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AMERICAN INSTITUTE OF MINING, METALLURGICAL, AND PETROLEUM ENGINEERS

ORAL HISTORY PROGRAM

Siegfried Hecker:
My Grandfather Saves the World Part

2018

TABLE OF CONTENTS

PART 1

00:00 Introduction

01:57 The Early Years - Born in Poland During WWII - Growing up in Austria After the War

04:30 First in Family to go to University - Case Institute of Technology

07:07 Influential Colleagues and Professors – Bob Smialek– Terry Mitchell – Lynn Ebert

10:01 Ph.D. Studies at Case - Effect of Multiaxial Stresses on Material Behavior

14:56 The Road to Los Alamos

18:48 Changes in the Educational Landscape - "It's so much richer"

22:25 Postdoc Work at LANL - Yield Surface Work

28:07 Industry Work at General Motors Research Laboratory - Applying Multiaxial Plastic Deformation to the Mission of General Motors

35:56 Leaving General Motors - The Anatomy of an Organization

37:13 Transition Back to LANL - Exposure to a Whole List of Other Really Intriguing Things

39:43 Plutonium Metallurgy - Plutonium the Most Challenging System to Work With

49:04 Being Drawn Into Uranium Work

52:13 Working With Russia Post Cold War

PART 2

00:10 Introduction

00:52 Director of Los Alamos National - Going From Scientist to Director of LANL

04:32 Division Leader of the Materials Science Technology Division

06:16 Heading up the Center for Material Science

08:11 Stepping up as the Director of LANL - January 15, 1986 - A Year Fraught with Disaster

12:15 Changing Relationships - Greatest Challenges as Director

14:40 Last Nuclear Test - Divider - September 23, 1992 - Defining Stockpile Stewardship 19:04 Plutonium Production Facilities - R&D

23:45 Federal Laboratory Partnership - Victor Reis - Assistant Secretary of Defense Programs at the Department of Energy

26:03 What it was Like Working with the Russians - Reykjavik Summit

27:58 Threshold Test Ban Treaty of 1974 - Russians out at Mercury for Kearsarge Nuclear Test

31:53 Americans at Semipalatinsk - Lab to Lab Cooperation - Visiting Sarov

36:01 From Confrontation to Cooperation - The Start of 25 Years of Cooperation

40:25 Benefits of Professional Society Membership - Platform for Presenting Papers and Networking

44:25 Advice about TMS Membership - University Student Chapters

46:01 No Regrets - I Don’t Think I Would do it Differently

49:08 The Human Genome - A Long Way From Nuclear Weapons

54:05 Non-proliferation International Diplomacy - 2 Minutes to Midnight

55:54 Los Alamos 50th Anniversary - From Russia With Love

01:01:21 Recommended Qualities for Young Engineers - Be the Best

01:02:52 Lessons from Academe, Industry, and the National Laboratories - Don’t Ever Sell Them Short

01:04:33 Metallurgy and Diplomacy in Russia and North Korea

01:13:11 What Keeps You Going - My Grandfather Saves the World

PART 1

00:00 Introduction

Nizolek:

Today is Tuesday, September 11th, 2018. This is an interview with Dr. Sig Hecker conducted as part of the American Institute of Mining, Metallurgical, and Petroleum Engineers oral histories project. The interviewer is Tom Nizolek and the location is Sig's office at the Los Alamos National Laboratory here in New Mexico. Sig is a Senior Fellow Emeritus of the Freeman Spogli Institute for International Studies and the Center for International Security and Cooperation at Stanford University. He is director emeritus of Los Alamos National Laboratory and the recipient of a remarkable number of awards and honors. Sig, due to time constraints, I won't be able to do you justice. To give our audience a sense, you are a member of the National Academy of Engineering, a foreign member of both the Russian Academy of Sciences and the India Institute of Metals, Fellow of APS, TMS, ASM, the American Academy of Arts and Sciences and Honorary Member of ACS. That was naturally a partial list, but, by any accounts, it's clear that you have a remarkable career. I'm very pleased to be here today to interview you. I hope that we'll cover your early career time, both as a postdoc at LANL and at GM before returning to LANL where you spent most of your career, and, of course, you're now at Stanford. I also want to cover your outlook on the engineering profession. But, perhaps, to start, Sig, can you describe any of your experiences during your formative years that led you to embark on this career, starting with your upbringing and schooling?

