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**Gary Rubloff**

**2000 Gaede-Langmuir Award Recipient**

**October 2000**

Good morning. I'm Gary Rubloff. I'm the 2000 Gaede-Langmuir Award winner for the American Vacuum Society, and I thought I would use this opportunity to talk to you about the award. This award is for inventive application of surface science and vacuum technology to the semiconductor industry, and for fostering an effective bridge between AVS research and manufacturing.  
  
Let me tell you a little bit about myself. I'm the Director of the Institute for Systems Research at the University of Maryland, and I hold a professor position in the Department of Materials and Nuclear Engineering as well as an affiliated position in the Department of Electrical and Computer Engineering. I started out as a physicist, receiving my Ph.D. from the University of Chicago in 1971. I did a post-doc at Brown University from '71 to '73, again in Solid State Physics, and then in 1973 I joined the IBM Research Lab in Yorktown Heights where I stayed for 20 years. From 1993 to '96 I was Associate Director of the Center for Advanced Electronic Materials Processing and Professor of Electrical and Computer Engineering at North Carolina State University, and in 1996 I took my position at Maryland. During my time at IBM Research I was in the science area, the Department of Physical Sciences, Silicon Technology Department, and Manufacturing Research Department.  
So I've prepared some notes for this interview, and I thought I would frame it in terms of a handful of questions which I would ask myself if I were the interviewer. The first one is: how did I get interested in surface science? Because I'm a physicist. In fact, a solid-state physicist.  
  
When I arrived at Brown University in 1971, I had done optical properties of solids in a number of ways, and I recall very clearly meeting Philip Stiles as well as other faculty members there, and specifically a blackboard discussion with Philip Stiles in which told me about some work that he had seen from Bell Laboratories using optical techniques to monitor chemisorption on an atomic scale in electro-chemical environments, by Jim McIntyre. And Phil had been sensitized to surface science through Peter Estrup, who was a very well-known surface scientist, well-known to the AVS community. He was also at Brown University. So we started talking about whether any of the techniques that I had been involved in for solids might be applicable to surface science problems.  
  
And we got very excited about this because it looked like, back of the envelope, it ought to work. Because I knew nothing about ultra-high vacuum or surface science at the time, but fortunately Phil had engaged another post-doc previously, Jim Anderson, who knew a lot about surface science, having worked at the National Bureau of Standards as I recall, and so we had what we needed to try and do something. And we started developing the marriage between optical techniques for solids and the surface science environment. And we were actually able to measure optical changes in reflectivity associated with very small monolayer levels of chemisorbed species on transitional metal surfaces. Sensitivity, as I recall, was well under a hundredth of a monolayer. So that was quite exciting, but it was not simple to make sense of optical techniques. We tried pretty hard to do that.   
  
My interest in surface science continued in 1973 when I joined Dean Eastman's group in the Physical Sciences Department at IBM Research, where the kind of mainline research direction was photoemission studies of solids and surfaces. And in fact, interest in the surface aspect of that had been stimulated by the fact that you can't measure solids, you can only measure their surfaces, basically. Charlie Duke, well known to the Society, has a beautiful way of saying this, I recall, which is that solids communicate to us through their surfaces.  
  
So in this group at IBM I got involved with other people looking at photo-emission studies of small organic molecule reactions, chemisorption and decomposition on transition metal surfaces and on oxide surfaces. This was a substantial amount of effort and I think it was a quite exciting time for me because I had a chance to really learn something very new and, of course, many of us would say that's the fun part of being a scientist.  
  
I also spent some time thinking about chemical reactions and developed some experimental techniques. This comes, I think, from my life as an experimental physicist developing techniques for strobing gas pulses onto a surface in order to time resolve isothermal surface reactions. And we were able to actually dose surfaces to monolayer saturation levels, watching the desorption and decomposition in real time, using photo-emission techniques.  
So if I was looking at problems related to catalysis, which I was at the time, the next question is, well, most of your career was in semi-conductor-type problems, so how did you change to that? And this really resulted from the fact that we had, at IBM Research, a very substantial group in surface science as such. There's a much larger world in a company like IBM with its Industrial Research Laboratory scientists, many of whom are rather interested in the role of semiconductors. And I thought, gee, wouldn't it be nice to learn something about that larger world, and maybe even to contribute to it?  
  
