CHEMICAL HERITAGE FOUNDATION

ROBERT MADDIN

Transcript of an Interview
Conducted by

Hyungsub Choi

at

Arlington, Virginia

on

22 April 2008

(With Subsequent Corrections and Additions)

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Center for Oral History
315 Chestnut Street
Philadelphia, Pennsylvania 19106

ROBERT MADDIN

1918	Born in Hartford, Connecticut, on 20 October

Education

1943	B.Sc., metallurgical engineering, Purdue University
1948	Dr. ENG., metallurgy, Yale University

Professional Experience

	Yale University
1948-1949	Postdoctoral Fellow
	Johns Hopkins University
1949- 1955	Assistant Professor, Mechanical Engineering
	University of Birmingham, U.K.
1954	Visiting Professor
	University of Pennsylvania
1955-1972	Professor, Metallurgy Department
1955-1972	Department chair, Metallurgy Department
1972-1983	University Professor
	Laboratory for Research on the Structure of Matter [LRSM]
1965-1972	Executive Committee
	Harvard University
1983-1986	Professor, Anthropology Department
1986-1989	Curator, Honorary Curator, Peabody Museum

Honors

Honorary Professor, Beijing Science and Technology University, China
Honorary Professor, Dali University, China
Fellow, Wolfson College, Oxford University, England
Distinguished Senior Scientist, Alexander von Humboldt Foundation, Bonn, Germany
Honorary Fellow, Japan Institute of Metals, Japan
Fellow, Institute of Metals, United States

Fellow, American Society of Metals, United States
Pomerance Medal, American Institute of Archaeology
Distinguished Alumnus, Purdue University
Distinguished Achievement Award, University of Pennsylvania

ABSTRACT

Robert Maddin begins the interview by briefly describing his childhood and attending school in Hartford, Connecticut; enrolling in Brooklyn College; and decision to study metallurgical engineering at Purdue University. Maddin then served in the Armed Forces during World War II before enrolling at Yale University for graduate studies. After Yale Maddin spent several years teaching at Johns Hopkins University's mechanical engineering department before accepting a position at the University of Pennsylvania. As the head of Penn's metallurgical engineering department, Maddin was responsible for its growth over the next 2 decades. During that time Sputnik caused a surge in scientific funding and led Maddin and other professors to submit a proposal for a materials science laboratory within Penn. With the proposal a success, Maddin then described starting up the Laboratory for Research on the Structure of Matter [LRSM] and the role the metallurgy department played in its formation. Maddin then offered details of LRSM operations and interactions between the chemistry, physics, and metallurgy departments within the facility. After being appointed a University Professor by Penn administration, Maddin had the freedom of teaching in any department and gradually shifted his focus towards the history of science. Maddin concludes the interview by describing his second career at Harvard University's anthropology department, and his interest in metallography and the historical usage of metal.

INTERVIEWER

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: **Arlington, Virginia**

DATE: **22 April 2008**

CHOI: Professor Maddin, I'd like to start with the very beginning of your life. So can you tell us some of the basic factual information there?

MADDIN: Yes, I was born in Hartford, Connecticut on October 20, 1918. I lived in Hartford until about 1939. My elementary school was in Hartford at a school called the Frank Brackett School. At that time, there was no middle school, so from grammar school I went to Weaver High School. Still exists in Hartford, Connecticut. I graduated from there in 1936. I was not quite sure at that time what I wanted to do. I lived in New York for a few months, took some courses in English literature at Brooklyn College, but I wasn't sure that that's what I wanted to study.

There followed about a year, year-and-a-half during which I knocked around the world, more of the United States and Mexico. Worked in a copper mine in Arizona. And when I returned to Hartford—had to be late 1937—I had decided I would go to some college where I could pursue either metallurgical engineering, in the old sense of the word, processing or mining, since I was interested in travel.

CHOI: Extracting ores?

MADDIN: Yes. But I didn't have enough money, so I worked for about a year, saved up money and got some help from my parents and enrolled at Purdue University in 1940. The course was four-and-a-half years. I did it in just under three years, I think, by working hard year-round, and received a degree in metallurgical engineering. I studied with a man by the name of John [L.] Bray, who at that time had one of the prominent textbooks in metallurgy, from the chemical end of things.[1]

Then followed three years in the Armed Forces during the Second World War, in which I was stationed first as a communications officer in Liberia for three months, and then in India and China for almost two years. I was—the phrase is mustered out of the Army, I guess, at the end of the war, and enrolled at Yale [University] as a graduate student. But here, I was—that primarily wasn't my decision, but I was asked to follow a mathematical physics part of the

[1] John L. Bray, Nonferrous Production Metallurgy (New York: J. Wiley & Sons, 1942).

curriculum, so the first year was devoted to mathematical physics.

From then on, I studied primarily deformation in alpha-brass. I was awarded my degree in 1948 and stayed on for an additional year as a postdoctoral fellow. That was 1949. I received an offer from Johns Hopkins University as assistant professor of mechanical engineering, and my wife and I and a child moved to Baltimore [Maryland]. In Baltimore, I was in the mechanical engineering department. There was no metallurgy, it was a small section of mechanical engineering. I do recall that when I left Yale, I was convinced that the field of metallurgy was changing, changing almost abruptly to more of the physics end of things—properties or structure, or how structure properties are interrelated. So one of the early things I did at Johns Hopkins was to try to enlist the physics people to work with me on problems.

The physics department at Hopkins, a very renowned department, was primarily in particle physics, except for one man who—the head of the department was a man by the name of—hmm, can't think of it right now. There was one solid state physicist, but he was a theorist. And I went to see him to see if I could get him to cooperate, and he said, "Sure, sure…"—he'd cooperate, provided I got him a graduate student, and he was interested in that, but it didn't work out.

In 1953, I'd been at Hopkins four years, and one of my fellow graduate students at Yale, two years behind me, from Mainland China, excellent student, had graduated. I asked him to join me at Hopkins. At the same time a letter from Birmingham, England, from Professor Robert W. Cahn, asked whether I would exchange with him. He would live in my house. I'd live in his house. And I'd spend the year teaching at Birmingham, and he'd spend the year teaching at Hopkins. That didn't work out too well from a personal reason. My wife was ill, and I had another child coming. So I asked [Neng-Kuan] Chen, the fellow faculty member, if he would go, and he said yes, he would go. But he then realized that because of the visa restrictions, if he left, he would have trouble getting back in, and he didn't want to endanger his stay. So at the last minute, my wife and I and our children at that time decided we would go to England. So I spent a little over a year there.

More importantly, while I was there, I met a man named Alan [H.] Cottrell, a physical metallurgist who really had a profound influence because I liked the way he put together his thoughts. We did some work together while I was there, mainly concerned with point defects and effect on properties. When I returned to John Hopkins, it was January of 1955. Within six weeks, I received a call from the head of the department at Penn [University of Pennsylvania] metallurgical engineering, saying he was leaving to go to the Continental Can Company as the director of research, would I consider going to Penn? We talked about it a bit, and I gave Johns Hopkins the opportunity to know what I was planning on doing. That summer, I went to Penn, 1955.

Should I stop here for a moment? Do you want to fill it out? Is that the sort of thing you want, that—

CHOI: You can finish that thought, and then I can ask some questions.

MADDIN: The summer I came to Penn.

CHOI: Oh, I see. Okay. So before we go onto Penn—

MADDIN: Yes, that's right. Okay. You can ask some questions about that.

CHOI: So just to make sure, when you moved to Penn, did you join as a department chair? Or were you just a faculty member and then became—?

MADDIN: I was a faculty member. Came as a full professor. I would point out, however, that the department was in shambles. There was one man, Norman Brown, who was there full-time. There was a man by the name of [Fred] Dunkerly, who was there part-time, and Dunkerly was nominally the head of the department. But the way Penn is set up in the administration of grants and things of that nature, an individual in his department had to have [Dunkerly's] signature to do any work or order anything on his own contract. That's the way it was set up. It means—and I brought two research grants, sorry, correction—one research grant with me from Johns Hopkins, grant of the Office of Naval Research on body-centered cubic single crystals of tungsten, tantalum, high-melting point vanadium. I could not order equipment without his signature. I could not hire anybody without his signature. And he was never at the university.

CHOI: Because he was part-time.

MADDIN: He was part-time. And it was very cumbersome. I complained to the university—well, normally when you complain, they tell you, "You solve it." So they made me department head.

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

MADDIN: Very small. There was one other faculty member, [Norman] Brown, one senior student, one graduate student and no other undergraduates. So it was a department in name only. Actually, it was more than that because the structure of Penn at that time because of the Moore School of Electrical Engineering.

Let me stop here for a moment and tell you a little bit about the structure at that time. There existed in Philadelphia, next to the university, the Moore School of Electrical Engineering. Had nothing to do with the university. The trustees of the Moore School decided they would try to ally themselves with the university, so they drew up an arrangement, and the arrangement called for many things—there was an endowment involved—that the head of the Moore School has to be called director or dean. Well, if electrical engineering was to have a dean, then mechanical engineering would have a dean. They were called directors. Chemical engineering would have a director. So I was named director of a school that had one other faculty member, one undergraduate, and one graduate student.

CHOI: [Laughter]

MADDIN: Amusing, isn't it? Well, the university was very much concerned, as it always is, about teaching. The raison d'être, let's say, for existing is teaching, along with research; but teaching is prominent. So any expansion of the department had to be on the basis of needed teaching. And with two faculty members and two students—one grad and one undergraduate student—one could not expand through the "teaching" mode.

Well, I argued, apparently successfully since it worked out, that since I was doing half the time on research the university is employing me as half time. I'm bringing up half the money, so that I can use the money they're paying me for the other half to hire new faculty, in effect. That's pulling yourself up by your own bootstraps. So I was able to go out and hire people, and they, in turn, wrote contracts, spent half their time on research, and therefore, they supplied money for further additions. So within a short time, we had a department of—let's see, myself, Norman Brown…six people. Graduate students increased because some came with me. Some came from Japan to study with me, and one from Korea, as a matter of fact, too. And we built up a group of about—well, here I'm a little shaky—perhaps ten graduate students, five postdoctoral fellows, and we increased the faculty to about six, and we were trying to attract undergraduate students. We were not too successful. We did bring in dribbles. It was never very successful.