01:57 The Early Years - Born on the Russian Front - Growing up in Austria During WWII

Hecker:

I grew up in Austria. As it turns out, I was actually born in Poland of Austrian parents as an accident of the war. I was born there in October 1943. My father was stationed in the German army on the Russian front, and they were allowed to take women and children along. I was born there. Things got pretty rough. Women and children were kicked out of there. I was four months old and eventually wound up back in Austria proper, and that's where I grew up.

I went to school until I was 13 years old. During those times, I don't think I had ever imagined that, first of all, I would be anywhere but Austria. Because when I was growing up, we lived in army barracks that were renovated, with no running water, essentially no central heat, no books of any sorts. So, I didn't grow up in an environment where I thought I would ever go to university. What I really liked doing when I was a kid growing up in Austria was playing soccer and skiing. My dream, when I was 10, 11, 12 years old, was to be a professional skier, like every other kid in Austria that grows up in the mountains, which is where I grew up - in a little mountain town, about 4,800 people.

Nevertheless, I was good in school. I really enjoyed math. I enjoyed science. However, for a variety of reasons, it turns out, my father never came back from the Russian front, and so I really didn't know a father. My mother eventually remarried, and then we joined relatives who'd moved to the United States. So, I got to the United States in December 1956. At that time I was 13 years old, and I came to Cleveland, Ohio. I went to a public school, which wasn't much quite frankly, but I was a good student. So, I started thinking I really should go to university. I was the first in my family to go to university.

04:30 First in Family to go to University - Case Institute of Technology

Hecker:

Being a reasonably new kid in this country, having to learn how to speak English, I thought, well, what do you want to do? This was going on to 1961. The big thing was nuclear science. So, I wanted to be a nuclear physicist. That's what I decided in high school I wanted to do. The local school in Cleveland was Case Tech, Case Institute of Technology, and I thought, well, maybe I wouldn't go too far away. I also looked at Carnegie Tech, which was in Pittsburgh and not too far away. So, probably, like every foreign-born kid that thinks about going to college in the US, I thought I should apply to Harvard. So I did apply to Harvard also. However, as things would have it, I wound up staying home in Cleveland.

I managed to get a scholarship to go to Case Tech. We didn't have any money in Cleveland either, so I couldn't have afforded to go to school without the scholarship. I still remember John Hunting scholarship, and it turns out you had to be a U.S. Citizen in order to get this scholarship. So, here I was in the fall of 1961, I was about to start Case, but I hadn't been in this country five years yet, which is what it takes to get citizenship. The folks at the scholarship committee were kind enough to say, “Well, if you apply for U.S. Citizenship, we'll take that as the intention.” They gave me the scholarship, and I wound up going to Case Tech.

I started in physics because I wanted to do nuclear physics. After a couple of years, I thought physics is fun, but I'm not learning anything that would allow me to get a job after four years because I would have no intention to go past four years. In my family, the idea was, they sort of take care of you until you're 18, and at 18 you go out. You're on your own. You have to take care of how you're going to make a living. I thought, well maybe at age 21 or 22, and then I would get a job. In Cleveland, most of the jobs, particularly at that time, were manufacturing automotive, steel, aluminum. So one of my colleagues said, "Hey, why don't we go look over at the Metallurgy Department at Case?" So we did. I joined in metallurgy and materials for my third and fourth years. That's where I got my B.S. Degree, at Case in 1965.

07:07 Influential Colleagues and Professors – Bob Smialek– Terry Mitchell – Lynn Ebert

Nizolek:

Were there any teachers that stand out in your mind, teachers or classmates that influenced your decisions to pursue metallurgy over physics?

Hecker:

