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University of California
Berkeley, California

Global Mining and Materials Research Oral History Project

Thaddeus Massalski:
The Development of Theoretical and Experimental Metallurgy after World War II

Interviews conducted by
Paul Burnett
in 2015

Since 1954 the Oral History Center of the Bancroft Library, formerly the Regional Oral History Office, has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the nation. Oral History is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is bound with photographs and illustrative materials and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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Thaddeus Massalski

Thaddeus "Ted" Massalski is Professor Emeritus of Materials Science, Engineering and Physics, Carnegie Mellon University, formerly one of the directors of the Mellon Institute, and Institute Professor. Ted Massalski was born in Warsaw, Poland, which he left at age 16 to fight with the Polish Second Corps in the British 8^{th} Army during World War II. He stayed in Italy to begin his college education, which he then completed in London and Birmingham. His academic career began in 1955 at the Institute for the Study of Metals at the University of Chicago. As confirmed by his curriculum vitae at the back of this oral history, Dr. Massalski is the author of hundreds of publications and several key scientific discoveries over the decades of his service to the field of metallurgy.

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Global Mining and Materials Research Project

For over twenty years, the Regional Oral History Office (ROHO) produced in-depth oral histories of members of the mining community, under a project called "Western Mining in the Twentieth Century," which was overseen by Eleanor and Langan Swent, Douglas Fuerstenau and others. http://bancroft.berkeley.edu/ROHO/projects/mining/index.html The 104 interviews in the project covered the history of mining in the American Southwest, Mexico, South America, and Australia from the 1940s until the 1990s.

ROHO has recently changed its name to the Oral History Center of the Bancroft Library, and with that change we proudly announce a new project entitled "Global Mining and Materials Research," which will focus on key transitions in technology, policy, and geopolitics that have brought mining to its current state worldwide.

Much has changed in mining industries in the years since the Western Mining project was in full production, including the increased globalization of mining operations, the decreasing concentration of mineable minerals in ore, increasingly complicated regulatory environments, new systems of environmental remediation, new technology for exploration, extraction, and processing, and new stories of political conflict and resolution. In addition to collecting interviews about mining engineering, metallurgy, and administration, we also hope to explore the history of information technology and data analysis with respect to mining, as well as the legal, regulatory, and policy history of the industries.

This interview was funded with support from the American Institute of Mining Engineers, Metallurgists, and Petroleum Engineers (AIME), the Society for Mining, Metallurgy, and Exploration (SME), the Association for Iron & Steel Technology (AIST), the Minerals, Metals, & Materials Society (TMS), and the Society of Petroleum Engineers (SPE). We are also collaborating with the IEEE to host these oral histories on the Engineering and Technology History Website, located here: http://ethw.org/Oral-History:List_of_all_Oral_Histories. Thanks also to former Western Mining Project Lead Eleanor Swent, Dr. Douglas Fuerstenau, and Noel Kirschenbaum for their advice and support while the Global Mining Project was being established. Finally, we are most grateful to Ted Massalski for taking time out of a busy schedule to speak to us about the evolution of the mining industry over the past forty years.

Paul Burnett, Berkeley, CA, 2015

Interview #1 March 16, 2015
[Audio File 1]

01-00:00:09
Burnett: This is Paul Burnett interviewing Dr. Ted Massalski for the Global Mining and Materials Research Project. It's March 16, 2015 and we're here in Orlando, Florida at the TMS Conference. So, welcome, Dr. Massalski. It's a pleasure to meet you.

01-00:00:32
Massalski: Likewise.

01-00:00:35
Burnett: It's customary in life histories, even if they're abbreviated, to begin at the beginning. So can you tell us a little bit about where you were born and where you grew up?

01-00:00:49
Massalski: All right. I think I've already mentioned to you earlier, I was born in Warsaw, in Poland. My parents were both professionals. My father was actually involved in the Polish government. My mother was one of the few women at that time who had a university doctorate. So I grew up in an – I wouldn't say unusual, but certainly a cultural background which promoted some of the things that happened to me when I was young. I never went to school. I was taught mainly at home. I started learning English when I was, I think, about six. I think the idea was that I would go to England for university studies one day. So, by the time I left Poland, which was very abrupt, incidentally, and I will mention this later, I spoke quite fluent English, which was an advantage.

In March of 1939 I sat for the examination to enter high school. The Second World War started on September 1, 1939. For America this happened a little later, in 1940, but for Poles that was the day. As you probably know, the Germans attacked Poland and very quickly reached Warsaw. But prior to that event, in March, I sat for the entrance examination to high school. My father decided that I should go to a Polish high school in a normal way. But I had some difficulty. I passed, as expected, all the examination subjects. But I had difficulty being admitted because there was prejudice against people who wrote with the left hand in those days.

01-00:02:59
Burnett: I am well aware of this prejudice. [laughter]

01-00:03:01
Massalski: Okay. I use the word prejudice because I don't know really how strictly this was enforced. But anyway, I spent the summer of 1939 learning how to write with my right hand. I'm now what they call ambidextrous. I actually can use both hands, which is kind of unusual. And if I want to show off in the class I move the chalk. [laughter]

01-00:03:30
Burnett: [laughter] You flip from hand to hand.

01-00:03:32
Massalski: Yes.

01-00:03:32
Burnett: Yeah, yeah.

01-00:03:33
Massalski: Although I don't do it as often as I used to. Okay. Then comes the war. Devastating to Poland.

01-00:03:42
Burnett: Of course. Yeah.

01-00:03:43
Massalski: We had actually another residence in addition to Warsaw, in a Polish city called Częstochowa, which my parents bought because my mother wanted to work professionally. She became a headmistress of a high school run by nuns, the sisters of Nazareth, a school for girls. In those days the government insisted that the person who was the director of the school would not be a nun. And so they found my mother. She accepted it. So there was a little bit of tension between father, who lived in Warsaw, and mother who lived in another city for a few years. And I was kind of shifted from one place to another sometimes.

I will skip the very difficult years of the war, except to say that I left Poland completely unexpectedly, at less than 24 hours' notice. A lady friend of my parents came for an afternoon visit, let me call her Mrs. S, and said that tomorrow she was going to Switzerland and, if they agreed, she would include me with the group of people who were going to go with her. My parents did not know this, but she was actually in charge of a secret underground transit route run by the Polish Home Army, whereby Allied pilots and other aircraft personnel who got shot down over Germany could be smuggled out of Poland to get back home via a neutral country. They knew from earlier instructions that if they survived the crash and managed to migrate without detection, from Germany into Poland, the secret underground Polish Home Army (the resistance) would take care of them, and taking care of them meant that they would be eventually assembled into small groups, provided with the Home Army manufactured false German passports, and essentially shipped to Switzerland via a complicated route.

These kinds of avenues of escape weren't lasting too long, maybe six months, because the Germans were constantly looking for them. The person in charge, like Mrs. S, was always promised that she (and also her children) would go with her in the last transit attempt. The Gestapo, the German Gestapo, would, of course, try to trace these attempts of escape. And from Warsaw, and from probably many other countries in Europe there were these secret routes going to neutral countries like Sweden, or Switzerland, or Turkey, or some other place. So this lady friend of my parents, Mrs. S, explained to my parents what she was doing for the Home Army, which of course was tremendously dangerous, if detected. She essentially said, "tomorrow I am going on a trip to Switzerland and if you agree that your son goes with me, I will include him in my party." I was then maybe sixteen.

Actually I was a friend of her son, and it was he who suggested to mother "let's take Ted with us." She had five children and they were, of course, included with this last transit group, but I think that in the party there were also about 15 other people, most of them Canadians and Americans, all with manufactured false passports, pretending to be German. And we went actually in a German military train carrying the wounded from the Eastern Front. So, overnight, I became a Hans Obermoser. This was my manufactured German name. I was supposedly born in Kiev, and I was one of the German escapees from the Red Army (which was now moving in from the east across Poland). It was, of course, all concocted.

To cut again a long story short, it didn't work as expected. In other words, when we got very close to the Swiss frontier, we were supposed to be temporarily housed on a German farm very near to the frontier. It was in Austria on the Lake Constance, Bodensee, which was a couple of miles from the Swiss frontier. And the German manager in charge there, the *Gauleiter*, was secretly bought (bribed) by the Allies through contacts from the other side in neighboring Switzerland, to be paid copiously after the war. And so he housed people who arrived, by pretending that they were kind of recruited farm workers; and then when an opportunity arose, they were shipped to Switzerland at night across a small bridge over one of the small estuaries over the river Rhine which was the border. However, it turned out that very soon after our group arrived, the local Gestapo already knew who we were, and so there was a raid one night and people got arrested, and some shot. Mrs. S got caught and later died in Buchenwald, one of the German concentration camps. I escaped into the fields and into the night. I had a brief session with my friend who had his four other young sisters now to look after. We tried to decide what to do. He said, "You go. I'll stay." And so I crossed to Switzerland overnight under a small bridge (full of Germans on top) and I managed to get to the other side. I bumped into a couple of escaping Frenchmen on my way, whom I encountered during that night when marching towards the frontier across the fields.

Later on, years later, maybe fifty, I visited that area again with my wife and the local people couldn't believe that I could have done this frontier crossing, because apparently the one-mile of the land before the frontier was all mined. And so I went over these mines somehow.

01-00:09:39
Burnett: You had no idea.

01-00:09:40
Massalski: No idea. No.

01-00:09:41
Burnett: And you just managed.