That brings me to a point where the forerunner to the LRSM [Laboratory for Research on the Structure of Matter] has to come in. Do you want to ask any questions at this point before I go over to that phase?

CHOI: No, please go ahead.

MADDIN: During the first summer I was there, I went to a Gordon [Research] Conference, where I gave a paper on point defects and interaction on properties. There I met a man from the physics department at Penn, a theoretical solid-state physicist by the name of Herbert [B.] Callen, and we talked about what should be happening at Penn.

I had gone to Hopkins with the idea that the future of metallurgy combined solid state physics, and they ought to be brought together into one unit. I couldn't do that at Hopkins. At Penn, I saw there was a possibility because there was Callen. He was bringing in a man by the name of Eli Burstein—you know him now from reading the file, solid-state physicist—as well as others. And we were able to attract someone from chemistry, Bob [Robert S.] Hughes. So we had the nucleus of an interrelated group—chemistry, physics, metallurgy. That was 1955.

These plans didn't progress too far until Sputnik came up in 1957. You were at that time back in Korea, or you probably weren't even born.

CHOI: No, no. Very far from that.

MADDIN: Yes, but you remember—maybe you were reading about it. When Sputnik was sent up in 1957, this produced a tremendous shock in the United States, especially to the scientific and engineering community. How could a totalitarian country produce a Sputnik when we, the greatest civilization in science, were not involved and could not do anything like that? So that was a great shock, and that brought together a number of very interesting people, one of whom was Herbert Holloman, a metallurgist, and also Vannevar Bush who was involved with computers at MIT [Massachusetts Institute of Technology]. And there were others who met in Washington [D.C.], and they said, "What can we do about the fact that we're so far behind, and the Russians—Soviets—had put up Sputnik?"

One conclusion of that committee was that there was a bottleneck in scientific personnel. We were not producing—first time the word appeared—materials scientists, people who could understand structure, properties, relationships. They put that before ARPA. ARPA at that time was the Advanced Research Project Agency of the DoD [Department of Defense]. The ARPA, as I understand, decided that they would ask for bids on an interdisciplinary field, which they would call materials science. Bids were sent all over the United States, and grants would be awarded to begin education and research. The word education here—I must be careful, because the government could not sponsor education. They could sponsor science. Education has to come through a different government department.

CHOI: Right.

MADDIN: Let's see. At the end of that meeting, I received a call from a classmate of mine; there were three of us who earned degrees at Yale at the same time. A person who was at that meeting telephoned and said, "Bob, look, prepare yourself. You're going to get a request for a bid. Other people will be getting requests to submit bids for a research grant." Well, I took that heads up as important item and went to the director—sorry, the equivalent of the dean of engineering. At that time he was called the vice president. His name was Carl [C.] Chambers; I said, “Look, let’s put a committee together.” He put a committee together. It consisted of me, Eli Burstein in physics, Bob Hughes in chemistry, and he asked a man by the name of Norm [Norman] Hixson in chemical engineering to chair the committee.

That committee met for, I think it was one to two hours every week, to put together that report which you see there. That was submitted, and three universities received grants the first go around—Penn, Cornell [University] and Northwestern [University]. Five more were approved a year later. That grant that we received was a bit over $6 million. So here we were at Penn, with money, but with no space. My department had one area of the third floor of what is now the Towne Building. No way of expanding. No office space. No laboratory space. We asked the government if we could use the $6 million to build a building, and of course, they could not. They were prevented from supplying money for bricks. But they did agree to reimburse the university ten percent of the total amount of money for a building as a “use fee.” In other words, we would supply the building. And then we could charge the government ten percent to use our space. So in ten years they will have paid for the building.

CHOI: Oh, I see.

MADDIN: You see that? And that’s the way the LRSM came about, the name itself is just a result of three people who argued about the name. So it’s a compromise.

Let me stop here and have you ask some questions.

CHOI: So I guess one of the questions that I had was what do you think that gave Penn, Cornell and Northwestern the edge in getting that first round of grants in 1960?

MADDIN: Good question. I think it was the enthusiasm shown in the report. We were reputable people from at least three different disciplines eager to work together. That, I think, was the glue. Cornell had it. They had some very good solid-staters. Forget their names now. Had some good metallurgists, and Hughes had originally been at Cornell. Northwestern was the same way. They had the ingredients, and it showed up in their report. MIT was later given a grant, as was University of Illinois and, I think, Stanford [University].

But ours, even though we committed the silly sin of not paginating our report—it’s not paginated, though, hmm? That was pointed out to us. So you can’t say, “On page 53, we said that…,” because there’s no page 53. The physics people were sincerely interested in cooperating with the metallurgists, as were the chemists. The best example of that would be that Alan [G.] MacDiarmid from chemistry and Alan [J.] Heeger from physics working in our LRSM received a Nobel Prize. So that’s the synthesis that can show up, but that sort of spirit showed up in that report, I think.

CHOI: So do you think that there always was this—in the context of the mid-1950s, before LRSM came to be, do you think that there was a sentiment of cooperation within these three departments preexisting before LRSM?

MADDIN: The reason I think that's a fascinating question is that to this day I don't know whether Eli Burstein, Bob Hughes, and I were of the mind to work together as a means of solving problems or because we saw this pot of gold. I don't think it was the latter. I was sincerely interested in understanding property structures. I think Eli was too because of the work he had done. Bob, I'm sure was. Later on he proved that himself. So if you ask me what I believe, I'm not sure, because there was an awfully large pot of gold at the end of the rainbow there that we had just been given, and there's nothing that will bring people together as sharing a pot of gold. But I think that Eli was sincerely interested in understanding the relationship between properties and structure. I know I was. And I think Bob was too. I can't answer any more specifically.

CHOI: Okay. So I guess next I had the question about hiring John Hobstetter, which I believe happened in 1958 or 1959.

MADDIN: Yes.

CHOI: And you were the director—

MADDIN: I was the head then. We had a—the man before me who was chairman, is Robert Brick. He had a number of graduate students—they were good graduate students—while he was head. One of them was Ray Smith, who became president of Michigan Tech [Michigan Technological University], now retired. But another one was [F.] Lincoln Vogel. As soon as I came to Penn, we set up a relationship because he was a very loyal alumnus, undergraduate and graduate student, and he wanted to see Penn progress in the field, his field. He was then working at the Bell Labs. He came to me and said, "Look, I've got a man here who is great. He'd make a wonderful teacher." That was Hobstetter.

So I contacted Hobstetter and talked with him, and he eventually came to Penn. He came as an associate professor without tenure, and within two years he was promoted to tenure, and within another three years he was promoted to professor. But his strong talent was in administration. As director of the LRSM, he did a great job. He was tapped by the president, then [Gaylord P.] Harnwell, to become associate provost of research, and he did a great job there. So that was how he came to Penn, and how he became associate provost.

Well, while in my department, he initiated a course in materials science which he taught along with [Louis] Girifalco at times, which was very well received by undergraduates. He later had one of his students, Edward Korostoff teach the same course to dentists because we had initiated a program where students of dentistry would be able to get a joint degree, their doctor in dentistry and a PhD in materials, and that was the basis in the course in which they studied later on. Hobstetter designed that course.

CHOI: So when you first contacted Hobstetter, did you know of his previous works on semiconductor material, silicon and germanium?

MADDIN: Yes.

CHOI: His role in Bell Labs.

MADDIN: I was acquainted with some of his work. He did a very brilliant piece of work on nucleation theory, as I recall. I looked at it—very well thought out. So yes, I did know about him, but I hadn't realized he was as good a teacher as he turned out to be. He was by far one of the better teachers that I've come in contact with.

CHOI: So if it was 1958-1959, that was already when this move toward an interdisciplinary research center was happening, and I was wondering whether he was brought in as this potential director of this new—

MADDIN: No, he was brought in pretty much to fill certain aspects in our department. He had a marvelous flair for writing, so we had asked him to read what we had written and put it into more acceptable language, I'd say. Engineers are not known for their promise as writers most of the time. So he was brought in for that reason. The first director of the LRSM was Norman Hixson, who was technically the chairman of the committee that put this all together. The second director was either Gene [Eugene R.] Nixon or John Hobstetter. I forget which. Do you know?

CHOI: Hobstetter seems to be the first.

MADDIN: Yes, as director of the LRSM, he did a very good job. I can't remember any errors he made as director because—actually, he had to decide, when there was an internal disagreement, there was a committee, the same committee that initiated the program that met each year to allocate funds from the budget. This is always a turf war. But I think he handled it very well.

CHOI: Let's go back to 1955, when you first came over to Penn. Who were some of the major players on campus in this broad field? Say, I know that Charles Price was the department head of chemistry at the time.

MADDIN: Charles Price was head of chemistry.

CHOI: And what about Frederick Seitz?

MADDIN: Frederick Seitz had already left when I got in there. I didn't interact with him because he left the year I came; I didn't know him. He went to Pittsburgh, I think. Herbert Callen was a big player in the physics department. And of course, Gaylord Harnwell was a physicist himself. So Charles Price, Herb Callen, to a certain extent, Bill Stevens, but Bill Stevens was in particle physics. And there was no one in chemistry other than Charles Price, but Charles Price has always been primarily an organic chemist, and his interest in inorganic chemistry, which is what we needed, was not that fervent. So there was no leader in the materials science sense from chemistry.

CHOI: So the LRSM was being established around 1960, and—

MADDIN: 1961 would be a more accurate figure, I think.

CHOI: And then the construction began.

MADDIN: The construction began shortly after that. Hobstetter and I had to raise money. The university could not give us the money for the building. We had to go out and raise it, and Hobstetter and I spent close to a year going to various companies to raise the money. It was not a good year because every place we seemed to go, the president at MIT, named [Karl T.] Compton, had been there before, so we always saw his footprints, and it's hard to raise money going to a company where they just gave money to MIT. But we raised enough so that we could start design—well, design had been going on all the time—we could start constructing. Does that answer your question?

