ceiling. And so I remember, we took him through the lab, and he couldn't look at anything. He said, "They have frescoes on the ceiling," that was the thing that impressed him the most, because of course, the equipment was just the same.

Sello: What a difference.

Gargini: Anyway, going back to this, in-- by '73, we had completed this CMOS project, and then Dick Blanchard said, "why don't you do what I do, you know, I work at Fairchild, and also I'm here to get my PhD," because he was precisely what he was doing working there. So he offered me to take a position at Fairchild, and I thought it would be interesting to go yet to the next step, that is to see (things) from the environment that I was already in (and it indeed was very different from the university in Bologna), because everybody (at Stanford) was very engaged in writing patents and publication, so then I wanted to see the real industry, and so I went to work, initially in Mountain View. And my test was, if, in two days, you can find the run, you're hired, because it was an absolute mess. All the runs were in a box with a piece of paper, but the system was so bad, that nobody really knew where the wafers were. And so my test, that I completed in a day and a half--

Sello: And the runs weren't numbered.

Gargini: Oh yes, but you know, you go to one station and it's checked out. "Where does it go?" "I don't know it goes to diffusion." "Well, where is it?" "I have no idea," right? So then I went-- eventually I found out this run, and so I spent five or six months there just for training, to understand--

Sello: Who was in charge at Fairchild at that time? Was that Wilf Corrigan, or was it [Les] Hogan?

Gargini: No, so Corrigan was still there, and then Tom Longo was there, and--

Sello: Okay, I've got that placed.

Gargini: So initially, I got my experience with diffusion. I remember one time bipolars were still very popular. So one of the problems was to create the collector that it took many hours to diffuse the collector underneath the epitaxial layer and so it was necessary, a very high dose of arsenic, and so in order to vaporize arsenic oxide, the furnace had two zones. There was a small furnace that was really supposed to vaporize the arsenic, and then it would go into the long furnace where it would stay for 14 hours, right?

Sello: like a pre-step. Yes, the long collection--

Gargini: A very long time. So I see this lady, and she loads the arsenic in the small furnace, and then she springs to push the wafer inside the furnace and then she comes to me smiling, and she said, this is going to be very good. And I say, “How can you tell?” “I can tell by how bitter it tastes in my mouth, I can tell that this is going to be a good---“

Sello: The arsenic.

Gargini: The arsenic. So I have no idea where she is today, and I don't want to know. But she looked at this, and I looked at her, and I said, "Do you know what the white powder is?" "No, but it tastes bitter, but not too bad." Okay, so and then, you know, another experience Gidu Shroff was in charge of Epitaxy [at that point--

Sello: Who was that?

Gargini: It was an Indian guy that then came to Fairchild to do epitaxy (Gidu Shroff) and then (later on) he came to work with Intel. And another interesting experience was that, you know, epitaxial deposition was using hydrogen. And the furnaces were using oxygen, and there was a single exhaust. And once a month, there would be a great explosion, because the mix of hydrogen and oxygen would reach the critical value, and the whole thing would blow up. And it took a year before people realized that maybe it was wise to have hydrogen in one exhaust and, you know, oxygen in another one, okay? So after the few months of training, then I got to move to the R & D in Palo Alto, in the--

Sello: Oh, did you?

Gargini: Yes, so I moved at the R & D, and at that point, Jim Early was in charge of the R & D.

Sello: Oh, he had recently just arrived there from Bell.

Gargini: Yes, just-- and then Gil Amelio had just completed his--

Sello: Gil Amelio, did you get to know Bruce Deal?

Gargini: Bruce Deal, yes, was still there doing oxide.

Sello: Yes, always, his whole lifetime.

Gargini: Always. I mean, how much oxide can you do, but Bruce Deal was something else, right? And Bill--

Sello: And surface states.

Gargini: Oh yes, surface states, they were so-- And then Bill Shepherd, there was one of the guys who put together, really, the epitaxial isoplanar process.

Sello: He was epitaxial guy.

Gargini: He was the I epitaxial guy.

Sello: He was pretty good.

Gargini: He was very good, and then Dr. Rich Schinnella was there also, and Dr. Conrad Deloca was there, and Dr. Art Lern so that was the team. And actually, it was a very enjoyable time, because the Fairchild R & D was somewhat in between Stanford and an industry, you had--

Sello: Yes, yes definitely.

Gargini: You had office for two people, you had technicians running everything, you write your recipe and people do it for you, and so forth.

Sello: And you're familiar with the term, “the engineers have the hand carried runs.”

Gargini: Oh yes, yes, yes. So--

Sello: That the operator, you have to get the permission of the operator to use the equipment.

Gargini: Oh, you-- we were not allowed, in general, you know, but then I was allowed, because they had to set up a new EPI reactor, and then that I could touch it, okay. And that was interesting, because Fairchild used to get very large contracts from the government, and in fact, we got a contract for 20 [charge-coupled devices] CCDs that, I didn't know at the time, they ended up on Voyager, and this was 1 million dollars in 1974.

Sello: Right. That's when Gil Amelio was in charge of that.

Gargini: Yes, that's why Gil Amelio was in charge of this.

Sello: Right, so you got to know him and--

Gargini: Yes, yes. And so the team was very interesting, but I got to learn something else that was going to be interesting for me afterwards. It was the fact that nothing ever transferred from the Fairchild R & D, to Mountain View. In fact, I personally transferred one technology in Mountain View, and by then, the industry was a strange mix. There were very knowledgeable scientist-engineer type people, and some other people had no idea what they were doing, especially manufacturing and so forth, these people had been hired from other industry because the volumes were going up, and it was felt we needed people with experience in manufacturing. And I remember going to the manufacturing manager with the run sheet that had all the instructions, and I felt like going to the butcher, and saying, “no, no, no, this, I can do it another way.” “No, I have a better way of doing this,” and it was clear that the level of knowledge of the person was simply adapting what I had brought to him, to the equipment that he had available to him. And this really set the elements, and later on convinced Gordon Moore and Grove, and Noyce not to have an R & D at Intel. And actually, it was simply, they got so disappointed by the Fairchild experience, that nothing would transfer, and then they realized that you had to force people to be into the manufacturing environment.

Sello: So you say here, you spent, like, seven years at the-- roughly at Fairchild.

Gargini: No, I spent about three or four years, because then I had to go back to Italy, because I had this—Fulbright (and these were the rules)

Sello: Oh yes, of course.

Gargini: And that's when I met the people at SGS. That was the time, because when I went back then--

Sello: Who was in charge of this? The guy from Motorola?

Gargini: So, Dr. Bedendo was in charge of manufacturing, and Dr. Berenga was in charge of R & D, and Dr. Ferla was doing everything. Dr. Zocchi was in charge of the Research

Sello: But the germanium was gone. Silicon was in.

Gargini: The germanium was gone. And the activity consisted in high-resolution cameras that could photograph a chip, and then do the layout directly out of the photograph, because at that time, there was no copyright, there was nothing. So that was the--

Sello: That's not an easy job to photograph.

Gargini: No, but they had incredible photographic elements, and then they would, at that point, computerize directly into the mask instrument, all these dimensions plus--

Sello: Each layer.

Gargini: All the layers. They would peel off everything, and then simply digitize and extract, essentially, mask information from reverse engineering, and at that point, it was perfectly legal.

Sello: What they call today, autopsy.

Gargini: Autopsy, that was legal, okay, at that point. And so, at that point, since there was an open offer from SGS to run wafers, for the university, that's what I went back to, to the lab in Bologna, this had been a standing offer for four or five years, that would come on this offer, and so I began running a few wafers there, and to make devices and so forth, and that's how I got to know several of the people there.

Sello: Did you know Guido Zargani?

Gargini: Yes, I know-- not as--

Sello: He was in manufacturing, I think.

Gargini: Yes, and Licciardello that is still there. Actually, he's still in charge of manufacturing. So I met several of the people. In fact, in '77, since, with another guy from the university, we were running a project there on using some components, we went there almost every week, to make-- to see the progress of our wafers and so forth, and by '77, I came back to the states for IEDM and one of these conferences that were held in San Francisco, and during my time at Stanford, I had met Craig Barrett, because we used to go swimming together, right, and he was a professor in material science at that point. And so, when I came back for this (conference), I ended up calling some of my friends, and I went out for dinner with Barrett. In the meantime, he had decided, “enough with theory, I'm going to go to the real industry”, and he was working at Intel. And so I said, “can I come to visit the next day?” So he said “sure”. I came, nine o'clock, the next day, I went to Mission College Boulevard, you know, and then I made it to the old plant in Bowers in Santa Clara One, and I show up and the guard says, “yes your interview team is waiting for you”. I said, “no, excuse me, there has to be a mistake“. And then there was a message from Barrett. “Sorry, I'm busy until noon, why don't you entertain yourself interviewing?” And that's precisely what I did. I had no idea, you know--

Sello: Who interviewed you, do you remember?

Gargini: Yes, George Perlegos. He actually had been one of my students at Stanford, so that was quite strange. And then--

Sello: Was Gene Myron involved within this at all?

Gargini: Yes, Gene Meieran was there, Ron Whittier, you know, all the people that were in--

Sello: My colleagues at Fairchild…

Gargini: Yes, all your-- so I-- all of the ex-Fairchild had moved there, right, so I said, I know you guys, I know your names, right? And I interviewed, but really, I thought it was like-- how should I say? Like a melodrama, it wasn't for real. I was laughing, having a good time--

Sello: Yes, excellent term. It was a melodrama.

Gargini: No emotion, right? It was a melodrama, so by noon, Barrett came up, we had lunch, he took me around, showed me everything, and I left. And then, you know, there was, after a month, I got a letter, and-- from Intel, an offer, and there is an offer, and I thought this must be a mistake. And after a month, I get another offer from Barrett, saying,” the first one wasn't good enough?”. I said, “Okay, make me an offer I cannot refuse”. Anyway, we went back and forth a few times, and by then I realized that, you know, the action was really here. You know, Silicon Valley was exploding, right?

Sello: Of course, of course.

Gargini: And so, by October, 1978, I came to Intel, working for Barrett, and at that time, he was working in reliability, and the problem he said that he had at that time, was that parts would come back, and it was extremely difficult to understand why they had failed. And maybe if we could do something new, like, do the reliability of oxide metallization, by the time we put them together, maybe they're not going to fail as quickly. So I said, “okay, that's interesting, but I want to do technology.” And so he said, “okay, I'll make you a deal. Do this, fix this for me for a couple of years, and then I'll get you back into technology.”