The first thing that I did was to change the group in which I worked to work with Paul Ho, who has become a very close collaborator and friend over many years. Paul was an outstanding materials scientist, with a surface science component at the time doing Auger sputter profiling and the kinds of AVS-like techniques that are used regularly in the thin-film science community. And so I came to work with him being a converted surface scientist and he was anxious to have more direct collaboration from a broader spectrum of surface science. So I remember our initial discussion and the decision to try to do some work on the silicide problem, which is the interface between transition metals and silicon and the formation of silicide compounds in interfacial contact with silicon. And to use techniques like photo-emission and Auger spectroscopies, to study the deposition and reaction of transition metals in sub-monolayer or monolayer amounts. And fortunately, we had, for a number of reasons, the ability to evaporate transition metals and to use surface science and a surface analysis environment; so we achieved some very quick results on that. And it led to a major program in which we spent quite some years.  
  
I remember something that I think was significant from my first discussion with Paul. We talked about what we might do together, and he mentioned Auger sputter profiling, which I understood, and he thought we should also do some TEM studies. I had some confusion at this, not quite knowing what that meant, because the reality was that as a physicist I knew nothing about materials science. I think this is a very important feature of growing in this research domain, working with people who know something you know absolutely nothing about. And to me, it's been one of the most exciting parts of my career, and if I would credit receiving the support and having the opportunity to do that and maybe being brave enough to try it.  
  
Paul and I began a program that grew quite large in looking at metal- silicon and silicide-silicon interfaces. There we made two fundamental contributions, I think. One is in recognizing how pervasive interfacial chemistry is at these reactive interfaces. These are highly reactive interfaces that form bonds, chemical bonds, and compounds, which really dominate everything that was available about interface properties. Secondly, we related these interfacial reactions directly to the electrical performance of these interfaces, where they're used routinely as Schottky barrier interfaces. And of course, it's a very fundamental problem and it has been for many years to understand what determines the Schottky barrier height at metal semiconductor contacts. I would say that the focus on silicon-silicide interfaces was particularly important in general in the field of Schottky barriers because the semiconductor is simpler than the compound semiconductors would be, and because we were able to identify the role of silicide formation in interfacial chemistry rather quickly, and I think it had implications for a broader set of Schottky barrier problems as well.  
  
This became a field in which there were probably half a dozen very profound research groups across the world working, and much of this work actually was centered within the American Vacuum Society, and in particular in the Electronic Materials and Processing Division, where Schottky barrier interfaces were a major thing for quite some years.  
  
Paul and I also took on another set of problems together in polymer interfaces, which was really motivated by the technology inside IBM where people were trying to build packages that had multi-layer thin film wiring of copper embedded in polyimide. It turns out that strategy is extremely important today on chips, but at that time we were looking at the packaging applications and there had been very little, if any, surface science to understand the surface chemistry of the polyimide surface and it's relation to metals that might be used, like copper, which would be deposited onto the polyimide. So we started by taking this seemingly, for a solid-state physicist, very messy system of polymer surfaces, and pretending it wasn't so messy and looking at it with the same surface science techniques we had already, photo-emission, Auger spectroscopy, and materials science techniques as well. One of the first things we were able to document at a surface sensitive level was the ability of the polymer surface to take up water vapor from the ambient and to expel it upon annealing, and to look at the absorption/desorption kinetics of water on this particular polymer surface. This problem is important still today in problems where people are looking at low dielectric constant insulators, which are polymers often, and eventually will be foams with large void areas where the uptake of water vapor and its subsequent desorption and interactions of gases with these kinds of polymeric and low-density materials will be very important to controlling processes for manufacturing.  
  
