CHEMICAL HERITAGE FOUNDATION

LOUIS A. GIRIFALCO

Transcript of an Interview
Conducted by

Hyungsub Choi

at

University of Pennsylvania
Philadelphia, Pennsylvania

on

10 June 2008

(With Subsequent Corrections and Additions)

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Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
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LOUIS A. GIRIFALCO

1928	Born in New York City, New York, on 3 July

Education

1950	B.S., chemistry, Rutgers University
1952	M.S., applied science, University of Cincinnati
1954	Ph.D., applied science, University of Cincinnati

Professional Experience

	E. I. du Pont de Nemours & Company
1954-1955	Research Chemist
	Lewis Research Center, National Aeronautics and Space Administration
1955-1959	Solid State Physicist, Solid State Physics Section
1959-1961	Head, Solid State Physics Section
	University of Pennsylvania
1961-1965	Associate Professor of Metallurgical Engineering
1965-1982	Professor of Metallurgy and Materials Science
1967-1969	Director, Laboratory for Research on the Structure of Matter
1972-1974	Chair, Department of Metallurgy and Materials Science
1974-1979	Associate Dean, School of Engineering and Applied Science
1979-1981	Vice Provost for Research
1981-1982	Acting Provost
1982-present	University Professor of Materials Science

Honors

1996	D.Sc (Honoris Causa) Hahnemann University

ABSTRACT

Louis A. Girifalco begins the interview by describing his parents' support of his decision to study chemistry; he also discusses his undergraduate and graduate education. Studying applied science at the University of Cincinnati, Girifalco did his Ph.D. research on the adhesion of ice to surfaces. The surface science thesis research naturally evolved into solid state physics when Girifalco began work for the National Advisory Committee for Aeronautics, which eventually became the National Aeronautics and Space Administration. During his career Girifalco met Robert Maddin, which ultimately led an offer of a faculty position for Girifalco at the University of Pennsylvania.

At Penn, Girifalco worked in the metallurgical engineering department and reflected upon the creation of the Laboratory for Research on the Structure of Matter (LSRM), as well as the funding process within LRSM. Fundamentally an interdisciplinary research institute, Girifalco spent time as director of LRSM and discussed his views on the evolution of the academic science research system and on the Nano/Bio Interface Center and other current interdisciplinary research institutes.

INTERVIEWERS

Hyungsub Choi is the manager for Electronics, Innovation, and Emerging Technology programs at CHF. Choi earned a Ph.D. from the Johns Hopkins University in the history of science and technology. He earned an M.S. in history of technology at Georgia Institute of Technology and a B.S. in engineering from Seoul National University. Choi took over the center's electronic materials program in November 2006. He has published extensively on such subjects as the history of electronic manufacturing in post–World War II Japan, RCA's transistor production, and solid-state innovations.

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INTERVIEWER: **Hyungsub Choi**

LOCATION: **University of Pennsylvania**

DATE: **10 June 2008**

CHOI: So, as I said, we would like to begin at the very beginning, so could you tell us the basic information about yourself, when and where you were born, and onward.

GIRIFALCO: Okay. Well, my birthday's there. It's July 3, 1928, in New York. I left when I was very young.

CHOI: In the city.

GIRIFALCO: My parents moved out of New York City.

CHOI: Uh-huh.

GIRIFALCO: Ultimately settled in central [New] Jersey, went to school at Rutgers [University], undergraduate work.

CHOI: And I see that you majored in chemistry.

GIRIFALCO: I did. I majored in chemistry. I had decided that I wanted to be a scientist when I was about twelve years old because I had read a number of popularizations, scientific popularizations. They were very exciting, and before going to college I had some jobs that were interesting. I worked for Ortho [Research Foundation in New Jersey], which at that time was a very small outfit, and I was fortunate enough to be there when [William] Oroshnik, who synthesized Vitamin A, was there. [Philip] Levine, who worked on the Rh factor, was also there.

The summer before I went to school I worked for Merck in a pilot plant for antibiotics. Streptomycin had been discovered by [Selman Abraham] Waksman at that time and Merck was trying to produce it. I majored in chemistry, and after leaving there I was looking for something to do. By the way, I knew very little about colleges or departments or science, or any of that stuff, because I was the first one to go to college. Neither my mother nor father did.

My father was born in Calabria [Italy] and my mother was born in New York, but I was the first one to go on to school, so I knew nothing about the system. The first thing I did was to take a fellowship in the Rutgers Agriculture School in their food technology department, and I was working on the biochemistry of beef. I got disenchanted with that after a year or so because there wasn't enough mathematics or molecular physics in it, so I switched over to chemical physics, moving to the University of Cincinnati.

Of course, I knew nothing at all about the schools. I was just lucky to go into the University of Cincinnati because at that time, they had a truly outstanding faculty—so many people from government projects like Los Alamos [National Laboratory] were looking for university jobs, so I even took electrodynamics from [Boris] Podolsky and people of his caliber in other fields. That Cincinnati thing lasted for about ten years, but I was there at the right time.