Sello: This was the time of the 1K memory device, do you remember?

Gargini: Yes, so at that time was already 4K, 16K, that was already the range, and so by beginning of 1980, he called me, the two years had gone by, and I managed, actually, to solve some basic problems. I'll give an example. We used to package parts in plastic, and at that point, the quality of plastic was not very good, and many of these parts would fail. And so I took several parts and baked them for different times, and actually, since they were memory, it was possible seeing the bit-map and I could see the diffusion of something from the edge of the die, because the failing bits, they began on the outside (of the die), and then progressed. “Something” was moving in. And so I concluded that it was just contamination coming from the outside, and so I modified the metal mask, so that it would seal off the edge, and this went away, and in fact, for as long as I know, we still had this rule, you know, seal off around the edge.

Sello: It sounds like-- capable of a patent.

Gargini: Yes, at that point, but Gordon used to say, write it on the silicon, don't write it on paper. You know, because he had gotten so dissatisfied with science, right?

Sello: One man in the world, Gordon Moore.

Gargini: So he didn't want us to publish, but to keep it as a secret, trade secret. And so at the beginning of 1980, Barrett called me, he was very apologetic, and said, “I can get you-- you know, you have done your part, and you solved a few problems for me, and now it's my turn to pay you back, so I have a job, I'm sorry, it's not very big, but it's Manager of Technology for microprocessors. We sell 20,000 per year. You have to do this 286. I only can give you 10 or 15 people. Stay two years behind the dynamic RAMs, you know, don't spend too much money, but at least it gets you back into the act“.

Sello: So this was the strength of Intel, the technical problem solving, and not the manufacturing production rates.

Gargini: No, in fact, if you go back, okay, when people left in the late '60s, from Fairchild, there were three groups. The marketing group went off and made AMD, the technology group made Intel, the manufacturing group made National. And if you look today, after 50 years, National can sell parts for 55 cents, and still make money, AMD is, let's make me a deal, right, they have deals with everybody, and Intel, we'll keep throwing technology until eventually the designers get it right. So--

Sello: Keep moving forward. That was Gordon Moore.

Gargini: Nothing has changed. Yes. That was Gordon Moore, so this was all, there was, and at that point, I inherited, after having had the Perkin Elmer, you know, in Fairchild, my first stepper, my first GCA stepper--

Sello: Was that your first stepper?

Gargini: this was my first stepper in 1980, you know, and all the rest was done with Perkin Elmer 200 and 300 at that point, and early on, when I first began, I had a K & S, Kulicke and Soffa, that was terrible, because the whole machine would shake and move, so it was almost impossible to maintain alignment. And the improvements came when Kasper came in, you know, but it was interesting, because I could look at the mask and the wafer and do the alignment and you could see that when the mask would press on the wafer, there was a shift between the two.

Sello: Between the mask and the image on the wafer.

Gargini: Yes, because the resist was so thick, and then the pressure was not well adjusted, so you could see the two things moving around until--

Sello: It doesn't take a Nobel prize to do that.

Gargini: So it was clear that that was not very good approach, right, and Kasper tried to do a proximity aligner, but in the end, you ended up somewhat touching and backing off, so it still has some of the drawbacks.

Sello: Now what was the mask, the photolithographic situation at Intel at that time. There was--was there a mask shop?

Gargini: No, so essentially, there was no mask shop, because the idea, again, from Gordon, was “we'll do only what adds value to the wafer. Everything else we will not do it.” And so we will not do R & D that is just a waste of time. We have the technology, let's squeeze it for the next 15 years, and we'll think about the next step afterwards.

Sello: Understood.

Gargini: And masks is not value add, that you can buy, you know--

Sello: So you could buy the masks you needed.

Gargini: At that point, there was no problem, yes, we can buy the masks. We would, you know, we would go as far as creating the Mylar and so forth, but then, you know, you would end up going outside. So it wasn't, and also when we began to do some masks inside, it was only one or two critical layers, because it was very difficult to get the specs we wanted. But by the early '80s, the ability of the GCA was really a major change, because all of a sudden, the resolution, you had the 10x instead of 1x, the resolution was very good, and the alignment was much better, than you could do when you aligned the whole wafer. The issue was, at that point, since each die is likely different in alignment, would the reliability be a random number? We didn't know. But we had problems, from one die to another, some of these issues were, some of the stuff we didn't know, okay?

Sello: That's right, that's right.

Gargini: And so we learned how to use the stepper, but then immediately we ran into a major problem (see 386 die size later on) , because in April, 1981, by accident, IBM selected Intel. This was more by accident than anything.

Sello: Really?

Gargini: The IBM group was reacting, simply to the Apple advertisement that they could do this PC, and at that time, IBM was selling the very large systems. And so Bill Lowe was given the assignment of slapping together, really, in a year, something that would be called the PC, just to show that IBM had presence (in the computer industry) from top to bottom, you know.

Sello: But still there was no mask making internally, it was still being purchased.

Gargini: Yes, essentially purchased, you know. The majority was purchased, and primarily we were cutting the Mylar, but then sending out, okay?

Sello: Sending out for the masks themselves.

Gargini: Put the mask--

Sello: Did wafer steppers come in for consideration at Intel?

Gargini: No, so-- and so the wafer stepper, since the-- it was becoming more difficult to get the resolution.

Sello: Because they killed-- wafer steppers killed the earlier company, the mask makers.

Gargini: Yes, so there was, you know, just for-- primarily for processors, because the die size was getting large, and so it was difficult to align and all the other elements. So what happened, just to go back to the selection from IBM, was that in one year, they were supposed to select the processor, and also to select the software. And if you go back and look at what happened, it's like a saga of errors, because the poor guy had the assignment to buy a 16 bit processor. At that point, there were many 16 bit processor in contention, from Motorola, from TI, from National, and Intel as well. But Bill had to do everything in one year. And so typically people would build the processor, but they didn't have the chipsets. And so Intel had a very odd 8088, that was 16 bit inside, but was 8 bit outside, so it needed to load two 8 bit words in order to load the full 16 bit, but it could use the chipsets of the 8080, that was, by now, six, seven year old. And I, from what I know, this was the primary reason why he selected the Intel die, because he could buy the whole set. Then the poor guy tried to buy the operating system and the applications. Nobody was paying attention, and I was told that he went to Seattle, and a smart guy bought overnight an operating system, and when the IBM guy pulled out a check for one million, said, no, I only want 100 dollars every time you sell one of these operating systems. And the IBM guy was so happy, because you know; this cost was going to be nothing for him. And as soon as he finished this project, immediately he got a promotion, and got to work on a real project, you know, real computers for IBM.

Sello: So he was from East Fishkill, he went back from IBM?

Gargini: Yes, so immediately got, you know, he transferred because this project was finished.

Sello: So where in this picture, can you say you became the PhD version of Dr. Gargini? Is there such a transition, or is that not the real--

Gargini: No, I was a gradual transformation, you know, essentially, and I think by then, you know, I was fully trained, essentially, so at that point, the problem was that all the sudden, making this processor, was becoming almost something that made sense, because it was-- I remember Ed Gelbach, took us out at the end of1981, because we had sold 20 or 30,000 units and that was a major accomplishment, right?

Sello: I would say so.

Gargini: And then, you know, the next challenge was really the 386, because the 386 was large, it was like one centimeter on the side, and so even on a GCA stepper, it was a single die per field and so that meant all it takes a defect. So we had to do something very strange, that is, we went back to 1x, and we developed the Ultratech 900, and the only reason why that got developed was because the lens arrangement was very strange. But we could place five or six die on a single mask, because it was 1x, and then we would print them out, and the one that didn't have defects was the one that was really used. So nobody at that time was using Ultratech, and that's why we made a contract for 100 at a price that you don't want to know, okay, and we bought 100 Ultratech 900. Well everybody was looking at us because they were just beginning to utilize the GCAs, and that was really the beginning. And then something even more interesting happened, because between 1982 and1983, at that point, there used to be a magazine called, "Electronics," that was really the gossip of all the events and--

Sello: Excuse me, Paolo, would you like to take a breath or a drink of water?

Gargini: Okay, sure, sure.

Sello: Yes, why not? Yes, I could talk to you about this all night long. Now were you an Intel Fellow by this time?

Gargini: No, no, no, no, so it's a long way to go. So at this point, we were just-- okay, are you taping again?

Sello: Yes.

Gargini: Are we on? Okay. So what was interesting in 1982, '83, was that this Electronics magazine published a result from HP that says, four suppliers, A, B, C, D in dynamic RAMs and the reliability and performance of A and B were very high, as compared to C and D. It only took a month to realize that A and B were really Hitachi and NEC, and the others were TI, Intel and everybody else. And that was the beginning of the crisis, because all of a sudden, Japan has very systematically, you know, absorbed the technology and copied the equipment and improved everything, and so forth, was becoming dominant in the semiconductor field of memory. And by 1983, '84, for the memory business, this was a major crisis. And it was made of two components. One component was that indeed, these devices were selling for prices that, according to the beliefs of the people in the valley, were too low. So the yield had to be incredibly high to be able to sell at those prices, and secondly, the equipment in Japan, had become much better than the equipment in the United States. Now, at that point, remember, 1982 to 1984, the US had about 90 percent market share in semiconductor, and 90 percent market share in equipment for semiconductor. And of course everybody thought this was going to go on forever. And by 1983, '84, we were in a full crisis mode, because indeed, it was clear, that doing reverse engineering, these lines were very straight, and the alignment was great, and so forth. And so there was instantaneously, this overnight realization that Japan had done something. Now, given the difference in culture, language and everything else, the level of understanding of the two groups was zero. And so many people went to Japan. I remember one time they flew all the Intel executives to Japan, but since no more than two could be on the same flight, somebody spent a lot of time booking 15 flights to have all the executive go there, and the executives came back, and it was hard to understand what they had understood, and then the general managers went there, and then the engineering manager went there, and then it came time to say, now we need to do something. So they came to me and say, you speak another language, right, why don't you go to Japan and trying to deal with them? Okay, that makes sense.

Sello: You speak another language-- Italian. This is Japan.