We also at that time, looked at the interaction of a number of metals with the polyimide surface, in particular copper and chromium, and learned that there was an important reaction in which relatively inert metals, such as copper or gold, diffused readily into a polymer and that accounts for a reasonable amount of adhesion. And adhesion is a problem extremely important to use thin-film wiring technologies. And in contrast, the more reactive metals, like chromium, would chemically bond with the polymer's surface and not diffuse in, because they formed such strong chemical bonds at the interface. That causes an even stronger adhesion of the metal to the polymer, but of course, when you're looking at the structure of the polymer, the breaking point may not be at the interface. It may be underneath. So there's a delicate balance between whether you want a graded interface or an abrupt but very strong adhesive interface, in producing the kinds of technologies that are necessary.  
  
So these two examples that I have given you really constituted for me, as a solid state and surface physicist, a very broadening experience. First of all, in bringing what I understood of surface science together with materials science that is an equally important part of any technology. Secondly, in numerous examples which show that both the materials and thin-film and interface properties depend not only on the composition of the materials and interfaces that are made, but how they got there in the process history. I won't take time to go into that in detail, but this is a concept that I believe is pervasive to understanding technological problems involving thin-film processes and science. Third, it made me realize that ultra-high vacuum, as practiced and advanced in the AVS, coupled with the capability to simulate manufacturing processes in a highly controlled and well-characterized ultra-clean environment, makes a very powerful combination for research that can underpin the technology. And finally, my work with Paul was so significant to me because he served, not only as a colleague bringing other skills, but also as a mentor in learning how to relate to the technology in the manufacturing world in IBM, because as a materials scientist he had been extremely productive in doing that.  
  
Okay. So everything I've talked about so far we've done in the context of the Department of Physical Sciences. How on earth did I move into real technology, meaning working in the technology department? Well, there came an opportunity to take what was called a technical staff position with the Vice President for Logic and Memory for IBM Research. At the time that was John Armstrong, who was a superb manager and actually went on to become IBM's Chief Scientist. And basically the job was to take a year off to work closely with him every day and to do his technology homework, which means he would pose questions and I would talk to his Directors. I tried to put together answers to those questions and presentations and statements of accomplishment and views on what the ten-year technology forecast for the entire corporation should look like. This was an incredibly broad perspective and the chance to become familiar with technologies way beyond what I knew at the time, and it gave me not only a much better perspective on IBM's technology, which is a shared technology with many other companies of course in the same business, but also a very deep appreciation of how to look at research in a strategic fashion, how to recognize which things are going to be important in the technology and business time-lines for the company as opposed to the things which are purely of academic or intellectual interest. Both have their place, of course, but working in a technology company, this was something valuable to me.  
  
When I finished my staff job with John Armstrong, I went on to the Silicon Technology Department in a management role as Manager of Exploratory Materials and Processes. And I had, during a period of, I don't know, seven or eight years, several different management positions that are kind of the first level of management and middle-management levels. And that was a very broadening experience as well, because here I was immersed in the kind of exploratory end of the technologies spectrum but very distinctly responsible, as part of a team, for the way silicon technology developed in the company.  
  
So the next question I'd like to address is what kind of research I did at that point, consistent with that change? I should say I actually continued to do research even though I was in management. That was a very important part of my career. So in the Silicon Technology Department, having worked on silicide interface problems and in polymer problems, I thought that the field effect transistor depends very highly on this ultra-thin gate oxide which controls its properties, and everyone has always said that silicon is the technology of the computer industry because of the oxide it forms when you grow the oxide at high temperatures. So I thought, well, that sounds like a pretty interesting problem. Maybe we should try to look at that problem. And at a place like IBM Research there are literally dozens of people who are experts in metal-oxide silicon analyzing technology. So I went around and talked to a bunch of them and I asked them what might be interesting to look at and problems needed to be solved and so on. And I had a fairly uniform consensus feedback that said don't bother; there's already so much research it's hard to imagine you can have an impact.  
  
I didn't listen to that because we had new tools to look at these ultra-thin materials, and so we tried some things and the first ones worked right away. And so we started learning important things about defect sites in MOS structures. We'd started by looking at conventional thermal-grown oxides on silicon and annealing these in a highly controlled ultra-high vacuum environment to see what would happen and what we found was that there were some peculiarities in x-ray photo-emission spectra and we subsequently identified that what was happening, from looking at scanning Auger and scanning electron microscopy, is that, as you anneal at high temperature, voids form in the oxide where the oxide is completely gone. And we recognized this as a reaction in which SiO2 reacts with silicon to form a volatile silicon monoxide product and that's how you create a silicon surface at high temperature.   
  
