



Thomas Eagar

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Interview by George Smith (Acting Director of the Dibner Institute) and
Arne Hessenbruch, on 6 May 2002, at the Dibner Institute, MIT.
Transcribed by Jonathan Coe.

George Smith

I am NOT, as such involved, but I'm starting to get interested, and this book [referring to Robert Cahn's *Coming of Materials Science*] has drawn me in very heavily. I've tended to be very skeptical, because of course the people, all the people I work with are really metallurgists, not Materials Scientists. [Richie Glue] knows less quantum mechanics than I do, and that's not saying very much. And you! I would think of you very

much as a metallurgist, trained at MIT. And you became chair of Materials Science! What's your picture of the change from metallurgy to Materials Science?

Eagar

Well, I have baggage, in that as a sophomore I took Cyril Smith's course, and the book *The Search For Structure*, and all that stuff, right? If we go back to the beginnings of Materials Science, and... ..this is sort of a Cyril Smith view, it was at the Sorby Centennial Symposium, you may have have come across that back in the 1960's. Cyril edited it. When I became a metallurgist, in the early 70's, they talked about metallurgy being structure-properties relationships. And then, Mert Flemings and a few other people kind of felt left out by that, so they started calling it processing-structure-properties, so by the time I was a Senior and a graduate student, it was processing-structure-properties, in about 1970, or actually 1972. Well, in '71 and '72 I served on an undergraduate committee to revise the undergraduate curriculum, I was the token undergraduate. Then they had a committee to revise the graduate curriculum, then I was the token graduate student the next year. That was the Morris Cohen committee. That committee was at the same time that Morris Cohen was coming out with the National Academy of Science report. You know that?

Arne

COSMAT.

Eagar

Got the whole thing?...I actually... [Marge Meyer] left me somewhere, the whole thing, I mean there was only one copy, you know this was before they made lots of copies of things. Anyway, I also, in 1973, while he was writing [Cosmat], was TA'ing for Morris Cohen, basically lecturing his course. And then in 1989, when they came out with Fleming's report, the NAS follow-up report, where they had the

tetrahedron, processing, structure, properties, and performance, that's kind of where it was, but really, that... and I say "1880," I kind of subscribed to the Cyril Smith view that it was the Sorby's ability to do metallography and look at an internal structure. They always knew, if you go back a thousand years before, people would see crystalline structure on fractures or things like that, and Cyril goes through all that in *The Search For Structure*, but they really never could begin to explain it until Sorby basically gave them the tool of metallography. Then in the 1920's, x-rays came along. So they had several...they started developing new tools. In fact in 1985, I wrote an article for the *Journal of Metals*, where I kind of said that Materials Science had matured because it now had a theory, quantum mechanics, to explain what they could measure in characterization, and they could now produce (by things like molecular beam method, things like that) on an atom by atom scale. So Material Scientists have this whole range of structural scales, from atoms on up. Sorby started out by showing them that you can measure structure in the microscope and correlate that with properties. But they didn't have a theory to tie the two together. And they really didn't have very good first principles fabrication schemes to build up if they did have a theory. So anyway, the theory came along, the characterization tools came along from 1880 through, they're still coming along today, but I mean it was first Sorby, and then it was x-rays, and then you have the transmission electron microscope in 1950, and things like that, and they could start to measure things on an atomic scale, and all kinds of other scales in between. But measuring something doesn't allow you to do much with it unless you have a theory to go with it. Well quantum mechanics gave the theory, except the problem was quantum mechanics didn't have the power until the 1980's when the computer allowed them to do something more than a hydrogen molecule. In quantum mechanics, they had the fundamental equations, but they just couldn't solve the complexity of the equations. Now, we actually can design materials on a computer that have never been built before, and predict what the properties are, And now we actually, in the 1970's, 80's, and 90's have developed techniques so that with something complex you can actually build things up atom by atom if you have to. That's not necessarily what you want to do, but you can. So I said, this is the 1985 paper I wrote, I said, "That's what was placing Materials Science in a whole new realm to move forward." Because they had this triad of theory, characterization theory, and

fabrication and processing techniques to build what they predicted. And I compared that to Biotechnology in 1985, and said, "It would be wonderful... Well, recombinant DNA gave you two of those: the characterization technique and the building technique." You got both, but you didn't have the theory. And at that time everybody said or people were saying Materials, Biotech, and Information Technology were the waves of the future. This is in 1985, now this is 17 years ago. I said "Well look. Information Technology is growing," (I didn't say too much about that but you could see the growth in 1985) "and Materials" I said, "really was poised, to be able to do some great things." And then, Biotech... and I was in favor of doing the human genome and stuff, although I'm not sure that was even out yet, but I was in favor of that, but I said "Until they develop a theory, they're not gonna be able..just because they have the characterization tools and the assembly tools, unless they know what they want to build, they're still gonna be doing things empirically." And that's a much slower process. And I think that's still true to a certain extent. Now they've got the human genome, and they're starting to develop others, then someone's gonna come along with the theory at some point, and figure out how these genes actually create these proteins and everything else. But they're not there yet, so there's still a lot of empiricism....But my thesis was that once you get past empiricism you have to have a theory to tie everything together. In Materials you have to have characterization, theory and then experimental assembly. Anyway, that was kind of my view of Materials Science. I wrote another paper about two or three years ago, where I said "Ok, lets look back and see if we can see what happened." Everybody has seen the growth of the Information Technology, and everybody still talks about biotechnology and growth. This was three or four years ago. The biotechnology boom hadn't quite taken off, although to a certain extent it has much promise still. But the thing about Materials is I called it, the paper was called "The Quiet Revolution in Materials Science and Engineering," and it's the "quiet revolution" because it has been a cost avoidance rather than a new business. Everybody in 1985 was predicting that Materials Science, Information Technology, and Biotechnology would create new businesses. And in Information Technology, and communications, and biotechnology, they have! But what are the new businesses in Materials? The employment is going down in the manufacturing industries. Steel companies have become four or five

times more productive in the last 20-25 years. But the growth and the consumption of steel isn't going up, because there's only so much you can "eat." So the problem, is there actually has been a revolution in Materials Science and Engineering, because we have this triad of theory, experiment, and stuff. We actually have had tremendous gains in productivity. I mean, the steel industry had doubled the productivity of the rest of American Manufacturing in the 1980's. For a whole decade! That might be necessity.

George Smith

By how much did it grow?

Eagar

It was like 6-8% a year versus three or four in manufacturing. And one percent, or zero, in the economy as a whole. I mean, you went from something like 6 man hours a ton to 1 man hour a ton to produce steel in a ten year period! Well that was partly because they cut all the fat, they probably got half of that by cutting the fat, but the rest of it actually came from technology improvements and rethinking the way they did things. Part of that is that "necessity is the mother of invention." They were going to go out of business if they didn't! Now it turns out they are eventually going to go out of business because we don't have the raw materials advantage that we had in the 1800's. We don't have the labor cost advantage we used to have. So frankly, the heavy metals producing industries, they're not glamorous industries that society wants to keep. They think of them as "dirty" industries they'd rather do offshore. Export your pollution, right? That's not to say that Materials Science, and you can take steel or you can take silicon, you can take either one, the productivity gains were tremendous. But nobody notices that because people don't purchase a material, you buy a computer. You're buying functionality, you're not buying a material. That was my thesis in 1998 or whenever I wrote that article on "The Quiet Revolution." Which was sort of sequel to my 1985 article, on the idea that you have to have this triad of theory, fabrication, and characterization.

George Smith

A quick aside first and then I have two lines of questioning.

Eagar

Well, by the way, I've got a student who has applied for a patent on a, he calls it a "linear metal foam," but basically it's just holes in a substrate. The application is to blow air through this thing for a semiconductor cooling heat sink. Well, what you need now, because this thing is less than a cubic inch to cool the semiconductor, whereas the actual pins inside your computer now are maybe 50 cubic inches. You have a tremendous space advantage, but you need an air compressor! You only need 30 psi, but we'd like to...

George Smith

No, we can do that...

Eagar

We want something...

George Smith

No, no, but that we can do.

Eagar

I know.

George Smith

We are doing things like that.

Eagar

In fact, I told Chris, he's trying to get venture capital and all this other stuff and start a business, he was actually one of the finalists in the 50K competition, but he finally pulled out, because it looked like he was close to getting real venture capital money. Intel's interested...it's the limiting thing on servers right now!

George Smith

Ok, let me pursue. These are two totally separate lines. You talk about theory, and I understand the point. Except I'm worried. You're right, but the ability to do computations in quantum mechanics beyond the hydrogen atom took off in the 1980's. But those are still not real computations. They're like the CMD computations. You make extraordinary simplifying assumptions, in order to get any kind of numbers out, and I look at them and I think of them more as engineering tools than physics.

Eagar

I'm not sure I disagree. I think I will agree with that, however, in some cases they've done very well, and the example I used to use when I was department head was Gerd Ceder who's now a full professor over there. He was an untenured associate when I became department head, and his claim to fame was lithium electrolytes for batteries. People used lithium cobalt and lithium manganese. I don't know what the anion was... maybe it was just lithium cobalt oxide and lithium manganese oxide. Basically, what they did is... Gerd basically developed this model, he was one of the first guys who could do ceramic systems as opposed to metals, where the bonding is non-directional, and everything. Anyway, and people couldn't do the ceramics because the bonding was longer range order, and as you say, they have to really do some very simplifying assumptions, and they can only do their calculations at absolute zero. We can't handle the entropy and so forth. I don't disagree with what you're saying, but what he did in that case is he was able to

take out the cobalt and the manganese out of that oxygen lattice, which you can only do on the computer. He showed that the highest voltage you would get is if you completely removed the cobalt and the manganese part of the anion, and you just had lithium and oxygen in that crystal structure. Well that's an impossible thing to make physically, but from that, they basically came up with an alloy, lithium aluminum manganese or something, oxide... I don't remember exactly what it was right now, but the compound, which basically they predicted in the computer, what they need in terms of the interatomic spacing. That was really all it was. You can change the lattice spacing in the computer a lot easier than you can in the lab. Then [Nyet Ming Chang] went off as part of this team and made it. And Don Sadaway and Anne Mayes measured its properties and it was the best of the lithium electrolytes, solid electrolytes, for batteries. That was kind of one of the things that helped Gerd's whole tenure case and promotion case. He was really, so far as I could tell, the first person to predict the properties of a material in the computer before it had ever been made. Everybody else was always, "Ok we made the material, now lets go do our fudge factors and show..."

George Smith

Yeah, we well realize that what you're doing there is self-fulfilling prophecies.

Eagar

Yes.

Arne

When was this?

Eagar

This was about 1995 or so they did this. But it was the first, and you know, I took his tenure case forward, and the letters came through, and

he was the first person to ever have, I mean that's what everybody was saying. And now the physicists looked down on him, because the physicists wanted to, they were interested in developing the fanciest new tools. Gerd was someone who basically said "Well what are the tools that are out there?" He would pull whatever tools off the shelf, from the physicists, that made sense to solve this problem. He was an engineer! He likes to think of himself as a scientist, but if you really get down to it, he's really an engineer, an engineer uses tools available, and engineer's not out there creating tools. That's the scientist's job. So, the physicists, I had to be very careful when I went out for letters, that I didn't try to get too many physicists who were going to look down and say "He doesn't have a computer program, a code, that he is the father of. Basically, I convinced Bob Brown, who was the engineering school dean, was that Gerd was a star because he could use any tool that was out there. He was intelligent enough to take the tools developed by others and apply them. Yes, they all have approximations, but he was able to put the right couple together, to come up with a prediction, which was a very useful prediction. And by the way, predicted the voltages out of 4.5 volts, predicted them within like a tenth or two-tenths of a volt. That's not too hard to believe that you can do that. All you're doing is changing the lattice spacing between the oxygen atoms. The other thing he did was that everybody really thought it was the lithium that was carrying the charge, and it turns out it was the oxygen vacancies. He kind of, not only did he, you know, predict it in the computer, but he actually, the computer told him what was counterintuitive to everyone else's assumption. Everybody figure, lithium's a wide ion, and it moves through there quickly, but no, it turns out it was the oxygen vacancies that were moving in the opposite direction. That came out, all those predictions, came out of the computer before anyone ever made the material. They made the material in almost the first time they made it, and they confirmed the theory. I'm not sure I can give you another example in the last seven years, that's maybe because I'm not department head anymore, and I'm not following all that, but it is going to come.