The idea of interdisciplinarity was okay at Cincinnati, and that's where I first learned of it because I was in the department called the applied science department, whose mission was to bring together different disciplines to focus on projects. This was even before ARPA [Advanced Research Projects Agency]. Unfortunately, they had a strange idea of doing it. Their idea was to make sure that you got everything in math, physics and chemistry that the mathematicians, physicists and chemists got and you had to take three qualifiers, so I don't have much patience with students today that say they're overworked. [laughter]

So I left Cincinnati and went to work for DuPont. I only stayed there eight or nine months because I looked around and I didn't want to do what they were doing. I was looking at people being there for five and ten years and I didn't want to be like that, so I went to work for NACA [National Advisory Committee for Aeronautics], from which I had previous job offer, at the Lewis Flight Propulsion Laboratory in Cleveland [Ohio]. While I was there, NACA was converted to NASA [National Aeronautics and Space Administration].

I was in the solid state physics group there, and did a lot of research on diffusion and defects in crystals. I became head of solid state physics section. In 1961, I got a call from [the University of] Pennsylvania asking me if I would want to come as associate professor to this department. I said yes and I've been here ever since. What else can I tell you?

CHOI: That's a career in a nutshell. [laughter] So a lot of the people who choose chemistry like to tell a particular episode that they had in their youth about how they first got interested in chemistry or science in general.

GIRIFALCO: Well, the first thing was a chemistry set. Of course, I was only twelve, didn't know what to do with it, but I was very much interested in what things were made of, and I had read books like *The New World of Chemical Discovery*. Who is the author of that? I don't remember, but he wrote two books. One was *The New World of Chemical Discovery*; the other one was *The New World of Physical Discovery*.

And I created a basement lab in which I did things like make esters and dyes and extracted stuff from mint plants and a lot of youthful chemical experiments, so I did a lot of organic chemistry in my basement, and it seemed very natural. Besides, I didn't know much. For example, I thought I had to major in chemical engineering. It came as a surprise to me when I found out I could actually major in just chemistry. I didn't have to be an engineer, so I was very naïve.

But it was a great choice—a great choice—because in physical chemistry you learn a great many things that physicists never really master. To this day, I believe that chemists and physical chemists learn thermodynamics in a much better way than physicists do, because in chemistry you have to apply it to a lot of real materials with the phase rule, chemical reactions and so on, but that's a prejudice. I don't know if there was an individual event, but it was a natural evolution out of my basement laboratory.

CHOI: Were your parents a factor in any way? What did they do?

GIRIFALCO: Oh, they were great. You know, I was their shining bright boy and they let me go ahead sometimes, not realizing that it was dangerous. I remember once that I had a terrible time because I had liberated bromine, and it vaporized and filled the house. Thank God they were away. I spent all afternoon trying to fan bromine out of the windows. [laughter] I survived all that, but that was one of my unhappy memories.

CHOI: Okay, so I'm quite interested in this department of applied science that you moved into in Cincinnati.

GIRIFALCO: It was a fascinating department.

CHOI: So when did this department begin?

GIRIFALCO: I don't know. Let's see. I graduated in 1950. I arrived there in 1951, I guess, and I left in 1954. Yes, 1951 to 1954. I got a master's and a Ph.D. there, in three years.

CHOI: So it seems that—it looks like a Harvard-type model that they're trying to get at.

GIRIFALCO: Well, of course, but I didn't even know they were trying to break new ground. All I knew was I had to do this work to get the degree, and later on, I realized what they were doing, but I also realized that they should have done it differently. Instead of creating a new curriculum, they simply combined the curricula of other departments.

CHOI: So tell me a bit more about the overall structure of the department.

GIRIFALCO: Of the department?

CHOI: Yes, while you were there.

GIRIFALCO: Well, they had a director. I don't remember if the official name was the school of applied science or the department of applied science. That, I just don't recall, but it was a sizable place. I mean there were eight or ten faculty members and about a hundred students, and I thought it was a wonderful place because I was doing research.

But I went back many years later and it was a terrible building. It was old, [laughter] poorly maintained, and at one time it was spilling over, and it was so crowded that I moved all of my experiments into a room under the stadium. [laughter] But there's not much to tell except that again, the professors were very good.

CHOI: They were from various disciplines.

GIRIFALCO: Well, yes, they were from various disciplines, but they were all members of that department.

CHOI: Right.

GIRIFALCO: We had an organic chemist and we had a couple of physical chemists and a fellow who specialized in surfaces, but those are the ones I remember. I do certainly remember the director. His name was [Walter] Soller, a very energetic, powerful person.

CHOI: So the school or department of applied science was more oriented toward the chemical sciences than physics?

GIRIFALCO: Well, let me think. Well, more towards physical chemistry and chemical physics, but we had people that were doing—for example, one experiment I thought was fascinating, was measuring the adhesion of things to metals. They would put like a paint dot or something that would stick on a steel ball and spin it magnetically in a centrifuge and measure the velocity at which it flew off so they knew the force it took to get it off. There was a lot of physics involved in that.

My own work was on the adhesion of ice to surfaces because it was supported by the [United States] Air Force for deicing, so I did a lot of that. A lot of gas flow work going on, through powders and membranes. So it was not what I would traditionally expect in a physics department, but looking back on it there was a lot of physics.

CHOI: So tell me a bit more about your research.

GIRIFALCO: Mine?

CHOI: Yes, Ph.D.

GIRIFALCO: My Ph.D. research.

CHOI: Right.

GIRIFALCO: My Ph.D. research was on the adhesion of ice to surfaces. Actually, it was really just the interfacial physics of water and ice with surfaces, and the experiments that we did were to dump powders into water and measure the heat involved. That's the heat of weighing.