And that was pretty interesting from a number of points of view, but it was even more interesting to look at the kind of operative chemistry here, because we then subjected these kinds of thermal gate-quality oxides to annealing conditions which were less drastic so that these voids wouldn't form but which would affect the electrical properties as measured in MOS capacitors that we formed already in the old oxides. And what we learned, by annealing under different conditions of oxygen partial pressure, very low oxygen partial pressures, is basically the formation of electrical defects looks very much like the same reaction I just mentioned, silicon and SiO2 forming a volatile product. And if the product was volatile but we supplied oxygen faster than we formed the volatile product, we could prevent the reaction from proceeding, and in effect prevent the microscopic inactive electrical defect from becoming electrically active. So the lesson was that new techniques can make new research out of old problems. That's what I took away from that experience.  
  
The next experience that I should mention is that we wanted to expand the kind of work we were doing when marrying ultra-high vacuum technology where one could carry out simulations of manufacturing processes, physical, experimental simulations, and the kinds of surface analysis and gas phase analysis that we have in a surface science environment. So we built a major laboratory, in fact several major laboratories, which linked multiple UHV chambers together, some of these chambers being able to carry out certain kinds of processes, others to carry out certain kinds of analyses, and the ability to carry out several steps in the manufacturing sequence in these highly controlled environments without exposure to air. This was essentially multi-chamber processing and analysis from the science point of view. But from the technology point of view, this exactly paralleled what was happening on the manufacturing floor where in those years, the mid-‘80s or so, manufacturing tools were becoming multi-chambered tools called cluster tools, which now completely dominate the equipment industry. And so we had a really significant time working with IBM manufacturing people who were planning equipment acquisitions and working with equipment suppliers to develop new kinds of equipment technology. That was actually a very fortunate synergy.  
  
And on the research side, because of the position I was in on the exploratory side of silicon technology, we used the same kind of technique not only for chemical vapor deposition and oxidation, which was my research program with my own group of collaborators, but also for physical vapor deposition which Jim Harper lead, and for plasma etching which Scott Levohan led. Both people very well known in the AVS. This work allowed us to understand the relation between surface condition or cleaning processes on an atomic scale and the way in which process integration would happen in subsequent steps. And in fact, there were some huge surprises there. One was that, for example, if you had too clean an environment you will have trouble making any kind of good electrical devices. And the reason for that is it's never perfectly clean, and if you don't passivate the surface properly and you do the steps wrong, the little bit of oxidizing impurities that you always have around will actually roughen the surface. So there were some big surprises there.  
  
We also did in this era a significant amount of work looking at mechanisms and chemical vapor deposition, and, in particular, looking at reactions on oxide surfaces where you're essentially looking at a selective process, so only defects can nucleate the growth of silicon on silicon dioxide. We were able to make some very rough morphology surfaces which are one way to make a high-density dynamic memory cell capacitor with a very large surface area without taking too much area of the chip away, by making a very rough surface and then building capacitors in that. That's actually a strategy that has been adopted in different forms by industry.  
So the work that I was involved in at IBM in the Silicon Technology Department was all well-related to IBM technology and manufacturing, and my associates in manufacturing and development organizations regarded the work highly and used that primarily as a knowledge and confidence base that they understood what they were doing and they had a back-up plan if they went down a certain pathway and ran into trouble. They used some of those lessons quite directly in some of the process-development work that they did.  
  