CHOI: Yes. Well, I was wondering why all these companies were involved, but now I know that they paid—

MADDIN: Yes, well, we got a lot of money from—

CHOI: GE [General Electric], Westinghouse…

MADDIN:—companies that were associated with Penn to start with. [Henry B.] Bryans had a big construction company, and he gave a lot of money. You'll see his name on the plaque outside. And—

CHOI: So were you involved in the design of the building?

MADDIN: Yes and no. Everyone was involved in the design. What we did once the architect was appointed—each member of my faculty had a particular laboratory for which he or she was going to be responsible. Doris [Kuhlman-]Wilsdorf was responsible for the metallography laboratory. At that time, electron microscopy was more available, so that would be included in that. Norman Brown was going to be in charge of mechanical testing. So each one would be worried about their—designing this space, and they all came together in that way.

CHOI: But I was struck by the way that the building itself was designed that had all the offices on the outside and the common space in the middle.

MADDIN: That was by design. The committee got together—one of the times they got together, we discussed the philosophy of interdisciplinary research, and we decided that the way to sponsor interdisciplinary work would be to have central facilities. For example, there would be a central metallography facility. It'd be manned or in charge by a faulty member. And any physicist, chemist or metallurgist—or chemical engineer, or electrical engineer—who needed that equipment would work alongside people from the other disciplines. There was a central magnetic facility, a central mechanical testing facility, a central metallography facility. I think for a while we had a central publication or reproduction area. The idea of putting all the offices on the outside was to have the central facilities in the center, and the students and the faculty would be around the periphery. That's why you see it that way today.

CHOI: So your office was in the LRSM building, wasn't it?

MADDIN: My office was on the second floor and would be the—the orientation. It would be the southwest corner of the building. There was a big argument in which the vice president of engineering tried to play a large role. You're bringing back some sad memories. Should a department be located in one building? Or should all departments be located in their buildings and have only a building for central facilities? The vice president of engineering wanted me to have my office in the Towne building. The faculty offices would be in the LRSM building. Same thing—the head of physics now has always been in physics. I refused. I said, "No, I will not be separated from the faculty," and I, in a sense, presented an ultimatum, "You want to do it that way, you'll do it without me," because I had a strong conviction that I didn't want to be separated from the faculty who were my colleagues and we were all working together. So that's why the metallurgy department is wholly in the LRSM while the others are not.

CHOI: Ah. So only part of physics and chemistry—

MADDIN: Yes. Well, I'm talking, now, as of 1983 when I retired. I've been back a couple of times. But there were no physics or chemistry professors in the LRSM building at the time I was there, by their desire. They didn't want to be. They wanted to be with their colleagues in physics. That's one of the reasons I wanted to be sure that I kept the metallurgy people together.

CHOI: So in a way, your department colonized the interdisciplinary environment.

MADDIN: [laughter] I'm not sure I agree with colonize, but that's a pretty good description.

CHOI: Interesting.

MADDIN: Since then I understand—it's only an understanding that—no, I'm not sure today whether there's any member of the faculty other than metallurgy in the building except the director of the LRSM, who's a chemist. But he may have two offices, one in the LSRM and one in the chemistry building. Who is it now? Do you know who it is now? Oh, I don't know.

But this interrelation did bring together and initiate interdisciplinary faculty. For example, we in metallurgy brought into and had an office in the building, [John E.] Fischer from electrical engineering. So within a couple of years, although he was an electrical engineer, he transferred to the materials department. We had, who else? There were others but I forget their names now.

CHOI: Huh. So you were saying from the beginning—the building went functional in 1965.

MADDIN: I don't know the exact year we moved—around that time, yes.

CHOI: So you were saying that from the beginning chemists and physicists did not have an office in that building?

MADDIN: Let me think about that now. Gene Nixon had an office in that building. He was a chemist. Eugene Nixon.

CHOI: Right. He became the director of the lab—

MADDIN: Yes, for a short time.

CHOI:—in 1969.

MADDIN: He had an office in that building before he became director. I'm trying to locate in my own mind where the offices were.

CHOI: So I guess the overarching question is where is the interdisciplinarity if it's a one-department lab. If the department—well, the School of Metallurgy at the time—were the only department occupying the lab, does that not defeat the purpose of setting up an interdisciplinary lab?

MADDIN: That's a difficult question to answer, Dr. Choi. The reason being is that I don't know what the results would have been had our department of metallurgy been in the Towne Building and the LRSM devoted only to research. I cannot predict what would have happened. That's not a question, necessarily, of physical locations. It's the question of personalities, pretty much. Some people, wherever they're located, want to be able to cooperate. I had—I won't mention any names—people in chemistry doing materials science work who would not even talk to the other people. That was his personality, very distinguished person. So personalities had a lot to do with it.

On the other hand, having, for example, an auditorium in that building where students and visitors gave talks brought lots of people together and lots of discussion together. That's part of the germination process. When someone from the physics department wanted to use the electron microscope, he had to use the technician, the electron microscopists. That brought in other people to discuss "what's the best way of doing that?"

Charles [J.] McMahon [Jr.], who was quite successful in setting up various spectrometry like the Auger [Electron Spectroscopy], worked with physicists to help him with some of the physics of it. That's interdisciplinary. So a lot of it has to do with the interests of the individually faculty and their personalities. I think—what do you think?

CHOI: Well, there are formal, measurable activities, and there are activities that cannot be measured as clear cut as it can be in other fields. So, for example, if we count the number of projects that faculty members from different departments come together and do a common project, did that increase after the establishment of the lab?

MADDIN: A scholar like you should be able to answer that if you go back to this period and look at the publications. See which are—

CHOI: Coauthored.

MADDIN: Coauthored. You can find that out. I can't remember too many except those by MacDiarmid and Heeger.

CHOI: That was a much later period.

MADDIN: That was a later period.

CHOI: 1970s.

MADDIN: Bob [John Robert] Schrieffer, who was a Nobel Prize laureate in physics at the time, worked with us, but I don't remember that any of his publications appeared with anybody other than physicists. I'm trying to think of my own publications during that period. My own publications were primarily with colleagues of mine in metallurgy, except for one or two odd cases. But you have to find out—it's an awfully good measure. I find that probably at the LRSM you would find not too many people where there are publications of members from different departments, where there might be in medicine, for example. I don't think you'd find it too often.

CHOI: Okay, so let me go back a little, back to your own research.

MADDIN: Yes.

CHOI: You did work on point defects and various alloy systems and—

MADDIN: Well, let me go back over that a little bit. I've had two different careers. At Johns Hopkins and at Yale I was working on the deformation of face-centered cubic crystals. One of the big problems, still is today to a certain extent, is the dislocation interactions in body-centered cubic metals. That's why I started, with Chen, to grow single crystals of tungsten, vanadium—not vanadium, but—yes, vanadium, tantalum, molybdenum, in order to study them. And in fact, Chen and I have a number of publications on deformation of molybdenum single crystals. We devised a method for growing them, and we were supplying single crystals to various scientists throughout the world, for example people at Cambridge.

The Navy then came to me and asked if I would be interested in setting up a commercial laboratory at Baltimore at the time, but I said no.

I also spent some time with Alan Cottrell on point defects interactions. I had a graduate student who came to me from Japan by the name of Hiroshi Kimura, who did a very brilliant thesis on point defects in, this case, copper. What he did was to produce a series of two papers on the mathematics and effect of point defects in copper which led to a very important discovery of collapsed disks.

I continued in that field, and that brought me into diffusion. I worked for a number of years on how to understand diffusion in solids. You can see the continuum because point defects are involved in diffusion. An atom diffuses by moving into an empty position and then to an empty position. The number of empty positions is a function of the temperature. The higher the temperature, the higher the number of defects. If you suddenly drop the temperature from very high, near the melting point, down to room temperature, you freeze in a larger concentration of point defects. These have an effect. They strengthen the metal.

So I was working in that area a while, and then I became involved—again a continuum—with trying to freeze the metal so quickly that I might freeze in the defect structure, and it's called splat cooling. This was a technique developed by Paul Duwez at Cal Tech [California Institute of Technology], and he produced what was euphemistically called at that time splat quenching. If he started with a liquid, a metal in liquid form, and flung it instantaneously against a cold medium, some compositions would freeze not in a crystalline structure but in an amorphous structure. So I began studying these structures for a number of years, I had some good students, an excellent post-doc, Ken Masumoto, who returned to Japan and formed his own laboratory devoted to amorphous solids.

In about 1970, I had been head of the department for 15 years, from 1955 to 1970. I decided I needed a leave of absence; so I went to [University of] Oxford as a visiting professor. While there, in my idle time, I wandered around the Ashmolean Museum, Oxford University's museum. It was a very wonderful museum, and became interested in history. So in 1972, I resigned my position as director. The university anointed me, I guess is the phrase, and promoted me to a University Professor, which means I could teach anywhere I wanted in the university. I became interested in early history, early history of metallurgy. From about 1972, 1973, I slowly abandoned materials science, and since then, I've been working with how did copper come into use. I've worked on the earliest copper in the world, in Eastern Turkey. And now, these days, I'm working on understanding the metallurgy of iron during the earliest years of the Iron Age. That's what I've been doing these last 25 years, 30 years.

CHOI: That's very interesting.

MADDIN: So I've had two careers.

CHOI: What I find fascinating is that this interest in very early history of what you had been working on—the metals—seem to be present, most prevalent among metallurgical engineers, like Cyril [S.] Smith, for example.

MADDIN: Yes, Cyril Smith. My model was Cyril Smith.

CHOI: And even back in my home country, Korea, there are a few metallurgists who look at the ancient bells.

MADDIN: Oh, I know many of them. We had a—we went to a conference in the southern part of South Korea, Gyeong-…

CHOI: Gyeongju.

MADDIN: Yes, Gyeongju. Oh, you know about that?

CHOI: That was the capital—that was the ancient capital.

MADDIN: We had a great meeting there, I met some—I don't remember their names. I've got them upstairs. But yes, you're right.