01-00:09:43
Massalski: I don't know how. All right. Then I got to Switzerland. Eventually I got to the Polish embassy there, which was the embassy representing the exiled Polish government in London. Poland ran an exile government during the war. And they knew my father and they were trying to decide what to do with the young Massalski. And after a while they decided I should go and join the army. And so again, through very complicated routes, I was shipped through France to Marseilles and I reached southern Italy later in a British destroyer. The Allies had already landed in the south of France and also captured a portion of southern and middle Italy. After I landed in southern Italy I volunteered to join the Army and eventually I became a signals trained soldier.

01-00:10:38
Burnett: So this was the British Eighth Army. This is the one that had to come—

01-00:10:41
Massalski: It was Montgomery's Eighth Army. There were three Polish divisions, or maybe more, maybe four Polish divisions. I had various trainings and I was finally trained as a signals person. I finished the war in uniform. There was a little problem with my age when I volunteered. I was a little too young, but we've engineered this. [laughter]

01-00:11:12
Burnett: [laughter] They needed all the help they could get, I think.

01-00:11:15
Massalski: Yes. Yes.

01-00:11:16
Burnett: Can I ask, were your parents in a persecuted position? In other words, did they feel that the timing was very propitious for you to go, or was it just an opportunity that presented itself?

01-00:11:35
Massalski: I suppose it was both. When I was leaving, we could already hear the Red Army guns. In other words, this was the time when the Red Army was moving through Poland. They may have been another hundred, two hundred miles away. But it was obvious that there was going to be a complete and major change. My father was hiding during the war from the Germans, and successfully, by various ways. My mother, my sister and I lived reasonably okay, considering the terrible war conditions. A Home Army uprising was

about to happen in Warsaw. But my parents knew that another huge turmoil was coming from the East, and so I suppose they decided during that afternoon when Mrs. S came to talk to us that there was an unexpected opportunity for me. They took the risk. I had no contact with them for the next thirteen years. You have any questions on that? This might take a bottle of red wine.

01-00:12:29
Burnett: Yeah. Or three.

01-00:12:33
Massalski: Or three.

01-00:12:36
Burnett: Well, there are so many questions, of course, about that time. And it was such an important time in your life and for so many millions of people. The question I had just for our purposes, I think, in this interview, is one thing that I always wonder about is for people who have gone through such harrowing experiences, what do they take with them to the world of comparative stability? In other words, when you became established in the United States how did those experiences shape your life in the United States, the decisions you made about your career? Did it have an effect that you can notice was different from those of others of your generation who did not go through that?

01-00:13:32
Massalski: Oh, of course. After my military career I was in Italy three-and-a-half-years in the end because when the war finished I went to a university there (Torino) for a year and a bit, still as a member of the military. That's another story. I had learned, of course, Italian already. So by that time I already spoke three languages, and quite a bit of German. And when I went, some forty years later, to the University of Turin, to receive an honorary doctorate there, I actually started my speech in Italian and they were nicely impressed. [laughter] But going back to your question. Maybe I can delay comments on this to sometime later because, you see, I may need to add a few bits more of information about to my life story first.

After the British army decided to pull out of Italy, when the post war years were coming to an end, some people, particularly officers, were encouraged to stay if they could get admitted to Italian universities and wanted to study there. And so, together with a few others, I went to the University of Turin, *Politecnico di Torino*, and enrolled to study chemistry there. I needed to take an entry exam, a matriculation exam actually, in Italian, in order to be admitted because I had no formal educational documents. I completed the first year and a bit more there, but then came an order that the British army was completely pulling out from Italy. And I had a choice of being returned to Poland, which by then went under communism and Soviet control, and of course, I wouldn't have done that, or, to go to Britain. And so I decided to go to England. I must have been well over twenty-one, something like that. And I went to London University, got admitted. Again, I took another matriculation exam because the British wouldn't recognize the Italian matriculation. [laughter] And so I was admitted to Imperial College in London to study, and I chose electrical engineering. But there were a lot of Polish people like myself in London at that time. Poles who, influenced by elder officers and others who had been in Britain during the war, or returned to Britain from war activities, were willing to be persuaded that there would be soon a third world war, and that we would go again and fight for Poland, et cetera. And I was convinced it was true.

So, it happened that I was in lodgings with a widow who had two rooms. She had a bigger room where a member of the exile Polish government actually was residing, and I had a little room as a student. And we introduced each other, but really didn't interact very much. But after a few months, sitting in her little garden on a Sunday—and I'm only mentioning this because this was a turning point in my life here—sitting in a little garden he got up and said, "May I have a word …?" And he said, "If you allow me, I have been observing what you're doing and you are not taking your studies very seriously. You're getting involved with lots of former military and political people from Poland, or Italy, who believe that there's going to be another conflict soon and that you'll be needed to go back and fight again for your country. I am of the opinion," this is this man saying, "that nothing like this will happen and that if you do not complete your university studies here, and at a high level, you will be working in a British mine, like others without education, or be unemployed." And he said, "I'm only saying this to you because I made a decision to go back to Poland despite communism there. I have my wife and two children there" (he left Poland when the war started). And some time later after he departed back, I learned that he was eventually arrested and put in prison, and died. Well, I shouldn't say this, of course, but anyway, to the communist government he was an enemy and so they'd done him in. But even his early remarks in the little garden already shook me. I sort of thought about it a lot and I thought, "Well, yes, indeed. I am here drifting about, but not doing very much about my future."

I was entitled to a university support because I was an ex-serviceman, kind of like a GI Bill. And so I decided to leave London and I interviewed at Bristol and Cambridge and then Birmingham. Cambridge would have accepted me because I had pretty good qualifications, but it was full of ex-servicemen. So I went to Birmingham University. There I decided I will no longer study electrical engineering, I'll study something different. And the registrar lady said, "Well, we have a small group being formed by a new professor—I think he was then what you call a senior lecturer. His name was Alan Cottrell. "And if you go and see him, and if Dr. Cottrell accepts you, you will be able to join Birmingham." And I went and got to an interview with Cottrell. Cottrell later became a very famous man. Sir Alan Cottrell later served as advisor to the government, et cetera, and a very well-known scientist too. At Birmingham, together with others, he was pioneering a new topic: *physical and theoretical metallurgy* about which I'd heard nothing.

So that was another turning point because I was extraordinarily lucky. The University of Birmingham department of metallurgy then had—for people who know the field, people like Geoffrey Raynor. I already mentioned Alan Cottrell. There was also Robert Cahn, there was Frank Nabarro, Jock Eshelby, Tsun Ko, Dan Hanson, to mention only a few. Later Chuck Barrett came to visit from the US. All these people have become really famous pioneers in materials science later, pioneering the new trends of understanding materials through basic principles. And Cottrell wrote a book or two on this. So did Raynor, a former Hume Rothery student, and others. And so I was lucky. Incidentally, as a comment, I had to pass yet another matriculation because the Birmingham U didn't honor the London U matriculation.

01-00:21:23
Burnett: It wasn't even a different country, it was just a different university in—

01-00:21:26
Massalski: No, the matriculation was called Northern Boards. You see, I had the, whatever, Southern Boards from London.

01-00:21:33
Burnett: Of course, of course.

01-00:21:35
Massalski: A lady in administration called me and said, "Well, you have to get a matriculation." And I said, "What's the difference between the two?" And the lady said, "You have to have a foreign language here." And so I said, "Well, a foreign language? Any language?" She said, "Any European language." I said, "Polish."

01-00:21:58
Burnett: Just happened to have that in the bag.

01-00:22:01
Massalski: She sent me to a professor of Slavic languages who turned out to be a Czech to take this exam.

01-00:22:11
Burnett: Great.

01-00:22:10
Massalski: And he interviewed me and said, "I don't know any Polish but where were you born?" And I said, "I was born in Warsaw." He said, "You passed." [laughter]

01-00:22:19
Burnett: Okay, well done. [laughter] Can I ask you—

01-00:22:24
Massalski: Please go.

01-00:22:25
Burnett: —because I would have thought—I completely understand your being interested in electrical engineering because all things electrical were absolutely fascinating to people at the time. It was changing the world. And metallurgy might have sounded to some like uninteresting.

01-00:22:50
Massalski: That's right.

01-00:22:50
Burnett: It's rocks and metals and so on. So how were you introduced to this new theoretical metallurgy?

01-00:23:00
Massalski: Oh, it was fascinating. It was nothing like what I expected. It was fascinating in the sense that it actually was quite a bit of physics and chemistry, sophisticated techniques, thermodynamics, and electron theory etc. Cottrell, I can't remember where he had his doctorate from, but anyway, he obviously had quite a bit of physics and chemistry. And so also, essentially, the majority of the professors, at that time, were not mostly teaching us how to make things, but also to understand how things work. And this has become my own habit too, in a way.

And so that was another turning point, you see. First of all, it was my going away from London and realizing why. It meant realizing that, well, as this Polish gentleman in London told me, let me call him W, he basically said to me, "You know, you don't know what's going to happen in the near future. What's going to happen is that people like you will have to work somewhere in low jobs, even though you know English, because you will be not educated enough. So you are wasting your GI Bill opportunities." So this set of remarks from W was a push from an older man with wisdom. The second turn was pure luck. I landed at a school, pioneering a subject and approaches to that subject that were all new and all challenging. So, I landed in the best possible school in the world at that time. To say nothing of the people contacts I was making for my own future, without realizing it.

Then came a third turning point, if I may mention. Incidentally, I got the B.Sc. degree and the department head, Prof. Hanson, decided that I should go and do research and continue graduate work, and I got a scholarship from the university to continue towards a doctorate. And then in my last year of undergraduate studies, in came an American visiting professor from the University of Chicago. His name was Charles Barrett. And he was a most unusual man in a sense that he had, what I would call now a rather American habit, to become interested and to get to know the junior people in the department, as well as the senior. And somehow he also befriended me, amongst others. There was this hard studying Polish boy, doing well. Barrett was there for a year as a visiting professor. He was a member of the Institute for the Study of Metals in the US, at the University of Chicago.