Okay now, let me come to the question which is stimulated by the fact that a fair amount of the work that I have done while at IBM involved research collaboration outside of the company. And a question that's interesting is how did that happen and what role did professional societies play in that? There are actually two examples I'd like to touch on. One is in picosecond ultrasonics, and the other in positron annihilation. In the first of these, we were in silicon technology trying to solve the problem. We were building a whole new generation and strategy for interconnects in semiconductors. We had the wires that connect devices and there are many levels of those that are needed. And one of the hard problems is to understand the micro-stress and strain in adhesion in these sub-micron structures and complex interconnects. We know how to measure stress and strain in micro-thin films, but of course we don't use micro-thin films for much; we build micro-devices. And of course, when you do that, many things happen that you wish wouldn't happen, such as changes in the interfacial properties that occurred because of the sequence of processing which you do, the patterning of these structures, so that materials are in very confined geometries and they have strange geometries.  
  
This, of course, changes the microstructure and the micro-stress and strain associated with those microstructures. So I was searching for ways to measure micro-strain and micro-stress in interfacial adhesion, which is one of the hardest. And I happened to be at a professional society meeting and I saw an intriguing talk on picosecond ultrasonics by someone I knew from my time at Brown. This was Humphrey Maris. They had developed some pulsed laser techniques to look at thin-film structures by using picosecond lasers to start a thermal wave which turns into an acoustic wave and would propagate and reflect from varied interfaces in thin films, in some measuring such things as very thin film thicknesses of a couple hundred angstroms. I thought this might be useful for micro-stress/micro-strain and so we started talking about that. It turns out it wasn't useful for that, but it was very interesting for other things. So we developed at that time collaborations on other problems because Humphrey Maris was looking for applications in the semiconductor technology arena. We had a bunch of them. He didn't know the semiconductor world. We didn't know the picosecond ultrasonics. So we worked together and we were able to actually demonstrate the ability of this technique to identify interfacial adhesion loss or potential loss at an incipient delamination at an interface, simulating void structures as small as five angstroms with very elastic fluorocarbon layers that we placed at the aluminum-silicon interface, and in detecting the formation of silicides at the metal-silicon interface buried beneath reactive metal.  
  
I'd like to think that this was an important step that went along to help them develop many other semiconductor applications, which they did. And in fact, the technique that they started in those days has now been commercialized and instrumentation can be purchased directly to do this kind of measurement in very complex interconnect structures.  
  
The work on positron annihilation happened because Kelvin Lynn from Brookhaven was also in search of some good applications for positron annihilation, and he saw some of my publications in defect micro-chemistry in MOS structures. So he called me up and we started talking, and we developed a collaboration that led to the use of positron annihilation to look for microscopic voids in these MOS structures. And, in fact, this technique has now become much more broadly used, not only for materials technology problems but also for bio-medical problems. So it's a very interesting technique and the collaboration was rewarding.  
  
But the question is how did it happen, because I worked inside IBM, and the answer is that leading industrial research labs have always encouraged their people to interact with the outside world, and that was very important, both in publications and in attending professional society meetings. And so I knew plenty of people in the outside world and that opened my eyes to these kinds of possibilities and it happens for all the people who work in industrial laboratories.  
  
Okay, let me come to the next question. I've spent some time in Manufacturing Research at IBM. What did I learn from that? I know I've said this already but all of these changes which I went through in my career were good changes because I learned lots of new ideas and I met lots of different people through them, and manufacturing research was no exception. I joined Manufacturing Research to manage the Process Modeling Group which did reactor scale and feature scale modeling, a very sophisticated chemical engineering and, in fact, some computer science as well. From that I learned the importance of modeling and simulation, which is pretty good for an old experimentalist like me by that time. Actually, modeling and simulation have become a major portion of my research program since then, so I now work in this area very actively. I also got a deeper appreciation of things which go on in manufacturing which are techniques that I now call systems engineering, though I never heard the term in IBM, but it's really systems engineering, and I'll get to some comments about that a little later. By that I mean sensitivity to our logistics and operations in the manufacturing domain, process control, sensitivity analysis and optimization modeling, and enterprise and information management.  
The next question that I have is how did I get to where I'm working now, which is the sensing of chemical species in semiconductor manufacturing processes and the control of those processes for manufacturing. Well, in the early ‘90s I decided, you know, my career is about at the halfway mark, and do I want to do this the rest of my career or do something else? And of course, like many people who spent so many years in the university, one thinks about being a professor. So I actually decided to go into the university there, and I joined the Engineering Research Center at North Carolina State University, where I had been on their advisory board for several years while at IBM, and I became the Associate Director of the Center there. And this is a Center which involves some outstanding researchers in semiconductor manufacturing process science and technology. So I thought, well, the last thing they need is another person who knows this. Let me do something else. So I thought about this and decided that process modeling and control for manufacturing could add some value. So I decided that processing and control for manufacturing could add some value and so I started work on using chemical sensors, primarily mass spectrometry, to detect depletion of reactants and product generation in chemical vapor deposition processes. And we hope that this is going to pay off. At NC State they had all the right equipment for doing this; all I had to do is set up the sensors. And we did that, and fortunately the first day that we turned on the sensors we got publishable data. And as I recall, we wrote an AVS abstract that day. It worked like a charm. So we were able to see the depletion of reactants and the generation of reaction products and to measure them to obtain a quantitative metrology which indicated how much reaction or how much deposition or how much thickness was taking place at the wafer surface. And as part of that, of course, I knew I had to do both the diagnostics and chemical sensing part but I didn't know the first thing about process control so I had to reach out to new kinds of people, both at North Carolina State and especially at the University of Maryland, where I learned there was a group of people who were very good at doing process control.  
  