George Smith

Ok, fair enough. You realize the same thing is going on in quantum

chemistry and physics. Long thought on the simplifying assumptions...a real interest in it. In the last few years, as computers have become more powerful, and being used to synthesize molecules. Now, let me ask the follow-on question, do you see any sign so far, of feedback from Materials Science Research into physics as such?

Eagar

No. Do you think that physicists would even think of talking to a Materials Scientist. I mean, this is the hierarchy of snobbery!

George Smith

Well, I understand all of that, but remember there was a *Scientific American* article, roughly a year ago, by a physicist out in the Midwest, I think it was Wisconsin, talking about the design of materials along the lines you're proposing. I found the article to be a sales pitch.

Eagar

Well, they're worse than that. I don't remember that one, because I probably just looked at it and threw it away, you know, read a few of the figure captions and decided "This guy is a physicist and doesn't know what he's talking about." I remember the one that was in *Technology Review*, about 4 or 5 years ago, maybe 5 or 6 years ago. They had a little linear induction motor made out of atoms. I forget how, I said this... oh, they called it a planetary gear. It wasn't an induction motor, it was a planetary gear. And they actually had four or five atom molecules that were the gears. They tried to draw these things as if each little atom... this came out of Lawrence Livermore, right? Some computational... basically it was a mathematician, who would work with some materials scientists, and it was all a big sales pitch. And I said "This is not a planetary gear, this is an interplanetary gear!" And the reason is anyone who has ever worked in Materials, knows that the surface atoms are extremely reactive, and if you put this in an oxygen environment, all these atoms would, you know, would oxidize, and you

wouldn't have this structure anymore. The other thing is, there's no lubrication here, the atoms, one on one, actually like to bond. I had three reasons why it was an interplanetary gear: you had to operate it in an ultra-high vacuum, and I don't remember the other two, but it was absurd!

George Smith

Briefly, the *Scientific American* article starts an unusual paradox, if you look at boundary constraints, there's so much larger material strength. But this is the paradox, that we now, in quantum mechanics are understanding, in such a way that we can now start constructing better.

Eagar

Well you know, and I read all this stuff about carbon nanotubes, you know, having calculated the strength of them. We did iron whiskers in the 1950's! They actually experimentally measured them, and it's not a theory, ok? And actually, one time in my class, three years ago, I was pointing out surface tension in my welding class, and I was talking about Van der Waals bonding and stuff, and I pointed out that, you know, the strength of an atom-atom interaction is millions of psi, and that should be equivalent to the surface energy created and one student said "How do you do that?" So I went back to my office, I had an hour after class, and I worked it out! It's actually just, you integrate $F \cdot dx$ to get the energy of the Leonard-Jones potential, and you compare that energy, per atom, to the area of surface, of what's the unbonded state. You predict that iron, to pull two iron atoms apart, $F \cdot dx$, that energy is equivalent to the surface energy created by iron, 1.5 Joules per square meter. You know, yes, I had to work it out, it took me an hour to work it out, mostly because I had to go back and remember my units conversion, right? It wasn't a hard calculation. This is a freshman physics type of calculation. But the units conversion got me all screwed up. But it works out. You can prove, all these people are presenting this as if it's a wonderful revelation! 50 years later! People worked this out! I don't know whether it was Cahn, whoever it was, but people worked this stuff out, I don't know, years ago. It's because they don't read the literature, or they need

to sell something, to people today and say "I'm new, I'm different, I predicted carbon nanotubes are the strongest things going." You know, I got criticized once, because I said the carbon-carbon bond was the strongest bond. This was in some *Technology Review* article I wrote. And some chemist spoke to me and said "No, it's the silicon-oxygen bond in silicones." You know, one's like 2.2 eV and the other's like 2.3! Or 2.25 or something! And "Oh ok, so I'm wrong!"

George Smith

Ok, quick comment and then I want to pursue this one step further. I don't know if you realize that this is something I followed in quantum chemistry. The Jones-Leonard potential has been derived, was derived for the first time successfully in the 1980's. And at that, only for helium.

Eagar

Oh yeah?

George Smith

If you actually tried to do the derivation from first principles, you get totally wrong results. And they got it for helium, it was a huge computational endeavor. The problem is you have to integrate, average, across all possible orientations. So they finally broke it enough to say it actually works for helium.

Eagar

Because helium's symmetric.

George Smith

You got it! Except it's not symmetric in electron orbits, so you have different orientation. Let me ask you a different question. Arne made me

look at your curriculum. Which I guess has changed recently, and Suresh has changed it still further, but I'll leave that alone. What I noticed, and he was calling to my attention, is a bunch of solid-state quantum physics courses. Do you presuppose the material in those courses, in your classes, you are very seriously giving them quantum mechanics. Do you expect your students to understand that?

Eagar

No. In fact, don't ask me to defend that new curriculum. That new curriculum is heading in the exact opposite direction of where I think the department ought to be heading. That curriculum was put together by some people who have never worked in industry and still think that we're producing PhD's to go on to academic jobs. As 10% of our doctoral students. 85% of our students go into industry. So our department, in their quest in this hierarchy of snobbery, likes to consider themselves, or they are more and more trying to consider themselves research or materials scientists. In materials engineering, there have been relatively few hires. In fact it's getting to the point where it's questionable whether we can even teach heat and fluid flow anymore. We were innovative in 1995 by requiring heat and fluid flow. We were the first Materials Department to even require heat and fluid flow of the undergraduates. But we're about one year away from having no one, except me, teach it, and I'm not going to. But they have not replaced the people, the materials engineers who could do that, and more and more, because all the students want to go into photonics. Which is one of these waves, I mean, this wave may be a fifteen year wave as opposed to a five or ten year wave, like advanced ceramics in the mid-1980's to early 90's. So maybe electronic materials is a longer wave and then biomaterials is coming along as a wave. But there's going to be something else. Whether biomaterials takes it over, you know, takes over electronic materials, but the industry is going to saturate. It's the old story of you know, the functionality... well I say old story! Christiansen got his claim to fame for the *Innovators' Dilemma*, that the technology outstrips the need. And people are going to quit buying functionality that they don't need. Do you need a 1.8 GHz computer to do word processing? Lets face it, most of the PC's in the world are just secretaries

typewriters.

George Smith

Except for the internet.

Eagar

Except for the internet, yes. But, well, you don't need a, do you need 1.8 GHz for the internet?

George Smith

Well they make sure you do by... put more crap on there!

Eagar

Well given that last mile speed, you don't need that!

George Smith

You're not really presupposing...well what you probably teach is practical, right? You're teaching welding among other things.

Eagar

Well, yes, but I teach it from a completely different point of view, but that's only to graduate students. When I taught undergraduates, I used to teach the sophomores physical chemistry. And that we did expect them to have a good foundation in thermal and physical chemistry. And they were supposed to get it with mechanics in the old curriculum. And they were supposed to get a structural properties course, which unfortunately, we had some of our materials scientists think that teaching space groups was teaching structural properties. Well that was never... I mean I was on the committees! Both the undergraduate and graduate committees, I

was on the committees that formed the curriculum when I was an undergraduate student and when I was a graduate student. I served on the lunch committee and the Cohen committee. The idea is that you would give some student the appreciation for the types of structures over the scales of size. Well, it turns out that of all the people who teach the course for 20 years, we never could get anyone to teach it properly, until finally, Sam Allen and Ned Thomas wrote a book in our curriculum series. Which began to do it, and that's the first book, but it hasn't really taken off because frankly, there's not really any single materials scientist who knows all that material from the get go, from atoms to [Regie Blue], you know, if you talk about size scales, you talking about eight orders of magnitude! Or seven orders of magnitude.

George Smith

Now, you're not presupposing...what about evidence? In his undergraduate and graduate, David Parks tells me that in the graduate program, really does presuppose the quantum mechanics principles - in higher courses. I'm dubious.

Eagar

When they talk quantum mechanics, they're talking about the concept that electrons can tunnel through an energy barrier.

George Smith

Let me use mine as a philosopher now, my terminology. I easily see quantum mechanics playing a profound heuristic role and serving as the basis for computation. But that's a little different from the science of quantum mechanics. Actually infusing the discipline. Which is it? Infusing or is it primarily heuristic and an underpinning concept?

Eagar

It's conceptual underpinning. I don't even know if I would give it the

glory of being heuristic. As a graduate student, I taught the chemical metallurgy course, which the first half of the course was Tom King, the department head, teaching blast furnaces. And the second half was Keith Johnson teaching quantum mechanics.

George Smith

(Laughs)

Eagar

And I said there's never been a broader course taught at any other materials department in the country! We went from

George Smith

[To Arne Hessenbruch] You have to see a blast furnace to believe it!

Eagar

We went from particles in a box to blast furnaces all in one course, and I was the TA for it. Nobody remembers this, this was basically teaching two separate modules. But frankly, [Tom King] was doing the old, traditional "teach them what the practice is out there." And Keith Johnson had been hired, I guess he worked with Slater, but he was a chemical physicist. He was using the x-alpha technique, if you're familiar, this was a way of kind of, when computers weren't as powerful, of just solving a single atom and coming up with symmetric boundary conditions. As far as I understand. Keith never got into something that complex, because that was research, and I remember [Tom King] once saying that Keith had just come to listen, this was when Keith was coming up for tenure, and he was all excited because he just figured out why permanganate ion was purple. Somehow in the calculations he had found some energy band or spectrum that gave the wavelength of purple. That was about the strength of what you could do, is you could predict that "the sky is blue." Bob Rose used to joke at that time that the

computational materials scientists were able to predict that copper melts below ten thousand Kelvin! That was about the level of their accuracy in 1970! Today, it's not really quantum mechanics first principles, but using Thermocalc and things, you can actually predict the melting point of metals, complex alloy systems, 12 component systems, more accurately than you can measure them. You can predict them within ten degrees with a fair amount of reliability. That's not quantum mechanics. That's basically just taking huge databases of thermodynamic data and fitting and finding the best fit.

George Smith

Which to me is engineering science.

Eagar

That's engineering science. Yes, it's not basic science. If we go to what people do today, yes, actually, people use the old x-alpha technique as kind of a tool. They don't use it for undergraduates, but they use it for graduate classes now. People are trying to use some of these programs-- actually one of these programs came out of the biology field-- for first principle calculations. But there was a \$50,000 program that Gerd got inexpensively, and he teaches a course on computational techniques in materials science. He's basically teaching them how to use some of the tools that you pull off the shelf. Physicists are developing them.

George Smith

I may try to take the course.

Eagar

I don't even know if he's still teaching it, but anyway. Basically, he's just pulling tools off of the shelf and showing them how to use them. It's interesting to me to see the tools the graduate students can pull off the shelf, whether it's Thermocalc or whether it's a first principles type of

thing. The first principles things are certainly, as you pointed out, are coming up with very very simplistic models, but that doesn't mean that they're not useful.

George Smith

Well, let me now summarize. You said one thing earlier that I now want to understand. Mainly, the time is coming, for these calculations to become more and more pervasive. Fair enough. What it sounds to me, is the science side of this is in contrast to the metallurgists I grew up with. Now remember, what I know as an engineer dates starting in the 1950's. These metallurgists literally taught me on the spot. They wouldn't have known about quantum mechanics, or anything!