Yeah, I would say the largest influence was probably my colleague, a fellow by the name of Bob Smialek. He's also a card-carrying member of TMS. Bob and I got to be good buddies. It turns out; it was an interesting background. Bob went to Saint Ignatius, which was a very, very good school, Catholic high school. I went to East High School in Cleveland. I had a minimal high school education. He had a great high school education. He was much better prepared than I was, but then we leveled out. He's the guy that had this influence by saying, "Why don't we go look at the Metallurgy Department?" So, we did. There in the Metallurgy Department, actually, I would say during undergraduate years the biggest influence during those days, I would sort of call the British invasion. This isn't just The Beatles. It was the British metallurgy and materials community that came to the United States. At Case, we had three very young British faculty. They were British graduates, came to be faculty at Case. One of them, it turns out he wasn't all that much older than I was, Terry Mitchell, came from the UK. He was a young professor at Case. He took Bob and me in during our senior year, and we did a senior thesis with Terry Mitchell. We got our first scientific publication in 1965 because Terry Mitchell worked with us. It was called Dislocation Pile-ups in Anisotropic Crystals. So, and the Dislocation Pile-ups in Anisotropic Crystals published in Physica Status Solidi in 1965. So I said, "Wow, that's not bad. It's 1965, I'm 21 years old, and I get a publication." That really helped to convince me that I should go on to graduate school. So he had a substantial influence. Then later when I came back, to Case for graduate work, my thesis advisor, a professor by the name of Lynn Ebert who was in mechanical metallurgy. He had an enormous influence and the influence there was mostly, he said, "Hey, look Sig, I'm going to get you out in the right direction, but you've got to figure out what you're going to do. That's where I learned you got to go on your own. You got to think things through. You've got to pose the right question for what you want to do. So he had that influence.

10:01 Ph.D Studies at Case - Effect of Multiaxial Stresses on Material Behavior

Nizolek:

What was your Ph.D. topic on?

Hecker:

What Lynn Ebert laid out initially- he was interested, at that time, in composite materials. I was initially interested in actually working with Terry Mitchell on much more of the physics side of metallurgy because that's what he did, coming from the UK, was metal physics. Lynn Ebert was a mechanical metallurgist, and actually, there was a strong line of mechanical metallurgy programs at Case. So he laid out the general area saying, "Look, fiber composites are just about to come into their own in terms of applications, be it aerospace, be it wherever. So. why don't you do something in fiber composites?" I had an interest in mechanics and mechanical metallurgy. I'd taken a lot of math classes and mechanics classes. So, we then sort of jointly defined an area that was my thesis topic, and that was to understand the effect of multiaxial stresses in different types of composite materials. So, it being a Ph.D. thesis, you had to go deep enough to try to understand the mechanics. So, I did a lot of development of actually somebody's plasticity theory to try to understand the transition from elastic to plastic behavior in fiber composites. Then, I built a very, very simple model composite of two materials. I picked them because they had interesting mechanical properties. I didn't pick them that, later on in my life, like in 30 or 40 years later, I'd actually come back to that because they were also important in the nuclear business. That was totally an accident. One of them was maraging steel. What's interesting about maraging steel, of course, there's a lot of fascinating metallurgy, but I didn't study that metallurgy particularly, I had to understand it.

So, maraging steel can have very high yield strength. For example, there's something called 350-grade maraging steel that has a yield strength of 350,000 psi. I wanted the steel that had a very, very high yield strength. So, you had a long elastic region, and then the inside of a cylinder of maraging steel, I diffusion bonded, a rod of beryllium. Now, it turns out that beryllium is a peculiar material. Of course, one of the things, it's toxic. So, you have to worry about beryllium, how you work with beryllium in a university environment. As I look back, some of those practices probably weren't the best in the world, but I managed to diffusion bond beryllium. So, the beauty of beryllium, it has this incredibly low Poisson's ratio. Poisson's ratio essentially tells you how much does a material contract if you pull it uniaxially, elastically. Most materials have a Poisson's ratio, somewhere between 0.25 and 0.35, or so. Beryllium is

0.05. That essentially means you pull it, and it doesn't contract. And, because the maraging steel wants to contract, you set up triaxial stresses. So, the idea was to set up this sort of modeled triaxial stress state in this composite and then go ahead and be able to predict its behavior and its transition from elastic to plastic behavior. So, that's what I did for my thesis. I started that in the fall of 1965.

I finished my master's and got a master's still from Case Institute of Technology. But, before I was able to finish my Ph.D., Case Institute of Technology federated, as they called it, with Western Reserve University and became Case Western Reserve University. I wound up getting my Ph.D. one year later in August of 1968. That was from Case Western Reserve. That work on my Ph.D. thesis got me intrigued about the effect of multiaxial stresses under deformation, both elastic-plastic fracture of materials. That's when I decided that's what I'd like to do, either for a postdoc or for assistant professorship. That's when I started looking around, but, in between, I had a stint at Los Alamos.

14:56 The Road to Los Alamos

Nizolek:

How did you come to Los Alamos for a postdoc after your Ph.D. studies at Case?