Gargini: So I said, “Okay, that makes sense,” right? And so you know, in 1985, I went to Japan, and just to tell you the situation at that point, GCA had developed G-Line, it was .35 NA (numerical aperature), and Bill Tobey and the Zeiss committee came to Intel and they told us that .35 NA was the highest that can ever be built, for all these reasons, for one day they explained it to us. And then they say, maybe we can build, you know, one or two at .38, but that's the limit, okay? So I went to Japan in December, and bought a Nikon that had a .42 NA aperture, and the main reason was, while the most conservative US and Germans had limit the number of elements to 14, Japan, since the tolerances had been improving, had told the lens maker to go to 18, 20 elements, and therefore, they could have much larger NA. So in December of 1985, I bought this, it was the first higher than .4 stepper to get out of Japan.

Sello: And this established Japanese lenses as capable.

Gargini: And that was with the Nikon lens, because the GCA, even though it utilizes some Nikon lenses early on, when they were making the step and repeat for masks, had switched to Zeiss by the time the GC8-4800, because the Zeiss lenses were better than the Japanese lenses. But there was like (you would expect), after ten years, Japan, again, had improved the technology, and so forth. What was interesting, all the experts in lithography in1986 told me that you cannot make a lens with more .35 NA, and I said, but I bought one. No, no, let me explain why you're wrong. I said, "I bought one, I have one in my lab."

Sello: This is on good evidence.

Gargini: So I think what was interesting was the level of disbelief, okay, I think people in the US were stunned, they couldn't believe that this equipment could be built.

Sello: How did the Germans feel? Zeiss and the rest, they were making lenses as good as that, weren't they?

Gargini: No, I mean, they thought it was impossible, because you know, they're very conservative, and you calculate and you do all the theoretical work, and you say 14 lens elements is the optimum. The tolerances don't allow you, because you know, the more elements that tie the tolerances to build this. So at the end of 1986, I bought another one that had .45 NA, and in 1987, they told me that it was impossible, because it cannot be built. And I appreciated that, and I simply thank-- thank you very much for your advice.

Sello: Thank you for the information.

Gargini: And in the meantime, I learned how to do business in Japan. It was very simple. After dinner, you know, you go singing Karaoke, and most of the people, the executives were somewhat scared. But for me, I was coming from Italy, I said, “okay, which one do we sing,” right?

Sello: And did you sing Karaoke in Italian?

Gargini: Oh yes, I mean, at that time, it was so-- it was funny, because the typical arrangement was, by 9:00, 9:15, the taxis are coming, and then they would tell me, stay behind. And we get everybody in the taxi and then me and all the Japanese guys will go and start the evening. And by one o'clock, we would write on a napkin the price of the stepper and the performance. I would put the napkin in my pocket, the next day I will make a copy, we both sign it, that was the contract. I've never written a contract with Nikon.

Sello: And that was the purchase right there.

Gargini: That was the purchase contract, okay?

Sello: Bravo.

Gargini: So that was a new experience, okay?

Sello: All I can say is, Madonna [(Oh my gosh).

Gargini: Yes, I mean, there was-- no, but actually, you know, it was simple, because you know, my grandfather was a negotiator, right, so on Thursday or Friday, people from around Florence would come to town, and he would take the two guys, they touch the two hands and say, “deal.” They had a little booklet and with all the numbers. So I had seen this technique before, so always dealt with Anelva, Nikon, Hitachi, you know, we would write on a piece of paper, and that was the contract. So that was the evolution of lithography, and in 1988, I bought the first Excimer, from Nikon that I put in Santa Clara One at the Bowers place, because eventually we were going to need this and it was interesting, because I went to some conferences were people where swearing up and down, you know, laser is going to be so expensive, can you imagine, half a million dollars. It will never come into my fab.

Sello: Talking about lasers, if I may just ask you a diversionary technical question. Where did MEBES and reticle generation by beams come into prominence, as far as Intel was concerned?

Gargini: So, by the late '80s, so then the masks had become really difficult, so they had to be done--

Sello: Another way.

Gargini: With e-beam and so forth.

Sello: Direct generation of a reticle.

Gargini: Generation, at that time, it was becoming necessary, and that's when we realized that it was complex enough, how you handled the data, the dense, it was the beginning. And Gordon was very reluctant. You know, it took us two or three years to convince him that we had to have a mask shop, and why can't you generate the Calma, the famous tape, and then send it out. So that's what we used to do until then.

Sello: I understand. He still was against it even as late as that.

Gargini: Oh, yes, yes. He was against, because, you know, as I said, “this is no value added, so why are you doing this?”

Sello: You're buying a service.

Gargini: You're buying a service, you know, buy the essentials, do all this stuff.

Sello: And you're not adding your own skills to it.

Gargini: No, you're not adding any skills to these things.

Sello: So did you become a fellow by this time?

Gargini: No, wait, it's getting--

Sello: I'm sorry, there's so much to ask you.

Gargini: No, no, so at that point, we-- you know, we bought the Excimer, adjusted to do tests and so forth. And at that point, it was still G-line, pushed the NA progressively up to .5 or so, and then it was the i-Line time, so at the end of the '80s, beginning of the '90s, was the time of i-Line, and in the meantime the things had changed. By now the microprocessors have become a reality, the disaster of 1986, in which we had to shut down dynamic RAMs, turned out into a major accomplishment.

Sello: A real blessing.

Gargini: Because in Livermore, where I was doing 386, we only had four inch wafers, so we had about 62 die on the wafer, of which only five were functional. It was impossible to have millions. And we shut down dynamic RAMs, and Gerry Parker, that was my boss at that time, said, “Grove gave me a fab in Oregon, because we shut down a brand new one micron fab, what do we do now?” And so that was when we moved the processor to Oregon, and all of a sudden, we went from one and a half to one micron technology, from four inch to six inch.

Sello: Now that was the fab, wasn't it, that [Ron] Whittier was running for the 256K RAMs?

Gargini: Yes, yes. That was his memory fab, and he could never produce at a cost that was competitive with Japan. And so at some point--

Sello: A very close friend of Andy Grove's.

Gargini: Yes, and at some point, you know--

Sello: I don't want to get personal, but--

Gargini: No, no, but Andy went to Gordon and said, "Okay, if this was not your company and you keep losing money for any part that you send out, what would you do?" He said, "I would shut it down." And so in 1986, that is the only year in which Intel lost money. It got shut down, but it was brand new. And so by transferring-- that was by accident, but it turned out the perfect strategy, because by transferring the 386 there, we went from 62 die to 392 die per wafer.

Sello: Fantastic.

Gargini: And in addition, they all worked. You know, the yields were even good, right? And so that allowed Intel really to be able to operate as a single source on the 386, and that made the difference. In the 286 Intel had given license to many companies, and only one company was able to find reasons based on one of the contracts that Whittier had written, and they were able, AMD, to explain that they had the right, or the X86 architecture. To this date, that's really the result of that contract.

Sello: I see, so all of this and at this point, the Japanese lens was still numero uno.

Gargini: Yes, numero uno, and then something interesting happened, that one-- when-- at that point, still the copyrights didn't exist much, so the fear was that the Japanese could have done to the microprocessor precisely what they had done to dynamic RAMs. They could have copied, put it on better equipment and so forth. So at that time, you know, with Gerry Parker, we discussed what can we do to make sure that we don't repeat the same errors, okay? And so it's a funny story, but you know, I was coming from my experience. The knowledge is, when Rome was founded, you know, Rome was a little town, initially, and they had to fight, as usual, with all the neighbors. And then after a while, they realized that fighting was not a very good deal. So there came a war with the people of Alba, they were nearby, and they decided, instead of a war, that is a bad deal, they selected three brothers on one side, and three brothers on the other side, and say, you know, the winner takes it all. That's very good on the economy, and we're done with that. So the three brothers on the Alba side were bigger guys, and the Romans were somewhat smaller. And by noontime, two of the Romans were dead, the other guys were wounded, but only one Roman was left, but he was very fast. So he began running, and these other guys, huffing and puffing, went after him, and when there was enough distance, he turned around and killed the first one, then he ran to kill the second one, so after all, why don't we do the same? The industry was frozen on three year cycle. And I said, it was not just me, but discussing--

Sello: For device development.

Gargini: Well, I mean, dynamic RAM was like a law of nature, was introduced every three years, right? And the point was “what if you go to a two year cycle?” Then it's going to-confuse - Japan, that is so systematic, to go from three years to two years, it's going to take them ten years before they even understand what we've done. And so we went to this. The second element, when we used to transfer something from development to manufacturing, manufacturing had the right to change all the equipment, because they were the experts in manufacturing, and we would lose one year. And then the second element was then, let's give all the manufacturing equipment to the development guys, since we had this fab in Oregon, and they're responsible not only to develop the technology, but to bring the yields to manufacturing level, and then--

Sello: That would be useful right to manufacturing directly.

Gargini: And then, to manufacturing, it was simply to copy exactly. And there were many other economical benefits, and I ended up, among others, explaining to the analysts what they thought was a really stupid idea, because you know, manufacturing guys are expert in increasing yields, and this was really taking them out of the equation, and so they didn't believe that this strategy would work. But it was actually, it was just the opposite. As time went by, this turned out to be an interesting strategy.

Anyway, going back to the lithography, the processor were getting larger, more complex, and at that point, we discover that we were reaching somewhat-- the limits of the existing steppers, even though they were useful, and we ran into SVGL that had developed with IBM, and government contracts, the scanner that was in a very unique, and you had already 248nm-250nm range of exposure wavelength, because they were using a mercury lamp, but they didn't need to narrow the line width. So we selected SVGL in March of 1994, SVGL for just two or three of the leading layers, the gate and contacts, some of this--

Sello: Critical layers.

Gargini: In the meantime, [Silicon Valley Group] SVGL was sinking, because they had lived off government contracts, and IBM contracts, and now they had to deal with the commercialization and some of these contracts were running out, so that really had cut down their staff. They could only produce seven or eight units per year, that was hardly sufficient. Furthermore, Canon was trying to really purchase, and so I would have never gotten involved if it hadn't been that in March 1994, we had selected as SVGL. And so they said, okay, since it's Japan, and see, why don't you go and find out what's going on? So in June of 1994, I spent the whole day in New York with this couple of executives of Canon at the time, to understand what their plan was, and then I spent time with Papken [Der Torossian] and all the other people at SVGL to understand what they thought the plan was. And then I draw my conclusions, and after a couple of months, you know, October, 1994, I flew both teams to Hawaii, and we put them on two sides of the table, and with a friend of mine, Japanese, and then between the two of us, explained to either side what the other side thought. And the next day, as the Japanese guys came back to the table, they start with the “Zannen nagara…” That means, “no deal”. So I heard all these legends that the US government intervened and so forth. No, it was very simple. By the time we explained to the two sides what they thought the deal was, it wasn't even close, the meeting of the minds, and so now we were left with the fact that SVGL had no money. Fortunately by then, Les Vadasz had gotten tired of technology, and he wanted to invest. So Gordon said, okay, “why don't you invest, do something, you know, keep yourself interested.”