Okay. And so later I was completing my doctorate at Birmingham and a year before I completed my graduate studies I got married to a wonderful English girl, Sheila Harris, who was, again unusual, because she studied French. And at that time that meant one year in England, one year in France, one year in England, etc. In France, she was associated with the University of Montpellier in southern France. So we got married. She and I came from what one might call a similar social class in each of our respective countries, so culturally and socially we fitted very well with one another. My family dates back a few centuries, is well-known in Poland, and so was hers. So we were okay. [laughter]

And Sheila was later expecting a baby. Then I got a letter from the head of the Institute for the Study of Metals, Professor Cyril Smith, saying that the Institute would like to appoint me as a post-doctoral fellow. There were only two post-doctoral fellow positions at the Institute so it was a very big offer. And Sheila didn't want to go to America, the baby was due in four months.

01-00:27:25
Burnett: So you were able to then, weather this change? She didn't want to come and yet you went?

01-00:27:32
Massalski: Well, I persuaded her.

01-00:27:33
Burnett: Oh, okay. I see.

01-00:27:34
Massalski: Yes. We finally agreed that we will go for the two years. This was an appointment for two years. And for '54 or '55, fifty-four I think, we went. Flew to Chicago. First experience. And I remember I went to the office of Cyril Smith, who was then the director, and introduced myself. He said, "Oh, welcome." He said, "By the way, we're very happy that you agreed to come and do you have any questions amongst other things?". And I said, "Sir, yes I do have some questions." I remember to this day his response. "This is America," he said." We're on first name [basis] here. So, you see, my name is Cyril, and what's yours?" I said, "People call me Ted." So he said, "Ted, do you have any questions?" And of course I had several. "What's my pay?" was one. He said, "Didn't I say this in my letter?" I said "No. I just received an invitation letter from you, mentioning a potential appointment, no money was mentioned and we just took the risk." [laughter] So he scratched his head, he said, "How about $3,000??" And I thanked him, of course, and I said it seemed very adequate. But later on I had to appeal to him to have some more money because I found that our baby was going to be born, and I had no

insurance, and all kinds of other expenses. Again, I'm cutting a long story short, but Cyril was very generous. My salary went to $5,000 pretty soon. [laughter]

01-00:29:25
Burnett: Yeah. That was about a teaching assistant's [ed. note: assistant professor's] salary at the time.

01-00:29:27
Massalski: Well, I went to Charles Barrett, whose office was on the way as I was walking from Cyril's, having gotten a raise. And he said, "How much did he give you?" And I said, "Five thousand." He said, "My God, I only get 10,000 and I am a full professor." [laughter] So those were the days.

01-00:29:48
Burnett: Well, when you got to Chicago, what was your impression of the United States, apart from the famous informality of the people?

01-00:30:02
Massalski: Extraordinary country. Extraordinary country in many ways, in the sense that, of course, it was very inexpensive comparing with what I was used to, although my income in England was always only a student's stipend. But I remember our first meal in Chicago was a dollar each, that kind of thing. And I bought my first car for $240. [laughter]

01-00:30:33
Burnett: Yeah, those would have been some differences, I imagine.

01-00:30:35
Massalski: You see?

01-00:30:35
Burnett: Yeah, yeah.

01-00:30:38
Massalski: So anyway, my daughter was born on the 2nd of April. We came in January. And the other post-doctoral—I mentioned there were two—was a German, Peter Haasen, who became rather famous later in Gottingen. So there were two foreign post-docs that year. And I remember I had to give a lecture, which was a custom for the post-doctoral fellows. And there were five Nobel prizewinners right there in the front row.

01-00:31:12
Burnett: Wow.

01-00:31:13
Massalski: Okay. One of them was Harold Urey. And later on I actually also learned quite a bit from him. So, again, my good fortune of having by chance come from Birmingham to the famous Institute for the Study of Metals. What

happened later was a bit unusual. Both Peter Haasen and I were offered permanent positions after the two years and we both turned them down.

01-00:31:48
Burnett: Why did you turn them down?

01-00:31:50
Massalski: Sheila wanted to go home. [laughter]

01-00:31:51
Burnett: Oh. So it was her turn to decide.

01-00:31:55
Massalski: Yes.

01-00:31:55
Burnett: You had done the two years.

01-00:31:58
Massalski: When I later came to Pittsburgh, this was another kind of recruiting attempt from the US. And I came to a place called the Mellon Institute in Pittsburgh. But that’s a later story. So far I have been mostly covering my personal history. And now perhaps it is time that I should tell you a bit more about my scientific career.

01-00:32:28
Burnett: Yes, absolutely.

01-00:32:28
Massalski: But this I hope helps you to see the kind of chances and opportunities that—

01-00:32:38
Burnett: Yes. And people saw your potential at different junctures and took action—

01-00:32:43
Massalski: Yes.

01-00:32:44
Burnett: —either to persuade you directly or to create opportunities for you because they saw what you could become perhaps. So you were at the Institute for the Study of Metals late 1954, ’55, into ’56.

01-00:33:00
Massalski: Yes. Yes. Fifty-six. Sheila went back home earlier with our newly born daughter. She was directly going home. And Peter Haasen and I went on a six weeks trip around the United States in his old Pontiac, and that was my first introduction to the vastness and the challenge and the beauty of America.

01-00:33:30
Burnett: You went on the road.

01-00:33:31
Massalski: Yes. We had done everything. Went to the Smokies and we went down to the Colorado River and the Grand Canyon, up and down various states. Visited Berkeley for the first time, and Stanford. We went through California. Crater Lake. I don't know whether you know.

01-00:33:54
Burnett: Haven't been there yet.

01-00:33:56
Massalski: So it was very exciting. Then Peter Haasen went back to Germany for family reasons, but he was also later on involved in my life in various ways. It's through him that I am now a member of the German academy and that kind of stuff.

01-00:34:10
Burnett: So this is almost like an early cohort of yours.

01-00:34:12
Massalski: Yes, yes.

01-00:34:13
Burnett: A cohort of two, I suppose, in that case. But yeah.

01-00:34:17
Massalski: Now, would you like me to start on a bit of science connections or—

01-00:34:22
Burnett: I think one of the things that you might be able to clarify is the emergence of what you call theoretical metallurgy. If you could describe a little bit about what metallurgy was like before, if it was a kind of more craft knowledge. What other sciences were brought to bear, what other principles were brought to bear to metallurgical questions at the time that you were studying?

01-00:34:52
Massalski: Yes. Well, interestingly, the students at Birmingham during the summer had to go, whether this was compulsory I don't remember, but they had to go on some kind of a vacation, summer practice. And I twice went to Sheffield, which was a center of English steelmaking. Okay. And got an exposure of what steelmaking was like. And it was, again, quite terrifying because it was very primitive. I remember I was an assistant to a guy who was quenching heated parts, I don't remember, but quite large pieces, but were heated almost to the red heat and then plunged into oil of some kind. Quenching process. And it is this guy who taught me that during this quench there was something that happened. He didn't know what it was.

01-00:36:09
Burnett: Right, right. It was craft knowledge. Yeah.

01-00:36:11
Massalski: But something happened. The pieces became very much harder and they were used I think for railway junctions. I became curious, you see. And there were big thick gloves that he had to wear. But he said that there's no need to do that. His skin became so thick with this plunging. Why? And I was curious. And he said to me, "Well, you see, by being able to plunge these without gloves I've got guaranteed my job because nobody else can do it." [laughter] Just a side comment. But anyway, I wrote essays on my experience, probably six weeks or something like that, each time. And I could see that the understanding of what was really going on was—and England was quite advanced already—but it was still rather primitive. So I wanted to understand more and learn about something called martensite which is a transformation that happens in iron-based alloys that makes them harder.

01-00:37:42
Burnett: Right, right. And so these are kind of engineering processes, like the Bessemer process from the late nineteenth century. So they worked out that these processes would be more efficient and they would have certain effects that would produce steel much more cheaply. But they didn't understand necessarily the thermodynamics behind it. So that the principles of physics would come later? Is that what has followed?

01-00:38:16
Massalski: Oddly enough, one of the teachers at Birmingham was a Chinese, Tsun Ko.

01-00:38:27
Burnett: Can you spell his name? Do you—

01-00:38:29
Massalski: T-S-U-N K-O. Became famous again. Tsun came from, I believe, Shanghai. Why he came to England, it must have been before the war. Okay. I don't know. And he was teaching thermodynamics which was, again, a subject quite new to me. For the first time I realized that there's something that governs behavior when you start mixing elements to make alloys, not only pure metals, but if you add a little bit of carbon to steel it becomes a very different material. If you add zinc to copper, it becomes different. In other words, I got interested in the thermodynamics and the understanding of alloys. And that has then become my interest. Cottrell, who was by that time was a full professor, went in the direction of understanding the deformation of metals, so-called "theory of lattice dislocations." You may have heard. The presence of imperfections in a metal that makes it pliable, et cetera. But I came under the influence of another professor, Geoffrey Raynor, who came from Oxford. And Geoffrey was schooled by a Prof called Hume-Rothery, in Oxford. And Hume-Rothery was a chemist who became interested in alloy formation and the rules of alloy formation, known as Hume-Rothery rules, which I got very much involved in later on. And so it is again contact between people. Hume-Rothery interests were kind of passed on to Geoffrey Raynor, who became a professor at

Birmingham. I did a thesis with Geoffrey Raynor trying to study an alloy system.

And when I returned to Birmingham, because that is what happened after those two years in Chicago, Birmingham University said, "If you want to come back you can become a lecturer." So I accepted a job in Birmingham and then started forming a research group, partly with Geoffrey, partly by myself. I obtained support for this. And my idea was to study Hume-Rothery rules, in other words.