Hecker:

The reason I came for the postdoc was very much because I came here as a summer student in 1965. Going back now to graduating from Case Institute of Technology four years, and then there were several major changes in my life. First, I graduated, and then the second, I got married. I married my wife, Nina, in June of 1965. So, I actually graduated one day, got married like three days later, and then left for Los Alamos, New Mexico for a summer job the following day. So, we came here on our honeymoon in June of 1965. Quite frankly, there were two reasons why I came to Los Alamos. Again, I was still this kid that came from Austria, came to this country, was still sort of really amazed by what this country would allow a foreign-born person to do. First of all, to come here, go to school, and then actually to be able to work at a place like Los Alamos. The whole key to my coming to Los Alamos, the reason I came was one, there was the fame of Los Alamos. If you think about it, this is now 1965, so 20 years after the Manhattan project. There was this incredible fame of this laboratory. However, the second reason for me was probably just as important. There were brochures in the administrative hall at Case, called Tomlinson Hall. They had this brochure of Los Alamos Scientific Laboratory, which is what it was called at the time. It showed, some of the facilities here, but the most important part, it showed a photograph of the ski area. I said, oh my God, I could go there and ski because when I came from Austria to Cleveland, as you might imagine, there wasn't much skiing in Cleveland, Ohio. The hills were maybe a hundred to 200 feet high.

So, just that attraction, of course, I knew I couldn't ski in the summer, but the attraction of mountains being able to get back in the mountains. So that's what brought me to Los Alamos first, and then my wife and I, as I said, we came for our honeymoon. So that three months here was really influential then, sort of in the rest of my life. First of all, that was the first time when I worked on plutonium. At the age of 21, I was in the CMR building, which is not far from this office, Chemistry and Metallurgy Research building. So, literally within a week, I was in the glovebox laboratories doing roll bonding of plutonium. I was under the strict instructions of my mentor as to how you work in a plutonium laboratory because that’s what you do in a plutonium laboratory. And so, I became fascinated by plutonium. That also helped then later on, in terms of deciding what I wanted to do in life.

So then the beauty of this place, having spent nearly three months here in the summer was great. That was part, the important part, of the decision as to why to come to Los Alamos in 1968 for a postdoc. Although it was a really close race because I almost came very close to taking a faculty position at the University of Illinois in 1968. But, in the end, I decided to come to Los Alamos.

18:48 Changes in the Educational Landscape - "It's so much richer"

Nizolek:

You said you almost went to become a Professor right out of graduate school. You've now returned to academia this time as a Professor at Stanford, and you teach students. How has the educational landscape changed since you were a student at Case? I know it's a very different subject area, but do you have any general remarks?

Hecker:

I would generally say, today; it's so much richer than it was back in 1965. Perhaps part of that was just because of my limited outlook on life, in terms of what I knew before I went to college and so forth. So, that could be part of it. For example, as I was going through Case, for the most part, I was just interested in science, mathematics, and then, metallurgy, physics, metallurgy. All those other courses that we had to take to me were mostly a nuisance. Why did I have to study world history or the history of art and those things? I still have those textbooks. But for the most part, I would say, I didn't have much appreciation for that.

I look at the students today at Stanford, and they have such a richer environment than what I did. So, I teach classes and when the students come to see me and ask for advice as to what to do. I'd tell them, "Look, I go back to my own experience at Case and make sure while you're there at school, get a broader education. I mean, this is the time of your life when you can do that." I was, I'm going to say pretty proud that I raced through from undergraduate school to getting a Ph.D. in three years. As I look back now, that was not a good decision. That would have been a time that I had a chance to, perhaps, take some courses in international relations or take some of the humanities or learn something about the world or learn more about entrepreneurship.

So Case was, actually, already starting to look at those issues as to what you do, to see how you get from science to technology. I didn't do any of those things. I was in a hurry. It turns out, by the time I left my Ph.D., my wife and I, we had two kids already. So, there was this other issue that somehow I had to go and make some money, more than I was able to get as a graduate student. But, it's just this much richer environment and, at least of what I find out at Stanford, there is this terrific interplay of research and teaching. That makes the teaching experience for the students, a much richer experience. At Stanford also, you have much more of the interplay of a sort of the rest of the world and the humanities and how they interplay with whatever your interest is. That's important, as well. So I think that to me, and again, this may reflect just my own limited background and upbringing, we're producing much better-prepared students today for that complicated world we're going to live in then when I got out.

22:25 Postdoc Work at LANL - Yield Surface Work

Nizolek:

So, you mentioned beryllium and maraging steel and how you worked on them during your Ph.D. What did you do as a postdoc here at LANL?