Sello: So that's when he started the investment arm.

Gargini: Yes. So he had just started this, so I went to Les with Parker, and we said, you know, we need ten millions for this company, and then we called Motorola and say, we need ten million from you. And then we called TI, we [asked for] ten million, and by the time SEMATECH found out, they said, “I'm in”. So we put together 40 million [dollars] and we bought SVGL stock at a price that I will not repeat. And what happened, we began buying SVGL and we put, you know, scanners, we put people in SVGL and so forth, and after a few years, Les looked like a genius, because he made a lot of money on this investment, and so then they began to say, “Maybe this investment activity makes sense. You know, Les, why don't you continue?”, and that was the beginning, essentially--

Sello: That's what started it. Now, did-- where did Bob Noyce enter into the picture as a stimulus? Is it about this period?

Gargini: So-- no, so no, if we talk about equipment in general, if you go back to the crisis of the '80s with Japan, you know, one of the outcomes of that was that we understood, since we began buying Japanese equipment, Anelva, Hitachi, Nikon that this equipment was never breaking down. That was a new discovery. And so in Santa Clara One, we built two adjacent chambers in which we would put a Japanese equipment in one chamber, and we will put a US equivalent in the next chamber, and then we will compel the CEO of the company to watch his equipment fail after two hours, while the Japanese equipment had been going for a week. And so in 1987, when SEMATECH was initially formed, there was the idea was to produce dynamic RAMS and static RAMS, and then the lawyers discovered that if we were doing that with 14, 15 US companies, we are violating all the laws that we were accusing the Japanese to violate.

Sello: Monopolistic, monopolistic control, whatever.

Gargini: Well, also similar to keiretsu, scheming, you know, all this stuff, right? So at that point, SEMATECH was left in1988, to making wafers, and the only way of being legal, at the end of the line, they would crash all the wafers, so there was some point, useless. And so since we had discovered that actually, what we could fix was really the equipment, then we proposed the same procedure, essentially to switch the direction of SEMATECH from making wafers to, instead fixing the US equipment, and that's when Noyce came, you know, he was the chairman of SEMATECH.

Sello: Of SEMATECH, right.

Gargini: And at that time, you know, essentially, we gave SEMATECH the whole procedure (of testing the endurance of the equipment) that we had developed here in Bowers, and SEMATECH created the name Iron Man (i.e., endurance test) that was essentially, the equipment was going to be put though the Iron Man until it broke down and then there would be the list of defects (failures), and the supplier was supposed to fix it (the equipment failures), and so forth, so the major benefit of SEMATECH was really to help, not the IC companies, but really, to help the equipment company to really come back to speed.

Sello: It kept them in business.

Gargini: To put them back in business, and then if you look in '92, '93 and so forth, then the erosion of the business leveled off at about 50-50. So essentially, we saved SVGL in 1994 with this effort, and the next year, 1995, another strange occurrence happened. We had a visit from

Mitsubishi, and again, Parker walks out of the meeting, and he said. "You've been sleeping." And I said, "What did I do?" So I look, and Mitsubishi shows me the design of a fab, and it's circular, because they have a synchrotron in the middle, x-rays right as you're going to manufacturing.

Sello: Madonna (Oh my gosh)

Gargini: Oh, okay, so let me see. So in 1995, I went to IBM, and of course they were very happy to discuss with me, and then I went to NTT in Japan, I went to Mitsubishi, collected all the data, and the conclusion was that it took them about five or six masks to produce a mask with the right dimension, and then it took another five or six masks to produce the right level of defects, but if you were willing to make anywhere from seven to ten masks, you could have made one layer. And so at that point, in the '90s, was really the middle of the gigahertz, megahertz rush, right, so making the gate a little smaller was real money because you could produce something, that was, you know, 2.3, you know, megahertz, all these kind of numbers, you know, that all the way ended up in 2000 at the gigahertz, but making something that was 10, 12, 15, 25 megahertz higher, you know, was valuable. And so in the meantime, Bell Labs was still trying to advertise the shaped e-beam scalpel and so forth. So the point was, what if one of these works, and so Parker said, “do we have anything?” So well, you know, we had this one million dollars small program at the National Labs, called EUV, but it's not going to be ready until 2010. “But does it get me to the table?” “Yes, you can-- going to be at the table and we can bluff these other guys, and we can get a cross license agreement.” And so we began in 1996, the government says that actually, we're shutting down EUV, so we invested 40 million, and then the next year in 1997, we formed the EUV LLC, and we got IBM to join, Motorola, AMD, TI, until 2001, we produced the first prototype was only .1 NA, but it was put together by Lawrence Livermore working with another couple of National Labs, and by the time we are ready to deal, x-ray and scalpel had disappeared. So by default, we stumbled in on something that hasn't quite made it to manufacturing, but is the only alternative that we have left at this point. And so hopefully they forecast it is the first machine manufacturing-like, this was 1996, would be 2010. So when last year, SML delivered the first manufacturing machine, I said, okay, I proved my point.

Sello: One last question. That's fabulous, fabulous. We're now looking at the end-- not the end, but the top end of Moore's prediction about-- Moore's Law prediction, and we're up into the tens of billions of devices per chip. And now, all of a sudden, we come up with vertical gates, so where do we go from here? Can you predict?

Gargini: I mean, if you look in any town, what you do, right, if you have limited space, you go up. So we only just started.

Sello: That's right, what's that going to do to the photolithography, where will it go?

Gargini: Well, I mean, we still, we need high-resolution lithography, hopefully we still need the EUV to come online. Right now, what we're doing is double exposure, so if you want to do 30 nanometers, you do it within two steps, 60 and 60 by interlace, and you can come up with the right number, okay? If you have a product that it has a high selling price, that price that is still doable. For those that, however, have to sell for a few dollars, that double the cost of lithography, so the cost of lithography, the traditional has been 20, 25, 30, it's quickly going into the 50 percent range, so that's really the downside that makes it difficult for people, they only have few dollars to play with, to remain in the game.

Sello: So, but technologically, there's-- you see no limit to where, at the moment, to where there still can be improvements relative to the making of masks, or relative to the lithography.

Gargini: So we're making this EUV masks, and we're being constantly decreasing the number of defects. We are now making devices in the 20, 30 nanometer, we know that they can work down to 10. That takes us to the end of this decade, and so we have still a few years to figure out what to do next. So still we have a lot of room to allow.

Sello: A lot of room for improvement. Okay.

END OF SESSION ONE

SESSION TWO, conducted by Daryl Hatano

Daryl Hatano: Well, good afternoon, I have the privilege today to interview Paolo Gargini, he's the Director of Technology Strategy and Intel Fellow, but for purposes of today's interview, he's also the chairman of the International Technology Roadmap for Semiconductors or ITRS. My name is Daryl Hatano, I'm with ON Semiconductor, but until very recently, I had been with the Semiconductor Industry Association or SIA, which sponsors the ITRS and so I've been on the periphery of all of this from the beginning. But today I really am looking forward to getting the insider's view of the whole history of the ITRS, and I think what we're going to be doing today is spending some time on the history, some time on the process, and really, also focus on the accomplishments of ITRS because I think it's been a major driver for the technology advancements in the industry. Paolo, if you would start by just giving us a very quick one or two sentence overview of the ITRS, and what is the purpose of ITRS?

Gargini: So the ITRS**,** it ends up being a document that is available for free to everybody, can be downloaded 100 percent from the website, and it contains trends for the next 15 years, on the major fields related to semiconductor from the process, design, reliability, packaging and so forth.

And it's a very useful document, because everybody can refer to the same document, so it's like the equivalent of an encyclopedia that contains all this information available to everybody.

Hatano: Great, if we can start now with looking at the history, ITRS didn't really start as an ITRS, just started as a national technology roadmap for semiconductors in 1992. Can you help set the stage for us in that time period? What was going on in the semiconductor industry at that point?

Gargini: Yes, actually, my involvement with road-mapping began much earlier than that. In around 1982/83, all the sudden, Japan was becoming the major power in the semiconductor, growing very fast, and they caught all of us by surprise because we knew very little of Japanese companies. A few of them were subcontractors, but we didn't expect them to be able to produce components that were much better than the ones that were produced in the US. And so we realized that it was important to get a level of awareness where this technology was coming from, and we didn't want to rely exclusively on buying reports from somebody, and so Gerry Parker that was my supervisor at that point, suggested that we should form our own internal analysis group that would do benchmarking and begin developing roadmaps for different companies, and especially in Japan.

So in that timeframe, 1983/85, I put together a small group, and, in particular, I hired people that could read Japanese, and we discovered, after a few years, by 1986/87, that actually Japan was extremely prolific in publishing technical data, and it has always been this strange attitude that what is published in Japanese is for the Japanese only, and only Japanese can speak Japanese and so forth. So they actually were incredibly open with the amount of information available. That was quite surprising. So one of the things that I was able to do by the late '80s or the early '90s, with this group, is really to develop roadmaps for different companies related to the rate at which they were introducing technology, die sizes and so forth. So actually, I took a 1991 book that I was producing at that time, and I want to show you just an example that contain, for dynamic RAMs, the die size, the sequence in which it was going to be reduced. They contain the spec of the different-- and most of these have been abstracted from this very interesting magazine called, "Nikkei Microdevices," to which I still subscribe. And if you look inside this magazine, this is a 1993 for instance, we would find the detailed litho roadmap and essentially very similar to the roadmap that the US was building at the time.

So when, in '92, I completed my ultimate contract, whereby I was able, in December, of buying a stepper from Nikon that was more advanced than what they were selling to any CEOs in Japan , the most advanced Japanese companies, then Barrett said, “now that we understand Japan, now it's time to go back and fix the US.” So I have two assignments: one is bring the roadmap close to what is really happening, as opposed to being more idealistic, and SEMATECH needs to evolve into something else. And so with this, I got involved in the 1993/94 NTRS (National Technology Roadmap for Semiconductors) process, and I discovered many inconsistencies.