CHOI: And I guess that's partly because the artifacts survive for such a long time that it allows the modern metallurgist to engage.

MADDIN: Well, the—you're right. The differences are fascinating because in the Far East, in Korea and Japan, China—and most of the technology diffused eastward from China—the written record is not as clear as it is in the Eastern Mediterranean, because there they wrote on clay, and we have excavated clay tablets. But in Korea and in Japan, you have to depend on what was written on bamboo in the early stages, very early stages. That's why the Japanese in Nara have the most developed laboratory on preserving wood, because they had to preserve it. I'm sorry to give you a lecture. [laughter]

CHOI: So going back to your early research, the first career, you dealt a lot with the Office of Naval Research?

MADDIN: Yes, they gave me a grant, and I think by the time they cut me off, I had been the longest serving person to have a continuous grant from the Office of Naval Research. Started first with a man by the name of Jules [Julius] Harwood, who was head—and ended up with a man whose name I can't remember anymore. But my grant came in 1949, I guess—maybe 1950—and I was gradually changed into my phase on amorphous solids; they didn't see any future to that field, which is rather interesting because the future did develop. But yes, I did a lot with the Office of Naval Research.

CHOI: And what was your experience working with them?

MADDIN: Excellent. They understood many things about academic research. They didn't ask you to do something you didn't want to do. They didn't even direct you to do something. They may have made mild suggestions, but they're only in the way of suggestions. They asked you to report periodically. They didn't push you. Your continuation of a grant depended upon how productive you were. So they were a very good government organization with whom to do business. They didn't require any security. Students could wander in and out. And I really think they were probably one of the best granting organizations until the NSF came along.

Oddly, the head of the entire Office of Naval Research is a man by the name of Dick van Orden, a rear admiral, who lives here.

CHOI: [laughter] Are you familiar with the name Sheldon Weinig?

MADDIN: Yes.

CHOI: He was the founder of the Materials Research [Corporation]—

MADDIN: Yes. Yes. Oh, I can tell you a story about him.

CHOI: Because I met him last year, and—

MADDIN: No, he—when I, with Chen and [Robert B.] Pond [Sr.], at Hopkins, developed a method for growing single crystals of refractory metals, we did so by having a large generator pass a large current through a bar of metal, and at the same time, we'd stretch the small bars as an electron beam come down, similar to zone refining that would met a section.

CHOI: This was at Hopkins?

MADDIN: At Hopkins, yes. That was essentially the technique. Shelly wrote to me and asked me how we did it. We told him how we did it. Later on, we saw the same unit he was then producing for sale, electron beam melting, and I've never quite forgiven him for not even acknowledging us.

CHOI: So have you met him?

MADDIN: I haven't seen him recently. Yes, I've met him, met him a number of times, but I haven't seen him in perhaps 35, 40 years. You have to understand, Dr Choi, that when I left Penn, or even before I left Penn, 1975—I've now worked entirely in history and not materials science. When I retired from Penn, I went to Harvard [University], but in the anthropology department, and I taught the beginning of the use of metals in the anthropology department. So I've sort of divorced myself from the field. I still belong to the Metallurgical Society and get their publications; but I can't even understand the field any more. So that explains the dichotomy, if there is one.

CHOI: Well, Shelly's living in Manhattan [New York].

MADDIN: Oh, he's done very well. I'm glad to hear that. Do you see him quite often?

CHOI: Well, I met him once. I talked to him a number of times over the phone, and I plan to go up again to see him.

MADDIN: Yes, I—he's a nice guy. I liked him.

CHOI: Yes, I just was reminded of him because of your zone refining. Hmm. So shall we take a quick break? We've been talking for about an hour.

[END OF AUDIO, FILE 1.1]

CHOI: So what I would like to do is to just go back to 1965—well, the early 1960s again—as the lab is being established. I just wanted to get a better sense of the activities going on surrounding the lab at the time. So, for example, you had to move your office. That must have been somewhat—

MADDIN: Oh, a great pleasure.

CHOI: [laughter]

MADDIN: I have to explain what I mean by that. Have you met Charles McMahon at LRSM? He's now a retired professor.

CHOI: No, not yet. You're the first one.

MADDIN: Oh, well, you met Dawn Bonnell because you—

CHOI: Yes, yes, yes.

MADDIN: Charles went to Penn as an undergraduate, MIT as a graduate student. His older brother went to Penn. His younger brother went to Penn. His younger brother, and I remember very well, a student of mine, and he always used to refer to our laboratory in the Towne Building as "Merlin's Laboratory," Merlin being the ancient medieval magician. It was really a rather poor arrangement, so all of us were very glad to move over to the LRSM—all, myself—Hobstetter moved over, I think. Doris Wilsdorf moved over. Norman Brown moved over. Lou

Girifalco moved over. So we all were very glad because we have new office space, good equipment, for the first time. And yes, I was very glad to move.

CHOI: So you were stationed on the second floor of the lab, and you—

MADDIN: Of the Towne Building?

CHOI: I mean in LRSM.

MADDIN: Oh, in the LRSM, yes.

CHOI: And you had your equipments in that center piece of the building.

MADDIN: Yes, a central core.

CHOI: Right, the core of the building, and people will just float by in that center core, working on various things, and so people are coming in and out from other departments as well. So how did that arrangement work? Did they have to put in a work order?

MADDIN: The way it evolved now, since I left—well, even before I left—if you wanted to get some work done in the electron microscopy laboratory, you had to have, in effect, a work order. It was charged. So if I wanted to use the optical metallograph, I had to account for the time in a budget sense, yes. It didn't use to be that way during the first, perhaps, seven or eight years of the LRSM existence. But then when the accounting became extremely important—not only the accounting to the university, but to the government as well, we had to have some way of recovering costs.

One thing that I'll interject here is that the offices around the core were primarily graduate students or postdoctoral fellows. The—let me see now. The east wing and the west wing were primarily faculty offices. The wing to the north and south were graduate students office, and there were many graduate students—I think five or six graduate students in an office. So they all got to know each other, and they got to know everyone else. We didn't, in most cases, mix metallurgists with physicist graduate students together. Why, I don't know. It would have been a good way to diffuse and inter-diffuse, but it never worked out that way. I don't know the—

CHOI: That's the—well, I guess one question is—so you had postdoctoral fellows who were assigned to the lab or to the department?

MADDIN: To the department.

CHOI: To the department. And graduate students assigned to the department.

MADDIN: Yes.

CHOI: So—

MADDIN: At that time, the LRSM did not have any personnel assigned to them like a—they could, I guess, with the arrangement of the committee, have a visiting professor come in, but that was only a temporary basis. A yearly basis, let's say, but they didn't have any people who were assigned to the LRSM because every faculty person had to belong to a department.

CHOI: So within the LRSM, you said earlier that—so all the faculty members who had offices in the building were in the school of metallurgical engineering.

MADDIN: Pretty much so.

CHOI: Apart from a few exceptions. But graduate students were from all different departments.

MADDIN: Yes.

CHOI: That's an interesting arrangement.

MADDIN: With a predominance of metallurgy graduate students living in the building. There were a number of graduate students who were taking their degrees in physics who have their office in the physics department but use the equipment in the LRSM. Again, it depends on their personality. If they wanted to mix, they mixed. And seminars and everything, they mixed. But I think we may have discussed many times how you force cooperation, and I don't think you can force anything.

CHOI: So wasn't there a concern that the metallurgy department is becoming a service department to physics and chemistry?

MADDIN: No, the head of each service unit, like the electron microscopy, was put under the charge of a faculty member. For example, I don't know who it is now, but there's now a complete building devoted for electron microscopy. There has to be a faculty at the head of that, but if he's any kind of a decent faculty, he assigns the operation of that tool to the man who actually does the work. So you might have at that time Doris Wilsdorf, the head of the mechanical testing facility, but she didn't actually approve what work was going on in there. Someone would come in and tell whoever was running the testing facility—I think it was [William] Romanov—"I have to find the tensile strength of this material." He would help them do it. He wasn't supposed to do the work himself. He was supposed to show the graduate student how to do it. The second time he'd come in, the graduate student was expected to know how to do it.

CHOI: I see.

MADDIN: The same thing worked with the scanning electron microscope. He would show the student how to use it and hope the student would be able to use it himself when he was assigned the time. If he needed help from the actual microscopist, he could ask for that help. He'd have to pay for it.

CHOI: So when was Lou Girifalco hired? Is that the early 1960s?

MADDIN: Late 1950s. It's a very interesting story on how he was hired. I went to a meeting in Cleveland [Ohio], late 1950s, and before I went, our faculty had been discussing how to increase our faculty, and in what areas. We didn't increase it at that time in terms of adding an individual, but more as an area in which we would find someone. And what we needed at that time, and we discussed this, is someone who understands the relationship between the physics and the experiment and how to prove a particular concept.

I went to a meeting in Cleveland and Lou Girifalco, I'd never met him, gave a paper. And I remember the nature of the paper. It was on creep of silver, in which he was using point defect theory and the experiments of creep to show how everything worked, and I was very much impressed with that. I went back, and I discussed this with the faculty, and they said, "Yes, sure, let's see if we can bring him in." And I called him and asked him if he would come and it generated from that, and that was late 1950s. Turned out to be a gem.

CHOI: And he also had some experience—

MADDIN: Hmm?

CHOI: Did he have experience in working with the electron microscopes at the time?

MADDIN: Say that—I'm a little hard of hearing.

CHOI: Electron microscopes? Was that one of his—?

MADDIN: No, he was primarily a theorist, but he knew how to relate an experiment—try to show what his theory was going to say. But he was primarily a theorist. I don't know whether he ever did an experiment really in his life.

CHOI: I see. Yes, that was the kind of person that I aspired to be as an undergraduate, a theorist without any experiments.

MADDIN: How long have you lived here now?

CHOI: Eight years now.

MADDIN: Your English is excellent. It probably had to be good when you got here.

CHOI: Yes, to some extent. Let's see. Then, when did the name of the department formally change to the department of materials science?