Eagar

They didn't even know "particles in a box," right?

George Smith

They were in another world. What's happened, is the application of fundamental science to specific problems in the materials realm, there's been a transition from virtually no attention to the possibility of using highly current science in application, to a great emphasis on it. Is that a fair summary?

Eagar

I think you're going back a little too much to the physicist, of what the latest thing is. I'll accept there's a ten or fifteen year delay.

George Smith

Ok. Fair enough. But twentieth century physics, fairly recent physics is being taught.

Eagar

Oh yes. I mean...

George Smith

And that's not true to the metallurgists who were coming up in the 1940's.

Eagar

In 1970, when I was taught, I was taught k -space, and reciprocal space, lattice to understand the band structure of metals. Which really was what Slater and others were doing in the 1930's. Right? It made it down to the undergraduate curriculum, by 1970.

George Smith

Ok, that's impressive.

Eagar

It was just probably in the graduate program in the 1960's, and so the time lag might have been 25 years at that point. I would say the time lag now it ten to fifteen years. But it hasn't shrunk to three or four years or five years,

George Smith

And it's only when it shrinks down to something like that, that the possibility of feedback into the science starts growing.

Eagar

That's why the physicists look at what the materials scientists are doing, and they say, "These are not sophisticated models. What Gerd Ceder did is he's using models that are ten years old." Well why is he using models that are ten years old? Well first of all, it takes about three or four or five years to recognize they exist, and then it takes a few years to learn how to use it, right? And then by the time you use it, by the time you get a result out, it's ten or twelve years at the earliest! Plus you have this kind of series of recognizing that technology exists, learning it yourself, and using it and applying it, and you have those three things, and that's going to take 12 years. I remember in 1984, when I worked for the Navy in Tokyo I went to Australia, and I met the guy who was the science master for Australia, and he happened to be a metallurgist. He worked for BHB. We were at a conference together, and he told me that the Australians had just done a study, to find that it took eight years for Australia to recognize--actually Australia was eight years behind in most of their science. Seven years of that was just recognizing that the work had been done somewhere else. It only took them about a year to catch up, once they recognize that the work had existed. But just sifting through the literature and realizing that over here, or at this institute in the Ukraine, or over here in Germany, or over here at Stanford or whatever, someone had come up with something really similar. It took them seven years to recognize these seminal contributions. Lets face it. They don't all come, they come from disparate places at disparate times.

George Smith

In physics, people are very aware of one another, of what they are trying to do in the backdrop. So there's very quick dissemination. Here it's individuals picking things up sort of in isolation, and playing with them until they become productive enough that it spreads. That's because you're not really doing research in these tools. You're doing research in the application.

Eagar

Right, and it may explain why [Bob Ranek], who was at NSF for like 30 years, kind of heading up a lot of their materials sciences, and he used to

say, "Look. The physicists don't murder each other on proposals like the materials people do." Why? Because the physicists, as you said, they all kind of know what the other guy is doing, and even if the other guy has a harebrain idea, you're a physicist, your kind of a liberal, and hey, you say "Maybe you'll come up with something." You don't have this arrogance that you know everything. In the materials science field, there's a few people who think they know everything, and no one else has ever learned anything. "Hand me down from on the high," you know? A transfer of knowledge. It's a very dysfunctional approach to assume that all your colleagues are buffoons. Now it may be true that 98% of them are, but none the less, to assume that all of them are...

George Smith

Let me put the capstone on this line of discussion, and then start the second line. So if Bob Brown asked you, you wouldn't recommend moving the Materials Science & Engineering Department into the School of Science?

Eagar

No.

George Smith

Is there anybody in your department who would be inclined?

Eagar

Oh there are three or four very arrogant people who think that they are great scientists and great physicists, and what they don't realize, and Bob Rose (Bob was my assistant advisor) and I always talked about the fact that they are half solid-state physicists.

George Smith

What's the size of the department?

Eagar

It's about 32 or 33 faculty. How many people did we train in physics?

George Smith

phD, graduate trained?

Eagar

No I know. Not that many. Maybe two or three, I'd have to go back.

George Smith

Chemists?

Eagar

Well, chemistry, you know, Gus Whit, who is retiring this year, had a degree in physical chemistry from Indiana. But most of them come out of Materials or Chemical Engineering departments.

Arne

Wuensch is crystallography, sort of physics...

Eagar

Bernie had a joint...who's the great crystallographer in the physics department at MIT...[B.E. Warner]. Bernie did a thesis in our department, but [B.E. Warner] was his thesis advisor. Bernie's the guy who thinks that teaching space groups is teaching structural properties. I

had to, as a tenured professor, had to mount a revolt. He was chairman of the undergraduate committee, he was head of the committee that created the curriculum. And I knew that what he was teaching was absolutely worthless to these student engineers. I mean if they were all going to go off and be physicists, it was probably the perfect course, but that's not what they were going to do. I had to lead a revolt. Bernie, who was usually a very kind and gentle person, Don Sadoway remembers it, one time I had finally brought it up again, and the undergraduates backed me up. None of the faculty dared to back me up, because Bernie is one of the most articulate people in the Department, and Bernie, when we had the vote, and the undergraduates on the committee sided with me--and the other faculty actually voted to make it a broader course rather than just space groups-- and Bernie just lit into me, in front of the undergraduate students and everybody else. He told me how an ignorant welder couldn't appreciate blahblahblah

George Smith

[laughs] You're not an "ignorant" welder.

Arne

When was this?

Eagar

This was like 1978 or so, I was probably still an assistant professor when I had led this revolt.

George Smith

Tom's never been known not to hold back on a strong view!

Eagar

Hey, you know, if they didn't want me for a faculty member, they could have denied me tenure, and so what, I've got a life, I could go on somewhere else! Which is what has bothered them ever since, you know? Because they know they can't shut me up.

George Smith

Let me go to the other line. When Mel Bernstein talks about Materials Science (he was really the first person I was around), he taught at Tufts for a while...

Eagar

Where is he now?

George Smith

[?]

Eagar

Oh.

George Smith

[Backow]...[?] He had the feeling that [Backow] was not going to listen to him and he wanted out. These are two people he knows pretty well...

Eagar

And he's absolutely right! A single conversation with [Larry Backow] will tell you that he doesn't listen to what you're saying.

George Smith

I know, and that's what happened. A single conversation, in private, and he came out of that office and wanted out. When Mel talks about Materials Science, what he emphasizes, is the generality of materials over the word metals.

Eagar

Yes?

George Smith

And to the extent that he was teaching a cute, a nice undergraduate course on materials, in which he emphasized the substitution of non-metallics for metallics and vice-versa. He went through the history of different bicycle materials.

Eagar

A typical approach. I've seen it many times, students like it.

George Smith

It's a good course and I'm very impressed by it. I'm curious about that in some ways, and I'll do it in my polemical way, which is probably surely wrong. As an outsider watching, of course you know I have a very slanted picture of the aircraft engine industry, and in effect applying the knowledge I got here to turbines, but generally, I got the impression that the metallurgists saw various materials coming to the forefront that might potentially replace metals. And they didn't want research on those materials to fall into other departments. They wanted to absorb it, but at the same time, the most widely used material in the world, concrete, they showed no interest in.

Eagar

Well, I think you're giving too much prescient knowledge to these metallurgists, ok? I look at it as first of all, when our department was started, it was course 3. There was Civil Engineering, Mechanical Engineering (course 2 at MIT), and there was Mining Engineering. Now there's not a lot of mining that goes on in New England anymore. There's not a lot that goes on in the United States. But it was in 1888 that they added the title "Mining and Metallurgy." And you have to remember where metallurgy was, you had Bessemer. Who had come along twenty years before, and all of a sudden the steel industry was growing. Andrew Carnegie was the richest man in the world. He was the Bill Gates of today. So why did you go from mining to mining and metallurgy? Well because metallurgy, the steel industry, was becoming the dominant industry. They were the semiconductor manufacturing industry of the day. And in fact even until 1961, remember when U.S. Steel wanted to raise prices and Kennedy had to stonewall them down, because it was going to cause worldwide inflation. It was like raising the price of oil, before the price of oil became the gold standard of...

[tape 2]

Eagar

During World War II, we bombed out most of the world's steel making capacity, so at the end of World War II, the United States had 75% of the world's stainless steel making capacity. Bethlehem and U.S. Steel had 40% of the world's steel making capacity between the two of them. You developed an arrogance among these businessmen that is unbelievable. From the days of Andrew Carnegie through Charles Schwab, who started Bethlehem, through, who was it Martin? Well, anyway, the guy that ran Bethlehem through the depression. It turns out that during the depression, the ten most highly paid executives in U.S. industry, six of them were at Bethlehem Steel. And I can't remember the guy who ran Bethlehem Steel at the time. He made a profit when Bethlehem lost money during the depression, because he got paid a bonus as CEO, for every pound poured, every ton poured. It wasn't how much money they (the company) made. When I joined Bethlehem Steel in 1974, they had just had their most profitable year ever, 1973. They

had been almost bankrupt in the late 60s, when they built the last integrated steel mill in the world to be built by a company. Every one since has been built by a nation. Bethlehem built Burns Harbor Indiana plant

George Smith

You realize I worked on that.

Eagar

Did you? Ok. So anyway, you know Burns Harbor, but Bethlehem was close to bankruptcy in the late 60's, but then when Burns Harbor started, it was an efficient mill. I remember in 1975, as a "looper," A "looper" was... Nick Grant had been a "looper" at Bethlehem Steel. They had the "loop" course, and back in the 1930's, late 30's, when Nick Grant went through it, he had been an undergraduate at Carnegie-Mellon before he came back to MIT.

George Smith

This is a term I don't know.

Eagar

The "looper?" They had the Bethlehem Steel loop course, it was very famous. They took all their college graduates, and for one year, they would put them six weeks in the blast furnace, six weeks in accounting, six weeks... you did these six week stints for a whole year, until as a management trainee, you learned the entire loop of the company. You couldn't go through the whole thing, but you went through like eight of these things, eight of these modules. Then you were a "looper." I was hired, I was one of 500 college graduates, hired into the loop course. Well, when I went into the loop course, it was no longer a year's training. I was hired in the research department, and I had been working there for seven months. So when the next summer, they ran it for 2

weeks at corporate headquarters in a great big auditorium. And in the mornings we would have lectures from the vice presidents. Tells you something about 2 weeks worth of vice presidents, about how many vice presidents we had. And in the afternoons we would take tours of the Bethlehem plants to see how steel was made. With our white hard hats to show we were management and such. We would just walk through like a bunch of prima donnas through the steel plant. That was the loop course and how it had changed. But I remember the vice president of finance gets up, and he says "It doesn't cost Bethlehem Steel anything to make steel because our coke ovens were built in 1911 and our blast furnace was built in 1912, and they're fully depreciated." and out of 500 ignorant little people who were supposed to be impressed, and I had the audacity to raise my hand and said, "I don't understand why it doesn't cost any money to make steel just because something's been depreciated." And his answer was, "That's because you don't understand finance." And, you know, that was true. I did not understand finance. But it wasn't until two years later that I took a finance course at Lehigh, that I realized that he didn't understand finance either! You don't save money by using something that's old and low-productivity just because it depreciated. On the books, on his books he might but in reality, you don't. And that's what killed the American Steel industry. But the point is why did we take metallurgy out? The department over there has got like 23 endowed chairs. Something like 17 of them come from the steel industry or steel people. Ok? If you look...

George Smith

You're even less that extreme at Carnegie-Mellon.

Eagar

Oh yeah.

Arne

"Over there" being the department...