01-00:41:16
Burnett: Can you pronounce—

01-00:41:18
Massalski: Hume-Rothery. Hume-Rothery is spelled H-U-M-E and then Rothery, R-O-T-H-E-R-Y. Hume-Rothery. He was totally deaf. So communication with Hume-Rothery was on a writing pad.

01-00:41:40
Burnett: And you were interested in the principles that he had elaborated?

01-00:41:49
Massalski: He developed something that become known as Hume-Rothery rules. He tried to determine factors which determine what kind of crystal structure an alloy would have. Since you are not a technical person, if you alloy A with B, and if A has a crystal structure—you know what that is?

01-00:42:18
Burnett: Yes.

01-00:42:19
Massalski: And B may have a different crystal structure, then somewhere along the way, as you change the proportions of the two elements, one structure has to change into another. But what happens, often, is that there may be a number of crystal structures that can happen in certain regions before you get to the structure of the B element, okay? And Hume-Rothery was able to predict what kind of crystal structures could form depending on something called electron concentration. In other words, now we're going to the electronic influence that actually predetermines the crystal structure. And so I became more deeply interested in the physics of things, as related to electronic structure, got involved in something called the Fermi surface. This was now very-low-temperature physics, discussing concepts that were entirely non-metallurgical, quite theoretical, but yet basically they seemed very important.

01-00:43:25
Burnett: And so this *is* the theoretical metallurgy of which you spoke earlier.

01-00:43:31
Massalski: Yes.

01-00:43:34
Burnett: Does it move between theoretical practice and experimental practice and back again? If it predicted it there were experiments that proved that this was the case?

01-00:43:52
Massalski: Yes.

01-00:43:54
Burnett: Were x-rays part of this? Were you actually imaging some of this?

01-00:43:58
Massalski: Oh, yes. You see, Hume-Rothery was handicapped because he had no x-rays in Oxford at that time. So at the time he was postulating crystal structure effects as related to electron concentration, this is now the late 1920's that Hume-Rothery did this work, he had to refer to crystal structure discoveries that were being made by other people, for example the work of Westgren and others, done in Stockholm, Sweden. A little later, now early 1930's, all this discussion attracted attention of a physics professor in Bristol, Nevill Mott, M-O-T-T, and his associate, Harry Jones. This is now all physics. And they tried to explain Hume-Rothery's empirical observations in terms of basic physics. They wrote an important book. And that fascinated me, in a sense, because you got closer and closer to—I had natural curiosity for fundamental understanding. So with many very able associates I also did quite a bit of work in this area later, experimentally. Mott received a Nobel later for his enormous contributions to materials science.

01-00:45:06
Burnett: Sure. Did you bring anything with you back from the Metals Institute in Chicago? What did you take with you?

01-00:45:17
Massalski: Well, I mentioned earlier Harold Urey. But this takes me in another direction. Urey was interested in the solar system. He made some discoveries about the elements, and I think got a Nobel Prize for that. But he and a number of other people at the Institute were interested in bigger things like origin of the solar system, et cetera, at that time. And so one day actually Harold Urey came to my little office and said, "Massalski," the habit was to call you by the last name. Anybody who was important was called "Mister." Anybody who was junior was called "Doctor"—[laughter]

01-00:46:11
Burnett: [laughter] I see.

01-00:46:12
Massalski: —at the University of Chicago. So Urey said to me, "Massalski, you are a metallurgist. I have a problem for you and I wonder whether you can look into this." And he for the first time showed me a meteorite, a metallic meteorite, you see, and he pointed out that there were diamonds, little tiny diamonds found in meteorites. And the calculations showed that to form a diamond by

carbon separating out from a liquid iron, or an iron alloy, one had to have enormous pressure. And if you calculate the pressure it turns out that the body from which the meteorite came had to be almost as large as the earth, and that did not fit into Urey's thinking. It turned out later that actually the diamonds came from only near the surface of a meteorite. As it enters the earth atmosphere at some 17 thousand mph and has this huge impact, colliding with the atmosphere, and there's a molten layer near the surface because of the friction, and the meteorite begins to melt. It's in that region the diamonds were actually found. But Urey didn't know this, and he thought maybe they're all over the meteorite piece, and that would mean that he might be able to calculate the size of the original bodies. And there seemed to be no such large bodies in the solar system. But anyway, it got me intrigued. And so I decided one day, if I have some funds I would study meteorites. And I have done quite a bit of work later, in Pittsburgh. So you see, again, contact.

01-00:48:01
Burnett: Right, right. The exposure to new ideas and different disciplines that were part of your growth as a scholar. So it's now the late fifties, I suppose.

01-00:48:16
Massalski: Yes. I am back in Birmingham. This was when the Sputnik went up and so on. So people like myself were being recruited to come back to USA, which didn't please my wife. [laughter]

01-00:48:40
Burnett: Okay. I'm sure.

01-00:48:41
Massalski: Yes. Actually, a member of the Mellon Institute in Pittsburgh stopped by. Mellon Institute was an independent big research place at that time. I don't know whether you ever heard of it. Built by the Mellon family, who were kind of rich entrepreneurs and bankers in those days. Okay. Eight-story building, about 800 people et cetera, who did research to support the industries that the Mellons owned. Not basic, but very kind of practical—

01-00:49:33
Burnett: Yeah, applied.

01-00:49:33
Massalski: —understanding, including steel, or whatever applied industries around Pittsburgh did. The Mellons understood that times had changed and that America needed more fundamental and advanced research and so they recruited a guy called Paul Flory from—I don't remember from where Flory was. One of the prominent universities in the east. Cornell I think. And Paul Flory became the executive director of the Institute. But the president of the Institute was a general, General Matthew B Ridgway. And above him was Paul Mellon. So this was a kind of nice structure of very influential people. And Paul Flory came on a visiting trip to Europe and stopped by in my office and said that there will be a division of metal physics that they were going to

form in the Institute and would I come and lead it. So another big opportunity. Lots of money for research. My personal salary then I think was going to be something like $16,000, which was a lot of money at that time. And so it was a tempting attraction. On the other hand, we were not really poor in England. We already had a house. My second child, Peter, was born in Birmingham after we returned. So we now had two children. Sheila felt that we could do very well.

01-00:51:38
Burnett: Can I ask your daughter's name? You mentioned your son's name but your daughter's name was?

01-00:51:44
Massalski: Her name is Renny. R-E-N-N-Y. Renny. She, incidentally, finished at Queens University in Canada, and Peter later went to school in England and then Oxford.

01-00:51:52
Burnett: In Canada ?

01-00:51:53
Massalski: [laughter] In Canada. And then she went and joined the Navy.

01-00:51:58
Burnett: The Canadian Navy?

01-00:51:59
Massalski: No. Precisely when she came home and I said, "Renny, what you going to do now?" she said, "I'm joining the navy, Daddy." And I said, "Which navy?" And she said, "Oh, American." She was born in Chicago. She was a born Yank, you see.

01-00:52:13
Burnett: Right, right. There you go.

01-00:52:15
Massalski: She spent twenty-two years in the navy, yes, and became a commander, then retired, went to school, and now she is a doctor of clinical psychology.

01-00:52:27
Burnett: Wow. So you inspire these chance departures.

01-00:52:35
Massalski: Well, you know—And then we also had a third child, Christopher, who was born in Pittsburgh. Incidentally, the two older children, when they came to high school age, went back to England to school because of mother.

01-00:52:52
Burnett: Right. Right. She wanted to have those roots, have that contact.

01-00:52:54
Massalski: Yes. Okay, so very well. Now where am I?

01-00:52:58
Burnett: You were asked to lead a metals research institute.

01-00:53:05
Massalski: It was called metal physics—

01-00:53:06
Burnett: Metal physics.

01-00:53:06
Massalski: — a division.

01-00:53:09
Burnett: Okay. So you were asked to lead the Metal Physics Division. And when did you join?

01-00:53:13
Massalski: There was quite a bit of hesitation, but finally I accepted the job. And this was the time when an immigrant like myself— they moved all our furniture to Pittsburgh.

01-00:53:26
Burnett: It has that advantage. But this was the time of Sputnik and the National Defense Education Act.

01-00:53:32
Massalski: Yes. Yes.

01-00:53:33
Burnett: There was a real investment in science education.

01-00:53:36
Massalski: Yes. And Paul Mellon, who was a philanthropic person, very interesting, married to an English wife, was determined that the Institute will become kind of a leading fundamental research place, doing some industrial research, as well, but moving into directions that America was not yet doing too much of anywhere. Okay. Paul Flory quarreled with some of the aspects of the control that was exercised by the family. And about two years after I came, and he was the person who recruited me, okay, he came and sat on the corner of my desk and said, "I'm going to Stanford. Do you want to come with me?" [laughter]. And he then got a Nobel later in Stanford for polymer work. He died, unexpedctadly, three years later. We visited the Florys in CA and so on. He was an interesting contact and an absolutely brilliant person.

01-00:54:08
Burnett: I've noticed from your résumé, more than a lot of accomplished academics, You have taken visiting professorships, like one-year stints. Is that part of your—

01-00:55:14
Massalski: Well, at my age I can admit it. Obviously they were trying to recruit me. And they were usually tempting places like Stanford, Harvard, CalTech, you know. Sheila didn't want to move. We established ourselves nicely in the outskirts of Pittsburgh, a very nice countryside, okay. We had six acres of land there. So anyway, we went for a year, generally the offer was with the intent of my staying and I did not stay. Was it wise? I don't know. Now that I'm at an advanced age I am sorry I am not living somewhere at the sea, either East Coast or West Coast. Although Pittsburgh is a very livable city.