Well, as it happens this leads to my next question. I'm now the Director of the Institute for Systems Research at the University of Maryland. The question anyone listening to me might ask is, what does this involve and how on earth does it relate to my research history? Well, it really started from my running into the previous Director of the Institute for Systems Research at an NSF meeting and we talked a little bit about potential collaborations and of course, one says that often, but it actually happened. So I started collaborating with the control people at Maryland in the Institute for Systems Research to complement the work I was doing at North Carolina State. As it happened I ended up going to the University of Maryland later for a number of reasons, but when I went there I knew I would do experimental work, but now I would be able to work closely with people who knew how to do process control and a number of other things which I would classify in the domain of systems engineering skills.  
  
What's surprising is that I should end up being the Director of the Institute for Systems Research, because systems research in that environment and in general meant control methodologies. It means communication protocols, and network management kinds of methodologies. It means systems integration, which is heavily software and modeling dependent. All things that don't sound like they have a thing at all to do with the kind of experimental physics background that I came from. But actually there's a lot of overlap there. The process of technology development and the application of operations research and many other skill sets in manufacturing are really all systems engineering, but they never used that term that I recall at IBM. Systems engineering means the development and application of structured methodologies to solve problems whose complexity is high because of the size of the problem or the heterogeneity of the problem, and the inherent dynamics, often on a different time-scale, that is part of the essence of the problem. Having seen technology development in manufacturing at IBM was pretty pivotal for me to be able to pass as a systems engineer of any kind. Also useful was the management experience that I had at IBM.  
  
Okay, one last question. I initiated the establishment of the Manufacturing Science and Technology Group in the AVS. How did this happen? Well, in the early ‘90s, maybe even the late ‘80s, it was pretty clear in the semiconductor industry that the competition in Japan was eating our lunch, and that soon the equipment industry would be dead in the United States because the competition from Japan was so huge, and once that happens we better worry about whether the chicken or the egg is going to survive either. Because most of the capital equipment costs in a manufacturing fab that makes chips will use the equipment that you put into the fab. So it would be an understatement to say that people were really concerned about our ability to know how to do manufacturing in the United States or in IBM, and how important is the Japanese leadership that was apparent at the time.  
  