Eagar

The department, yes. In our department. And one of them is actually a chair of ferrous metallurgy. This is a guy gave four chairs, a Greek steel man, he gave them when I was department head. The first chair, or two chairs, he gave two chairs. He actually gave about 7 chairs to MIT, one in Chemical Engineering, his Master's thesis advisor, and he upgraded a junior chair to a full professor chair. Then he gave four million dollars with which we created two chairs, the [Metoulla?] and the [Establa Solophotus?], two chairs he gave for his parents. And he gave those gratis. No strings attached, didn't have to do anything. When he came in to form the [Tom King] chair, of course Tom King was his thesis advisor, and the chair in his and his wife's name, and [Vicillian?], [Venet?], and [Solophotus?]. The [Tom King] chair is a chair of metallurgy. And the [Solophotus] chair is a chair of ferrous metallurgy. I called up Bob Brown, after he came into my office and said he was going to give me three millions bucks, after he had already given us four million bucks, and this was like a year and a half later, and I said, "Bob, can we accept these?" And Bob said, "Oh sure!" Bob has no compunction whatsoever. He would take it, and what does he care? Screw him! [Solophotus] will either be dead, or if he comes back, he can't get his money back. That's the ethics of Bob Brown. And that's the ethics of MIT, so far as that goes. But that's one of the things, I mean I always tell people that's one of the reasons I stepped down. There's a thousand reasons I stepped down. But that's one of them! I know, and [Vicillian] calls me, every time he comes back into the United States, because he realizes that he can trust me, and I say, "[Vicillian], you know..." what I did is, I created a good friend of his, [Claude Lupus], Who was an MIT graduate student at the same time. They're both of Greek origin. When Claude's mother was dying in Egypt, and he was living in Australia, he had to fly through Athens to get to Egypt, and he stayed at [Vicillian's] house. This was back in the 1970's. They're best friends, and I got Claude, well Bob Brown killed it, but Claude was a full professor at Carnegie-Mellon, and went off to Australia and worked with the World Bank and other things. He had a degree from France, it was the equivalent of a doctorate in economics before he came to MIT to get his doctorate in metallurgy. He made full professor at Carnegie-Mellon and went off to become the

advisor to the CEO of [A-Max] or whatever, and worked in industry, basically as a consultant for the World Bank type of things, and made mega billion dollar projects around the world for twenty years. He wrote... at the end of that, wrote a textbook that's being used at about half of the materials departments to teach graduate thermodynamics. He also won an award, a practical award from AIME, ok? He wanted to come back because his kids were going to start college in the United States, and I figured this was the perfect person, sixty years old, twilight of his career, make him a professor at MIT. Bob Brown wouldn't have it.

It was basically, you know, give this guy...well anyway, what he did was let me make him a visiting professor for five years. So Claude's treated as a second class citizen, but he has the name [Vicillian Solophotus] on his chair! And Claude is as much as a physical metallurgist as anyone else. [Vicillian] is very happy, but what happens when Claude's five years is up? I don't have to find anybody, I'm no longer part of that! Well it's not as if Tom Eagar's going to be quiet about the fact that MIT is going to use this to hire some photonics person in the chair of ferrous metallurgy! Ok? It's deceitful, it's dishonest, it's unethical, I don't care what you call it, but it's the MIT way of doing business.

George Smith

Can I read you correctly? This is still...

Arne

Yes, I have it all on tape, but you haven't signed anything yet!

Eagar

I'll sign it!

George Smith

So there's not a metallurgy department in a very real sense?

Eagar

No, no, no. They have not hired, actually, they did hire one metallurgist, who will show up in the next year, but that's probably the first metallurgical hire in ten years. Mert Flemings actually fought very hard, got a lot of flak from the steel guys, to drop it down to 25% metallurgists. When I was a student it was 70% metallurgists, 25% ceramists, and 5% polymer people. Or polymer "person," I guess. Just one or two! And they wanted to build up polymers in the 70's and 80's, and then Flemings actually, for all of my problems with Mert Flemings, he actually did a very good job of balancing it so it was 25% electronic materials, 25% ceramics, 25%..., well he never got it down to below about 30% metallurgists, and 20% polymers people. And, well, I mean I completed it. And at some point while I was department head, we were 25% of each. But it never had this thing where the steel guys wanted to bring the other things in--the steel guys wanted to keep the other stuff out! Why is concrete out? Because it's a competitor to steel!

George Smith

I understand. But I gather there's somebody in Civil Engineering taking a Materials Science approach to concrete right now.

Eagar

Well they have to, because there's more tons of that used than steel. However, they've hired several people in that field, but it's very hard for them to get tenured, because you have people like Bob Brown at the top, who says, "We don't want someone in concrete," just because it's one of the most heavily used materials, and it has the potential to be improved dramatically. Ok? And the science has been done to show that, now you just need someone to show and prove the processing economics to do that. But Bob Brown wouldn't be interested in that because he gets his Materials Science off of the front page of the *Wall Street Journal*.

George Smith

Let me try something. Of course his background is applied mechanics.

Eagar

Yes, but he considers himself a Materials Scientist, because when he first started Mert Flemings gave him some money, some seed funds out of the Materials Processing Center, to do some problems on silicon crystal growth. So Bob Brown thinks he knows more about Materials Science than I do, ok? Actually, he thinks I'm a dinosaur in Materials Science. We actually shifted to 15 or 20% metallurgists, and that's mostly just because some of us can't wait until we die. The department now is trying to shift to almost all electronic materials and biotechnology. Which is interesting, because when I started out as department head in 1995, I determined that we wanted a soft-tissue biotechnology...They had an opening, supposedly, for a biomaterials person, but Mert Flemings and everyone else in the department was thinking what I call a "hip corroder." Basically, looking at vitality and how metal implants corrode in the body because that's all they ever knew, from the 1960's on. That was what they thought of biomaterials. I went, and I talked to Doug Lauffenberger, and I realized the type of stuff that Bob Langer's doing, the type of stuff that they're doing in Chemical Engineering on soft-tissue engineering was the real future. And that's where people would be using the principles of polymer science. It took me two years of bringing in candidates for a faculty position in biomaterials before the faculty finally got a vision. We must have brought in a dozen, some of them very senior, and some of them, well most of them junior, candidates. And I was getting faculty coming to me and saying, "What are you bringing this person for? This is a Chemical Engineer, they don't belong in the Materials Department!" It took me two years, and all of the sudden, after about two years, the faculty had the light hit them. They realized that soft-tissue engineering was what really the future of biomaterials is, and so now, they're running "whole hog" into it. The pendulum swings too far to a certain extent. But none of them, I mean fortunately, you know, the wicked leaders, he who they despise, the good leaders, he who the people revere, and the great leaders, he who the people say, "We did it ourselves!" I actually was the great leader in biomaterials in that department, because nobody in that

department would now associate me as the person who forced them into soft-tissue engineering. But I fought them for two, two and a half years!

George Smith

Ok. I want to continue this line, but I want to do something different. I mean, from my background, when I hear ceramics, when I hear composites, my eyes tend to roll.

Eagar

Well, same here! I wrote articles on that!

George Smith

Well, you know, Rob's article on the heart valve is one I constantly pull out, and tell people these are materials whose time has not yet come.

Eagar

And will not come.

George Smith

And will not come for forty years! Well that's Rob's view. But now, David Parks offered the following description of the change in Materials Science. He said, "The old organization," and he was at Illinois, "Was a group of specialists defined by the Material they worked on."

Eagar

Yes.

George Smith

And a curriculum built around the individual. And that's just not true anymore. What you're doing is generalizing across materials constantly.

Eagar

Yes. That's true, and that came from... it came actually first out of the [Wench] committee. It was led in part in the background by Morris Cohen. If you want to go up to Swampscott, you might want to interview Morris Cohen. Morris, who was a steel man, he was revered by the steel industry, was the guy who led the charge nationally to take it from a metallurgy field to a broader field that covers Materials Science and Engineering, and looked holistically at the structure and properties relationship you had been looking at in steel and copper for all these years. The same things apply to other things. Which is why I laughed at Buckytubes being so strong when we were doing iron whiskers in the 1960's and the 1950's.

George Smith

I had lunch yesterday with [Dudley Hershey], and he of course, created the technology...

Eagar

You know these are people reinventing the wheel. Morris actually had the vision, and through COSMAT, he pushed that vision that it really was a holistic field of Materials Science and Engineering. I think, although you'd have to talk to Mert and Morris I think [Herb Polyman] was one of those, where all but everyone hated the man. And I only met him at few times in my life. He was one of the more arrogant people, but he was at MIT,

George Smith

Yes, I understand.

Eagar

But not in the Materials Department.

George Smith

He was incredibly arrogant from everything I hear.

Eagar

He belonged at Harvard. I mean there was absolutely no question.

George Smith

See I saw traces of him at GE.

Eagar

Well, you know what I always say. MIT is the second most arrogant school in Cambridge. You can quote me on that too.

George Smith

You don't have to be quoted, everybody knows that! That's simple truth!

Eagar

So the industry in the 1960's and the 1970's was still feeling they were on top of the world. And I remember when I went to Bethlehem Steel in 1974, as a young employee, they had 25% of the world's steel industry sales, where they had 75%, but the guys that had been hired after World War II were now the managers 25 years later. They still thought in terms of "We control 75% of the world's steel industry." But they only had 25% of it, but they had the arrogance to try to do it. And that was the

beginning of the end of the steel industry in the United States. They would not change, they were proud of their 1912 blast furnaces, because that was what made them profitable for all they knew. They were idiots, they were morons, I don't care what everybody else says. They were not looking at new technology. They were living in the past, they were flying, they were taking corporate jets from Bethlehem, Pennsylvania, down to Florida on the weekends to play golf. I can give you all kinds of stories. Just total corruption, I mean, well I don't know about "total corruption," but they were not businessmen. They were just people who had worked themselves by the corporate lobotomy to the top of the heap, and now they were taking all the perks they could. And I could tell you, we could spend hours on the perks these guys had. The automotive companies, and Kodak, and all these others never topped the steel company in terms of taking care of their executives. So those steel companies Were very upset for the next 15 or 20 years that MIT and other people were moving to this more holistic view of Materials Science, rather than metallurgy.

George Smith

I would think they resisted perhaps?

Eagar

Oh yes. And I don't think we were the first department to change to "Metallurgy and Materials Science" as the title.

George Smith

No, you weren't.

Eagar

Ok. There were one or two others, but it was only because they didn't have as many politics to fight at the other schools that did it. I think you'd probably find that an individual at each one of these schools who

was on the COSMAT committee and stuff, and so they did it earlier than we did, but only a few years earlier.

George Smith

It's interesting to know that the steel people resisted. [Bernstein's] an interesting case, because of course the handbook of steel, he co-edits and co-authors, and he had become just an outspoken proponent of broader materials.

Eagar

Well, it's a no-brainer! I look at what the polymer folks are doing today, and I say, "They've discovered alloying." Ok? What was discovered 150 years ago, they've now discovered!

George Smith

Well, 1500!

Eagar

Well 1500. Well in terms of starting to understand, but yes. We've been doing it for years before, but we didn't really understand what zinc did to copper. But in any case, what it was that it was harder to alloy in polymers. You can't just throw them together, you actually have to synthesize them together as block polymers, but that's nothing more than alloys, the polymer analog to alloying in metals. And now, guess what? Instead of monolithic silicon, they're going to silicon germanium, and the compound semiconductors and stuff. Well, surprise surprise! When you marry two materials, you can actually enhance some properties at the expense of some others, which is something people don't always realize. That you don't get your bang for your buck in every area.

George Smith

There's no free lunch.