01-00:56:15
Burnett: Yes, it's very beautiful.

01-00:56:16
Massalski: A very livable city. Sheila was almost willing to go to New England at one time because it was closer to "home." Yeah.

01-00:56:27
Burnett: Yeah. But I imagine, as with your earlier educational experiences, it represented exposure. You were exposed to research programs.

01-00:56:39
Massalski: Oh, absolutely. You see, my first visit was actually to CalTech. This must have been about 1970. So about fifteen, fourteen years after I came back to US. No. We came in 1959-60 to Pittsburgh. So, anyway, a few years later I went to CalTech for a year to work with a Prof. Paul Duwez. D-U-W-E-Z. Paul Duwez. Who was, again, a most challenging person. He came originally from Belgium, and then was at the University of Illinois for a while, and then he took a chair at CalTech. And he was pioneering something entirely new. And that was— what happens if you quench a molten metal at enormously high speed? At that time, it meant taking a tiny drop of a metal or an alloy and shooting it against a cold copper plate. Very rapid chilling. And so you could reach a cooling rate of, say, a million degrees per second. And then there's no time for equilibration, or thermodynamics to play a part—it's all a kinetic situation. And Paul Duwez discovered that all these phased diagrams that Hume-Rothery was working on became sort of only semi-valid because there was no time for the normal thermodynamic and electronic forces to exert themselves. So, in the end, people discovered that you could form a glass form a metal alloy, a *metallic glass*, in a metallic system. Most unusual. You may have heard the term.

01-00:59:00
Burnett: In doing research just for this, yes, but I had not heard of metallic glass before. And so this is one of the things that comes through because of the kinetic force. So these are the microstructures of metals that are affected by different kinds of manipulations, be it temperature changes, or movement of phase boundaries, or kinetics of some kind, right, over time. Very brief, very intense, changes, and you can really get unique structures that can then be studied for their properties.

01-00:59:36
Massalski: Exactly. I will return to the metallic glass story later. So, you see, already twice I came to really very challenging scientific situations in materials science. The study of meteorites, when I came to Mellon Institute and I had pots of money for research of my choice. I formed a group to study meteorites because I wanted to test these ideas, diamonds, et cetera. And before leaving Chicago, incidentally, Urey said, "Massalski, if you discover some kind of "unusual biological" things in meteorites based on carbon, you've got yourself a prize." [laughter] So there was a temptation, and I had the money. So I formed a group of three PhDs and we did quite a lot of study of metallic meteorites and published some interesting things. And, in fact, we were able to calculate the size of the body from which this particular meteorite came, you see, by finding the rate at which the sample cooled. But I won't bother you with details. But, essentially, by cooling a metallic material based mostly on iron and nickel, you take it through different transformations, and even though you can have thousands, or millions of years available for cooling, gradually equilibration breaks down as you continue cooling, and from the traces of the nickel distribution in the sample you can attempt to calculate the potential size of the object the meteorite came from, at least I thought so. So, you see, the ideas were back there from the Institute for the Study of Metals.

01-01:01:20
Burnett: From my very unschooled perspective, because this is the era of Big Physics, high-energy physics, which is subatomic, right. So it almost sounds like some of this research is high-energy solid state physics. Is there a sense in which there's an influence from the Big Science of subatomic physics?

01-01:01:48
Massalski: Yes. And, of course, in the process, when I was at Mellon Institute and later, I became involved in nuclear energy. I've been a consultant at Lawrence Livermore for years, and also Oak Ridge and Los Alamos, et cetera. And so I got involved in other curiosities. The reason, again, is that I was a developing expert on alloys and people needed this kind of help. So, this exposed me to a lot of things which, of course, were classified. But nevertheless, this was a very interesting area where, again, I thought I made some contributions.

01-01:02:30
Burnett: Absolutely. I bet. I think this might be a good time to pause and then we can regroup and start over.

[Audio file 2]

Burnett: This is Paul Burnett interviewing Dr. Ted Massalski for the Global Mining and Materials Research Project and this is March 16, 2015 and this is audio file two. So last we left off you were just talking about the influence and shaping of these visiting professorships that you undertook. And you were about to tell us a little bit more about the influence of the CalTech visiting professorship. Can you talk about how that shaped your work?

02-01:03:14
Massalski: Well, as you may recall, I mentioned that Paul Duwez pioneered something which has become known as *splat cooling*. That is, taking a very small amount of liquid, of a metal or an alloy, and shooting it explosively—kind of gun technique really—against a mostly copper target, because copper absorbs heat very rapidly—against a target, copper target, which would therefore spread this liquid alloy or metal and instantly solidify it. The cooling rates can be extraordinarily high although the whole process takes a fraction of a second. But in terms of cooling it could be a million degrees per second. And this was fascinating. Although this had very little to do with the kind of interest in the electron compounds and Hume-Rothery phases and things that I developed earlier, which is what we were partly continuing at Mellon Institute. There, I had funds for research so there was this meteorite group, but the majority of activity happened to be in the alloy stability area and alloy theory area that I brought from England. We built, for example, a very low-temperature calorimeter to measure electronic specific heat, liquid helium range, so one point something, 4.2 degrees absolute [-268.8 degrees Celsius]. And this was to determine the density of—this is physics now—density of states, of electronic states, in order to argue that it is the electronic features of alloys that brought about their stability.

Then I came back from CalTech, you see, and I became interested in this idea that you really do not need to try and understand all this phase stability, if you cool liquid metals so fast as Paul Duwez was trying to do, and the new properties were very unusual and challenging. And so I developed another group. We actually built a copper wheel because it was realized very quickly that you can splat little bits of metal against a plate and study them by x-rays, for which you require very little material. But if you want to *use* this material, to test properties, you have to make a lot of it. I don't remember whether it was Paul Duwez, or somebody else, came up with a newer idea that you could project the liquid metal against a rapidly revolving copper wheel, *a wheel technique*, instead of a splat. So that you could start now with much larger amounts of a liquid solution, of liquid alloy, which you'd compress and then expel out, again explosively, and so out comes a stream of liquid and it hits this fast rotating wheel, and chilled, and otherwise big, copper wheel, and you make a ribbon. And this ribbon is being continuously peeled away from the wheel because of fast rotation. So you can make a lot of long ribbon. And then people learned that you can make the projectig lip opening, of the container holding the hot liquid, into a slot, so that you could get now a bit of a sheet of metal coming from the wheel instead of ribbon. And this is where people were saying, "Let's do some technology with this stuff." So, for example, let's make something that is cooled so fast that you actually make a *metallic glass*. So that is what I mentioned much earlier. Glass is essentially a liquid that has been frozen at sufficiently high speed not to have a crystal structure. The glass in your window here has no crystal structure. It is frozen, but a frozen "liquid". Okay. Well, it was not realized that you could also freeze metallic liquids and other liquids so that they can become glassy. But the speed with which you have to do this is many orders of magnitude higher. In the case of ordinary silica glass, the silica liquid you can cool in a pot and chill relatively slowly and it will still become glassy, of course now a solid "liquid." Of course you can cool silica even more slowly and make it crystallize, but then it wouldn't be transparent. But you don't have to cool it very fast to retain solid glass. However, to do this with metals you have to use very high speed. So the next answer was all kinds of high-speed techniques that were being developed. Some of these researchers have pioneered discoveries around the world and now almost anybody who's interested in amorphous alloys has some kind of a wheel technique that can do it.

02-01:08:25
Burnett: And as you were making these new materials you said, "Let's make some technology with this." I imagine there's an immediate interest in application. So if you could make a glass out of a conductive material would the conductive properties change? Are those the kinds of research questions—

02-01:08:49
Massalski: Absolutely. And then, you see, what became of interest is that you can make something quasi-amorphous, quasi glassy. This is a new word. Again, Nobel Prize came out of this recently. I will take you to this in a second. But just to fill in, here I came back to Pittsburgh. We established ourselves there. I really have had quite a lot of financial research resources and so I was able to broaden my research interests. Colleagues came from Japan, Professor Mizutani, Professor Suzuki. Professor Noguchi, who developed this very-low-temperature calorimetry. This was pure physics. To study essentially electronic specific heat, to understand these electron compounds of Hume-Rothery, which was stemming from Hume-Rothery rules. We were working on this, but other things were becoming equally interesting.

After I returned from CalTech I went on an appointment to National Bureau of Standards for one year. This is an institution near Washington, DC. There was a practical mission for my appointment that had to do with the way National Bureau of Standards was going to develop in the future. But I also came in contact with a bunch of entirely new and very bright scientists doing all kinds of fascinating things. And recently, the director of one of the groups there reminded me that it was me who persuaded them to build a wheel, a big wheel of copper, which I got from ideas of Paul Duwez. And he said, "Ted, we built this wheel because you suggested this to us." Why did he say that? Because later on a post-doctoral [researcher] from Israel came and studied some aluminum-manganese alloys, not necessarily to try to get "glass," but just to see what happens when you rapidly chill Al-Mg alloys. And he discovered something that nobody has ever seen before, he discovered a crystal structure that showed pentagonal symmetry. And pentagonal symmetry is not allowed in eqilibrium crystallography. [laughter] I don't want to go into much science here but there was this young man called named Dan Shechtman who came as a post-doctoral [researcher] and he was presented with this wheel, which they were excited about throwing all kinds of molten alloys against. And he for some reason that I don't remember now studied the manganese-aluminum system. And then did diffraction, by x-rays or electron diffraction, and he showed pictures showing pentagon symmetry. And that wasn't supposed to be possible. In fact, I remember he came to my office and showed it to me." Professor Massalski, you co-authored a book with Charles Barrett, on crystallography, years ago." And he said, "What do you think?" And I must say, I wasn't really happy about this. Neither were many other people. But it turned out that Shechtman made a discovery, okay. He took it back to Israel and eventually got a Nobel Prize for the discovery of quasi-crystals. But I won't go into this here because there developed all kinds of emotional arguments as to who did what. I was not involved.