I was using a microcalorimeter that was built by a predecessor of mine before he got his master's degree, extremely sensitive with 126-junction thermocouple. What you did was put powder in a bulb and evacuate it so you had the powder in an empty, evacuated bulb. Put it under water, break the bulb, let water rush in, measure the heat, but the PV work in the bulb was important. You had to know the volume of the bulb to calculate that, and we found that the limiting factor in the experiment was the strain energy of the glass when you broke it. We had bulbs specially made so that they were uniform, but they weren't perfectly uniform, so the strain energy of the glass varied from one to another. These were calorimeters that could measure a micro degree difference. So I worked all that out, and did a number of experiments in which I pre-absorbed stearic acid on the surfaces. The stearic acid blocks the active sites, so with partial surface layer you get a partial heat and a measure of the surface energy distribution over the surfaces. I think the important thing that came out of that was I worked out a theory relating the interfacial tensions and surfaces to the surface tensions and energies, and that has stood the test of time and those results are still being used.

CHOI: And after your Ph.D., you quickly moved into solid state physics. How did that happen?

GIRIFALCO: Right. Well, again, that was a natural evolution. I was working with solids, solid surfaces, and about that time Fred Seitz had published a great big book, *Modern Theory of Solids*, which was the first book on solid state physics that I read. It was the only existing book on that. You know about that? Yes, and I was fascinated by that book, and somehow or other I got interested in defects starting from the surfaces, got the point defects. That led me to diffusion, and I continued studying solid state and that was it. It seemed like a natural thing.

CHOI: So your research at NASA was mostly on point defects.

GIRIFALCO: Point defects and diffusion, right. Both experimental and theoretical. I started—I don't know if I was the first, but one of the first, to do calculations on the structure around point defects in solids, for example on relaxation and using bad potentials. Eventually, I gave up because I knew that those potentials weren't good, but we got a lot of good information out of them.

And when I came here [Penn], it was obvious that diffusion was important for so many things that were being done. Since I had been interested in point defects, one of the things that led me to it was a set of experiments, and I'm not sure why I did those experiments, at NASA. We looked at void growth in silver crystals during plastic deformation. I think I got interested in that because I had heard that plastic deformation increased the number of point defects—I don't remember how I learned that—and so I thought it would be interesting to look at.

And I had a young lady, who was a collaborator, who did the hard work of measuring the sizes of the voids as a function of time, and it was obviously a diffusion problem. So I worked out the theory for how vacancies diffuse to a grain boundary and nucleate to form voids and the void growth. It fit the experiments pretty well, and it was really on the basis of that work that I was asked to come to Penn.

CHOI: So Bob [Robert] Maddin knew about your work from?

GIRIFALCO: The reason he knew about it was because I had organized a conference in Cleveland. One of the things about Cleveland was it was far away from other centers and I felt a little bit isolated, so I asked a fellow named Jay Denes, who was a Brookhaven [National

Laboratory] scientist, to be a consultant. I had met him at Gordon [Research] Conferences, and he came to be a consultant and we did a lot of work together, but I still felt that was not quite enough.

So I organized a conference on diffusion, which was a very hot topic at the time, and everybody came, and one of the people that came was Bob Maddin. He listened to my paper on the vacancies in oxides and silver, and he thought that was interesting and asked me to come and give a seminar at Penn on it. I came and gave a seminar, and sometime after that they called me. It's the way things usually happen.

CHOI: Right. Bob also worked on point defects, too, to some extent, didn't he?

GIRIFALCO: Bob Maddin had an interesting scientific history. He seemed to, in several areas, anticipate what the next big thing was going to be. He is not the big name in work hardening, but he wrote a few papers before anybody else was in it about that, okay? And it was important to do that.

He did the same thing with strain-enhanced diffusion. Before anybody else was worrying about the enhancement of diffusion by plastic strain, he did some experiments on it, and to this day we don't know if those experiments were correct or not, but it initiated a field. Then he did a high quench. It's the so-called splat cooling. He was, I think, the first one to take metal and cool it so fast that it didn't crystallize. It stayed as a glass, and he called it splat cooling because what he did was very interesting. He had a rotating metal wheel, copper I think it was, and then he had a bar which was pulled back like a spring, and on it a blob of molten metal. They would let go and the thing would shoot across the empty space, hit the wheel, splat out into a thin film and it was very rapidly quenched. That was really the beginning of the study of metallic glasses.

So other people became famous in these things because they stayed with them, but Bob also did several things. He was very busy building a department. You have no idea how much time that takes. Maybe you do. So I thought that was kind of intriguing, and people here were doing stuff for which diffusion was obviously very important, so I continued the work that I had started on diffusion at NASA.

CHOI: So what was the attraction of Penn for you to move over? Was there an option for you to stay on at NASA for a while?

GIRIFALCO: Yes. I had it made at NASA. I was the head of the solid state physics section. I didn't have to do anything but run research groups and everything was nice, but I had always wanted to teach. I thought teaching would be the most really great fun and, although I did have self-determination at NASA, it was still government and mission-oriented. One of the things I like about university is you that had a greater degree of control over your own work and doing what you wanted to do. I knew about Penn, and I liked Maddin. I came and met the people here. They were doing interesting stuff. Philadelphia [Pennsylvania] is a nice place, so I came.

CHOI: In 1961, you came to Philadelphia.

GIRIFALCO: Right.

CHOI: What was the state of the department at the time?