Eagar

There's no free lunch. Anyway, so the metallurgists still dominated the industry. A lot of them were wealthy and gave a lot of money to the department to create chairs, but they have been somewhat chagrined to see that the department shifted. Although in the late 1990's, many of them actually have acknowledged the wisdom of the department for having switched. Because most of the steel guys realize that the steel industry is gone. You have to remember that the steel industry was where our students went! Where did Tom Eagar go when he graduated? Bethlehem Steel! They hired 70% of the graduates, and that's why 70% of the faculty were steel people. Why is the department swinging towards electronic materials and biotechnology? Because that's where they're hiring students. The problem is the faculty still think that they're producing professors. Not students for industry. And the faculty don't like to think of themselves as engineers. My tenure case, one of the letters, and I know the person who wrote it because I got to read my whole tenure case when I was department head. This is one of my colleagues on the faculty. He described me as a "pure engineer."

George Smith

I would describe you that way.

Eagar

Yes. I describe myself as a pure engineer. I am proud to be an engineer. I'm one of the only people I know who is proud, working in academia, who is proud to be an engineer. I've always said that it would be wonderful if more than 20% of the faculty in the School of Engineering at MIT were engineers!

George Smith

Parks is another one.

Eagar

Parks is another one. Yes, he's one of the 20%. Now I also say I wouldn't hire more than 20% of my colleagues as a consultant! Most of them are scientists who wouldn't be able to figure out any complex problem, make a decision on it, in the face of uncertainty, and have a chance of being right. I'm a pure engineer, and so I've accepted the fact that I'm a pure engineer. I'm proud of the fact that, although I don't like to use the proud word for religious reasons, but I actually am "pleased" to describe myself as an engineer. And I've come to realize that I am in a tremendous minority among academics. And it has created, I'm somewhat of an outcast because of that, or looked down upon by these other people. But I realize they're all third rate scientists. The people in the engineering schools who like to parade around pretending they are scientists, if you took them into a physics department, they'd be laughed out of the room, ok? And anyone who looks at it objectively knows that. But these people, I mean some of them have a tremendous amount of arrogance. They think that they're doing the most wonderful stuff in the world, and all they're doing is second rate engineering. If they would recognize they were engineers, and accept it, and be pleased with the fact that they are engineers, they would do a much better job than they do. But they like to think of themselves...

George Smith

You're preaching to the choir.

Eagar

But you understand that this hierarchy goes from the scientists at the top, down towards the lower lever, the engineers. What I learned, in biotech, is that clinicians are beneath the engineers. Ok? Because they are totally empirical. But there are very valuable clinicians out there. The thing is all these people add value to society, but what irritates me is that they

can't respect each others' contributions.

George Smith

I have one last question, and then I'll be...this is the sort of thing you like. Is to hear this hierarchy. I have a minor question, in a way it's a selfish question, They pay very little attention to the mechanical behavior of metals, thinking of fracture mechanics. Which of course is the one area of your entire field that I know little about.

Eagar

That's because the scientists, don't even...do you know fracture was left out of the graduate curriculum 20 years ago? And I brought it up in a faculty meeting and [Reggie] looks at me and says, "Yeah you're right!" I said, "We're not going to teach fatigue or brittle fracture in our graduate curriculum?" And everybody kind of looks around the room and [Reggie] looks at me and he says, "Yeah you're right it's not here!" No one in the room had even realized they had left it out.

George Smith

Of course, you know my view is you learn from failures more than anywhere else in the real world, but you know that.

Eagar

Yeah, I know that.

George Smith

Now it's a hybrid field, because it's both Mechanical Engineering and Materials Science. Is that right? Should it be a hybrid field?

Eagar

I actually think it's one of the strengths, in fact as a freshman, the way I chose Materials Science and Engineering, first I happened to have 3.091 with [John Wulff], and I had [Jack Hash], who won the [Goodwin] Medal as the best TA at the institute. And my house tutor was a Materials Scientist. But the real reason I chose it was it was the only department in the school of engineering that had both science and engineering in the name. I didn't know if I wanted to be a scientist or an engineer. I didn't even know what a scientist or an engineer was at the time! But I knew I wasn't ready to make the choice. And because of a host of reasons, but one was because science and engineering were both in the field. And I actually think that's a strength. I wish the faculty actually recognized where they lie on the spectrum of science and engineering. They are all on one end of the engineering side of that spectrum. What they consider Materials Science, the physicists would consider applications. They don't understand that there is this other half that goes all the way back to the fundamental science. You're talking about feedbacking control, well that's because they don't even recognize that Materials Scientists think that they are fundamental physicists, ok? And the fundamental physicists look at them as these applied guys, and so there's a total disconnect. There's no feedback at all, and maybe you could speed things up a little in the world if maybe they developed a little respect for each other. But that's the whole respect thing that irritates me so much. These people don't respect other people's contributions. I'm used to people in my department looking at me as the far end, the engineering end of that spectrum. And I don't mind being there, but I have had a lack of respect from my colleagues for the work I do because of that, and I think that [John Wulff] can go back and point to that, and there's some other faculty who could go back and point that they were on the further engineering side. It doesn't bother me, because I got a better record than any of them. Ok? So I can kind of thumb my nose at them and say, "Screw you!" But it would be a heck of a lot better if the collegiality were better.

George Smith

The collegiality is pretty good though. The mechanical behavior of materials with mechanical engineering, and of your people, I hear

nothing but respect from [McClintock], Parks, yes, all of those guys.

Eagar

Subra has his own appointment over there, and he graduated from that department. And I think they...

George Smith

That's the problem. It's that that department doesn't know very much metallurgy.

Eagar

You said [Rob Ritchie]? Yes, he was in that department, and he is very much in my work, he's more of a mechanical engineer. I think they get along. As well as anybody. And they had a certain respect for [Reggie]. Well, they did.

George Smith

But [Reggie] had a lot of respect for them.

Eagar

[Reggie] had a lot of respect for everybody. [Reggie] was one of the few people, who, you go around the country, and everybody asks how he's doing, ok? He's genuinely loved by many many colleagues. I keep trying to get [pointing to Arne] his counterpart who's French, Bernadette, to interview [Reggie] in French, because I think they would hit it off profoundly. But because he's not a central figure in Materials Science, as you look at the literature, and they don't realize that he's a giant! In fracture mechanics.

Arne

Well, we're not just interested in the central figures.

George Smith

I understand, but [Reggie] would be a very interesting person, especially with her interviewing him, with his Parkinson's disease, his English is really difficult.

Eagar

[Reggie] bridged Mechanical Behavior and Materials extremely well. And he is a wonderful person, he respects everyone, and therefore had a lot of respect from a lot of people. But within the materials community, he was looked down on for the quality of his work.

George Smith

That's strange.

Eagar

Well that's not strange, he's at the engineering end of the spectrum!

George Smith

I know, but his work is a model of excellence.

Eagar

Well, still, I could say the same thing about mine compared to any colleague I know in my department, ok? and I'm not trying to be arrogant here.

George Smith

Well, I know, I hired you!

Eagar

You can walk through my office and look at the awards up there! And that's another thing that maybe we ought to bring up. You mentioned that at other schools there was one or two people. That is the model of Materials Science. Back in the 1940's and 1950's, and I can't go back any further, from what I gather from people, in the 40s, 50s and 60s there were five or six "dons". There was Morris Cohen in physical metallurgy, there was [Norten] in x-rays, there was, it became, Kingery in ceramics, there was [John Chipman] in chemical metallurgy, and there was the other [Norten] in physics of solids, the two brother [Nortens]. These people controlled the department. There were five or six fiefdoms in the department. They told the department head who they wanted to hire as junior faculty, they typically would hire two junior faculty to compete with each other, and these guys were basically slaves for the don, and they would cast one aside or both aside when it came to tenure time. [Ken Russell] likes to say that he was the guy who broke the system, because he and [John Breetus] were hired in to work with Morris Cohen and [Ben Averback]. [John Breetus] was, well, Tom King basically chose [Ken Russell] over [John Breetus], [Breetus] is now at [Olin], against the wishes of the physical metallurgists. So [Ken] likes to say he was the guy that broke the system. Why did [Tom King] do that? Because he and [John Elliot] were hired in to be [John Chipman's] gophers back in the fifties. And [John Elliot] had risen to the top, but this was a case where both of them got tenure, and [Tom King] actually went on to become department head, and he wanted to break the system. That's the way [Ken Russell] tells it. Tom Eagar likes to say that he was the first junior faculty member who refused to go on and work as a slave for a senior faculty member. I was offered the opportunity my first year. My first year budget was fifty dollars, and we can go through that, well actually I'll tell you this story, because this is on tape, you might as well hear it.

Arne

Exactly!

Eagar

I was hired in to fill [Bob Moravian's] spot. You know [Bob Moravian]?

George Smith

Yes.

Eagar

Bob had been told he wouldn't get tenure if he stayed in solidification, the same area as Mert Flemings. I was a graduate student at the time, I was going to borrow a strip chart recorder from him, and I had worked in his lab with Flemings back when I was an undergraduate. I was a graduate, and I needed to borrow a strip chart recorder, and I went to Bob's office, he wasn't there, I walked out and here he is coming up the steps from [Walter Owen's] office on the third floor, we were on the fourth floor. He walks in, and I say, "Hi Bob," and he says "Hi," and he walks in and I follow him into his office, and he's literally picking up books and throwing them across the room! Bob was never a very calm guy! And I said, "What's the matter Bob, you look upset?" And he says, "You're damn right I'm upset, you know what [Walter Owen] just told me?" And I said "No, what did he tell you?" And he said, "He told me that if I stayed in solidification I wouldn't get tenure, but if I switch to some other field like welding, I can get tenure." Because MIT already had Flemings, who was only in his forties, and we didn't want two people in the same field. I said, "Well what are you going to do?" He says, "I'm not going to switch to welding, I'm going to stay in solidification!" And I said, "Well then you won't get tenure! Can I borrow your strip chart recorder?" And two years later, he didn't get tenure, he went to Illinois, and they hired me to fill the slot.

George Smith

Welding.

Eagar

No, no. They didn't hire me to do welding, Nick Grant wanted me to do powder metallurgy and become his gopher. Mert Flemings wanted me to run his laboratory in solidification. But I knew that welding was a possibility and they were interested in that, so I basically said I wasn't going to work for anybody, because I had seen what had happened to these people when I was a student. And I didn't work for anybody, and they basically, within the first three months that I was on campus, the department wrote me off. And they gave me a secretary, who was on the fifth floor of building 13, I was on the first floor of building 8, had the little short door (if you know the short door). That was my office. I was sharing it with a graduate student. He got the better chair because he had been there longer, ok? And had this old desk from the nineteen tens or something. I went to [Joe Docey] and I asked him, "Can I get some decent furniture?" Because this was graduate student furniture, basically from the graduate student office. In any case, they gave me [Kathy Liden] as a secretary. [Kathy Liden] was a wonderful young women, but she was not too bright. She had been [John Elliot's] secretary, and he was over in Japan, he called her up long-distance, which back in the seventies was a big deal to call long-distance from Japan, and he says, "Send me this manuscript immediately!" This was before we had word processors or email. So she sends it to him, surface mail! Kathy, well that was kind of her level of intelligence. And I was behind [Ken Russell]...

George Smith

These things don't get...

Eagar

I was behind [Ken Russell] and [Joel Clark], I was third in line. [Joel Clark] was another assistant professor, [Ken Russell] was a professor.