MADDIN: You know, that must have occupied the subject for at least six meetings under Hixson's direction. Burstein, Hughes and I would argue it. Burstein, coming from the physics department, wanted to call it Laboratory for Condensed Matter. Hughes really didn't care. And I was insisting upon having some way to indicate the structure properties relationship. So this was a compromise. Eli agreed, and I agreed we'd call it the Laboratory for the Structure of Matter, and he agreed not to call it condensed matter.

CHOI: Yes, condensed matter makes more sense for the physicists.

MADDIN: That's exactly right.

CHOI: But the metallurgy department itself…

MADDIN: Metallurgy department started off as metallurgical engineering. Then the next name changes—changes with the nature of the field itself, not necessarily the LRSM, although it had a big influence there. It changed from metallurgy to materials science. Yes, and then it changed to materials science.

That, I think, is an evolution as a field itself. You have to keep in mind, too, that in the middle of the 1960s, I organized a journal published by Elsevier in Holland called *Materials Science and Engineering*. It's now a well-known journal. It was vetted out of my office. I was the editor. My secretary was also partially secretary for the journal. So having a journal that became fairly well known, materials science as bridging the gap between engineering, and the department seemed to go that way. That was not the primary factor. The whole field was changing, and today you won't find a person who studies metallurgy anymore. He tells you materials science. That's sort of snobbish too, in the sense that metallurgy used to be considered—probably still to many—as like a blacksmith, heat 'em and beat 'em. Materials science has more of the aura of a science. That's what I mean by that.

CHOI: Yes, it's the only discipline within the engineering school that has science within the title. So I guess I was getting at the education side of it, the curriculum change, and you outlined a little bit of that in this paper that you did with Hobstetter.

MADDIN: I've forgotten what I wrote.

CHOI: You emphasized four areas that should be covered by the new curriculum, which is quantum mechanical foundations of materials, statistical thermodynamics of materials, kinetic phenomena and defect phenomena, which looks like a very solid foundation for—

MADDIN: Yes, that makes it heavily physics-oriented, doesn't it? Hmm.

CHOI: So was the quantum mechanics foundation actually taught within the metallurgy department in the early-1960s?

MADDIN: I think Girifalco taught a course one year on that. I don't think it was part of the required curriculum, ever became part of it. Graduate students—these are graduate students, I think, or undergraduate?

CHOI: Graduate.

MADDIN: Graduate students. At my time, the graduate student had to fulfill course requirements, and I forget what they were, but one of them was physical metallurgy, which really brought into it quantum mechanics as well. I know I taught one for two years, and I'm really vague on these points there. I can't really answer your question too well. Many of those concepts were not taught as the title shown there, but came within the course that a person taught. I think I, and other people, taught point and line defect theory.

If it were purely a course in quantum mechanics, we would advise them to go to the physics department. We would confer with the teacher in physics and tell them that we would like the students to cover that, keeping in mind that they come from a materials science background and not from a physics background. That's not to water down the course but that the emphasis is going to be on structure. So yes, it did come in, but not as in those names, although I think for a couple of years we did have courses called that.

CHOI: Quantum Mechanical Foundations.

MADDIN: But you can see what that's going to do. If you have a course in metallurgy or materials science called Quantum Mechanics and right across the street in the [physics] laboratory they have the same course, it sets up an area of friction. It doesn't always work out because at one point in Penn's history, and at other schools as well, there may have been at least four different thermodynamics courses being taught—thermodynamics in chemistry, thermodynamics in physics, thermodynamics in metallurgy and thermodynamics in chemical engineering. And all of the concepts were the same. If you try to eradicate that, you ran into trouble because—well, let me give you an example. At Harvard, I spent a few years after I retired from Penn in the anthropology department. Students in anthropology—archeology, primarily—needed to study statistics. They can't excavate a site without understanding statistics because they excavate probably two percent, maybe five percent of a whole site, and to reconstruct the whole picture, which brings in statistics quite a bit.

The graduate students, after two years, were assigned to a statistics course in the mathematics department. After two years, they came to us at Harvard and said, "We don't want that course. We don't like it. It is not applicable. So we were forced then to ask the math department to change their course, or teach our own course in statistics as applied to archeologists, which is slightly different. So that's why I say you can't teach those things and expect to get along with your colleagues and with your students.

CHOI: In here, you also say a very—make a very interesting point here, and I quote, "Particular attention must be paid to minimizing all barriers to contacts among the interacting groups. The most serious of these barriers may be architectural. A flight of stairs can be inhibiting. A street to cross can be intimidating. Artificial departmental barriers can also be formidable. On the other hand, every occasion must be seized to bring the group together in a stimulating atmosphere. Seminars and colloquia, but less formal discussions over tea are often more so."

MADDIN: Yes, that's a good—I had forgotten that point. We always in the LRSM sponsored, at least while I was there, a tea where all the students from different departments would at times come in and take their share of the biscuits we supplied. And that was good for mixing.

Let me make a point here. I don't know if we want to record this so turn it off for a moment.

CHOI: Okay, we're back. So was that kind of a tea an unusual event for Penn, for example?

MADDIN: Students looked forward to it. Some students would come down as the biscuits and things were being laid out.

CHOI: Well, grad students never say no to free food. [laughter]

MADDIN: And they really enjoyed that, I think.

CHOI: And this was hosted by the lab, LRSM.

MADDIN: Yes, they paid for that. Not departmental. I think the LRSM paid for it.

CHOI: Interesting. So I guess overall what you're saying is that it's more of this informal atmosphere that the lab created that made a big difference.

MADDIN: Yes, very informal. Thinking back at it—and I haven't thought back about it many times—that afternoon tea was probably the most important part of the students getting together during the day. How that spelled out later, socially, among the students, I don't know. We have no way of knowing where they lived and what they did. But I know they were interested in learning each other's work.

CHOI: That's interesting.

MADDIN: At least in one case, I had a graduate student who met her future husband in one of those teas. [laughter]

CHOI: That happens all the time. So throughout the 1960s, the LRSM became more or less established and became a fixture on campus. In the 1970s, the funding model changed from ARPA to NSF.

MADDIN: Yes, that's right.

CHOI: Were you involved in that process?

MADDIN: Yes, I was informed about it. I didn't know anything about it. I was informed about what we're doing. It didn't really affect how we operated. I think we had to present, as I recall, an extended budget for three years or so, and it was understood it would be renewed for three years, so we had a three-year continuity. I think that was it. You're taxing my memory now. But it didn't really make any difference between the way ARPA worked and the way NSF worked. It was seamless. We didn't see any difference in our operation, as I recall. There was an awful lot of talk about how this would change the nature of it, and it never did, as far as I know. Remember, I changed the area of my interest to early history. I was, hence, not fully aware of the workings of the LRSM.

CHOI: And throughout this period, you maintained your office on the second floor of LRSM until you retired.

MADDIN: Well, I gave up my office when I gave up the department chair and went to an office on the fourth floor. My office was on the southeast corner of the fourth floor, very nice corner office. The corner offices were the most desirable offices. There was a pecking order. If you were on the faculty longer, you got one of those. Because they were bigger.

CHOI: Yes, bigger, and you had windows on two sides.

MADDIN: Windows on both sides, yes. I kept that office from 1974 until I retired in 1983, nine years.

CHOI: Was there a noticeable difference in the personalities of the lab directors through these years? Was there a change from Hobstetter to Girifalco to Eugene Nixon, Donald Langenberg?

MADDIN: Oh, yes.

CHOI: And then Chad Graham for briefly as acting director, and then Alan Heeger in 1974. So the directorship changed from the metallurgy/materials science department to chemistry to physics somehow throughout this period. Did that make a difference in the operation? Or did any of these have any specific personalities that made a difference in the lab—

MADDIN: Oh, there were differences, but I don't attribute them to the discipline of the director as much as I do to the nature of the funding by the NSF. When NSF took over, shortly—not shortly after, but a few years afterwards, government funding for research started to decrease. So more and more, faculty that had been sponsored by the LRSM were encouraged to seek other support. In other words, write more proposals, because we saw that the funding was decreasing, not keeping up even with inflation, and we knew the handwriting was on the wall. And today, I think most of the money comes from independent research grants the faculty obtains from some other agency, NSF, perhaps. But that change of nature, I don't think was as much with the discipline of the director, as much with the change in the funding pattern of the government.

As I recall, every one of those directors, except perhaps one or two, were collegial, interested in research in materials—and were not primarily or not at all interested in turf, in maintaining turf. In a couple of cases, an accusation could be made.

Look at the men who were director there. Hobstetter became a very successful administrator. Don Langenberg went on to become the president of University of Iowa, then to the president of the University of Maryland and then chancellor of the Maryland university system. Girifalco went to the administration, has done a lot of work, mainly in the relation of science to the Wharton School's curriculum. He's written a couple of good books on that—*Mathematics of the*—something or other. I've seen one of them. Seen two of them. Who were the other directors?

CHOI: Eugene Nixon.

MADDIN: Nixon was not the most successful. He was—I hope I say he is, I don't know whether he is still alive. But he was a very retiring man, and he preferred never to get into an argument with anyone. So he was not as effective as the others. Not that he wasn't effective. He was not as effective as others. [Graham] was similar, the same way, as his term at LRSM and later on as head of the metallurgy department, or vice versa. He did not conduct a very collegial program in the sense that he didn't consult with the faculty before he made a decision, necessarily. He made his own decisions without collegial input, and that is not something you do with faculty people. It's not what you want to do.

CHOI: And of course, Alan Heeger went on to—

MADDIN: I don't remember Alan Heeger's time there.

CHOI: UC Santa Barbara now.

MADDIN: Yes, I've seen him since then. I saw him in Santa Barbara, and I wrote to him when he was awarded the Nobel Prize and got a nice letter in response. Heeger is a very fine gentlemen, and he ran a good program, although by that time I was in history, so although I had my office in the LRSM, I didn't have too much contact. I saw him at tea when I went down, but I was teaching in the anthropology department, or American Civilization, wherever I could get somewhat interested in teaching. So I was not really a member. I wasn't even on their budget, the metallurgy budget. I was on the provost budget.

CHOI: Right. Because you were the University Professor. That's interesting.