GIRIFALCO: It was really a nucleus department. Before I came, and some other people came with me at about the same time, I think it was about four or five people. I came in during the growth spurt that brought it up to about ten or twelve people. They brought in a chemical metallurgist named Geoffrey Belton, who was a world class fellow, and Irwin Parthe, a great crystallographer. We were all so young then. Of course, they brought in Charlie McMahon, who you may or may not know, who else did they bring in? Dave Gaskell, who was also a chemical metallurgist, but he ultimately left for Purdue [University].

CHOI: I think I studied one of his textbooks.

GIRIFALCO: Yes. Well, there was one textbook in many editions, and he moved out to Purdue. They brought in a fellow named Neal Durdaller, who ultimately died in a car accident; and Herb Herman, who ended up at – where did he end up? Some really good university in New York, I think. So I was at the leading edge of the great growth spurt. Doris [Kuhlmann-]Wilsdorf was here at the time. She was already quite famous.

CHOI: So I assume that the fact that Penn already had the corporate grant was a big attraction.

GIRIFALCO: Oh, sure, because I was guaranteed support from the word go.

CHOI: Right. So what was the kind of support that you were guaranteed?

GIRIFALCO: Beg your pardon?

CHOI: So what was the support that you were guaranteed?

GIRIFALCO: Oh, well, the LRSM [Laboratory for Research on the Structure of Matter] was supporting my research in diffusion. I didn't have to go for a grant.

CHOI: Right.

GIRIFALCO: It was here. LRSM had local grants. They gave out the money. The government would send them a block of money each year and they would locally give it out with an executive committee, and it was much better then than it is now for several reasons. One is the grant was enough to support a professor. A professor didn't have to go out and get five or six different grants to support a group. The second was that they saw a student through. Once they took on a student, they supported him until the end. Nowadays, students can get cut off any time because there's not enough money. The third was that there were adequate funds, and they provided a lot of great capital equipment. In fact when I was director of LRSM, at the end of the year, I used to get a phone call that says, "We have capital equipment money. Do you need any this year?" So this was unheard of. I had a director's emergency fund. If somebody was running out of money for something I could give it to him. Try that today!

CHOI: [laughter] So when you first moved in to the Penn metallurgical engineering department at the time, where was your office?

GIRIFALCO: In the Towne Building. Terrible office, it was old and the paint was peeling. Couldn't wait to get here [LRSM], but I had a wonderful time.

CHOI: So all the department faculty were here [LRSM]?

GIRIFALCO: All moved over here, yes.

CHOI: And I was quite interested in the fact that metallurgy was the only department that moved its entire department at Penn.

GIRIFALCO: Right. Well, there were several reasons for that, one of which is that chemistry and physics, of course, were much too big; and the other is that all of metallurgy was materials science, whereas in chemistry and physics only part of theirs was materials science. However, some people did move over as individuals.

We had some physicists and some chemists in this building. They usually kept an office also in their departments, but we had people here from chemistry and physics that had offices here and worked here and mingled, so there was a good deal of mixing. We had joint seminars all the time, of course, and while we were the only entire department that moved, other departments had faculty and laboratories here.

CHOI: So did the physics and chemistry faculty rotate from time to time or did they just stay on while –?

GIRIFALCO: It was pretty stable. You mean the ones that were in this building?

CHOI: Right.

GIRIFALCO: Yes, it was pretty stable, because when you choose an area of research you don't change it every year.

CHOI: Right, but my understanding is that individual faculty members had to submit a proposal to the executive committee of LRSM.

GIRIFALCO: That's correct.

CHOI: And they approve or not approve –

GIRIFALCO: That's correct.

CHOI: – that depending on the topic.

GIRIFALCO: Right.

CHOI: So if faculty member A was doing good work and then he submitted another proposal and that got denied, did he or she have to move?

GIRIFALCO: He wasn't forced to. I mean next year he might submit one that was okay, but the faculty kept pretty close touch with what was needed and what was going on because it was always a topic of discussion. The executive committee had a representative from each department on it, so the representative from our department, for example, would come back to departmental meetings and say, "This is the direction the LRSM is going. This is what physics is doing. This is what chemistry is doing, this is what these individuals are doing. That's for your information if you want to submit proposals."

CHOI: Maybe you're not the perfect person to answer this question since you came in at 1961, after things had been figured out to some extent and LRSM was already on its way.

GIRIFALCO: Say that again.

CHOI: I mean, the question that I had was, was there a substantial difference in the practice of doing research before and after LRSM came to be at Penn? For example, was there more collaboration among faculty members from different departments?

GIRIFALCO: The LRSM created collaboration in many ways where there had been none. Departments are very insular at universities usually. Maybe Penn was a little better than others, but I don't think a whole lot. [University of] Illinois was the only example I knew where there was a close collaboration between physics and metallurgy, but departments didn't know what was going on in other fields.

For example, physics departments and mathematics departments knew about dislocation theory even though they weren't using it or doing it. On a visit to Russia at one time, I found that the metallurgists did not know dislocation theory because they were so separated from math departments and physics departments. In the early days these were mathematical problems, and they were so separated from metallurgical engineering problems and so on. Nobody had told them about dislocation theory. They didn't even read each other's journals.