In addition, I went inside the process on how data was generated. Between 1985 and 1992, I spent almost three years in Japan. I attended many conferences, and in some of the conferences, they were showing how the SIA roadmap was inconsistent in some places, especially, about dynamic RAMs. And actually, one of the-- for instance, you could find publication that had, in excruciating details, all the trends that the NTRS was trying to publish at the time, but if you look at the NTRS for instance, the publication was limited to a very simple table with a few numbers and then there were many, many pages of text; just roadmap and no numbers. And so it was interesting for me, that when I picked up the initial publication of the 1991 workshop in 1992, and I looked through this, there was no reference to gate measurement. I looked and there were dynamic RAMs, one number only; and then there were data only every three years, and the die size for dynamic RAMs were incredibly big. And there was not awareness that that was just the introduction. Let me see if I can find-- I had one that was very interesting. So two things; First of all, this Japanese roadmap had a gate (information) in excruciating details, you know to the transfers of dynamic RAM in production and similar information for microprocessor, and these data were not in the roadmap, and they were clearly identified by quota (quantity), just about, okay?

So I discovered that there were many elements where the roadmap was very light. Most of all, what was disconcerting for me, that in public conferences in Japan, they were showing, this is the SIA data and this is the real data, right? So I didn't understand why there was this discrepancy. So when I joined the group in 1993, the process was already in progress. Owen Williams was very organized, was very-- driving a very tight ship and was keeping everybody in line, and I look at some of the numbers, and since-- I'll give you an example. Since the roadmap was using the die size at introduction, and this, very quickly, in the next six months was immediately reduced by 30 percent, another six months by another 30 percent, those were the ones that were really going into volume manufacturing. But one of the assumptions was, take the large die size, and make the assumption that that is the one that goes into production, and then you come up with defect numbers that were completely impossible to realize.

So when I came on the roadmap, I began arguing this point, but it was dominated by many people that were highly theoretical, and I understood, after a while, that the motivation was that, at that time was still very popular, to submit proposal to the government for funding, and one of the problems that had come back from the government, the national labs, university and industry, would go and propose programs to the government, with completely different roadmaps and so the idea was to create a document that was the collective wisdom of all these components and this would be an element from which everybody can point to the same data, so it was a very good reason for doing so. In fact, I was surprised that, as soon as the 1994 roadmap was completed, in the same session, everybody began writing white papers to submit to the government for funding. The other thing that was very interesting was that it was very accurate in the first five years, and then the subsequent ten years were just an extrapolation on numbers. And some of the numbers, there was no intelligence, how you would obtain these numbers.

In fact, to give you an idea, if you look at the 1994 roadmap, polysilicon was going to be the dominant material, you know, all the way to 2010. There was no concept that, at some point, this needed to change to another material and so forth. So I think it was very good in the first few years, but then there wasn't a systematic study projecting trends beyond five years, and trying to put realistic elements. So when-- in 1996, the 1997 roadmap started, since I have been very vocal, Owen said, "Okay, why don't you give the opening presentation?" We met in Dallas, we used to meet at the hotel right at the airport. It was very convenient, so everybody could fly in and fly out. And so, together with Alan Allan, we put together a booklet of about 60 pages, that I handed out to all the different people, showing the inconsistency of the '94 roadmap, and what I was proposing for the '97 roadmap.

It initially created an uproar, because I was contesting defect numbers, die sizes and there were no gate-- there were no frequency numbers, and many other elements. So it was a very vivacious conference, and this discussion continued for the next few months, and then, after a while, you know, I had the opportunity to sit down with Owen, and Owen said, "You know, actually, thank you for doing all this. Actually, I would like you to be the Vice Chair, because many people have the tendency to provide a little information but not too many people want to be involved with all the aspects. And it seems to me that you have been doing some of this, so it would be ideal that if you would do this for me."

So I accepted, and indeed, in the 1997 roadmap, for the first time, for instance, I had the idea that by the middle of the next decade, 2004/5/6, we had to introduce high-k metal gate. And at the end of the meeting, especially the university people were terrorized, because they thought they were not going to think about it until 2010. And at that point, this was in '97, and as I, in the next six years, you had to be ready, so, and it was really a terror for all of them all the sudden, something that was very low key project, all the sudden was becoming very important. The benefit of it was that indeed, fortunately for all of us, many of the universities began working on it, and with the exception of a few publications, nobody has really taken it seriously, because people that, many time, I remember in the past, like in the '80s, there was the one micron barrier. “Nobody can do less than one micron.” Then when lithography was able to produce results less than one micron, there was the next number, nobody can do quarter micron and so forth. And so progressively, the mood had gotten from extreme pessimism, to tempered optimism. Anything can be done.

And so all the sudden, saying that we had to change the fundamentals, that was a major element. One of the convincing elements, I plotted the trend to gate oxide thickness in a different way. Instead of having thickness versus time, that I had seen many times in the past, instead of thickness, I counted the number of monolayers, and I showed to people in '97, that we had about seven or eight monolayers, and if you eliminate the one or so every couple of years, by the time comes, 2005, you'll be down to one monolayer. So it has nothing to do with the quality of the oxide, it has nothing to do with the equipment, that after one, there will be none. And this, again, you know, created panic, and I remember attending 1998, '99 and 2000 IEDM conferences, and nothing was working, because everybody could deposit some high-k layer but then the mobility would be destroyed and so forth. So this created quite a change in the pace of the roadmap. I would say it went from the transition, from being a document to write white papers, to a live document that was now pointing out some problems, and progressively, we said, this is like running into a wall, and then it became a brick wall, and then it became a red brick wall, and the idea was, not that these were fundamental walls, but if you don't so something there will become one of these walls.

Hatano: And it was red brick wall because of the color coding in the ITRS (and NTRS).

Gargini: Of the ITRS. So until then, there was not much emphasis in the color coding, but that was another idea, how do we make this relevant for people? And so we emphasized this yellow and red and white. They became all the sudden it was a sea of red after five or six years, and then everybody’s awareness really increase.

During that time, what has occurred in Japan?, By 1995, the economy that had collapsed in 1992 had not recovered yet and, the US equipment suppliers and IC makers had regained market share, Japan had not been able to make an inroad in microprocessors, so all the sudden, there was this feeling that something had gone wrong. Let's look at what the US has done, and maybe we can find the secret in this, to really regain leadership. And so they put together a think tank, and the came up with some conclusions. The US had SEMATECH; SEMATECH had been instrumental in revitalizing the US semiconductor industry. The US had SRC, they had revitalized the energy in the university, and the US had the roadmap. So with these three elements, Japan had none of these, these were elements that had really allowed the US to regroup after the initial inroad made by Japan, and if Japan would do the same, then we could regain leadership. And so by 1996, they began forming SELETE [Semiconductor Leading Edge Technologies] that was the equivalent of SEMATECH, they began STARC [Semiconductor Technology Academic Research Center], that was the equivalent of SRC, and they also began discuss 300 millimeter, that actually began discussing standards in April 1994, and then the last element that they were studied, how to form a Japan roadmap. And I think that would have been a disaster, because then we would have had direct conflict between the US roadmap, and the Japan roadmap. So I thought that, to be honest, that was the most damaging element, because the world would have been completely confused. So as soon as-- of course I found out in the bar at two o'clock in the morning, and they say, "Yes, yes, but we're forming our own roadmap." I said, "What do you mean?" "Yes, we're going to have the JNTRS." "Oh, what is that?" "Oh, the Japanese roadmap. You have so many errors and so forth, okay?"

So then, you know, again, I went back to my experience, and so, if you remember, what, in "The Godfather," said, I want my friends next to me, but my enemies even closer, and so I said, okay, so what I'm-- so I know a very simple way of diffusing this problem, I'm going to make him part of the roadmap, something they have-- becomes our roadmap, it's not us against them, and so forth. And, to be honest the people in Japan, they are very wise, and so I discussed with some of the people that realized this was going to be a conflict again, like the mid '80s, when the US government, some of the people told me they have flown 100 times to Japan, in negotiation to settle dynamic RAMs flash memory and so forth. It smelled very similar to that kind of confrontation. And they realized that even though the intention of the think tank, maybe SELETE was okay, but this was very bad, there was going to be major confrontation. so we discussed, “now what should we do?” And they say, “okay, you know, indeed, if you absorb what we're trying to do into the-- and making it international roadmap, then it, at least remove the contention between two opposite and divergent roadmaps.” So with this idea, I discussed this proposal with the SIA board. They were somewhat laughing at me and say I was going to spend the next three years before they even pay attention to you.

But then I discussed, actually, with people and CEO of Japanese companies, said, you know, actually, the best strategy is really to use the WSC, the World Semiconductor Council, because if the US makes a proposal, how can we say no? That would be really contentious on our side. So I explained to George Scalise, and he put it on the agenda of the [World Semiconductor Council] WSC, so in March, 1998, the SIA board, somewhat laughing, approved, yes, make it international. George Scalise put it on the agenda of the WSC in April, and they all accepted, and the next day they got invitations in the mail, and now they were really on the spot. So then I called the first meeting, July in 1998 of the International Technology Roadmap for Semiconductor (ITRS), and we hosted the first meeting in San Francisco, on July 11th, 1998, and everybody came extremely suspicious. And so the question was, “Is this a secret SEMATECH scam? Is this really open, or are we just going to get some of the data and so forth, okay?” So that was the first meeting, and at that meeting, I presented some of these results of some of this project for the 1998 roadmap, essentially that what I said that we can edit at the end, that we extended on April 23rd, that the WSC, 1998. We had the first meeting on July 10th and 11th in San Francisco, and then the follow up meeting in December, 10 and 11, 1998, SFO, and the results, that 50 percent of the 1997 NTRS tables had to be either changed or modified or corrected. And once we let people modify and indeed, we published-- this was the draft of the 1998, and we viewed the new logo, that was completely different. The previous one was somewhat reminding people of Texas, as you can imagine, and this one, instead--

Hatano: Because of the star.