They were over right next to her. This was before word processors, and I would handwrite out something, and she was supposed to type it up. I could write a three line letter and it would take me a week to get it back. Anyway, so Kathy came down one time to give me back one of my short little letters, and she says, "Oh Professor, what account do I charge your stamps to?" I didn't even have an account, because in the old days, you always went around in one of these fiefdoms of one of these dons and they took care of you. Well I hadn't been willing to go along with this old system, of the fiefdoms so I had nothing. So I went up to [Joe Docey], who was the administrative office, and Joe refuse to admit this anymore, and I said, "Joe, I don't even have an account number to pay for my stamps!" And Joe kind of hems and haws and says, "Well Tom, why don't you go ask some of the secretaries and they'll give you some." I'm supposed to go beg, as an assistant professor my job is to beg for stamps from secretaries, alright? So I immediately go across to [Walter Owens] office, he's the department head, one of the three times I saw him before tenure. And I said "Walter," I gave each one of them a different problem, "I don't even have an account to pay for my long distance phone calls." And Walter thinks about it and he says, "Well, we'll give you an advance on your [Deserd] liason program funds." So he's going to give me a loan on what I can earn. That's discretionary folks, ok? So I went up to Mert Flemings, who was head of the committee that had hired me, Mertie had always liked me as an undergraduate when I worked in his lab, he always thought I was a Senior when I was actually a sophomore. That's what scared me away from working in his lab! But I used to fix all the equipment, and the graduate students all liked me because they would break the equipment and I would fix it. Anyway, I go up to Mert, he's sitting on a million dollar a year DARPA contract, which was a lot of money back then, and he's got the only chair in the department, the [ABEX] chair. And I go in, and he'd hired me, he felt some responsibility at that time, and I said, "Mert, I don't even have an account to Xerox my proposals!" And he hems and haws as Mert will often do, and he says, "Well Tom, I'll give you an account number, but let me know if you spend more than fifty dollars." That was my first year budget! Ok? As a faculty member. I went back down to my office, that little door, and I sat there at my desk and I looked around at the walls...

George Smith

You didn't throw books?

Eagar

I didn't throw books, I said, "Oh. So that's how it is. It's sink or swim." And I swore to myself, "Ok, it's sink or swim, we'll see whether I can swim or whether I'll sink, but if I do swim, I'm going to make sure that no junior faculty member ever has to go through this again." So anyway, I like to think that I broke the system. And the way I broke the system had nothing to do with me necessarily. In 1980, or 1979, I got my first ONR contract in 1977. My first contract, August 1st 1977. One year to the day that I had actually started on campus. And two years later, [Bruce Batuddle] at ONR, my contract monitor, calls up and says, "What would you do with half a million dollars a year?" Well, that's a lot of money back then. What was happening is they were starting to get an increase in their ONR funding, but they couldn't hire any new contract monitors, so ONR had decided they were going to pick key areas of interest to ONR, and put a big slug of money in. And the first one they did, they did it with [Newnam] and piezoelectric materials at Penn State in 1978. And the second one, they chose an untenured, assistant professor...

George Smith

But on a topic of great interest...

Eagar

But on a topic of tremendous interest and at a university where they would love to see some students coming out in that area. And it turns out I only got \$405,000, but nonetheless, all of the sudden, come 1980, I had a DOE basic energy sciences, an NSF, and an ONR, and I had \$600,000 a year in research, which was only topped by Harry Gatos and [Kent Bond] in the department. And they both had junior faculty runts working

for them, and big organizations and everything. I had more money per individual manager than anybody in the department. Mert Flemings was starting the Materials Processing Center, and he came over and begged me to put my contract through his center. Because that would all of the sudden show his center, he had a \$360,000 NASA grant, if he had a \$400,000 grant from me, he would all of the sudden have a nearly million dollar center overnight. He was begging me to put...

George Smith

You see the irony of this, given the questions of the website, is you're talking about a very complex social-political structure in this department. It essentially has nothing to do with the issue of Materials Engineering versus Materials Science. It has to do with the hierarchy of MIT, whether wrong or not. The obvious question is whether this is peculiar to MIT, or is this just a distortion? Now at this point, I basically have to drop out for two reasons. I've got a board meeting, but has this been useful to you?

Arne

Oh yes, very useful. Very useful.

George Smith

I apologize for taking...

Arne

Are you in a rush?

Eagar

I'm going to have to leave in a little bit, I was supposed to be on the 10:45 but it was cancelled this morning, so I'm taking a later one. But,

no, we've got a little bit more time actually.

George Smith

You want to ask the question, I see you've prepared...

Eagar

He's got characterization in here!

George Smith

That looks like a damn good book.

Eagar

It actually does.

Arne

There's actually very little characterization in it.

Eagar

Well it says optical microscope, that's Sorby. He doesn't have x-rays in here, but he does earlier have Braggs...

George Smith

Yes. [Grines] are featured in chapter 30.

Eagar

No. R.W. Cahn is one of the great guys in Materials Engineering and so

forth. And he's also a colleague of Morris Cohen's.

George Smith

[To Arne] Have you interviewed Morris Cohen? [To Tom} I want to thank you.

Arne

No. This is actually something I'd like to do.

Eagar

Well you better do it soon! I don't know really how much of this does go on to other schools. We produce 15 or 20%. Actually we produce 15% of all the doctorates, or we did, in the country, so I would expect that some of this probably gets carried over.

Arne

I'm sure it does. That's the way of the world.

George Smith

Well, MIT...you should understand, I'm on the visiting committee at Caltech, MIT's is very very different from other schools. The department heads have incredible power.

Arne

But political structures have an impact wherever they are.

George Smith

Fair enough. David's the one who made me realize that book existed. You'll see my pad. It has [Jed Buchwald's] name on it.

Eagar

Ok, well good to see you again.

George Smith

It's good to see you, and I may get you involved in this [world of steel]. The problem is to figure out why the step is vital.

Eagar

Oh, ok. [?]

George Smith

Yeah, but they're doing...getting 40,000 psi...

[tape3]

Eagar

So what are your questions?

Arne

Well, one of the things that I would like to get at is the impact of the processing on the field as a whole. Because while I tend to get stories coming from the other end saying about the impact of the development of the theories and so on. We've talked a lot now about the institutional inertia perhaps, something like that? But a lot of the story of the field of materials research is really driven by market, by processing, and is a demand rather than a supply story.

Eagar

That's definitely true industrially. There's no question about that. In terms of the field being defined among the academics as having processing, as much as he and I don't get along now because of the way he treated me as department head, Mert Flemings really is the guy who has carried the banner. You have to understand, Flemings was not the brightest of students. He was ok, but he started out, and there was a guy named Taylor (I'd say Frederick, but it wasn't Frederick Taylor). Anyway, Professor Taylor in the Materials Department. [Flemmings] and [Dave Bergoni]; and [Dave Bergoni's] and interesting person, you should talk to Dave. You've have probably come across his name. He was President of Case Western Reserve. Actually, his MIT number is [...Eagar writes number down...] (Because Dave is a very thoughtful guy) I'm pretty sure that's it. 4252. Dave and Mert Flemings and another guy [Ed Hucky], were three roommates as graduate students. And they all worked for Taylor, who had the foundry. And Taylor had come from industry and had fantastic industrial contacts in the foundry industry. MIT had a foundry, they could melt several tons of steel in their foundry. It was on the top floor of building 35. They had a weld lab at one end, which was [Mel Adams], and they had [Taylor's] lab at the other end. And Taylor had a consulting business. It was quite a lucrative business, and it turns out that [Dave], well they all graduated, and [Hucky] went to the University of Wisconsin at Madison I think, as a faculty member, Mert went off to [ABEX] and then came back two years later, working with [Taylor]. [Dave Rigoni] went off to, I can't remember where he started out, but he was dean at Dartmouth, he was Provost at Michigan, he was actually the guy who gave Chuck Vest his tenure at Michigan. When Chuck Vest was offered the job at MIT, he called up Dave to find out whether he should take it. I've heard this from both Chuck Vest and Dave. And Dave says, "Well there's two reasons why you might consider it. One, is they don't have a medical school, and two they don't have a football team." Anyway, Dave ended up as President of Case Western Reserve University, and then in his mid-fifties, he actually sort of got kicked out, I don't know the details at Case Western, but he sort of got pushed aside. He came back to his friend Mert, and he got an appointment as a senior lecturer, and he wrote the

two undergraduate books in thermodynamics, Dave and I used to teach thermodynamics together to sophomores. And anyway, Dave will know some of these stories and stuff, back from the fifties, and he's a very thoughtful guy. He's not a typical metallurgist, he has been at manager at the universities, and I actually have a lot of respect for Dave. So he would teach his thermo in the morning, and he'd do venture capital in the afternoon. He's now seventy years old, and he still does his venture capital, but he no longer teaches for the last four or five years, but I think he probably still has a phone number here at MIT. You can leave a message and it will actually probably give you his venture capital firm in Wellesley. But Dave's a very interesting guy, but in any case, Flemings, the three of them used to go, these guys used to go out and do the consulting for Taylor's business. ... Mert was brought, Mert was kind of working along the old Taylor-ism stuff, as a young assistant professor working for... Taylor who was one of the dons, one of the bigger dons, but you know Mert was the junior faculty grunt working for him. And when Taylor died, Taylor had done very industrialized, very applied research. The dean at the time, Gordon Brown, a New Zealander, a prim and proper New Zealander supposedly, he was the guy who took MIT into engineering science in the late 50's. Around the time of Sputnik and everything else. And basically, Mert was brought in and told by John [Chipman], who was department head, and sort of like [Moravian], said, "If you stay in the foundry business, you won't get tenure. You're probably not going to get tenure around here because we don't want this kind of applied engineering science." Mert actually has told me this story, he went home, and he said that night, he was basically told he wasn't going to get tenure because he was in too applied of a field. He's told me the story. He went home and he that night decided to get rid of the huge furnace, you know, and just turn the whole thing into solidification science overnight. Because otherwise he wasn't going to get tenure. So he basically threw out the Taylor empirical stuff, and he rebuilt it, and he really built up the field of solidification science, which is, well he followed along some things that [Guy Rudder] and [Chalmers] Rudder and Chalmers were up in Canada. Anyway, he kind of followed along some of the stuff that they had started, but he really, with the quality of MIT students, really took it off. He did tremendous things for the field. And certainly deserved to get tenure and he did get tenure because he switched to engineering science. But, then you come

along with COSMAT, and Morris Cohen pushing that we're going to have Materials Science and Engineering as opposed to just metallurgy. We need to look broadly at the field and all the classes of materials, although concrete was excluded. I never heard anyone bring up concrete and I think they always said, "Well it's a commodity or something." They didn't think of steel as a commodity, but they knew that plastics was a growing business.

Arne

Bernie actually had a feasible argument for this kind of thing, he says that if you have something where very little capital and research goes into it, a cheap material, like concrete, then it's not a topic in Materials Research. It has to be...

Eagar

Well.

Arne

an expensive material, one that you sink stuff into in order...

Eagar

I have a plot that I've used, actually, I stole it from [Jack Westboro]. It's an internal GE report. And it shows this on a log scale. The log of tons used, [drawing] yeah the log of tons versus the log of price. Ok?

Arne

Of any material?

Eagar

Structural materials.

Arne

Ok.

Eagar

Structural materials. And you have diamonds over here, at about a million dollars a ton. It might have been \$100,000 a ton back then. And you have stone up here, crushed stone, at like ten to the thirteenth tons, I mean something incredible. You have concrete, it's not tons it's pounds. And you have steel. Ok? Steel is like ten to the eleventh, I think stone is ten to the thirteenth, and concrete's like ten to the twelfth or something. And it goes all the way down here, and you have, you know, there is a wonderful correlation in this very narrow band, I can get this for you if you want, but it if you look at the...

Arne

What time is this?