Nevertheless, the LRSM over a period of time created a system of collaboration that was much, much greater than had ever existed before at Penn. My opinion is that other universities would tell you a similar thing. The ones that we were most like was Cornell [University], and I had friends there, so I know that that was true for Cornell. From that point of view, the ARPA program was an enormous success. It was also very successful from a research point of view, but from the systemic point of view of how science was done, it was very successful.

CHOI: Would there be any way to measure that, in your view?

GIRIFALCO: Well, there is a way to measure it, but it requires too much work. You can look at all of the papers that are published and see who the authors are.

CHOI: Right, and see how they cross-fertilize.

GIRIFALCO: Right. I don't know if ARPA or the National Science Foundation has kept statistics on that, but it would be an interesting question to ask. Find the person who's responsible for these things now at the National Science Foundation and ask them if they have any knowledge or statistics. In fact, there's a guy in town you might want to call about this, Joe Bordogna.

CHOI: How do you spell his name?

GIRIFALCO: B-O-R-D-O-G-N-A. Okay, he's an electrical engineer, but for a number of years, he was deputy director of NSF. He just came back to Penn a few years ago. He was from our electrical engineering department. He was a dean here, and he would know if there is any measurement made of the degree of interdisciplinary. You could ask Pat for his number. She could track him down for you or you could look him up. You can google him and find his CV.

CHOI: The reason that I'm asking this is as I have been searching information about LRSM, what people usually cite as a prime example of interdisciplinary research at LRSM is the Alan Heeger-Alan MacDiarmid collaboration, which lead to the Nobel Prize.

GIRIFALCO: Well, it is a prime example, and it's there because they got the prize, right?

CHOI: Right.

GIRIFALCO: Okay, but there are many others.

CHOI: You say that there are many others, but when I went through looking at the Penn archives under the stadium, as your office was like in Cincinnati maybe, the system was that individual faculty members submit proposals to LRSM executive committee.

GIRIFALCO: That's correct.

CHOI: So I was thinking that might rouse an interest in the field of materials science broadly, but that doesn't necessarily promote collaboration among faculty.

GIRIFALCO: Well, there are various definitions of collaboration. An important factor, which does not involve person-to-person collaboration, is that people are opened up to what's happening in other fields, so our students took quantum theory from the physics department. Our students take courses in the chemistry department. Right now, I think there's more collaboration than there ever has been.

You know, you take our polymer people and our theoretical people like [Vaclav] Vitek. They work with people in other departments all the time. A great collaboration has been between Campbell Laird in mechanical engineering and John Bassani. Jack Fisher, his collaboration with the physics people like [A.T. Charlie] Johnson and [Eugene J.] Mele and many others, is terrific.

I mean the nanotube field has in fact resulted in a number of publications, which are people from different departments. I myself have published a paper in which my name exists with Paul Heiney from physics, so the collaboration is real. Person-to-person, it grew over a period of time. As you would expect, it's not going to be instantaneous. People have to get to know each other. But I can tell you from observation that regardless of any objective data you try to generate or stories you hear beyond MacDiarmid and Heeger, this broadening has been very real and collaboration one way or another has been very, very real and I think an amazingly powerful development in American science.

CHOI: Professor Maddin talked about the weekly tea time that you had. There was a tea time within the lab?

GIRIFALCO: Yes, we always would be down there. Now here's another one for you, okay? We had a faculty member named Solomon Pollack. He's retired now. He and I were good friends. His research specialty was thin films. One day, he decided that he was going to study biomaterials. This was before the big bio explosion.

So he did that, and he ended up working with a fellow in orthopedics named Carl Brighton, and one of the things he was looking at was the electro-potentials generated by stress in bones. The practical results of that, by the way, was that putting electrodes on bones in old people accelerated the healing, but that was a collaboration—extra-departmental—collaboration that has had historic influence here at Penn.

We now have a bioengineering department in Penn, which essentially would not have existed without Pollack and his work. There were things called bioengineering before, which was really looking at X-ray tubes and things of that sort, but the collaboration with the medical part all came out of the LRSM idea of interdisciplinary.

So that's the kind of thing that you won't get if you do what I say, look at papers in materials science. It goes beyond that. We had a fellow working on dental materials, which by the way was first initiated by Hobstetter [John], who went and taught dental materials in the School of Dentistry. You can't get much more interdisciplinarity than that.

CHOI: Right. [laughter]

GIRIFALCO: Okay? So I don't think those things would have happened if there had not been an LRSM because there was no philosophical focus for such a thing.

CHOI: Right. So how was John Hobstetter as a director?

GIRIFALCO: Magnificent. John Hobstetter was magnificent in everything he did. He was a superior person. He ended up being vice provost of research at the university. He created the modern research policy for the university.

CHOI: And then how would you characterize that?

GIRIFALCO: Beg your pardon?

CHOI: How would you characterize the modern research policy for the university?

GIRIFALCO: Oh, it was very enlightened and very straightforward. We had trouble because there was a government program here that had some secrecy to it many, many years ago, and it was under criticism. We looked and found the university didn't have any standard policy for research.

So we created a policy that stressed openness, stressed academic responsibility, and allowed for some secrecy for patent purposes for a limited time, like a year. We could not do any work that required continuing security. So it's a very good policy, and it has offshoots, like what you do with patents and income from it and so on, but it's all written down someplace. You can find it if you want.

But [Hobstetter] was the originator of that and he was a great director because he established the policies by which the LRSM functions to this day. He set up the executive committee. He set up the reporting system, and the reason he was good for this is because he had the most integrative mind that I had ever met. On anything, he would bring together all the knowledge and coalesce it into an integrated whole.