Gargini: Because of the star, and because SEMATECH, you know, if you look at the back, you know, this was published by SEMATECH, and so they came up with the star of Texas. Probably this was not appropriate, so we changed it to this, the look at the world with ITRS on top, and then they began to believe. Now they said this is real. And then you know, all the regions really began staffing this activity, and the idea was that after the first three roadmaps that had been done, you know, the workshop and the activity in the US, then the next one was going to be really international. And then the change was that with the 1999 roadmap, we were going to start to rotate. It's not just the US, we want to show that is for real, so in December, 1999, the meeting was in Tokyo. And then we began this methodology whereby the first-- the spring meeting is in Europe, the summer meeting is in San Francisco, and the winter meeting rotates between Korea, Japan and Taiwan. And at that point they were caught in the process, and so collectively we began looking at these charts and that was 1998/99, and 2005 (when major changes were required in the CMSO process) was extremely close.

But now what we obtained, and not only the US university were working on high-k metal gate, this is right in the middle, just as an example, and the whole world was working on it. And so I had outlined in the 1998, the first meeting, this roadmap. Essentially this said that by 2004, or so, we need to change the gate, we need to change the technology drastically. At that time, I called it equivalence scaling, that is the designers will never know what we've done to them, but we changed the materials, we changed many things, then we need to change the level of integration with package, and then by 2010, we need to be serious on real nanotechnology, okay? Of course the names that evolved that had the non-classical scaling, this became SOC, 3D and all the other stuff, and this became nanotechnology that really began in 2000 and so forth, okay? The idea was really what the ITRS has become, is a document, and that of this document that even research organizations and consortia place their attention onto it, and then eventually goes to the suppliers and all through to the IC companies.

So with this methodology, we switched also to a yearly roadmap, because the technology was changing so much that the idea, originally, we do the 1992, 1997, maybe by 2000 we do another roadmap. It would have been obsolete the next year. So we switched to a yearly roadmap (process), and Owen Williams, who initially, and for very good reason at the time, has said that the book should be only 200 pages, so since there were 10, 11 groups he gave 15 pages to each one of the groups, and then the introduction and so forth, and just to give you an idea, by 2005-- sorry, this was still the roadmap methodology but. now counting something like 800 pages from the original 200, and we realized that this was going to go out of control. So by 2005, we switched to CDs and web and so forth, because it had really become an encyclopedia and it was not possible to limit people to 10 or 15 pages.

Hatano: I know the executive summary is about 70 pages.

Gargini: Yes, just the summary is what it used to be, right? So with this, you know, we came to 2000, and it was fortunate that we had already gotten the whole world to synchronize for the next steps in CMOS (technology), and then, all of a sudden, in January of 2000, President Clinton announced nanotechnology, the NNI [National Nanotechnology Initiative] project was beginning for the US. In the past, only a few companies that were really involved with government programs, but this practice was not an industry-wide kind of (government-industry) cooperation.

But the point was that, indeed, by 2020, we had to create new devices. In addition, crossing this major 100 nanometer (barrier) was imminent for the semiconductor industry, so it was time to change the model of interaction with the government, and not only in the US, the governments in general, whereby, many times, the governments had a tendency to invest for ten years, and after ten years, there is the expectation that the industry will pick it up and do something with it. It was clear in this case, that the opportunity of bootstrapping the investments of the government--because the US began in 2000, Japan in 2001, Europe, 2002, immediately everybody was doing nanotechnology--you could get the highest value by co-investing and participating actively, as opposed to waiting until 2011 and say, “let's see what the governments have done, is there anything for me?”

So one of the elements was to form especially the Emerging Research Devices group, at that point, included a group of people that had never been included in the roadmap. We took physicists, many people that were really working on pure speculative projects, and material science people. They were not involved with the roadmap before, and we had this group that began mapping, if CMOS comes to an end, what would be after this, and after silicon, what would be available? Questions that nobody had had the courage to really ask until then, at least, nobody had taken them seriously in the industry. But by forming this group, that was really spearheaded by the SIA--we met in the [SIA] office down in Technology Road that was the easy place to get together. This group, by 2003, published fundamental limits of silicon that showed that, indeed, something else needed to be done. By then Europe and Japan were fully up to speed, Korea was beginning, and Taiwan realized they had to do something. So from the emphasis of the roadmap in the '90, in which it began simply collecting the inputs of government, academia, and industry, more or less, like a diary; this is what's happening then, it (ITRS) had become now, an instrument which was forecasting major transitions in the semiconductor industry and calling people to invest and cooperate in a pre-competitive way, and this a new element (on how to cooperate Internationally).

With the stage set in this way, I got invited by [Mihail] Mike Roco, to go to one of his NNI presentations, in 2002 in Washington. It was there, because Mike wanted to show to the politicians, that indeed, he was investing the money wisely. But after the meeting, I began talking to Mike Roco, and said, ”actually, we have many commonalities, maybe we should join forces.” And this dialogue began to include more and more people than ever have been involved with the roadmap; theoretical physicists, people that had been working on low temperature physics and so forth. All of a sudden and they could see the similar equations or problems were actually also to be solved with the semiconductor industry. And in October of 2003, I hosted a conference in Oregon, whereby I invited, besides Mike Roco and some of his key people. Mike Roco is from NSF, in addition, people from NIST, DARPA, DOE and so forth, people from the universities, people from different companies. We spent two or three days looking at opportunities, and out of this we formed four or five working groups, to identify commonalities between what the NNI was doing and what the semiconductor industry wanted to do. And with this, by 2004, in June of 2004, I presented to the SIA board, the proposal to really initiate a nanotechnology research initiative sponsored by the members of SIA, that would be synchronized with the NNI activity. By 2005, this was fully ratified, and in the November meeting, Barrett, like negotiating chickens in a market, asked everybody to contribute to this first one million and initially we had five companies that was easy to divide, that was 200K, and then a sixth company came, so everybody calculated it was 167,000, and let's forget about the decimal points, and so at the end of the meeting, in November 2005, six companies from the SIA board had signed up for this initiative. And immediately in January, we began co-investing the first one million with the NNI project, you know, in key universities.

Hatano: And at this point, you're also chairing the SIA Technology Strategy Committee.

Gargini: And at that point, fortunately, I was chairing the SIA-- so that allowed me to play this dual role, you know, go directly from theory to first step of implementation. So that was a very fortunate period. I think, between 2003 and 2005, we really set the foundation for research, primarily in the United States, but even in Japan, Europe, and Taiwan and Korea, that is really just beginning to pay off, in that we begin to see some of these devices being possible devices that could be useful in the second half of this decade.

So with this, the initial experimental work in international technology working groups became real, and then, in the meantime, the semiconductor industry was reaching consumers more and more, and so wireless techniques were becoming more and more popular, and so we added also a chapter that was taking into consideration, analog and wireless. Actually, many companies were skeptical, because this was not using silicon, it was using III-V again, III-V had tried to come into the mainstream, for a long time, but given that they couldn't produce wafers in a size comparable to what the semiconductor industry could do, they have remained almost in the background. So with this, the consideration for III-V began in the ERD [Emerging Research Devices Group]. The first element of relevance, and low and behold, the introduction of germanium into the source and drain process was creating strain that was enhancing the performance of transistors.

So in all, the forgotten semiconductor was coming back again in the front stage, and it began like in a concentration, 10-15 percent and devices now has almost 50-50, germanium and silicon--so the source and drain, you could call them germanium source and drain, as opposed to silicon source and drain. Then by 2007, introduction of high-k metal gate, about ten years after the initial NTRS and the ITRS had pointed to something that was needed in that timeframe, became a reality, proving that, indeed, it was necessary to call the alarm ten years in advance. So people began paying more attention to what was in the ITRS 10 years out and 15 years out, believing that the probabilities of this becoming reality down the road, was becoming more and more real.

And with this, indeed, we put also tri-gate on the roadmap, and we began in 2002, 2003, forecasting, maybe in the next ten years, this will become relevant, and indeed, in 2011, it became a reality, and now we come to the point that actually III-IV have been transferred from the [Emerging Research Devices] (ERD) to the [Process Integration Devices & Structure] PIDS group, that is more the group that looks at the next five years. This is the group that looks at processing integration and device structure, and is really focused more on the next five years, as opposed to the ERD, which is focused ten years out. So the ability of the roadmap to evolve and change, and not becoming what many people accused it to be in 1999 or 2000, just a repetition of geometrical scaling, in that was just a bunch of old people just repeating the same numbers, changed under their own very eyes into something that really has evolved. Now we introduced this year, MEMS [Micro-Electro Mechanical Systems] that nobody thought they even could be mapped in some form similar to the roadmap, and we are now with 17 groups from the 10, 11 that we began in the early years, meaning that the number is almost double. And nobody has complained the fact that we have analog, or III-V or RF [Radio Frequency] because in the meantime the consumer has absorbed this new technology very eagerly. And it has turned something that was more of a curiosity into something real.

So actually I have a good story that somebody from STM [ST Microelectronics] told me. They are one of the largest producers of MEMS. They had done some prototyping of MEMS to be used as a gyro that you can look at direction; that's what's used nowadays when you flip around your phone or your tablet; you know the image rotates with it. The results were excellent until the parts were packaged, and then the part became very inefficient. So the guy that was telling me this. He went to Japan, and there had been, as usual, an earthquake. So he asked the question, "How is it possible that these buildings, they have been staying around for hundreds of years and no damage?" And somebody said "because they have a pivot," and essentially the whole temple can swing. He realized that in the moment he was assembling MEMS, he was simply clamping them down, and the trick was really, for the assembly of this gyro really to assemble only at one point so that the MEM could, indeed, move. And when you put them into a package, and you clamp them down, of course they lose their capability because they are frozen. So he came back anxiously and asked the people in packaging to do it in this way, and all of a sudden the results were impressive.

Hatano: I wanted to move now towards process. How do we get all these different working groups together from around the world, academics, industry, governments and somehow come up with a final product at the end?

Gargini: Okay, yes. When I was first introduced to the roadmap in 1993, the process was very straight [forward]. There were two people from the university, two from the industry, two from the government, and then there was the possibility of adding, ad hoc, a couple of experts in the field. But the groups were monitored very strictly to make sure that a single component wouldn't overwhelm another component. And by 1997 people began to feel comfortable with each other.

Then the number of experts kept on increasing. So in reality, it began to migrate to a group of experts almost independently from where they were coming from. When we went to the international process we had to revert back to some order. They wanted to make sure each region would have two people and two people on the IRC, that is international map committee, and so forth. So the process for the first couple of years was somewhat restrictive.