Eagar

This is 1962 that he actually did it, and if you look at iso-market size lines, and they look like this, so this is steeper than the isomarket size. Which means that if it's a, if you can drop the price of the material by a factor of 2, you should increase the usage by a factor of four. Ok? Based on the slope here. Which means that your market doubles. So if you actually, and I've used this argument a number of times, that we really should be pushing for reducing costs and processing of materials, rather than looking at more elaborate materials that are always going to be boutique materials that no one uses much. In any case, getting back to Flemings, and the COSMAT stuff, basically that's when Flemings started politicking with Morris Cohen that he should do processing-structure-properties. Because in the sixties it had always been the

structure of properties. And Flemings started saying, because he was in the processing side of the department, he was one of the only guys! He was the one arguing that we should add fluid flow, he taught a heat and fluid flow course. He is basically trying to carve out a niche for himself in a department that he didn't quite fit in. Ok? And he did! Very effectively. And he convinced a bunch of the rest of the people. Now the people in industry loved it, because they knew processing was where they made their money. So you're right about the fact that industry knew it, but if you look at academia, I think I have to give credit to Mert Flemings, for really being the guy who led the country; you notice in here actually, he's quoted about as many times as anybody.

Arne

We could talk about it then in a similar way to the way we talked about physics sort of seeping in with a time delay, that the market is sort of seeping into the academic world with a time delay.

Eagar

Yes.

Arne

Does that make sense?

Eagar

Yes. There's, the problem is the time delay, well there is feedback between the two, and it goes both ways. Ideas and knowledge seep into industry with a time delay. And in fact, this idea that the field is broader than just metals is something that came from academia, and has seeped back into industry with a time delay. Industry now accepts it, but in the seventies and eighties, they were fighting it tooth and nail. They were very upset, and threatening on withdrawing their support. It turns out they were going to withdraw their support because the profitability

was going down, ok? But they used it, and they come in and say, "Well we're not going to support you anymore like we used to, because you're not supporting us like you used to." And so in a sense, the whole broadening of the field to look at other materials was kind of a major thing. Another person you might want to talk to is Harry Gatos.

Arne

Yes.

Eagar

You know Harry?

Arne

Yes, well I don't know him but...

Eagar

But Harry was really the person who brought electronic materials into the department. And he had been an undergraduate in the department, and I think he worked in mining. I don't remember exactly what he worked in as a student. He went out to Lincoln Lab and he became head of the whole solid-state division within a few years. Of course they were growing semiconductors. Harry became "Mr. Gallium Arsenide," you know, before it was popular. And in the mid-sixties, he was offered a full professorship in the department, and he brought a million dollars worth of equipment, which was a lot of equipment, with him from Lincoln Lab. And basically set up his own little fiefdom, he was another fiefdom, all of the sudden we had a new fiefdom in electronic materials they've never had before. And Gus Whit, who could be an interesting person to interview, to show you what they did to junior faculty, I don't know if you're interested in the internal politics. Gus was a physical chemist, he had been hired to work with [Phil DeBruin]. [Phil DeBruin] was a Dutchman who was a great man at [froth flotation], which is a

way of recovering a good part of ores from the ores. DeBruin was kind of the last man standing in mining. And in 1962, they had a vote on whether to continue mining in the department. And with one dissenting vote, John [Elliot], the department decided to drop mining. Well [Phil DeBruin] was still at MIT, and he became head of the graduate committee and graduate admissions and stuff. Gus Whit had been hired originally to come and work for Phil DeBruin. Gus was a surface chemist, a physical chemist from the University of Innsbruck or somewhere in Austria. And when he got here, he went in to meet with Tom King. 1962 was when John [Chipman] had stepped down, in '65 and Tom King became the department head, and Tom King called Gus in when he first came to MIT, and Gus thought he was coming over to work and be a gopher for Phil DeBruin in mining engineering, and Gus says, "We no longer have mining engineering in the department, we've eliminated it. You should go over and work with Harry Gatos in semiconductors." Ok? And Gus says, "What do I do?" Well, he went over and worked with Harry Gatos in semiconductors. And the two of them from the mid-60s through 1990 were the powerhouse, basically, in electronic materials. Until we finally started hiring some more in the eighties, and then the two of them retired. Well Gus is actually just retiring this year, but Gus really hadn't done anything for the last twelve years in terms of real science or research. But anyway, that's how they brought in electronic materials. They imported Harry Gatos. And that was one of the first electronic materials groups in the country.

Arne

How do you see the impact of national politics and funding, lets say DARPA, and the NSF grants?

Eagar

DARPA was very important in setting up the Materials Science Centers around the country. That was a big move, there's no question about it, in the early 60's, a tremendous boost to the whole field of Materials Science.

Arne

Was there a field before that?

Eagar

Well actually, it's not clear that there was, I mean you really have to talk to Morris Cohen or some other people of that genre. I was in elementary school, I was in grade school, or high school or something during that period. But certainly, that was one of the things that switched, helped push it from, helped push the academics from thinking of it as always "metallurgy," because certainly the Materials Science centers were not set up just be metallurgy. The military, you know, knew they needed all types of materials and they wanted to support all types of materials. NSF only inherited it because of the...

Arne

Mansfield?

Eagar

They inherited it because of Mansfield. And they continued to support it although they do a lot of sabre rattling, but it's still kind of a central thing to to the whole thing. So I think DARPA's deciding to fund the material science centers was or it might be that it was the beginning, but I can't tell you though.

Arne

Yes. No it was. Then in your time, what's the, has it shifted...

Eagar

The funding?

Arne

boundaries around? Is it?

Eagar

Well...

Arne

That must've been...

Eagar

Yes. It has, I mean it used to be. After Sputnik it was mostly government funding. I mean I remember my old thesis advisor [Bob Rose] had all kinds of money in the sixties. In fact he used to tell us the story, the joke was "While you're up, get me a grant!" All you had to do was kind of... Well actually, to tell you a little more of the story that I had heard, because of the Manhattan Project, in World War II, you had John [Chipman] working on a lot of the physical chemistry. He was working on trying to create crucibles for uranium and stuff. There was no material, Uranium is too reactive, and so they were working on the physical chemistry of what they could use for crucibles to melt the stuff.

And Morris Cohen was working on, well somehow, I don't remember what he was doing. But there were projects going on for the Manhattan Project. And I remember John Wulff telling the story, he was on the outside, he was one of these processing guys that they looked down on. But he knew uranium was very important, but he didn't know exactly why. And he had been studying trace elements in oils around the world. And he dropped in at a cocktail party and said, "You know there's a lot of uranium in the oils in the Balkans," or something like this, and the next morning there were two secret service agents in his office asking him what he knew about uranium. That type of thing. But anyway, what happened is because of their help on the Manhattan Project, Norton in ceramics, Cohen in physical metallurgy, and I think someone else.

There were two or three grants that came from the Atomic Energy Commission that basically just funded these guys for the rest of their careers. I mean the Atomic Energy Commission became the Department of Energy and stuff. And it turns out I remember in the early eighties, Kingery and [Kobel] still had Norton's old grant. That was the last one remaining of this kind of gravy train of funding that you get as the equivalent of one and a half million dollars or two million dollars today. They would just get it for just kind of telling people in Washington, "This is how much money we need next year." It was sort of a reward for what they had done on the Manhattan Project. After Sputnik, it got to be very easy for even junior faculty to get funding. I remember when I started on the faculty in 1976, 25% of all NSF grants got funded. And if you were from a place like MIT, you probably had a 50% to 60% chance, probability of getting a grant funded. Today, or in the mid-nineties, it dropped to like 5% of NSF grants are funded. And if you're from MIT, it probably cuts your odds by half, because the other schools out there. What happened is in the seventies, a bunch of other schools found they couldn't compete on quality of proposals, so they started competing in Congress. And then at NSF, they started competing by sending more of their faculty off to be rotaters.

Arne

This is in the seventies?

Eagar

This is in the seventies and eighties. And a lot of these people basically just had it in for the schools that have been on these gravy trains, ok? I mean in the late-eighties we lost the National Magnet Lab to Florida, and that was a pure buyout by the state of Florida. Alright? And MIT was judged to have the best proposal, but the politics at the NSF were such that they decided they couldn't turn down the money from Florida, plus they wanted to send the message to these elite schools, that they weren't going to continue to just get things just because they had better proposals.

Arne

I talked to Dale Corson at Cornell, and he spent half of his life in D.C. He said this is the way it always worked, there's no way you can run a department or a school without taking D.C. seriously.

Eagar

Well, yes and no, I mean that's one way to do it. And in a sense you could say the guys who worked on the Manhattan Project and made their contacts in D.C. were using their "old boy" connections to get their grants. But that's not what happened to me. Ok? I was hired on as an assistant professor, I was still working at Bethlehem Steel. I talked to some professors on the phone and they said, "Well you ought to go talk to so and so in Washington." And I called up some of these guys. One of them was Bruce MacDonald at ONR. And Bruce actually was willing to let me come down from Pennsylvania to talk to him before I even started at MIT. And he told me what ONR was interested in. Now Bruce happened to be an old MIT grad, he was one of Morris Cohen's students. And he was in charge of the welding program, and he wanted, you know, and I was a welding engineer at Bethlehem, and that's how I got to know Bruce. I then talked to my old house tutor, who actually was a welding, head of the welding group at Union Carbide. They were doing a project on submerged dark welding of titanium, they didn't see a market for it, but they did have some useful results for the Navy, and they were not going to ask for a renewal. So Dave says, "Well why don't you write to the Navy, maybe they're interested in this." And I did, that was my first contract at ONR. I found out what the Navy was interested in by working connections, and that's why it helps to be at an elite school, because you have some of the connections. Bruce MacDonald was an old MIT person, and so he wanted to help if he could, but Bruce is an honest enough guy that if you didn't write a decent enough proposal he wasn't going to help you. But I found that when Dave Hill told me that, you know, the Navy is interested in this, and we're not interested in pursuing it as a company anymore, why don't you write a proposal? That was my first proposal, and the first one funded, and since then, I did a good job for the Navy, and I'd serve on committees

for them, or I'd spend part of my summer working with their engineers, but not politicking. Ok? I've continued to be funded by ONR, but I have not had to do politicking. I have been continuously funded since 1977, 25 years by ONR, based on the quality of my work. Ok? Now at NSF, I wrote a proposal to this guy [Bob Ranig] Bob Ranig was a very ecumenical guy, he was from UPenn. I called him up and said I'd like to come and meet with you, because that's what Tom King and Bob Rose and other people, the faculty of MIT said, he was head of materials science then and Bob says, "You don't need to come see me. Just write me a proposal. Tell me what it is you want to do, how you're going to do it, and if you're successful, so what?" Ok? That's been the outline of my proposals ever since. Those three things. I had continuous NSF funding until two years ago. I got kicked out for what I consider one of my better proposals because they kicked me over to the engineering side, not the materials side, where they do these panel reviews. And those things are vicious. You have a bunch of people from these other third-rate schools who come in and they don't even know what they're talking about and they say, "Oh this one's from Stanford, this one's from MIT, we're going to kill it because we hate these guys. They always get the money and we don't." Ok? It's a terrible system, and I lost my NSF because of that, but I had continuous funding from NSF. Now that's partly, not necessarily because of the quality of my work, in terms of NSF peer review, but it was because when Bob Ranig, and when he left, Bruce MacDonald went over there. They knew I did engineering applied work, but did good science behind it, I mean I had good fundamental science behind it. And they liked my type of work. Bob Ranig told me once when I was a young un-tenured professor, I said, "Bob, I understand you had, you wanted, you had a lot of proposals on Fermi surfaces, and that's the type of work you do." And he said, "Tom, last year I had 33 proposals on Fermi surfaces, and I funded one of them. I had one proposal on welding, yours, and I funded it." Ok? So there were all these materials scientists trying to be pseudo-physicists, and Bob didn't care about that. So it was really Bob Ranig and then Bruce MacDonald who liked the quality of my work. I never even walked through NSF until the early 1990's. I never walked through the door of that building. And so I've had people tell me the same thing you were told at Cornell, and I say, I've said, "Not true." Ok? The department of energy, I ended up getting in, there was a committee that looked at what materials science the DOE