MADDIN: But I remember Alan Heeger as a very gracious, considerate man. He still is, I think. I had dinner with him in Santa Barbara in the early '80s.

CHOI: Yes, and one of my colleagues who's working on—his specialty is in the history of nanotechnology and the roots of what became nano. He did similar interviews with Alan Heeger and Alan MacDiarmid, separately, before Alan MacDiarmid passed away.

MADDIN: Yes, I heard that.

CHOI: And they were actually talking about the LRSM being this place where they—

MADDIN: Yes, it's a place where they were synthesized. That's where Alan MacDiarmid came to Alan Heeger and said, "Look, I think we ought to be able to do this," and Alan said yes, and they discussed it. That's what led to the work. So that was a good example of a chemist and a physicist getting together to compare their thoughts on the same issue bear fruit, and it did. It was an excellent example. I put that more to the personality of Heeger than to MacDiarmid. Heeger is a very outgoing person, gracious, listening to anyone who wants to talk to him.

CHOI: Okay.

MADDIN: In other words, let me summarize that. I think the existence of the LRSM had a lot to do with their cooperation. The primary reason they got together is because of the existence of the LRSM. What made it work, I think, was Alan Heeger's personality. So the factor there is two—there are two reasons why it worked so well. One, the existence; and the other, personality.

CHOI: Well, I had one other thing that I wanted to bring up, which is an article written by Norman Hixson in 1961 in a magazine called *Pennsylvania Triangle*.[2]

MADDIN: Yes, I don't know that. Let me see that. May I? Yes, go ahead. I don't remember ever seeing that.

CHOI: And he makes this point—well, this article is about how—in 1961, so right after the contract was signed, and the construction was about to begin.

MADDIN: 1961? Okay.

CHOI: And talks about interdisciplinary research and how it should be set up on campus. And he makes the point, and I quote, "it was obvious that some of our traditions had to be stretched considerably, perhaps even broken without in any way damaging our most important ones, the quality of our graduate and the independence of our thought." So that quote really caught my eye because he was thinking about there was this tradition that was about to be broken or was in the danger of being broken if we were not careful at the time. So—

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[2] A. Norman Hixson, "Pennsylvania's Newest Venture," *Pennsylvania Triangle* 49 (December 1961): 11-13.

MADDIN: I don't know what he's—I think what he's getting at is that—and I don't quite agree with him on that, by the way. In a university, departments are well defined. There's a barrier between the departments. Let me give you an example. When I taught—when I left Penn to go to Harvard after I retired, teaching in the anthropology department, I was teaching the same subject I did at Penn but under a different aegis. I was teaching the beginning of the use of metals and alloys going back to the earliest history. Because the department is so rigidly defined, it's in the anthropology department rather than the history of science or some other department. That's what I think he means. We're in danger of eliminating the department. But I think that's a good thing, not a bad thing.

Departments are much too rigid. Look what happened to the field of biophysics. A man at Penn by the name of Bronk, Detlev [W.] Bronk, who synthesized the field while he was at Penn, went down to Hopkins to become president, and he single-handedly broke down the barrier between physics and biology, and that's where biophysics come from.

I think materials science has to break the barrier, not with the physics—with the physics as we know it today or knew at that time, but it has to sort of lop off the solid state sciences into one field, and it won't do that. Physics will not let go of that, and that's why I don't think it will succeed, and that's what he probably means by being in danger of. But I think we would be better off at the university if the discipline was defined—

Well, let me—I'm losing my train of thought. Let's give you another example. Archeology. If you look at Penn or at Harvard or at [University of] Chicago, you find archaeologists in the art history department. You'll find them in the classical archeology department. You'll find them in the Middle East department. They're all archeologists. And if you ask them, as Oxford did a few years ago, to become one department, the departments wouldn't let them go because you're taking a stellar person from my department away from me.

If I went to physics and told Bill Stevens at the time, "I want Eli Burstein," they'd say, "Ha, ha, ha." They'd think I was foolish because he was one of their chief men in the physics department, and to let him go into a new department was something that no one wants to do, and that's what I think they meant by that. Do you follow my reasoning?

CHOI: Yes, yes, yes.

MADDIN: It just won't let it—it's a question of turf, but it turns out in the archaeology example that the archeologists I had in mind in the art history department are two of the most prolific, well-known people in our history. To let them go really diminishes their department, they think. Well, I won't beat the point.

CHOI: Okay, so shall we take another break?

[END OF AUDIO, FILE 1.2I]

MADDIN: As I recall, Dr. Choi, there was a director of the program, and he had an executive committee, almost always a representative from three departments—materials science, chemistry and physics. Each year—I forget what time of the year, probably in the early spring or early part of the year—everyone who was receiving support from the monies that came from NSF—the LRSM call them and they were asked to resubmit their requests for next year, and a letter went around to the communities, engineering and science, asking anyone who would seek support to send us the nature of the work they wanted to do, and how much money it would cost. Those requests came to the director, who passed it on to the executive committee. We were given time to absorb them. Then we would discuss at the executive meeting which ones we thought were worth supporting.

If, for example, someone from metallurgy asked for support, and he hadn't been doing very much with it—dogging it—we most likely would not have continued his support beyond a certain time because there are people involved, not only the faculty and the graduate students. So we couldn't cut anyone off summarily without some consideration for how his graduate students were being supported. This worked pretty well. There was an awful lot of turf protection, but I don't think it really got out of hand, and you could always tell when someone is wanting, for example, to put the laboratory next to the chemistry building. [laughter] Sorry about that, but you get the picture. That's the way the administration went on.

We would discuss—on a continuing basis, but not every week—the output of each of the scientists and faculty had that year or previous year. And in fact, I remember the LRSM—I think the NSF sponsored this through the LRSM—went to a research company up in the Boston area, Ajax Corporation or something, that evaluates government agencies, and they asked the—I forget what company; you can find it—to evaluate all of the LRSM-type materials sciences laboratories—Penn, Cornell and so forth—to find out whether the money spent in the past was worthwhile.

CHOI: In the 1970s? If it was NSF.

MADDIN: Yes, 1970s. That's right. I remember it because of my work on amorphous solids was cited. But that is part of the oversight that was being placed on us by the NSF and by us ourselves on our own people.

CHOI: So the executive committee met every week? Or—?

MADDIN: I think so. I think during the early stages, every week.

CHOI: And it was a committee of three plus the director.

MADDIN: Every year it would change. In effect, I would say, "You do it for the next two years," and ask a faculty to do it, but the committee almost always structured as one from metallurgy, one from physics and one from chemistry. It was not always the head of the department. It was never the head of the chemistry department—yes, one year, it was. It was never the head of the physics department. What was the name of that man? Nice man in chemistry, head of the chemistry department. I've forgotten his name now. He was on the committee. So it was always a representative. And I don't remember anyone doing it for more than two or three years at a time.

The oversight of the central laboratory, central facilities was pretty major, pretty much made by the director. He may call in someone to say, "What do you think of what Bill Romanov is doing in electron microscopy?"—or whatever field it was. But that was on an ad-hoc basis pretty much.

CHOI: Was there a case where a metallurgist was cut off from LRSM funding?

MADDIN: I expect there were cases, but I don't remember.

CHOI: Then what would that mean? Because his office will still be within the lab, but he won't get funding from the lab.

MADDIN: There was an understanding—and I'm afraid I insisted upon it more than anybody else—that the metallurgy department occupied space in LRSM. As a member of an academic department, not necessarily as a member of a research organization. And I remember pushing that as hard as I can because I didn't want in the future to see somebody saying, "All you metallurgists, go back to the Towne Building," so I wanted to establish a right of eminent domain, if you want put it. You follow my reasoning? So the metallurgy department—that's why if you go to the LRSM, and you look at the board on your left that has all the people in it, it has the LRSM people, and then below it there's the materials science department separately. That's the reason for that.

CHOI: I still don't follow. So—

MADDIN: No, no. Keep asking. Maybe I can get to it. Maybe I'm not answering. You asked specifically, "Was there anybody from metallurgy ever cut off," and I says, "I think there were, but I can't speak…"

CHOI: Right. No, I mean, the part where the importance of keeping the two separate for you.

MADDIN: Yes, well—let me give you an example. Take the Museum of Archaeology and Ethnology at Penn, the one on 33rd and—what is it? Whatever the street is. It's a museum. Its existence came about through efforts by the state of Pennsylvania to set it up. The anthropology department has offices in the museum. Therefore, they have a position in the museum and a position on the faculty. Suppose the curator or the director of the museum, who may not be a faculty member, but he always is, says, "You in the classics department, you're taking up space I need. You have to go back someplace else." So that department has to find someplace in the university if he's going to agree to be eliminated.

I didn't want the situation to arise that some future director of LRSM would come to the department of metallurgy and says, "You have three people here who have nothing to do with the LRSM. They're doing their own research, independently. They shouldn't be here." I didn't want that situation to arise because I had no home other than the LRSM, "I" that is as a department. I originally came from the Towne Building. I can't say, "I'm going to go back to the Towne Building," because that's not open to me. So I wanted to be sure that in perpetuity, metallurgy would be located in the LRSM building as an academic department. There's a distinction there.

CHOI: So even though a couple of faculty members were deemed by the LRSM executive committee to be not as relevant to the whole mission of the center, to the lab, they were still retained within—

MADDIN: They would still retain their office. It's up to the department, if they're not prolific in any other way—when it comes promotion time or raise of rank or salary—to reflect that, but that's independent of LRSM. That's a departmental function. Purely academic.

CHOI: So how did the people from other departments respond to that? I'm sure they weren't very happy.

MADDIN: They weren't very happy about it, and it produced a lot of argument, and I expect I didn't make any friends that time because I was insistent upon in, and I was insistent with the vice president at the time, the dean, to back me up on that. Of course, the dean of engineering was happy to back me up because he didn't want another department going into his space there.

So there's a lot of politics involved there. But my point of view was quite simple. We were primarily a university academic department, not primarily a research section. Slight distinction.