In fact, I have a famous paper of his. He didn't publish a lot, but I have a famous paper of his on point defects and semiconductors and you look at it now and say, "Oh, yes, sure. That's all pretty standard stuff," but he created it by bringing together a lot of different things. So he was ideal director for the LRSM. Bob Maddin brought him here. I don't know if he realized just how good he would be at it, but he brought him here and Hobstetter set up. He helped design this building. It was his idea to have a core and a ring around it.

CHOI: Yes, I was always interested in his career, that he came from Bell Labs.

GIRIFALCO: That's right. Bob brought him here from Bell Labs.

CHOI: From the metallurgy division.

GIRIFALCO: That's right. He was a metallurgist, but what a metallurgist! I mean he read and knew everything. He absorbed everything.

CHOI: In his obituary it says, if I remember correctly, he acted as a liaison between the metallurgy group and the physics group within Bell Labs.

GIRIFALCO: And chemistry. Oh, at Bell Labs.

CHOI: Yes, at Bell Labs.

GIRIFALCO: Yes, I knew a little bit about that but not much. I knew nothing of his career at Bell Labs, but he sure was a liaison here among everybody.

CHOI: Right.

GIRIFALCO: And one reason that he could do that was because he was very highly respected since he was so smart. He could understand anything you told him.

CHOI: That's very interesting. I'm really sorry that I can't meet him.

GIRIFALCO: Oh, it's a shame.

CHOI: Yes.

GIRIFALCO: Yes. Well, he retired and in his last few years he was quite ill. He had balance problems and kidney problems, and I used to go see him a couple times a semester and it was sad to watch him deteriorate, to see a great mind like that.

CHOI: So when Hobstetter went on to become vice provost, you took over as director [of LRSM].

GIRIFALCO: Yes.

CHOI: And was it –?

GIRIFALCO: I think I was the second. What year was that off the top of your head?

CHOI: 1967.

GIRIFALCO: 1957?

CHOI: 1967.

GIRIFALCO: 1967, so yes, I think that's right. I think I was next. I was the second director, right.

CHOI: So how did that happen? Did someone just give you the offer?

GIRIFALCO: Yes, they set up a committee, the usual way they make appointments at universities. They set up a search committee and I had no knowledge of it. I was ignoring pretty much all that stuff because I was busy, and one day they came and said, "Would you do this?" I said, "Sure. Why not?" [laughter] Because I admired Hobstetter and I saw that the LRSM was important and in those days, it was an easy job. It wasn't a hard job.

CHOI: Because everything had been already set up?

GIRIFALCO: Not only was it already set up, but all you had to do was write your reports every year and do good science; no politicking, no fighting for funds, no visiting Washington [D.C.] every other day, you know.

CHOI: And how was the relationship between the university and ARPA? Was it a hands-off mostly policy that ARPA took or did they try to gain some level of control over what you were doing?

GIRIFALCO: Their control was through the annual reports and through a site visit, and then they rated how you were doing and compared you to the other laboratories at other universities on how you were doing. Otherwise, they were hardly ever involved. Every once in a while there would be a government conference, I guess, on what was important for the future. And there were commissions that were set up to try to analyze where science was and where it was going.

Fred Seitz and others had a thing called the solid state panel, of which I was a member, and they would get together a couple of times a year and try to sort out what was needed in solid state. Of course, everybody read those things and was influenced by them, but at the end of the year you submitted your report to ARPA, and later NSF, set up your budget, asked for a certain amount of money, and then you got notified as to what you were getting. After that, the executive committee took that money and distributed it. Now, don't misunderstand. In the budget, you did have a statement of what kind of research you were doing and where it was going to go and who the faculty members were, all the usual stuff, but that was the mechanism of government oversight, site visits and the report.

CHOI: Did they in any way try to steer the direction of a particular topic of research?

GIRIFALCO: Sometimes, but in those days, you have to remember that the community included the government people. It was much less adversarial than it is now, and the reason it's adversarial as it is now is because money is short and so these guys in Washington have to satisfy a huge number of constituencies. They have to be very careful in questioning everything.

But, you know, we had people from materials science departments take three-year leaves and go to work for ARPA, running the programs, and later NSF, running the programs, because in addition to the bid report there were little reports, smaller reports, and continual contact. It's not that they were deaf and dumb until the end of the year. They were part of the system.

CHOI: So I was trying to understand how this whole budget thing works at the lab.

GIRIFALCO: Now?

CHOI: Back then.

GIRIFALCO: Oh, back then?

CHOI: So you get a huge chunk of money from ARPA.

GIRIFALCO: Right, which we asked for, for specific research directions.

CHOI: Okay, and how do individual faculty proposals fit into that?

GIRIFALCO: The individual faculty wrote up what he wanted to do and sent it to the executive committee. The members of the executive committee read it. They had discussions and sometimes arguments among themselves as to how much money to allocate for different areas of research. Then they would put a stamp of approval on the ones they chose, notify the faculty member and he'd go to work. It was all fairly rapid compared to today's lengthy processes.

CHOI: So the individual faculty proposals has to fit into one of the areas that the lab had proposed to ARPA?

GIRIFALCO: Yes, with an exception. There was always room for new initiatives, and they were labeled new initiatives. But don't forget, the faculty members were the ones that put the information into what areas of research they were going to go to, so it was no surprise to the executive committee when you got a proposal from a faculty member saying, "I want to do this."