02-01:13:08
Burnett: Right. But others were claiming some kind of priority?

02-01:13:13
Massalski: I think the prize should have been given perhaps to three people, not just one.

02-01:13:17
Burnett: Right, right. Well, there's something skewed about identifying one single person when so often it's networks, it's communities that produce knowledge.

02-01:13:28
Massalski: But that led to another new huge field which was developed also from other inputs around the world, called nanotechnology. Initially by very high rates of cooling, but eventually by some other techniques, you can develop particle sizes in a solid which are in a nano, ten to minus nine [10^{-9} meter] range, okay. And now, again, technology's growing and people are making all kinds of nano-applications and they're growing into industries. Again, this is kind of big development. I have not really done much nano-work myself, or with associates. But it all follows from that business of rapid cooling. It seems.

02-01:14:17
Burnett: Yeah. Rapid cooling was really the key.

02-01:14:20
Massalski: Yes.

02-01:14:21
Burnett: And you mentioned something earlier and I want to make sure I'm following this. You were talking about someone's initial experiment with rapid cooling and you said that this potentially missed some intermediate states if it cooled more slowly.

02-01:14:39
Massalski: Yes.

02-01:14:42
Burnett: Was that a research program that you understood with a different set of experiments by—

02-01:14:48
Massalski: Well, in discussing portions of my career I sort of missed to mention something that you may have seen in my summary, the so-called *massive transformation.* You see, when I did my PhD work with Geoffrey Raynor in Birmingham he wanted me to study the Hume-Rothery phases that were forming in copper-gallium system. The idea was that you would make some samples, one gram, little tiny samples, a number of them, and then quench them in water from a high temperature in order to retain the high-temperature phase and its crystal structure, in which they were existing when you made them first. Quenching allowed you to retain the structure in a metastable way down to room temperature, essentially without any structure or composition change. Then I would heat the quenched sample back in a furnace to a temperature at which I wanted to find out what the new structure will become at that temperature, and how the sequence of structures changed. I know it sounds a bit complicated. So the process was: make an alloy, cook it somewhere a very high temperature where it was one phase, quench it sufficiently quickly so that you retained what was at high temperature, and then cook it up again at some lower temperature to see what will happen. Am I clear?

02-01:16:21
Burnett: Yeah. No, that's very clear.

02-01:16:23
Massalski: In doing this in copper-gallium, I discovered that something didn't work. Namely, I cooked a range of copper-gallium alloys at a temperature of about 600 degrees centigrade, which is kind of beginning of glowing, maybe a little below that, with the idea that I would promote the formation of a single phase—and the structure then, in Cu-Ga alloys, should be so-called body-centered cubic, BCC. And the idea was I would then retain that body-centered cubic structure at room temperature via quenching and then cook it up, maybe raise the temperature higher to about 450 and find out what the structures would be. Do they follow the sequence predicted by Hume-Rothery, or not? Okay? That was the challenge.

02-01:17:16
Burnett: It was confirmatory experimental research that goes back to Hume-Rothery's—

02-01:17:21
Massalski: They were called electron phases but sometimes electron compounds because Hume-Rothery believed that electronic structure determined what crystal structure was selected. Okay. So I cooked these copper-gallium alloys at 600, quenched them, and the first thing to do was to do some x-ray work to confirm that what I quenched from the high temperature was still there. It *wasn't*. It

disappeared! [laughter] In fact, the crystal structure after quenching was close-packed hexagonal, not BCC. That was not supposed to have happened. And I discovered something called a *massive transformation*. That is a transformation which can happen in solid state quite quickly, because one solid structure is changing into another solid structure during the quench, while the boundary between them moves very fast from one structure to another. Such transformation can be very fast because there is no change of composition at the boundary between them, as the boundary moves. So, even though I quenched the samples through a field which we call a two-phase field, the phase that formed, that formed during quenching, consisted of only one structure (hexagonal), and of the same composition as the phase I planned to retain by doing the quenching. And so I wrote a paper. In fact, I wrote it in Birmingham. I took it to Professor Cottrell, who was still there, a distinguished professor by now. And he said, "Massalski, that's interesting. You should publish this and soon." And so I published in 1958, before I returned to America, published that paper. Okay. It was immediately challenged by a lot of people. "Couldn't happen. Couldn't be." You see? Why? Because it was argued that it violated again the rules of crystallography and thermodynamics. I argued that the only way that this could happen was that there was a semi-amorphous layer between the new growing grain and the matrix into which it was growing, in other words, transforming. I argued that that layer was perhaps amorphous. That there was no close contact between the growing phase and the disappearing phase. No crystallographic contact. You see, in the concept from martensite, which we mentioned earlier, the idea of crystallography is that in order to take a single crystal structure, a grain, and form another grain in it, it is cheapest for the new nucleus of the grain growing to develop some kind of crystallographic relationship into the grain to which it's growing because that lowers the energy of the surface. So the theory has been created, which is very valid, provided that you do it slowly, valid that there's a crystallographic orientation relationship between the parent and the product. Massive transformation violated that principle. There was no crystallographic relationship between one and the other, which means at the interface there had to be disorder. And so this was challenged by a lot of people and there were a number of symposia. I think I was involved in three Massalski symposia. People argued. I don't know whether I can put it this way here. The argument won, that it is possible. Okay.

02-01:21:27
Burnett: And then from there elaborating new principles? Because now you've said that this violates these basics, but the downstream consequences of this?

02-01:21:39
Massalski: Well, the new principle is that what you see in the product is a function of both thermodynamics and kinetics. If you give yourself enough time the kinetics carry out—Thermodynamics rules the behavior of an alloy provided you give it enough time for these rules to take effect. If you do a very, very rapid cooling you can destroy all rules. Okay? In massive transformation you

destroy only partial rules. One partial rule was that there should be an orientation relationship between the parent and the product but in this case that doesn't seem to be. And this applies, of course, in practice because in practice people today manufacture things at various speeds deliberately to upset the thermodynamics.

02-01:22:48
Burnett: And this is, I imagine, why Paul Duwez's work resonated with you so well.

02-01:22:55
Massalski: Absolutely. Because he was one of the first researchers, you see, also trying to understand the behavior of alloys. And he came to this concept that suppose you have a copper and silver and mix it in all proportions, 1 percent, 2 percent, 3 percent, et cetera. All right. Since both copper and silver have the same crystal structure, so-called FCC, face-centered cubic, there should be a continuous range of solid solutions. However, if you take alloys of copper and silver, say 10:1 % percent difference, in the phase diagram, that is the story of what happens when you cook at various temperatures these alloys, the story shows that actually a so-called eutectic system develops, which means that the solution, based on silver, even though it's FCC and the solution based on copper, which is also FCC, come to a certain limit and then a mixture of these two solutions is present, as a two-phase mixture, even though they both have the same crystal structure. Very unusual. And Paul Duwez thought to himself, "Well, if I do it fast maybe I can maintain this complete miscibility all the way, where thermodynamics dictates that it should be a phase mixture, but kinetics can suppress it." And to do this he did this splat cooling, by taking little bits of alloy, shooting them against copper, okay. But the cooling was not enough. And then they developed the big wheel and then you make ribbons. And the first proof of Duwez, that you can overcome thermodynamics, was to demonstrate that the copper-silver system can have a continuous set of solid solutions, although they are only metastable.

02-01:25:02
Burnett: And this begets so much research, entire research programs, certainly yours, right?

02-01:25:09
Massalski: Oh, yeah.

02-01:25:10
Burnett: Not Duwez's work but your work plus his leads to all kinds of consequential work. Does martensitic transformation also play into this?

02-01:25:22
Massalski: Martensitic transformation is something that doesn't involve diffusion. That's something that, as a physicist, I had to explain to my colleagues in the physics department because they couldn't believe that the crystal structure can change to another crystal structure at temperature of liquid helium. Okay. Nothing moves! [laughter] So martensitic transformation is when the transformation

happens by so-called movement of dislocations. That is, this is now a lattice effect. Dislocation is an imperfection in a crystal, which can move at any temperature. Now, it doesn't involve diffusion, it moves like a wave. The atoms originally which were there stay where they are but the wave moves and disturbs other atoms and so you can have a movement of a dislocation without any diffusion. And this can happen then at any temperature. So martensitic transformation is what often happens in steels, as it was eventually discovered. Martens was a German who I believe first discovered it. And the martensitic transformation involves a whole host of transformations that are diffusionless. Not quite, because, as usual, you know, the whole research area has become quite fancy. You can have a thermo-elastic martensite and various other forms. But the basic difference between diffusion-control transformations and martensitic transformation is that one involves imperfections; the other one doesn't.

02-01:27:08
Burnett: Right. And in elaborating these new physical properties of these materials under these conditions, you're trying to understand the basic principles. But the practical applications are enormous. I can think of the aircraft industry that wants aluminum that's denser, lighter, or in other cases more pliable. Could you talk about the interest in your work that may have come from industrial concerns, or the military, for example? I know you can't probably talk about some specifics. But generally speaking what were the big bottlenecks in material science and engineering applications at the time that you were doing this work and did that have an impact on what you were doing?

02-01:28:07
Massalski: Well, massive transformation occurs in plutonium alloys. [laughter]

02-01:28:15
Burnett: Okay, so there's that. [laughter]

02-01:28:18
Massalski: So it is interesting.

02-01:28:19
Burnett: Yes, yes.