One of the major responses to this was the formation of the Sematech Consortium to try to basically protect and develop and nurture the semiconductor equipment industry as well as the chip industry in this country. It was a major challenge. And because inside IBM and the larger U.S. environment this competition for understanding the practice of manufacturing was so clear, I thought of the notion one day that, well, since I'm very involved with the American Vacuum Society at the time, I think on the EMPD Executive Committee, we try to have special topical conferences with the AVS and maybe it's time to do one on manufacturing. So I asked my sidekick, Michael Liehr, who is also a very important collaborator and mentor and friend at IBM, whether he'd be willing to go off on a limb with me and propose a couple of conferences. He said sure, let's do it. So we did. It was very warmly embraced as a suggestion by the AVS in '92. Charlie Duke was a big help in starting this, so we ran a topical conference in 1992. John Armstrong, by then the Chief Scientist at IBM, helped also to get the right kinds of high level people to this meeting. We ran a Topical Conference in '92 and then were asked to do another one in '93, and we did that.  
And then the question was what should we do with this? Because it's very exciting and what should the Society do with it? And at that time it wasn't clear. The initial thought was, well, you should become a Division, but of course a Division lasts forever, and maybe we need to be a little more careful. And I think it was Rey Whetten, or a group of people he spoke with, who came up with the notion to start a Technical Group in Manufacturing Science and Technology, which is actually what we did. And there are now a handful of technical groups in the AVS including what we call MSTG, the Manufacturing Science and Technology Group.  
  
And this actually set a precedent for a flexible organizational structure, a Technical Group as opposed to a Division in the AVS, which may have a finite lifetime or may turn into a permanent Division or may stay a Technical Group. One of the key features of the MSTG was that we brought many people from industry. It was actually dominated by industry, which is a very healthy thing for a professional society. In general, technical societies tend to be very academically centered. So I think it contributed a lot and I think many of the kinds of issues the MSTG has emphasized have now been assimilated into the work of other Divisions and, hopefully, I think this represents an enhanced vitality for other Divisions. This is all good news in my opinion.  
  
So I learned from this that, first of all, work with this Society which is very flexible and ambitious. Secondly, welcoming to industry. And third, perhaps most important, there are lots of new fields that become important that don't have a professional home. Certainly manufacturing, in my opinion, was one at the time, and I could point to a handful that the manufacturing group at AVS was now accommodating in trying to nurture.  
  
So I've talked about a handful of questions that I hope are of some historical interest, and hope I've given you something to think about. Let me just make a few more comments on the lessons that I've learned through my career in changing several things that I've been involved in.  
One is, I'm a scientist, not an engineer, though I try to pass. And I regularly get paid to pass. But scientist bring real creativity into technology. Not just me, but in general. Let me give you an example. When I joined the Silicon Technology Department, I spoke with Tak Ning, who is an esteemed expert on bipolar technology and an IBM Fellow. And I remember the mini-lecture he gave me when we had a chance to talk informally. He said, it was really important that people like you, who come from science, from physics and chemistry, to work in technology because you bring different perspectives and different ideas to the technology. You can have a huge impact. And I think he's right about that. It's that mixture which makes for exciting progress. And I think it's a mixture of science and technology, but also the different domains within engineering and within science. Secondly, timing in research is really important, and I think I feel this because I've done it wrong and I've done it right. The work I did in pulsed-gas dosing to time-resolve surface chemical reactions I think was too early for the kind of infrastructure that was available to be a successful research program. On the other hand, we were just right in the ultra-clean and the integrated processing in which we brought new tools to old and new problems.  
  
Also, the work environment is really important. IBM Research was a superb place to do science and technology together, and I thank my lucky stars for having been there at the time I was. Management and staff experiences, sabbaticals, any kind of change to see a larger world, very important to get out of the work environment. And I really think it's important to consciously think about, if not do, a change of job and research directions periodically. It's what keeps us alert and excited, I think.  
  
Finally, people really, really matter. When I think about the mentors and collaborators that I've had that taught me things that I knew nothing about and when they do that for you, the learning curve is infinitely faster than reading books and reading journal articles, and it's a lot more exciting. So I've been extremely fortunate. I'm sure I would never get an award if I hadn't been so lucky with the kinds of mentors and collaborators that I've enjoyed. Working in cross-disciplinary teams is increasingly required for the problems that really matter. And so one really can't get away from the importance of working with a mentors and collaborators; the mentors because they know something that you don't know and vice versa. And from a personal standpoint, I think that that's most exciting way to work in research. It certainly accounts for whatever vitality I've been able to have in my research program.  
  
So I hope that gives you some feeling for the background that led to where I am today as winner of the Gaede-Langmuir Award, which is a tremendous honor and I deeply appreciate it. Thank you.