But as people became more comfortable with each other, then the groups, even though they still have components from the three different elements-- and in addition suppliers have become very active part of the roadmap, as well-- we rely more and more on expertise than being strictly watching the numbers as they are. So normally what happens, we try to alternate. One year we completely rewrite the roadmap. And the next year we just adjust the tables and reflect a few of the changes. In this way, there is a year of preparation and a year of hard work. So in this respect, the level of confidence among the different regions has become such that different chapters are led by people in different regions. And so, for instance, in this moment Europe is very strong in “More than Moore”, that is techniques that add functionality without pushing the scaling to the limit. And for different reasons-- for instance, the fact that consumers want many functionality, and it’s not necessarily like producing dynamic RAM [where] you have to be on the leading edge of the technology. And so typically the European group leads into this field. Packaging, many times, we have a strong contribution from Japan, or even Taiwan because of their origin, and so forth. Then we got many professors from Japan involved into this activity. So the leadership of the different groups somewhat rotates, or is distributed depending where the expertise is.

Normally in the April meeting we have the review of what happened with the previous roadmap. And we identify the places where we think additional work is needed. Then in the July meeting we have two or three days of workshop, and then one day where we present a draft of the roadmap to the public. In this meeting we identify who is supposed to work in the next couple of months to complete all the chapters to make sure that they are essentially completed by the time in December when we meet in Asia, whether it’s Korea, Taiwan, or Japan. Then normally the roadmap, by January, is available on the web. Since we have embraced all the new techniques, one of the advantages is instead of just intermixing tables with the text; we make references in the text to specific tables. This can be simply link to the specific chapter so that very detailed tables can be generated without disrupting the text, just as an example. The roadmap has really become a web instrument because if we were to publish a book now I’m afraid it would be several thousand pages to have all this information.

Hatano: What were the more controversial technical issues that you had in one of your working sessions? Very different business interests, different focuses, especially as the industry has evolved over time in more specialization. Describe one of those situations and how that was resolved.

Gargini: Yes, like in any new enterprise, the first two or three years were the most difficult. One of the elements that were at the center of the discussions was that Japan historically had been on a three year cycle. And by then it was ‘98, ’99, especially logic has been solidly for ten years on a two year cycle. And agreeing on this two year and three year was very difficult. So we ended up splitting the roadmap in two elements-- was the only way of doing it-- in a dynamic RAM three year cycle and a processor two year cycle. But on the other hand, reality was lurking in the background because this quadrupling of the number of bits every 3 years was becoming impossible given the extremely large die sizes (required). And so the dynamic RAM industry realized to go to a two year cycle, doubling every two years was much more cost effective. And so even though this was very contentious in ’98 and ’99, then it progressively disappeared as the industry by the 128 mega-byte, then 256, 512, 1 gigabyte, 2 gigabyte, it began doing 2x every two years. So the process (i.e., technology pace of 2 years vs. 3 years) was—going to fix itself because of the limitation on the field size whereby the die were becoming too large. And so-- but this (i.e., 2 years vs. 3 years) was very contentious. And as I said, we didn’t have a resolution.

Another one that was very contentious was the lithography field. As many times-- we actually ended up in a very strange situation because as I said, in 2001 the first prototype of [Extreme Ultraviolet] EUV was built in the National Labs in Lawrence Livermore. But in the meantime the lithography industry was transitioning from 248 nanometers wavelength to 193 nanometers wavelength. But the problem was that with EUV being viable 2010 and beyond, and looking at the limits of 193nm the way it was produced at that time, dry, coming upon the industry by 2005, 2006 there was definitely a gap that needed to be filled. And that became somewhat strange because we look for the next wavelength. And according to the expert and so forth 157nm was a viable wavelength, even though calcium fluoride is birefringent, most of the experts believed that the penalty was only two or three nanometer. And with this in mind then we began working actively on calcium fluoride and so forth. And this, however, was very contentious because some-- this technology was embraced by SVGL aggressively-- that by then had become ASML, they had got purchased in 2001. Japan was somewhat reluctant, still wanted to stay at 193. And so this was another contentious point. And then, what happens many times, reality takes over, that is finally NIST measured the deviation of the light on the birefringent condition, it was not two nanometers. It was twelve nanometers!. That was completely unacceptable.

That lead to the fact that it would probably a single ingot calcium fluoride would yield only one or two lens elements that needed to be rotated by 90 degree to offset—the birefringent effects and very quickly the cost of 157nm lithography was skyrocketing. But one beneficial element was that everybody had understood that EUV was not going to happen in 2005 or 6. So this brought the attention of all the companies on this problem.

The easiest solution was to default to immersion lithography using 193nm exposure wavelength. That actually resulted in a very beneficial investment for the stepper companies because they could take the basic optics they had developed at 193. They could utilize the technology from calcium fluoride for the condenser or the early element because the power of the laser would ablate otherwise some other materials. By simply adding immersion they now had stretch to NA [Numerical Aperture] 1.35 and of course anything beyond one is impossible in air. And so in this way they indirectly got the motivation to try something because EUV was not going to happen immediately. And this has been beneficial because if we didn't have this (solution) in this moment we still, (assuming that everything works out), have two or three years before EUV is really cleaned up. (This means that) The source power needs to become appropriate, that good mask infrastructure exists and the masks have low defects. So I consider this failure a really lucky event (i.e., the early failure of 157nm lithography leading to the realization of 193nm immersion lithography). A contentious element-- and actually the failure I think became a success because only two or three machines (157nm) were built. But everybody realized that something had to be done, and they took control of their own destiny as opposed to waiting in a passive way to be instructed by the IC companies (on what to do next), and came out with the technology that, fortunately, is taking us at least through the next five years. Hopefully by then the next technology, hopefully EUV will be functional.

Even though at the last meeting-- as you know EUV works at 13.5 nanometers. At the last ITRS meeting, somebody says it's time to think about 6 nanometers. This came from the lithograph group that includes suppliers and also users and the university because since we are so late in the introduction, if we have to do at the end of this decade the 6 or 7 nanometer, maybe we are at the limit of these techniques. And beyond 2020 we need another technology that needs to be comparable to 2 or 3 nanometers, that has to be at a smaller wavelength. So I think I consider this a constructive failure that really stimulated the industry. Of course, people in the ITRS were called fools for calling 157 the next generation. And, indeed, it was not. But you know in the end the results came.

Hatano: That's what counts, I guess.

Gargini: That's what counts, right.

Hatano: How about wafer size? Has that been contentious in the roadmap process?

Gargini: Yes, that is still contention. Actually wafer size is still contentious. Actually, Gordon Moore is a chemist. One of his early accomplishments in the early '60s was really to demonstrate that you could only produce ingots about 3/8 of an inch with the purity (required) to make good semiconductors. The person has been most surprised about wafer size transitions has been Gordon Moore. So we made it a tradition that for each wafer size we can produce that when we can produce the first perfect wafer, we put (each time) one of them in his office. So his office that is still open here in [Intel] Santa Clara on the wall has, going from just about 1 or 2 inches to 12 inches, perfect memory and logic wafers plastered on his wall. And every time we have started--

especially in the ‘90s, he called me and said, “You know you’re wasting time. You cannot make 300-millimeter wafers at the right purity. This is impossible.” “But we got to try anyway, Gordon.” “And also, this double every two years has got to come to an end.” “But we’ve got to try anyway.” So I find it ironic that the person that really initiated all this was the one that was actually in the mid ‘90s becoming more skeptical about this. So I think while the 6 inch transition and the 150 millimeter and the 200 millimeter were lead by single company, one case Intel, the other case IBM, I think that the transition to 300 millimeter was, again, precursor to this international cooperation that has been, in my mind, characterizing the second half of the ‘90s. By 1993, the conversion to 200 millimeter had been completed, and it was clear that we needed to look at the next wafer size transition. So again, if I look at the Nikkei Microdevices in 1996 this is me and Ken Thompson [referring to a photo]. At this point we were advertising the next transition to 300 millimeter, and we also hypothesized that one day we have to do 400 millimeter or larger. And SEMATECH that had been instrumental rebuilding the U.S. equipment industry, by 1994 was beginning to discuss 300 millimeter. On the other hand, at that point I have become the representative to the technical committee and Barrett was on the board.

I did a survey in the 14 companies, and so forth, that made SEMATECH in the beginning of 1994. By then all of them were using at least 50% equipment that was not U.S. So having then a program that, by definition, had to be U.S. only was really problematic. And so on June 28, 1994 in Texas, in a building that says, “Please leave your guns in the car and don’t take them into the building,” I said that we should have an international program for 300 millimeter and duck very quickly because people were throwing tomatoes at me because that was impossible to be in the middle of Texas-- at that moment the funding of SEMATECH was 100 million dollars from DARPA and 100 million dollars from the member companies. And so it took then another 18 months until everybody came to the realization, looking at this survey, that everybody was already using 50% foreign equipment that it made sense to make it international. Then Barrett and I were discussing and Barrett says, “Why should we pay for the whole world? Make the rest of the world pay with us,” and we formed i300i that then accepted international membership. We had to do high-level surgery to make sure that the money that was still flowing from DARPA into SEMATECH was separated. We took a whole floor-- like the fourth floor in SEMATECH and we isolated two [SEMATECH] companies, but by April 1996 we had six U.S. companies, three Korean, three European, and one Taiwanese. So we had thirteen companies. And when we came to Munich for the first meeting, and people were asked to vote for a chairman, there was only one person that everybody knew that was me. So by default I was voted because I was the only person that everybody in the room already knew, since I had done a lot of benchmarking and cooperation. And so I remained chairman from April 1996 to April 2000, when the transition-- when i300i actually absorb the lithography portion of SEMATECH and became International SEMATECH. And actually this international absorption, by 2003 had absorbed the whole SEMATECH into international SEMATECH. And this, indeed, was pioneered by the roadmap.

And so in 2001, when the conversion to 300 millimeter was just imminent-- really began in 2002, then I say it's time to think about the next wafer size conversion. Everybody says 400 millimeter, but if you do the ratio of the area you get 1.89. You don't get 2.25, and so forth. So we inserted in the roadmap 450 millimeter at a time that nobody was even paying attention. And it was not until 2004 that I had a keynote at Semicon West where people saw 450 and said "Where is this coming from?" "Well (if you) look at the last three years of the roadmap, it's 450. It's already decided." And to this day nobody has questioned it because the roadmap for three years had established that 450's the right size. So again I see a role of the roadmap in paving innovation.