02-01:28:20
Massalski: You see? Et cetera. So obviously I got a little bit involved in that direction. But basically that was a big challenge and it was primarily a personal challenge because there were people who absolutely argued that—in fact, some of my postdoctorals—I had at Mellon Institute, at various times, some twenty-three postdoctoral people who continuously explored and argued about various ideas . Most of them did outstanding work. As you know, actually you can see this from my publication record. I very rarely published things only by myself, but occasionally I kind of wrote a summary. But basically, I always worked with associates and often all you have to do is to appoint the right people and keep out of their way.

02-01:29:11
Burnett: Right, right. That's often what folks say when you—yeah.

02-01:29:15
Massalski: And they will do the work and argue it out. And I had very, very good fortune in having worked with some people from Argentina, some incredibly able Japanese, some people from Europe.

02-01:29:35
Burnett: So you were a visiting professor at Harvard, CalTech, Oxford, UCLA, Stanford, University in Buenos Aires, and Krakow, as well.

02-01:29:47
Massalski: Yes, many more.

02-01:29:48
Burnett: And many more.

02-01:29:49
Massalski: But I only mentioned the ones that you may be familiar with.

02-01:29:53
Burnett: Right, right. And as we've said earlier, you had an opportunity to influence and be influenced.

02-01:30:02
Massalski: Yes. That's very important if you can do this. But it often necessitates travel, visiting professor appointments etc. And I sometime faced some difficulties here. Because as I mentioned earlier, my wife kind of resisted moving to different places in US. She really didn't like to move around too much. Once I got involved in a new place, I was so absorbed in the scientific interactions, and my wife had to reorganize the home, and look after the children, schools, et cetera, and she would rather be at home, okay. But nevertheless, we did go. We also spent a very pleasant year at the Naval Postgraduate School in Monterey, which was a very good experience. Again, they cooked up a chair for me there and I turned it down. [laughter]

02-01:30:49
Burnett: But it was, again, a great opportunity to—

02-01:30:51
Massalski: A fantastic opportunity. Why? Because, you see, Naval Postgraduate School is interested in things that affect ships. Okay. And so the research performed by professors there, it focuses on things that can happen to ships or that can improve ships. And so that was a very challenging situation, too.

02-01:31:24
Burnett: I'd like to turn a little bit to the work that you did as an editor putting together these massive volumes of phase diagrams. Can you talk about how that came to be?

02-01:31:37
Massalski: I see, you saw that in my summary. Well, again, as I mentioned earlier, it just happened through contacts. As I mentioned earlier, I was persuaded to spend a year in Washington, DC at the National Bureau. It was then called National Bureau of Standards. It's now called NIST [National Institute of Standards and Technology]. My role was complicated there because the idea was at that time that the Bureau of Standards should be detached from the Department of Commerce and act as an independent national institution of standards, not related to any practical industrial control. And I was a little bit involved in this, and so I went there to take an appointment for a year. My wife didn't want to go and so I flew on Monday mornings to Washington from Pittsburgh and returned on Thursday afternoons. Fifty-two weeks or fifty weeks, lots of flights

02-01:32:48
Burnett: Wow, that is a—

02-01:32:48
Massalski: Yeah. Ninety times.

02-01:32:49
Burnett: That was a commute.

02-01:32:52
Massalski: Yeah. It worked out very well in the sense that I had a friend who was a widow in Washington and she offered me to stay in McLean, Virginia in her house, and I was there Monday, Tuesday, Wednesday. Three days, three nights. Then on Thursday afternoon I'd fly to Pittsburgh. Pittsburgh was Thursday, Friday, and then on Monday I'd fly to Washington again. But that's a personal story. Why did you ask this question?

02-01:33:33
Burnett: Oh, about the volumes of phase diagrams.

02-01:33:35
Massalski: Oh, yes. So, you see, there I came in contact with people who were telling me, "Well, look, Ted, these phase diagrams, we've just looked at this one, or that one." They're not sufficiently clear. They are not quite accurate enough." And I got very intrigued in various discussions and, finally, the ASM [American Society of Metals] persuaded me to start a program of re-evaluating binary phase diagrams. What I mean by this is this. Now we know well over a hundred elements in the Periodic Table. But considering only the elements that we typically mix to make various alloys and compounds, we play with about ninety. If you consider how many ninety elements you can mix to form binary systems, taking just two elements, A, B, okay, it's about over 4,000 combinations. Four thousand two hundred, something like this. If you start mixing three elements together you run into hundreds of thousands of options. It's a factorial equation. If you start mixing four elements it goes into millions. But the most important question was how accurate are at least the binary

phase diagrams? And I became challenged because it seems that these binary phase diagrams were determined mostly by metallurgists years ago, who didn't know too much about thermodynamics which govern the formation of phases. And a German professor compiled the knowledge, at first, about some 760 binaries.

A German named Max Haasen, just before the last war, determined the form of the known binary phase diagrams by looking at the published literature. He drew up the best possible binary phase diagrams that could be obtained by combining the information published by others, from the literature. But that was not enough, okay. And when I was in Chicago there was for a time a visiting assistant of Haasen named Kurt Anderko who actually worked with Haasen to determine these phase diagrams, and he felt also that the job needed updating. And, at the Bureau of Standards, years later, we talked and talked, mostly with ASM, and finally I convinced myself also that this whole job needed to be done again. And so I was appointed editor-in-chief of this project by the ASM (without pay) and I decided that this should be done internationally and so I appointed thirty-four editors in various countries around the world. And it became a big project. The ASM spent millions on this and we raised much money from industry. It was a ten-million, or more, project, okay. So I got my name (and those of assistants) on all these volumes [laughter].

02-01:36:44
Burnett: I interviewed another scientist in a—

02-01:36:46
Massalski: But, again, they are no longer adequate. We need to do more.

02-01:36:50
Burnett: To do more.

02-01:36:50
Massalski: Sure.

02-01:36:52
Burnett: I interviewed another scientist in a completely different field who had found that these encyclopedic texts or tables for, in this case, drugs were—he couldn't trust them in the end. He said over a period of time he decided that he was going to go and undertake a project to get the most reliable data on each of these drugs and how they behave in the body. But that served him in his career, in that by having this in mind he was able to understand new things about drugs fundamentally. Did this exercise for you have any impact on the kind of research that you were doing or the kinds of questions that you were asking, or was it more of a service to the community to get this material properly done?

02-01:37:49
Massalski: A very difficult answer there. It depends, I suppose, on the circumstances and the situation. But you see from the accounts that I've been outlining, I seem to become challenged by unresolved things. I don't really stay with the same topic. I suppose at the back of one's mind is to help move an obvious need to a possible solution and also benefit as a result. So I embraced this phase-diagram project. I don't know. I was involved with it for about ten years while of course doing many other things as usual. I also published a large number of binary evaluations myself, mostly on gold alloys, as a side job, mostly with a very able Japanese, Prof Hiroaki Okamoto. We also published some advisory papers on diagrams with advice how to discover which are very likely to be wrong, at least in part. Okay. And then we formed a worldwide group, APDIC, the Alloy Phase Diagram International Commission. I was a founding member of this and this APDIC essentially encouraged people to accept the phase diagrams that we, and others, have developed. APDIC is still active today.

02-01:38:49
Burnett: To be the standard.

02-01:38:50
Massalski: To be the standard. And so, as I travel around the world, these volumes, or discs, are now found in many libraries. Indians contributed a lot of stuff, particularly on higher order systems, three or more elements, because they have done a lot of research stimulated by these projects. Also Germans, Poles, Italians, English, Americans, you name it. Whoever wanted to work and contribute could become a co-editor of some special section of binary or higher systems. Altogether we evaluated about four thousand, some only partially.

02-01:39:25
Burnett: Well, you described essentially—you don't like to rest easily with one problem. You like to move from one problem to the next and identify, and keep moving, as it were.

02-01:39:38
Massalski: Yeah. Keep moving. Perhaps sometimes too often. And here I want to mention that, you see, basically, thermodynamics is not sufficient to describe what may happen to an assembly of phase particles under stress or heat. Thermodynamics is interested in, given a substance and its crystal structure, and assuming that it has not too many grains, just so-called single crystal situation, what will be this crystal structure and its overall stability. But now new words and new concepts have crept in to the world of materials: metastability, instability etc. So, for example, in an average alloy, like this one here, it consists of grains. Okay. And these grains are such that if you use a microscope, or electron microscope, you can see that they abut against one another and in between there's an interface boundary. If you cool metals very rapidly these grains don't have much time to grow as they form so they remain very small. And if you cool them at enormous cooling rates they

become *nano*, become so small that the abutting regions, the grain boundaries, are as plentiful as the substance in between them. Okay. And then, again, the thermodynamics breaks down because now what is beginning to govern the structure of this material are the grain-boundary regions which are so plentiful. So that creates entirely different properties that have little to with thermodynamics. Cottrell, as I mentioned earlier, he studied the lattice dislocations to find how metals deform, and how they become strong. But that assumed that you had big grains and that the dislocations moved mostly inside them, under stress. And when they came to a boundary between two grains they had to go into another direction into the next grain, because of structure crystallography, and that kind of stuff. So grain boundaries clearly contribute to strength. But if you have grains, through fast quenching or cold working, that are a fraction of a fraction of a fraction of a millimeter, then the dislocations cannot move much because as soon as they start moving they hit a boundary. So the whole nanoscience of materials is now developing with big new possibilities. But I need to return to my earlier story, the phase diagrams. So, again, I'm going to change the topic here a bit, but you might—

02-01:42:02
Burnett: That's fine.