As it turned out, I think—I forget who it was who was cut off, but the people who were cut off probably had other sources of money anyways. They weren't cut off from using a central facility, for which they had to pay, but they were cut off from other support for their research. For example, suppose a faculty member is saying, "Your work is not what we want to continue sponsoring, so after the end of this year, you're no longer a member of this—you're no longer doing research under the sponsorship of the LRSM. He may have other research that he's doing. He could do it in the LRSM building. He would have to pay for everything, but not for his office space. Nobody paid for their office space as such.

CHOI: I see that. But what I find very interesting is the singularity with which the metallurgy department had this kind of an ownership of the LRSM as opposed to other departments, how that came to be. If it was created as a trilateral cooperation of the three departments, what made the metallurgy department more claim and ownership in that whole enterprise?

MADDIN: I follow your point. How it is since I left, effectively in 1973, let's say—I stayed there until '83, but I was no longer involved there. That's a good example. I didn't receive any money from the LRSM, yet I occupied an office there. I continued my work. And nobody, as far as I know, resented it.

CHOI: Well, that's maybe perhaps it was because it was you, and a long—

MADDIN: No, but the point is made that suppose I were not a University Professor. I was just an ordinary professor. The same situation would have been there. The fact that I had a more elegant reputation just adds onto it because that means if they grumbled, they grumbled alone so I couldn't hear it.

But I'm trying to think of an example of someone in my department—I can't think of any, but there had to be somebody. Let's see. I'm sure I recall—but not vividly—cutting off people who were not doing work that we thought should have been done by them. But I don't remember any repercussions from it—lasting repercussions.

CHOI: But my original question was how were you able to make the argument that the entire materials science department should remain as a group within the lab as opposed to the other departments who had only selected members who came in?

MADDIN: Dr. Choi, all I can remember is that that was a very severe battle, and I forget my logic I used at the time, but I virtually insisted on it, not quite giving an ultimatum—"If not so, we're withdrawing completely from the department."—but the threat was there. I would not have gotten away with it, by the way, but the threat was there. But I do recall very vividly arguing that point, saying, "As a department, we will not be separated." I remember the dean of engineering, who wanted me to have his main office in the Towne Building, and saying to him, "No, I will not be separated," and the same thing for other people. I say you cannot run a cohesive program where you're separated from the faculty, and I still believe that.

How it would have succeeded had I and other people who were prominent in the program lived in a different place, I don't know. I know that in physics, very, very few of the stellar people in physics maintained an office at LRSM. Their main office—their only office—was in physics.

CHOI: Well, I can see a logic there that within a physics department, only a small number of solid state physicists were involved, and in chemistry, inorganic chemistry.

MADDIN: The problem, as I envisioned it—I can articulate it now and probably had been able to articulate it at that time. To have a truly synthesis of a new field like materials science, we would have had to lop off solid state physics, lop off solid state chemistry, bring them all together in what is now the LRSM, have them all live there. We were not able to do that. The physicists would not let us. The solid state physicists themselves didn't want to. Their glue was to the physics. They didn't want to be tarnished by the mundane metallurgy department. You see the logic there? And I can understand that.

As far as I know, there was no materials science department throughout the United States that is truly a new discipline in the sense that they now have people—maybe—well, maybe I'm being a little rash. In 2008, some 28 years later—no, I don't know what the number is. Almost 30 years later. Today, I see people on the metallurgy faculty who come from polymer chemistry. I see people from mechanical engineering who are now in the metallurgy department, electrical engineering. That was not so prominent during my days there. Apparently it's more prominent there. The MIT faculty, at one point, had gone so overboard from bringing people in from other disciplines to join their faculty that the big criticism they realized right away that the core of metallurgy was no longer there, and they had to make amends for that because the core of materials science has to be the relationship between structure and properties, and that is new metallurgy. Well, that's pretty strong, but that's essentially it.

CHOI: I know a little bit about MIT, and I know that they had a very strong physics dominance in the field of materials science at MIT, and John Clarke Slater—

MADDIN: I know John, yes.

CHOI: Who was instrumental in setting up the materials research center at MIT. So my impression there was that the solid state physicists were taking the lead in setting up the MIT center.

MADDIN: Well, the only person I know there was that woman. What's her name? A distinguished lady in electrical engineering. She had a big part of it. My memory's getting so bad. Well, I've forgotten anyway. But I was fairly close to people in the MIT department like Morris Cohen and Cyril Smith, when he was there, but he was an Institute Professor with Mert Fleming and other people there. And my impression there, no…not impression, the statements they made to me was that they had gone so overboard in bringing in people from other disciplines that they had not nurtured the core of the metallurgy section that had been structure/properties relationship, and they started to switch. I still think that the core of materials science is the structure/properties relationship, however you want to define that. All right, okay. I'll stop there.

CHOI: So did you have any interactions with other materials centers like Cornell and Northwestern?

MADDIN: Not very much. I had a little bit with Illinois. At MIT I was a close friend of Morris Cohen's, and besides, I lived in the Cape Cod [Massachusetts] area, so I was going up there quite often. And when I was teaching at Harvard, I had students from MIT come over and take my course. So I had a more working relationship, but I didn't have that with Cornell nor with Northwestern. I knew Walter Owen when he was there. Illinois, not too—a little bit with Illinois. Very little with Stanford. That's pretty much it.

CHOI: Does the name Robert Sproull mean anything to you? Sproull?

MADDIN: Oh, yes. Sure. Sure. He was at Cornell, and he was probably the most distinguished solid-stater. He became president of Cornell, Sproull. Yes, I met and talked with him, but I didn't have much interaction with him, no.

CHOI: All right, so, let's move quickly forward. The other thing that I'm somewhat interested is how this model of interdisciplinary research affected the way that science and engineering and research was being done on the Penn campus. Do you see this as a model that was replicated in subsequent ventures?

MADDIN: Yes, I think so. I think the emergence of bioengineering at Penn and at other places probably was helped by our experience in materials science. In fact, there were people in materials science, LRSM, who spun off into that area, like Sol [Solomon R.] Pollack, and others into the bioengineering aspect. So I think they benefited from what we did. I'm not sure it came in everything they did later was from a blueprint they got from us, but I think they were affected by it.

Where else? You know, there was a little interplay with electrical engineering through people like John Fischer, and there was certain interaction with chemical engineering because of John Quinn—more John Quinn than Stu Churchill. Mechanical engineering is a little bit different. That's another story completely. Civil [engineering], no interaction, as far as I know. The biggest interaction was the development of the bioengineering.

CHOI: What about the medical school? You mentioned that the dentistry students came—

MADDIN: Well, that's an interesting, at least to me, story. When I first came to Penn in the 1950s, shortly after I came there, I was appointed to a committee by the Senate, a faculty trustees committee. An effort to get the trustees to see problems the faculty had and vice versa. And at one meeting, I met a man by the name of Lester Burkett, a member of the trustees, who many years earlier had been dean of the dental school. And we were talking about the education of a dentist, and someone—either I or Burkett—made some statement to the fact that dentists should understand materials because they need materials for filling. They need materials for facial construction and things like that.

So I returned to my department and suggested—this was to John Hobstetter—that perhaps he should design a course for materials science that students of dentistry would take, and that was the beginning of that particular course, a letter to which I think you referred to. I think you referred to the letter. But we initiated a program that permitted a student who had finished his dental work, or was almost finishing, to enroll in our department for a Ph.D. in materials science at the same time he was finishing dentistry, a joint program. And we had a number of students. The most successful one was a man, an Algerian man by the name of Louqassi Armand who is now doing great work at the University of San Francisco. So that's how that generated. It was sort of interdisciplinary, but didn't bring too many disciplines in.

CHOI: Not as much as with the medical school?

MADDIN: Not as much with the medical school. The medical school had a bigger interaction with what turned out to be bioengineering.

CHOI: Once-removed interaction.

MADDIN: Yes.

CHOI: So have you been keeping up with what Dawn Bonnell has been doing?

MADDIN: No, I—

CHOI: The NBIC, the Nano Bio-Interface Center?

MADDIN: No, I've heard some great reports about her, and I've met her, and I think I saw her at a gathering of the faculty last November or December, but I don't know what she's doing scholarly or scientifically. I did visit the department previously—I'll have to tell you about that—and asked to see her, but she was on leave, so I never did see her. I would have asked her what she was doing. She was in Europe someplace or something. Quite a good addition to the materials faculty, I think.

CHOI: So in general, have you been following what—in the scientific—well, U.S. scientific engineering?

MADDIN: Not really. I've been mostly involved in history, almost completely.

CHOI: So any observations on the recent nanotechnology field?

MADDIN: I have a very impressive book that [Eduardo D.] Glandt put out called *Nanotechnology at Penn*, which shows pictures and little bios of every member of their unit. I'm very much impressed with that. As a matter of fact, I told him so. Is that what you're referring to? Not necessarily that book, but the actual program?

CHOI: I was referring to more generally in the United States in recent…

MADDIN: Well, I don't know anything firsthand about it. I read what's published in the newspaper. I read the almanac that comes every week from Penn, so I hear about it. I read about it online. But no, I haven't kept up with it.

CHOI: Okay, so moving onto your second career.

MADDIN: Yes.

CHOI: So how did this all begin?

MADDIN: Well, it's hard to find the one aspect. I think I was subliminally interested from my conversations and observance of Cyril Smith, first thing. His scholarly work and his scholarly attitude is what, I think, really impressed me. The next thing is that in 1972 when I was promoted to University Professor, which meant I was free to teach in any department I wanted. I had a visit from the director [of the University of Pennsylvania Museum of Archaeology and Anthropology] at that time, Froehlich Rainey, and he came to me with a problem. He said that some people in present day Iran were suspected of pooling their resources in a little village and buying up gold and having some artisan construct something and going 50 or 60 kilometers away and burying it and then arrange a few years later to dig it up, and the value would enhance many fold. The problem was could I tell the difference between old gold and new gold. And I thought about the problem, and I said, no, I couldn't because so much of the gold in use is remelted old gold, that you can't have a trace element profile because it would be contaminated. But that got me interested a little bit more. Not in that problem.