And I say doing it for a good purpose because, indeed, only changing to 400 millimeter wouldn't be good enough. So you always have to go to incubation, and so 2006 ISMI was formed within SEMATECH; 2009 we began negotiating money with the state of New York. This money was issued in 2010, and now we have a major initiative underway to extend this. In the meantime, SEMATECH moved from Austin to Albany. So now is all consolidated, and is the right place to initiate this 450-millimeter initiative. Two weeks ago, during the plan of discussion at Semicon West, the discussion was based on the problems and challenges it was not "this cannot be done." So from 2001 introducing the ITRS, 2004 Semicon West, 2006 ISMI, by 2011, ten years we come together, and hopefully in the next three or four years we'll put all the elements for 450 that, indeed, comes international via the ITRS international in that Samsung, TSMC and Intel have been spearheading this activity. And whoever else wants to participate will be welcome.

Hatano: Great. Few more questions. Where do you see ITRS going in the future? Look at it out ten years from now, and what do you see?

Gargini: Well, I mean I think that we made a major transition from silicon only to including other compound semiconductor, you know besides germanium and so forth. We have absorbed (in the ITRS) physicists that never used to participate to the road map. We have now included groups that address the consumer's need like MEMS, analog, RF. So I think the ITRS will continue to anticipate the needs of society by at least four or five years, if not longer. This will influence the universities to put in place the foundation of the studies that then pave the way, ten years later, for the industry to really take over. So I see that especially from a system point of view, the system becoming the dominant factor and then listing all the elements that are needed in the system, not just a few dies, but in this moment the multiple die in the same package, all this 3D packaging and elements.

New needs will come in. I can see that as III-V comes in, then the entire RF portion becomes easier. If a look at the NRI (Nanoelectronics Research Initiative) proposed devices, possibly for instance, tunnel devices could be an alternative. Even though tunneling was discovered in the late fifties by Professor Esaki in Japan that got the Nobel Prize in 1973, but tunnel transistor become a possibility only if you use compound semiconductors. So once, in the next three to five years, these compound semiconductors make their way into manufacturing, then a new wave of devices using compound semiconductor can make their way in (the semiconductor industry standard CMOS process). Then as we go beyond 2020, and now you're down to a few nanometers, the industry will have to really deal seriously with quantum computing and quantum effects in general; because I think once we reach the few, two, three nanometers, we cannot accomplish improvement by scaling the size of the devices down, but you have to increase the functionality. In principle, when you start using quantum effects, you have an infinite universe of functionality if you can tap into it. They allow you, in a finite space, to have the equivalent—(of what) we have nowadays with three bit per memory cell, or four bit per cell. We may be able to elaborate using millions of bits per cell at that point. So at that point a different type of people will need to contribute more-(to the ITRS) - maybe mathematicians, or people that have to develop completely new algorithms and new software. So maybe that the silicon—and after the advent of III-V, the CMOS technology comes to an end. And a completely different set of people that can use this (technological) capability to extract completely new (computation and memory) capability will come in place. Like it appeared that TV had come to an end, and now there's interacting TV, all this software, wireless, making TV a new instrument. The cathode tubes used for the early TV, and so forth, have been replaced by these flat panel displays and technology is not longer the issue. Nowadays the challenge consists in making sure that TV talks to your telephone, talks to your PC. So that everybody's on the social network. Maybe software and mathematicians and people that can understand this (new environment); this will be the people that make the new roadmap in ten years or so. So I think it's important that we are not restricting ourselves to what appears traditional because if I look at the people participating in the road map nowadays and compare them to the 1993 and '94 people, (I can see that) the expertise required are now completely different. The goals are completely different. So I think as long as we are open-minded, and we continue to adjust to the new needs, the roadmap can continue indefinitely.

Hatano: Maybe new systems company involvement?

Gargini: Yes, systems companies may become the real participants. You know, (the question is) "how do you map systems?". And you can standardize in a lot of parts, like we've done in standardizing process steps because the integration is what counts. But then we reach the limit to what you can do. But how do you interconnect and extract value. So we have to be--

Hatano: And if we get the political problems solved maybe China?

Gargini: OH yes, well we have (still) few items (to solve) but I'll leave it up to you to solve the problem, right?

Hatano: And how do you see the leadership of ITRS going forward?

Gargini: Well, you know, I think, despite everything, the two regions that have been most involved in pushing the leading edge have been Japan and the U.S. And Europe has been already switching (from pushing the leading edge to concentrating) on functionality. It's already a different type of roadmap. And (on the other hand) Korea and Taiwan have been more interested in specific items like memory, packaging, and so forth. So I think the mix of Japan, U.S., and Europe seems to be where the leadership resides. And, in fact, even in the NNI-- we formed for the NRI, this IPGWN (International Planning Working Group for Nanoelectronics) this working group (is made of representatives from Europe, Japan and the US) that rotates among the three regions. We see again Japan, U.S., and Europe are the most engaged. I don't see a desire for leadership from Taiwan and Korea as much as they are maybe more concentrated on the next three to five years, as opposed to these other countries (who worry more about long term roadmap). They seem more inclined to speculate on the future.

Hatano: What is your funniest story in all the times you've been involved with ITRS?

Gargini: Funny story with the ITRS?

Hatano: That you're willing to share…

Gargini: No, I understand. I'm trying to think. I think we have to be very creative in the way we find low cost locations where we can have the ITRS. And a few years back when we were in Europe, the only location that could be found was a church. During the fifteenth and sixteenth century, religious wars were very devastating in Europe, and so many times churches had to be built disguised as houses. So from the outside this looked like a housing complex. In the inside it was a church, and it had many little rooms and so forth. It was built in a different way. It was built in a very skinny and tall, and had speakers all around it. And Linda Wilson [ITRS project manager] couldn't come to the meeting. And Alan Allan that is always one of the driver and in the past he was a preacher, himself. So I think he got in the middle of the church and felt inspired. And then the voice of Linda Wilson came from the speakers. And I think he switched back to his old mode (as a preacher), and say, "Sister Linda, I can hear you in the sky. I can hear your voice. Hallelujah." So I said to Alan Allan, "Switch back (to the ITRS)." And everybody took it for what it was. And it was a very interesting place because it was all built out of wood, and so you walked around and everything was creepy because as you set in the room everything was creaking (because of people walking by in other rooms). People were afraid of cracking or were these fears of ghosts and so forth. So that was the strangest roadmap that I can think of.

Hatano: What city was that?

Gargini: This was in Amsterdam. I think it was-- I can find the specific place. They (the European organizers) have found it-- and it was very strange.

Hatano: Good. What would like to convey to young people about the importance of learning about the history of ITRS, or computers, semiconductors? Why does history matter?

Gargini: I think history is a double-edged sword in that it tells you what already happened and you have to make sure you don't fall into the trap of just making more of the same. If you look at things have happened in history through the ages, even though they may appear different, but if you abstract yourself from the details, then you discover that the difference is simply in the methods and implementation. Since I was born in Florence, I had no choice but to deal with the renaissance because every place I would go I would have twice life size pictures of Galileo, Leonardo, Machiavelli, and so forth. As I was reading these memories or books that they have put together-- especially Machiavelli was trying to explain how to run organizations. So, for instance, one of the elements he said in many words that I'm trying to paraphrase was-- this was 1513, so you can imagine it was quite some time ago. He said that when you deal with an organization you will find that if you want to try to (make a) change everybody will be your enemy. At best, you can get some people that think they could take advantage of the new organization that are somewhat (supporting you in a) lukewarm way but still just looking at you (and doing nothing).

But in the moment success comes it completely changes the organization, then you have so many fathers (of success) and so many people (coming forward and getting involved). And as I've gone through, now, forty years of experience in the industry, I've seen this over and over again. Those that are already in a power position make, as time goes by, their number one goal, to maintain the existing organization. And people that may see some benefits coming to them from a new organization are somewhat willing to help him, but only so much, right? So this was just to give an example. Looking at history (we can see that) the solutions proposed at that time to eliminate (physically) some of the opponent is no longer acceptable nowadays. The Borgias were going around with poison in the ring eliminating on the spot the opponents, this approach cannot be utilized in the same degree anymore (of course). But you can see same winning techniques that are now without bloodshed, but you can see the strategies are the same ones. So if you don't look at history, you run the risk of repeating errors that have already been eliminated. But you shouldn't be looking at history just so that you do more of the same. Given the present circumstances, and given the knowledge and how some problems have been solved,(you should think) how can you apply these solutions to different situations where the people-- the clothing is different, but the instrument-(you need to use to solve the problem)- if you abstract the problem and you try to see what situation of the past is similar, then you can very successfully utilize elements that have been utilized before to solve the problem.

So I encourage young people to be knowledgeable about the past. They shouldn't just memorize it and repeat it brainlessly. They should use it as an example of how some problems have been solved. And as they deal with new problems, they should identify (also) new solutions, taking advantage of whatever bits and pieces solutions already exist but it will never be precisely the same solution that has been used in the past to solve similar problems. So I think the ability to abstract from the specifics and defining(in a very abstract way) the problem and the solution is key to really take advantage of history and utilizing it to project into the future and finding new solutions.

Hatano: Great. Any other comments you wanted to make as we close?

Gargini: I think I started in the roadmap essentially somewhat from a strange point of view, having seen all the criticism in Japan. I tried to make it international to minimize contention, problems, and confrontations. It actually-- these (ITRS) people discovered, like in many cases before-- that instead of being just competitors, they had many problems in common. And they actually became surprisingly good at working with each other. Now (the ITRS) it has become an institution that if I look at some statistics, it is used on the average in a conference ten to fifteen, even twenty percent of the papers, they mention the ITRS at one point or another because when people have to find some data, they can always default to the ITRS. So I think it has been a very good experience. It has gone beyond my expectations. And I hope that as I continue to work on it, and whoever will do it in the future, will keep an open mind and be willing to realize new trends and include them in the ITRS.

Hatano: It's been a great contributor to the advancement of semiconductor technology and all the electronic gadgets and everything else that we all enjoy now and will for some time to come, so--

Gargini: That's what we hope.

Hatano: Thank you very much.

Gargini: Thank you.

Hatano: And thanks for your time.

END OF SESSION TWO