02-01:42:05
Massalski: Returning back the phase diagrams, the same situation also arises here. In order to determine phase diagrams, the way I did it for copper-gallium, (and I accidentally discovered the massive transformation and turned sidewise to work on this). For experimental phase diagram work, you have to make lots and lots of samples of different composition. Then you have to cook them and then find out by x-rays or otherwise what are the crystal structures that you see and so on. But, as I pointed out, as the number of systems you need to study increases, as you start mixing more and more elements, the number of systems to study increases. In the binary systems we have some four thousand, or so, to determine. But if you start mixing three elements it is over one hundred thousand.

So in comes to an entirely different proposition, and people have begun to ask: How about calculating a phase diagram first, using thermodynamic and electron theory laws? Instead of working experimentally. Okay. So I got involved in this also. Now you can say in addition, all right, let's also consider to start the calculations in an entirely different way. Let's propose a needed property, okay, say resistance to corrosion, and then look for a phase diagram, and an alloy in it, that will fulfill this needed property to some extent, rather than start with the phase diagram first and only later study the properties after we have learned what phases are there stable. And so I'm getting involved a bit in this recently. This is now a field of very fast computer calculations. Tremendous progress is developing here. In fact, let me try to talk about this a bit.

02-01:44:02
Burnett: It’s so interesting. I think we touched on this a little bit earlier. But that technology has a tremendous amount to do with what is possible to see or conceptualize at a given moment. And so when you were describing this earlier thermodynamic model you’re dealing with a single substance, a single crystal structure. But when you are talking about these assemblies of particles, the behavior becomes much more complicated. And there’s visualizing that on the one hand with x-ray crystallography and then there’s also the calculations involved, which I imagine get much, much more, tremendously more complicated. And so computing power is a huge part of the story. And computing power itself is derived from an understanding of the complexity of the materials, right?

02-01:44:53
Massalski: Absolutely.

02-01:44:55
Burnett: And the processors themselves are made of that substance.

02-01:44:56
Massalski: Absolutely. Absolutely. Yes.

02-01:44:58
Burnett: Yeah.

02-01:44:58
Massalski: But, you see, if you were an experimentalist, and I really consider myself basically an experimentalist because I’ve done lots of experimental work with others, it’s difficult to educate yourself that actually you can calculate properties or structures and it may be OK. [laughter]

02-01:45:20
Burnett: Because it’s so complicated. It’s so—

02-01:45:21
Massalski: It is complicated and it’s so unreal, okay, to calculate things at these speeds. The last paper in the list of my publications, the very last, is something we published a year ago. So, you see, at my age I am still exploring—

02-01:45:48
Burnett: Active, yeah.

02-01:45:49
Massalski: —pushing research. Have a look at my list of publications here. You can see something like 200, or more. But look at the very, very last.

02-01:46:01
Burnett: Two-oh-seven, 2013? Oh. *Some Questions and Progress in the Field of Phase Stability*?

02-01:46:11
Massalski: No, no, no.

02-01:46:13
Burnett: Ah, the next? There we go.

02-01:46:18
Massalski: Yes, It's 209.

02-01:46:23
Burnett: [reading title] "Comprehensive Search for New Phases and Compounds in Binary Alloy Systems Based on Platinum-Group Metals Using a Computational First Principles Approach."

02-01:46:33
Massalski: So, again, I got involved with another group of brilliant people using fast computers at Georgia Tech this time. Okay. Prof Stefano Curtarolo at Georgia Tech is full of new ideas about computation, and we discussed the tremendous capability they have of calculating phase stabilities from first principles. But I tried to suggest to them that this can be even further extended to a new concept in the sense that, as I said: you postulate a desired property, for example a specific crystal structure. Then you calculate among hundreds and hundreds of possibilities, because you don't know which alloy will turn out most likely to have this property. Which alloy is it going to be? We can specify we are seeking this chosen property, but not this, and not that. And now let's calculate if we can find two elements, or more, which when mixed together in the right proportion will come close to that property, or feature (say crystal structure, for example, or corrosion resistance, or etc). So it's sort of a reversed approach. And it is likely to take hold as computers get faster and faster.

02-01:47:26
Burnett: And it's not trial and error. You're designing in a sense the material you want and then going into nature and try find it, that are handled by computer, to come up with potential crystal structures that may fulfill your need.

02-01:48:30
Burnett: Does this feel like a sea change for you? A fundamental change in metallurgy and in materials science?

02-01:48:37
Massalski: Well, it'll take some time to be fully acceptable, and it's not for everybody, and it's not in every situation. Somebody still has to make steels. [laughter]

02-01:48:48
Burnett: That's right. Absolutely. [laughter] But for special materials, for new materials. You talked about this research that you were doing in the fifties and sixties and those of others contributing to quasi-amorphous studies that eventually led to research in nanotechnology.

02-01:49:14
Massalski: Yes, and more will come.

02-01:49:15
Burnett: Can you talk about some of the applications that have come out of your research? You did a lot of work on aluminum and things like that. Can you talk about some of the benefits that materials science has achieved during your lifetime that really stood out for you?

02-01:49:36
Massalski: I know what you're asking me and I have to make a confession. I have not made too many very practical applications. My curiosity always went to understand what's going on. And, again, I make a confession. Today in materials departments particularly, and somewhat increasingly in physics departments of universities, people are encouraged to take patents. Okay. And if you look at somebody's résumé, he's published fifty papers and has fifty patents. The deans like this now, and et cetera. I have never registered a single patent. Why? I sometimes regret it. But that was the tradition that I learned actually in the British university. If you are a professor you do research, exercise your curiosity, and then publish it for everybody. The world has changed and now sometimes you do research on something that you do not want everybody to know, not right away. It's a pity but it happens. And secondly, it is considered desirable by universities to spin-off research of the professors into forming new enterprises, new companies, new growth of industry et cetera. And I was listening recently to a talk, by our new president at Carnegie Mellon University, Subra Suresh, I don't know whether you've heard the name. We have recently appointed a new president at Carnegie Mellon who is an Indian by origin, actually very good friend of mine. Very capable. We've edited a journal together in past years so I know him well. He gave a talk, and he was saying that perhaps people don't realize that, but, per post-doctoral fellow and faculty, we are the university that spins off more companies, et cetera, than any other university. I didn't know that. Yeah. So, this is a rather entrepreneurial kind of thing. And I see it as extremely valid, and important, but I was not brought up in this culture. Okay. So when I see my colleagues, particularly younger colleagues who discover something like the massive transformation, which is useful and has possibilities, I would never have thought of patenting it. But today perhaps I would, you see. And also today we consider that universities perhaps should be a source from which we spin things by developing the more fundamental ideas into practical applications. But I have not really been as conditioned to do this as others. That's why I'm digressing and commenting about spin offs, because you once or twice asked me how did my research activity go into opportunities. People have used my ideas of course, in various directions, because I've done so many things, okay. But I have never thought of actually taking a patent. Perhaps I should regret this?

02-01:53:40
Burnett: Do you think there are advantages and disadvantages of the transformation of the university to this kind of structure?

02-01:53:51
Massalski: Oh, we are touching a very sensitive subject. And I'm glad you said advantages and disadvantages. In a way, I am not happy with what is happening. You see, the second time I returned to America, this was a time where America thought that there was not enough fundamental research going on here, and so they were trying to bring people like me, also many of my colleagues from abroad, to start a group of metal physics, you see, or something like this. To look at explaining what's going on in these materials. Use physics, chemistry, crystallography, et cetera, et cetera. And I could get easy research money from the NSF, or Mellon family. And so that was okay because companies like Gulf, US Steel, Westinghouse, G.E., you name them, Bell Labs, ran their own research labs for industry, including even some basic stuff. They have discontinued this. US Steel razed their research building. They want to do research that pertains to the products they are making, okay, and they don't really want to share this with other institutions because of competition. And so, gradually, the research that industry should be doing has been transferred too much to the universities. So that now there's pressure on junior faculty to do what I would have considered in my time really industrial research, rather than fundamental research. Physicists are less affected by this because they are researching more fundamental stuff. But material scientists are quite a lot affected by it. I personally think that there are some dangers in this, that we should go a little bit back towards the model where if you are a professor at a university you're looking more for explanations rather than applications.

02-01:56:23
Burnett: That's very interesting. Well, I think we should perhaps wrap-up for now. It's getting late. I wanted to give you the last word. Do you have any advice, perhaps stemming from the comment you made just now, for young material scientists coming up through the system into their new careers going forward?

02-01:56:53
Massalski: Interact with people, number one, don't get isolated. And number two, don't be blind to opportunities and use them when they come. It's a funny thing. Many of my students and my post-doctorals, et cetera, have been asking me, "Well, so now after this what should I do?" I always say, "Go and attach yourself to the most respected and accomplished person that you can think of and try to persuade them to take you on," because you learn immensely from people who are knowledgeable and critical and you can benefit from this throughout your career." And all the good turns in my scientific career were simply that: I happened to be somewhere—a situation got engineered somehow, such that—I happened to be where something interesting was going on and I was free to follow it later if I wished. And so if I were advising my own son where to go I'd say, "What do you want to do? This? Who are the

most accomplished people in this area, person in this area. Go and join him, at least for a while." It is marvelous to be able to do what you want to do, and reasonably survive financially. That is what universities should be for.

02-01:58:19
Burnett: And it seems from your description, you talked about your scientific curiosity and your moving from domain to domain and question to question. But the other important feature of your career is that you have been at the center of a network, that you seem to be bringing people together as much as you—

02-01:58:40
Massalski: Yeah, but that's good luck.

02-01:58:41
Burnett: That's good luck. [laughter] But it's important in that you've been in all of these different institutions, you've gotten to know all of these key figures. And you've been able to connect people. I think that's an important feature of an accomplished scientist. Yeah. I want to thank you so much for taking the time to speak with us.

02-01:58:59
Massalski: Thank you very much, Paul.

[End of Interview]