should be funding, and [Kent Mullen] who was a faculty member here, served on that committee, he might have even been chair of it. He was kind of a golden boy that had gotten in with the DOE group from the old AEC stuff. During the Kingery and Kobel ceramics stuff. So he was in tight with those people because of some of the "old boy" connections, and Kent comes back and he tells me, "Oh we said welding was a high priority item. So it'd be good to send the proposal in to DOE basic energy sciences. So I got together with [Joe Sekelly], and we sent one in and we got it funded. Now that was rocky for, well they funded us for six years and then they dropped us the year we won an award for our paper. Which I always thought was sort of interesting. And then a year later, there was another report at DOE, the Packard committee report, which basically says the national labs and the universities should get together and do joint research. [Ken Hansen] in Nuclear Engineering over here was on the board of Idaho National Lab. So they got together and they said, "Oh, well we ought to do some joint MIT/Idaho National lab thing." And they put together this big program. Dave Parks was part of it, and I was part of it, and all of the sudden it turned out to become eventually, for a while, my biggest contract for DOE. That stayed for about ten or twelve years, and finally I got refunded separate of that program, and I'm still funded by that program. I've had a six month hiatus one time, just because of their funding cycle. But I still never have walked into the DOE, Gaithersburg building. Ok? So you can say this but I mean yeah, I've worked closely with ONR, but they took a plier on me before, I did walk in that building and talk to Bruce MacDonald, because he's willing to take the time to talk to me, but in terms of all this other politicking? Now since then I now have another DOE contract that came because some guy from the University of Alaska came six or seven years ago and said, "What can MIT do to help the University of Alaska?" And so we set up this thing, and we actually have been going through Senator Stephens, who's head of the Senate Appropriations Committee and is from Alaska. And we've been able to get some funding, which now is my biggest contract, when it starts up again, and that's pure pork! Ok? If you want to call it that.

Arne

You can call it whatever you want.

Eagar

But that basically is pork. It's pork going to the University of Alaska and then they ship some to us. So it's not as if I've never played that game, but I didn't start playing it until like 1996, and I've been here for two decades before I started playing the game. So yes, there are the "old boy" connections in Washington, and those are probably the most common. But is it absolutely necessary? No. I guarantee that it's not absolutely necessary.

Arne

I mean Dale Corson went up to a very senior position within the university administration, probably in that kind of position it's more important.

Eagar

And frankly, MIT has not been playing that as well as they used to. Our senior guys, well Chuck Vest is down there, but you look at our Provost and other people... Nam Suh is down as Director of Engineering at NSF, but I can't think of anything, and that was more than ten years ago. I can't think of anybody else. We've had Millie Dresselhaus who was number two at the Department of Energy, she was head of the energy sciences, but she only did it for a year. [Ernie Monitz] was down there in physics and energy sciences, but I can't think of anybody else at NSF. And we haven't really done rotations through ONR, other universities have done that, but MIT hasn't, MIT faculty haven't been willing to do that. It's not lucrative enough.

Arne

Hasn't there been a shift in funding in some way, government funding? Has it become more and more private?

Eagar

Well, and the reason for that is there used to be a 50% chance of getting an ONR, or NSF contract, if you sent it in from MIT, but now you have a 2% chance. Ok, less than average. So what happens? You go for industrial funding. Well, there's been more and more industrial funding as industry has dropped off its own in-house labs, so there's been more funding. Actually, I've always said, "Well if industry decides to go fund people, MIT will do just fine, because they're going to look for the best places. And we get the best students." So they're going to come and fund us and we have more industrial funding than anybody else by a factor of two. And that's true. We no longer can get the government funding like we used to, because we don't have the inside tracks, and in fact being from MIT is now more of a detriment than a plus in many cases. Sometimes it's political, but sometimes it is merit based. So there's not...I'm going to have to go here and catch my flight.

Arne

How much time do we have left?

Eagar

Another 5 minutes.

Arne

Let's just make sure I ask this question. You said before that for religious reasons you couldn't use the word proud, were you being flippant or?

Eagar

No. I'm actually a Mormon, ok? I'm a Latter Day Saint. And pride is a sin. Now most Mormons don't do this, but I actually have tried to expunge the word proud from my vocabulary. Now actually when I used

it, I used it in terms of other people being proud, but in terms of saying "I'm proud," I actually have done pretty well, I'll now say I'm pleased. I don't say I'm proud of my children, I am pleased that my children have done well or whatever. It's just more of a psychological thing than anything else. But yeah, it actually, it is something I've done. It's not typical Mormonism but it is more of a philosophy that you're not supposed to be proud and arrogant.

Arne

The MRS, the Materials Research Society, is a different kettle of fish from the MIT department.

Eagar

Yeah, but Harry Gatos was the founder.

Arne

Harry Gatos was one of the big guys, but in my opinion it would be Rustum Roy...

Eagar

Well there were a few others but..

Arne

Rustum Roy always claims to be the...

Eagar

There's an arrogant guy. Rusty's fine, but Rusty claims lots of things that aren't necessarily true. Rusty was one of, he got it together, but it was really Harry Gatos', if you just look at what they were doing originally,

it was Harry Gatos as the brainchild. He was the loner, he was this electronic materials person among all these other people who didn't even know what electronic materials were. He knew the people at Bell Labs who were decent physicists and stuff, and he decided that he ought to do something. And he got a few people, like Rusty and a few others, but it was the group of them that did it, but the guy who really was the brainchild behind it was Harry.

Arne

Were you a member of the MRS when they started?

Eagar

No. Because they were a bunch of scientists, physicists, and I'm on the far engineering side of it. I didn't join it for years.

Arne

So in the 80s you also didn't join them? Did you go to their meetings?

Eagar

I've never been registered at one of their meetings, I've been to some of them when they're over here. I've been to some of them when I've been asked to talk, but I, usually it's the Boston meeting, and I just kind of slip in without registering, that way I don't have to pay the registration fee. I am a member now, and I think I joined in the late-80s just because they were growing and it was important to kind of read the things, and I edited one of their MRS bulletin things in the early eighties. But it's the same type of thing. I've never been part of the Center for Materials Science and Engineering, I have never put in a proposal to them. Why? Because I've been able to get plenty of funding on my own, but they gave it out in smaller lumps, and it was just a bigger pain. I mean I can get better funding, and again, I'm on the far engineering side, they were trying to sell to the NSF that they were this wonderful Materials Science

group, as half baked physicists, right? And I didn't fit that model, ok? Everybody thinks of me as the industrial guy, but it turns out it's sort of funny to me because I've had relatively little industrial funding. Ok? I've had mostly basic energy sciences, NSF, and ONR. Ok? And I've always basically had more fundamental science funding than all these other guys who claimed that they were the fundamental scientists.

Arne

But from my understanding of the MRS, it is that it's focus is actually much more towards the application end and the engineering side, but certainly...

Eagar

Well, from a physicist's point of view, that's true. From a materials science view they're into fundamental science.

Arne

Is that right?

Eagar

Well yeah, they're kind of middle of the spectrum. Ok? The physicists look at them as more applications oriented, but the materials scientists look at them as, oh, they're more fundamental. Because that's what, I mean there's a lot of support from industry for MRS, but, but even so, I think it depends on whether you're a physicist or whether you're really a materials scientist. You know, the spectrum I talked about before, where you have a physicist here,

Arne

Sort of the pure, applied...

Eagar

Pure applied, and you have engineering all the way over here, well Tom Eagar is over there, but The materials science is right here in the middle. Ok? The physicists look at materials science's applications. Materials Science and Engineering is really here, and this is Materials Engineering, and this is Materials Science.

Arne

So the MRS is not really your place.

Eagar

No. The MRS is primarily, well, actually, I probably ought to, they're probably more central than Materials Science and Engineering.

Arne

I should have asked you before with the Mormon question. Do you have any, do you do your science differently because you're Mormon?

Eagar

No.

Arne

It has no connection? Two different worlds.

Eagar

I think I do it more objectively. Because, well more honestly, and I don't know if that's necessarily because I'm a Mormon, although it doesn't

hurt. But I've always had a problem with these people that say, "Oh, well Buckytubes are wonderful." Ok, I've written all kinds of articles about people running off on these bandwagons of materials. Back in the 80's when, again, when people said ceramics were the future of everything, just like George doesn't believe it because of the fracture toughness, I never believed it, and I used to write articles that said this is, you know, that the ceramists don't know what they are talking about. They had never discovered fracture toughness. And that actually happens to deal more with my honesty, ok? Now, and the other thing I think is I manage my lab quite differently. The reason I've got nine best paper awards, whereas most of my colleagues have zip, ok? And I have lots of other types of honors and stuff, is not because I get along well with my faculty colleagues. I mean my department head now, Subra Suresh, is a politicker. I mean he's the fellow of this society, a fellow of that society, a fellow of that society. I think he has one best paper award. Ok? He's actually a very distinguished person and very well thought of, but I have nine best paper awards, and he has one. I'm an honary member or a fellow of two societies, and he's an honorary member of seven or eight! Ok? He goes to the meetings and schmoozes. I don't go to any of the meetings and schmooze, because I'm not interested in that. I lead my department, I lead my research group, I don't manage it. And there's a distinct difference, and a lot of that comes from my leadership and management training as a member of the Mormon Church. Ok? I'm a Bishop for the church right now, as far as that goes, but... I'm here to educate the students to learn to be professionals. SO I have to define a problem, and let them flounder for six months, or sometimes more. and if they flounder completely, I have to be able, you know, in a few weeks, to completely repair the damage and get them a thesis and get them out. Which has occured a couple of times. But in general, I'm there to give them a lot of flexibility. I provide the resources, the research money, and the resources, and I critique what they do, but I actually let them do it. And I actually sometimes, with some people I end up forcing them to work on one of their weaknesses and other people I let them work on their strengths, and it really depends on where I think they're going to grow and develop the most. And I think actually that is part of my religious upbringing. It is that the important thing is developing the person, and I don't really care about the research results. And I've actually, a few say, "Oh well," and then other faculty say "Well I

proposed this and then they have a timeline of what they're going to do." And that's what they try to do. Once I get the money, other than the general topic area, I don't care what I do. I let the student go off and do whatever they want. And the thing is, when you have bright students at MIT, that's the way to manage them. You lead them. You don't manage them. And they will produce much better things than I could ever produce. And they have! And that's why, you know, I have these nine best paper awards. Because they are bright students, and they actually, I've had a number of them come in and they're used to being told what to do. I mean John Elliot used to bring students in once a week and he would go over their lab notebook line by line. He would tell them how to clip the ends of the thermocouples, ok? If a student doesn't come see me for six months, I might say, you know, ask my secretary to get an appointment, just tell them I want to see them and find out how they're doing. But usually I will walk through the lab or a luncheon seminar or something, and we'll talk. But I'll let them go for six months and I'll never bug them about what they doing. They know what their problem is, I've told them, you know, and they have to get back to me. And at first, some of them figure this out very quickly by talking to their older colleagues. Some of them don't learn it for six months, but after a while, all of the sudden, they learn it, and lo and behold, then they catch fire, because they realize the thesis isn't going to get done if they wait for me to do it. And I've had a number of them, I take them to lunch when they finish their doctorate, and one on one they ask me about things, and I've had a number of them say, "You ought to push the students harder." And I said, "You didn't feel a lot of pressure to finish your thesis?" And they said, "Oh I felt a lot of pressure." I said, "But it was self-motivated." "Yeah. Because you weren't pushing me." I said, "Yeah, well don't you think you actually felt more pressure than if I had been pushing you?" "Yeah!" Ok? "And so you learned to do it yourself, right?" And that actually comes from my religious background of the progression of the individual is more important than the task.

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