I think the next thing that piqued my interest was when—I forget who it was from the museum came to me with two bronze bracelets. Not decorated, just bronze bracelets, and told me that they were—and he said 6,000 years old, fourth millennium, and they came from Thailand, and would I study it and see what was done. That got me interested because they let me take a piece out of it. Normally you can't take a piece out of an artifact.

All of that led to interest, and my time at Oxford at the Ashmolean Museum—had a curator there by the name of P.R.S. [Peter Roger Stuart] Moorey, now dead—curator of Near Eastern Art, with whom I had many conversations, and he came later to Penn, and I met with him for lunch. He got me interested, saying that metallurgists have a lot to contribute to information the archaeologist needs.

So little by little I became interested, and about the same time, Cyril Smith wrote me a letter to say that there was an excavation in Eastern Turkey called Cayönü Tepesi in which pieces of copper are suspected of being native copper. And said he had some pieces of copper that were found at Cayönü that date back to the eighth millennium B.C., 10,000 years old. He suspected, and a very prominent German metallurgist confirmed, that they had been worked and annealed, that is he showed long annealing twins. I examined the copper at the request of the director of the excavation, a man by the name of Robert Braidwood. This is going back some years into the early 1970s—middle 1970s. And I looked at it, and I said, "No, they're wrong. This is not normal annealing twins, but it was annealed during geographic time, not by someone who had worked it; that the working had been induced in the copper by means of geological forces, the earth moving. This was a slow recrystallization process.

So I got involved with that, and then the more I did, the more interested I became, and it led to that. And having the authority to do pretty much what I wanted, I initiated a course in anthropology on the beginning of the use of metals, attended almost always by archaeologists and history of science students, not many, but mainly archaeologists. The course was very successful. Taught it a number of years. I also taught a course in American Civilization with a man by the name of Galvin Anthony on materials used in eighteenth century navigation equipment, like astrolobes and various navigation—and mainly brasses. So we taught a course there. But little by little, the more I became interested, the more I became interested. So it fed on itself.

And by the time I was due to retire, I reached my 65th birthday, I had been asked by Harvard to join its faculty. I knew I was already committed, and that's how it generated.

The odd thing about it, and I always find this ironic, is that I taught the same course. It could have been in the metallurgy department. At Harvard it was in the anthropology department. At Penn it was in the anthropology department. Same subject. That's the ironic thing about it.

CHOI: But the students who took the course—

MADDIN: Hmm?

CHOI: The people who took the course were from different departments.

MADDIN: Yes, mainly—at Penn, they were pretty much all anthropology students. At Harvard, they were varied. There were some undergraduate students who saw the write up in the student book, who said, "I'm interested," and there was a young lady from MIT who came over—she was a materials science student—said she would like to take my course.

I say that because that's a very interesting story. I taught the course. That summer, this young lady called me and said she would like to come talk to me about a future in early metallurgy. So I told her to come down; her name is Blythe McCarthy. She says she comes from the Baltimore area. She would like to pursue her doctorate in materials science but with a strong interest in history. At the time at Hopkins in the metallurgy department, the name by Robert Green, there was also a man, an Israeli, by the name of Rosen. And I said, "They have an effort there," which they did. "Why don't you apply to them and see whether they'll accept you and do—

CHOI: Was this the lady who worked on the Titanic?

MADDIN: It may well be. I'll tell you what happened anyway. She got her doctorate, and I forgot completely about her. About two years ago, I went a lecture at the Freer Gallery in Washington devoted to Asian Art, by Tom [W. Thomas] Chase, a man I know, who's a conservator. He used to work at the Freer.

So I went there, and he saw me in the audience. We know each other, and he came up and said hello, and he says, "Would you like to see my laboratory?" So there was time before his lecture, so I went with him, and he introduced me to the people. There were three or four people there, one of whom was this Blythe McCarthy. Now, I did not remember her. He introduced, and I said, "Pleased to meet you," and she looked at me and said, "Don't you remember me?" And I said, "No." And she told me, and I remembered the incident. She is now the head of the conservation laboratory, and she's probably the one who did the work on the Titanic rivets.

CHOI: I think so because that was a project sponsored by the National Institute for Standards and Technology.

MADDIN: I don't know who did that, but Blythe McCarthy's now well known there.

CHOI: Right. To work on the remains on the Titanic to determine how the ship went under.

MADDIN: Yes, I don't know that. I know the recent stories about the rivets being—having too much slag in them. That's why they failed. I heard that story ten years ago. I don't really believe it. They examined four rivets.

Okay. You asked me how I became—and who took the course. Various people. Primarily anthropology students, though. There weren't that large of number, 30 or so.

But my recent utterings, let me put it that way, like the ancient mariner, is trying to understand the metallurgy during the early years—the metallurgy of iron during the early years of the Iron Age in the Eastern Mediterranean. You have to add that because the Iron Age in the Far East is different than the Iron Age in the Near East. It's a phase difference. Iron was first introduced in China in about the seventh century B.C., which is almost a thousand years later than it was in Eastern Mediterranean.

CHOI: So how do you do the research for something like this? Do you collaborate with archeologists? Do you collect the specimens and—

MADDIN: It's very interesting. First of all, you have to get the archaeologists, or at least the director of the excavation—if he's excavating it at present time—to allow you to take pieces. I could do the work simply by taking drillings, but that only gives you the element composition, which is not always what you need. You need a little hunk so you can look at the structure, tell how it was worked, what was done, its thermal mechanical history.

That means that in most cases you have to convince a curator in a museum that what I will supply to him, if he lets me take a sample, is more important than the destruction of the artifact; over which he has control and is tasked with keeping it as it was. That's not always easy to do. Quite often, if he has two knives, let's say, one of which is broken, so it's not for exhibit, he'll let you have a piece of the broken one which is adequate. But more often than not, he's not going to let you take a piece of an item that is exhibited and show big notch taken by a man, which you can see in some exhibit down in Philadelphia.

Well, what I do is then study the metallography. I will determine the trace elements in there. Let me set up a hypothetical problem. On a shipwreck off the coast in Turkey that sank in 1300 B.C., there was a cargo of copper and tin. Let's say the copper consists of 160 ingots of copper. So you take a piece from each one of the—a representative sample, let's say. And you have elemental analyses on all of them by some reliable quantitative technique, like ICP or something. So you have a matrix, let's say, copper 1 to 20 here. Trace elements right in here. Arsenic, zinc, lead, so forth and so on. If you have all those values, you can set up a statistical program to see if there's any grouping. Suppose that three of the ten group very nicely in a tight group as compared with all the others. Then you can probably safely say—not with certainty, that these three came from a different location than the others. That sort of work.

What else can I do? Well, metallography, element analysis, physical analysis. These days I can't, but people can take a sample of iron for the accelerator and date it from accelerated dating and get the date on it. Normally, you can't do that. You need a big accelerator to do that, and it's done in a few places in the United States, a few places in Europe. I personally don't do that.

Another thing that I do not do that I should be able to do, that if I were younger I would do, is measure the isotope ratio in lead. There is copper. Quite often one of the trace elements is lead. Lead has a number of isotopes that are natural; are not affected by melting, smelting or anything like that. If you know the ratio of, say, 206 to 207, which you can measure with a masspectrograph, and you know the ore sources around the area, and you have the same ratio in one ore source as compared with these, then you can make a fairly good one to one conclusion that that lead came from that body of copper, whatever it is. So that's something I can do. Can't do personally, but can get done. So a lot of things I can do.

CHOI: So since the early 1970s, you have been working on this.

MADDIN: Yes.

CHOI: And you maintain a lab first at Penn and then up at Harvard.

MADDIN: Yes, and I set up a small lab in my own home on Cape Cod, with a microscope and grinding things like that, in my basement.

CHOI: That's very interesting. So when did you stop actually working in the lab?

MADDIN: A couple of years ago. I went to Penn to use their facilities, the LRSM, and studying a very interesting problem. Do you know about the Dead Sea Scrolls? Well, it's a long story. Just after the end of the Second World War, a Bedouin shepherd sold a scroll that he had found in a cave in the Judean desert. That was bought by the Israelis; they found that plus other fragments showing that it was a story written in the third century before Christ, written by, it turned out, a sect called Essenes. This was secreted away in these caves; these are known as the Dead Sea Scrolls. Many volumes have been written about it.

About three years ago, a piece of zinc sheet was found in an adjoining cave. The question was: is this piece of zinc sheet the same age as the scrolls, roughly third century B.C.? The big question here is that zinc was not discovered as metal zinc until the third century A.D., some 600 years later, and in India. Zinc cannot be smelted readily because it vaporizes and oxidizes. The vapor pressure of zinc is very low, which means that a retort system must be assembled so the zinc would condense on the walls. That process was discovered by an Indian archaeologist, Hedge. The date on that was third century after Christ.

If this zinc sheet was 500, 600 years earlier, how is it there? Interesting question. Furthermore, there is a piece of zinc sheet in the Agora in Athens, which a number of people have investigated and showed that the zinc was well dated to before third century before Christ. A piece of the zinc that I was referring to was sent to be from Jerusalem. I went to Penn to use their equipment to see what I could find out. I used their electron microscope and their optical equipment , and that was the last time I used a laboratory.

I have to supplement that a little bit. In 1989, I'd been out of Penn for six years. I was awarded a Distinguished—I'll give you its full title—Senior Scientist Research Fellowship by the Alexander von Humboldt Foundation in Germany, which permitted me and my wife to spend a year in Germany. So I went to Northwest Germany, to a place called Bochum. If you know Germany, it's a—do you know where Dortmund is and Frankfurt? Around the Ruhr district, Bochum is one of those cities (a big city, 500,000). It has probably one of the most foremost museums, mining museums in the world, and has a research laboratory. So I spent time with them, and I've been going back pretty much until about five years ago. I use their equipment. By the way, they have extremely good equipment, much better than we have in this country.

That's pretty much it. I'm still doing a little writing, but I'm not doing any laboratory work at all.

CHOI: Okay. I don't have any more questions.

MADDIN: All right, well—you can turn this thing off, I guess.

CHOI: Well, thank you very much for your time.

[END OF AUDIO, FILE 1.3]

[END OF INTERVIEW]

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