CHOI: So the lab proposal that you submit to ARPA was produced by a bottom-up procedure?

GIRIFALCO: Yes. Oh, of course. It's always bottom up. I mean you can't do it from top down. Nobody's that smart.

CHOI: Right. [laughter]

GIRIFALCO: Except, of course, our President. [laughter]

CHOI: Okay, so you submit the proposal to ARPA and then in the following year, you accept proposals to fit those.

GIRIFALCO: Right.

CHOI: Those objectives that you set forth.

GIRIFALCO: Most of which have already been written, right?

CHOI: That's interesting.

GIRIFALCO: How else would you do it? What would be an alternative system?

CHOI: I mean I'm interested in that two-tiered system, right? Perhaps the other way might be just accept proposals and kind of collate them into a whole and then submit that to ARPA, but you're saying that there was a kind of a filtering process at the executive committee level.

GIRIFALCO: Yes, the director did not submit this report in a vacuum. With the executive committee, they knew exactly what was going on in a laboratory. They knew what everybody's research was. They'd discuss their research in the executive committee meetings. There were presentations made of the research.

One of the first things I did when I came here was to present my research. There were site visits where we had people come from ARPA who'd come and listen to these presentations. Everybody knew what was going on and when things were phased out, they were phased out over a period of time. Also, they were phased out because other more exciting work was going on, often by the people that were being phased out.

CHOI: Right. So it's a fairly organic process at work, right?

GIRIFALCO: It's a bottoms-up process, bottoms-up administration, because the executive committee was chosen by the faculty in their departments. They were there really to do the organizing, the collating and to do the dog work. And then there were some times a decision that had to be made because they didn't have enough money for what everybody wanted to do, and they had to support this instead of that.

And sometimes it was a little bit unhappiness. For example, when we would go through a transformation from one area of research to another. When we got big on surfaces, other things had to slow down and go, but it was gradual. There was a period of time where everybody knew it was happening. It wasn't like General Motors where the guy comes out and says, "Okay, tomorrow you guys are doing surface chemistry." [laughter]

CHOI: It seems that the way that you describe that period during the 1960s is something like the good old days.

GIRIFALCO: You know what? That's the way I think of them, and I don't know how much of this is simply because I'm old. But if I have to think objectively, there was a reality to it. It was the good old days for several reasons. First, we were in a growth phase. Science was growing throughout the country, and scientific budgets were growing throughout the country. Don't forget, Sputnik had a powerful effect on that, and the materials laboratories were a new experiment with a beautiful idea. We were publishing like crazy, learning lots of new things, and getting to meet people we never met before. Above all, our students were relatively well protected. If I put in a proposal for a student's support and I got that support, I was pretty well assured that they were supported right through to their Ph.D.

CHOI: As far as I know, one of the purposes of the ARPA grant was to produce Ph.D.s.

GIRIFALCO: Exactly. Yes, but that was as important in their minds as the research production. Of course, we succeeded in that, and one reason that we succeeded is because they protected the student's education for four years.

CHOI: So it seems that all the stars aligned at that time: you had plenty of money, and the size was small enough to warrant informal interaction among all the faculty members. That's not exactly what science administrators face nowadays, right? [laughter] They're always worrying about the lack of funding and the size is relative to the extent that –

GIRIFALCO: Well, you know what? At the time, we wanted more money. We were worrying about the lack of funding. But it was a different kind of thing than it is now, because in truth there was money, and if you had something really good to propose you always could get money, but we always wanted more. It's the nature of the beast.

Education and research and medicine, I think, are three areas in which no matter what you do you can always do it better. No matter how good an education you could deliver, you could find ways to improve it. No matter how great your research establishment, you could find ways to make it better and make it more efficient. No matter what disease you cure, there's always another one to cure and you could do better on the ones you've got a handle.

So the demand for funds in those kinds of fields will never stop no matter how much you get. We had money to support our group and our students and so on, but you know what? If I had a 25 percent increase, I could get two more students to come here. Why can't you give me a 25 percent increase? [laughter]

But it was not a case of misery. It was not a case of bitterness. It was a case of you want to do more and that's never going to change. The model, I think, as you so rightly said, was a coalescence of factors that made it possible to look back now and, indeed, they were the good old days. The LRSM director today has very little money to spend. There's no faculty member that I know of that is supported completely by LRSM funds today. No matter who they are, the LRSM support is a minor percentage of their funding. When I was there, it was a major part of their funding and, in fact, many people, it was a total part of their funding, which makes life much easier.

Estimates have been made that the amount of time that is spent by academic scientists in administering science, by which that means writing proposals, writing reports, making presentations, doing reviews and so on, is 25 to 33 percent. Now think of that. A quarter to a third of our time is spent doing things about science that has nothing to do with science. If that system was streamlined, we could immediately increase the scientific productivity of this country by at least 25 percent. We had a streamlined system in the early days.

CHOI: And somehow it was lost in due course, in your view. So the streamlined system, in a way, deteriorated through time.

GIRIFALCO: But it's very natural. Programs like this are often in some factors and in some ways victims of their own success. When you get to be big and there's big money involved, Congress requires more accountability and the congressional version of accountability is much more detailed than what our version might be.

Then when you see successes like MIT [Massachusetts Institute of Technology] and Penn and Cornell and Columbia [University], other people want some of that, too. So you get proposals for new centers and they have to respond to that somehow, so there's more administration. The NSF, for example, has to go to Congress and justify their budget. They've got to justify budgets for the materials labs they have today and they've got to go in there by telling them stuff.

Where do they get that? They get that from professors writing it up, and they get instructions for it. "Do you have any nuggets?" was a favorite term, and these guys run around for nuggets. Then we have to have an audit. They come down and give an audit, and all of the files are opened up. They look at it very, very carefully because they have to know where the money's been going, so the deterioration is very natural. In our view, it's deterioration. In their view, it's an improvement in accountability. So competition, large budgets, accountability, political factors, they have an effect on the system.

CHOI: Mm-hmm. And all of these tend to go hand-in-hand, I would think.

GIRIFALCO: Yes and what has an effect on our system? The Navy's need for materials; the recognition of the government that materials are important for defense; Sputnik; growing budgets; economic boom times, and the idea that science was going to be the cure-all, which of course it never is. But all those things came together and said, "we've got to do something."

For twenty years, it had a wonderful run, didn't it? And the system was built in such a way that now it accommodates pretty well to the tougher system, although at a cost of time, but our department and materials science at Penn is thriving. It had its season origins in the early days. What else can I say?

CHOI: [laughter]

GIRIFALCO: I think that I'm enthusiastic about it probably because it made my career, right? I came here. I was happy. I was doing what I liked, and so some other people might look at it a different way.

CHOI: I'm just looking at your CV here. So you were a director of the LRSM for two to three years until 1969. Then you became chair of the department in 1972, so you were moving in and out of these offices, right?

GIRIFALCO: Yes.

CHOI: Because?

GIRIFALCO: I'm not sure why. [laughter]

CHOI: [laughter] So the department chair's office is in that corner and the director's office is downstairs.

GIRIFALCO: Downstairs, right.

CHOI: [laughter]

GIRIFALCO: Not a heavy move.

CHOI: Right. And you became associate dean of the engineering and applied science right after that. You were vice president –

GIRIFALCO: In those days, Joe Bordogna and I worked closely together. He was associate dean for undergraduate.

CHOI: And then you occupied the position that Hobstetter had for a while.

GIRIFALCO: Right.

CHOI: So you're right, that your career spans this twenty-year…well, that the height of your career spans that twenty-year period where…

GIRIFALCO: Right.

CHOI: That all these wonderful ideas about science and wonderful funding resource available, which was not the case after the 1980s.

GIRIFALCO: That's right.

CHOI: Which was characterized by intense international competition, worries about competitiveness.

GIRIFALCO: Right.

CHOI: Misgivings about whether all these fundamental research funding is going to be of any use.

GIRIFALCO: That's right. Yes.

CHOI: And whether we should do more practical research than basic.

GIRIFALCO: Even before then many industrial labs were closed down. I remember U.S. Steel had essentially closed down. GE had cut way back. Ford Motor Company as well.

CHOI: And then in the twenty-first century as the nanotechnology boom happened, partly as a result of that post-1980 period, and trying to regain this leadership position for the U.S. scientific community. It's interesting that there is this continuation of practice that goes back to the early 1960s.

GIRIFALCO: Mm-hm.

CHOI: And even as they are aiming for slightly different goals, the institutional framework itself has remained pretty constant.

GIRIFALCO: Isn't that interesting? Yes. Well, it was a good idea.

CHOI: Yes.

GIRIFALCO: And both good ideas and bad ideas can persist for a long time, but this was a pretty good one.

CHOI: So were you at all involved in the NBIC, the Nano/Bio Interface Center?

GIRIFALCO: Not really. I wasn't involved in any organizational part of it. I've done a lot of research on nanotubes. I worked out a lot of statistical mechanics of molecules inside nanotubes and worked out the energetics and interactions of that and I published a lot of that stuff. Right now, I'm only peripherally involved. I'm doing some research with a colleague on metals inside of nanotubes, but, hey, I'm going to be eighty years old. The young people have the energy and the ideas. [Laughter]

CHOI: So how does that all work together? Say Dawn Bonnell, for example, right? She's the director of the NBIC. She's a member of the LRSM and she's a faculty member at the department of materials science and engineering, which seems like a very complex organizational network that is –

GIRIFALCO: Why?

CHOI: – like superimposed at the same time.

GIRIFALCO: Well at Penn, I guess like other places, there's both horizontal and vertical organizations, right? If you're going to do interdisciplinary work you've got to organize with people across boundaries. That's what the institute does. Penn is somewhat different than many other universities, it used to be more true than now because other universities are catching up, but Penn has been kind of entrepreneurial.

Most of the good things that happen at Penn happened by some faculty member going out and being an entrepreneur, both in an academic sense and in a financial sense, the major reason being that Penn had such a small endowment. [laughter]

So if somebody wanted to do something they had to go raise money and find people to do it, so there was an atmosphere at Penn that faculty members had to do things for themselves and that's still true, so Dawn went out and essentially she was the fireball that did it.

Dave [David E.] Luzzi was the fireball that did that other one where they were financed by the state. He's left us now, as you know, dean—where—at Northeastern [University]? I forgot. But the individuals, faculty members, are usually the driving force between new initiatives. It's not usual that it comes from some administrator. I've been involved in several of these.

That's it?

CHOI: That's it. Thank you very much.

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]

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