Hecker:

So, when I came to Los Alamos, at that time, as I had mentioned, I was particularly interested in the effect of multiaxial stresses on material behavior. So, when I did plasticity analysis for composites during my Ph.D. research, I looked into the state of the art as to how does one deal with the transition from elastic behavior, that is reversible behavior, to plastic behavior in materials, and how do you describe that? I found that there were a couple of very simple theories. There's something called the von Mises yield criterion, for example, that students in metallurgy and mechanics learn. There were people doing research in several universities. I remember, particularly, a guy by the name of Aris Phillips at Yale who looked at the very nature of the yield surface and did experimental work to see whether you can map out the elastic to plastic transition under multiaxial stresses.

So, during the end of my Ph.D. work, I said, this is an area that simply isn't well defined and what it needs is more experimental work. Aris Phillips had, essentially, just about closed up shop, and there was hardly anyone in the U.S. doing yield surface work. I said that's what I'm going to do. So, I came to Los Alamos for a two-year postdoc, '68 to '70, and I did nothing on nuclear at all. That was the beauty of Los Alamos. They, essentially, said, "Look, why don't you come back, you have been here as a summer student, why don't you come back as a postdoc? You can do whatever you want it. We'll support you; you do whatever you want." I thought that was a good deal. Besides not only do what I want, but my salary went from $4,800 a year as a graduate student to $12,000 a year!

My wife and I thought we'd sort of died and went to heaven. In addition, they let me do what I wanted to do, and that is to do yield surface work. So, the beauty of Los Alamos again, was they had people here who could do anything, make anything, measure anything, advise you on any theory on anything whatsoever. So, I came here, and I said, "Well, look, I need to be able to create multiaxial stress states. The two major ways of doing that are you take thin-walled tubes, and you either pull them, push them, although you can't push them very far because they'll collapse. Then, you internally pressurize and as you internally pressurize and pull and the push that gives you multiaxial stress state." Later on, eventually, we went one more step, which is, if you can add torsion to that as well; that is torsion plus tension, or compression and internal pressure. However, during my postdoc, we didn't have a machine that did that, but we did the internal pressure. Because of the enormous machining capability we had at Los Alamos at the time we made these intricate samples. Machined out of full stock, out of aluminum, copper, and then machined down to a thin wall. Then, I would go ahead and put these in an Instron machine to be able to do tension or compression, internally pressurize, put strain gauges on and then start to load them. What I was particularly interested are there such things [as] a corner on a yield surface? So, the von Mises criterion is an ellipse. It's smooth, and it turns out, if you have a smooth yield surface and you load elastically, and you get to that yield surface, to that boundary, then the direction of the plastic increment is fully defined.

If it has a corner, then there's an ambiguity. It's not defined. That could have a lot of impact as to how the plastic flow actually occurs. So I spent a couple of years doing these experiments. It took me, maybe, not quite six months to set up the experimental apparatus, the machining of the tubes. It was, again, because of Los Alamos. So we machined these tubes, and I had this super machinist, here, at Los Alamos by the name of David Murphy. He was so good in the machining that he did, in order to not influence the structure in such a terrible way that you could never know what you had. We published a paper together, a technical paper on how you could machine these thin-walled tubes. So, I did that, and then, went ahead and measured the yield surface and at least convinced myself, and I think a lot of the community, that, in most likelihood, at the very tiniest level when plastic flow first starts, there actually is a corner on the yield surface. So you're going to have to deal with that uncertainty. The bottom line, then, is what I did was to study the multiaxial flow experimentally and particularly to define the yield surfaces in simple materials like aluminum and copper.

28:07 Industry Work at General Motors Research Laboratory - Applying Multiaxial Plastic Deformation to the Mission of General Motors

Nizolek:

After your postdoc at Los Alamos, you spent a period of time working at GM research labs. What precipitated that career change and what brought you back to LANL?

Hecker:

So, one of the things at Los Alamos at that time, the postdoc program was set up such that the general rule was you do your postdoc, and you leave. So, they did not encourage postdocs to stay. That was probably a good thing because I think I would have been inclined to stay because, by the end of my postdoc, we had three kids, three daughters. This is a beautiful town, beautiful place, great place to raise your kids. But, the general sense of the University of California, which, of course, ran Los Alamos was you do your postdoc, and you move on. So I thought, okay, I'm going to move on.

The first choice was actually at that point then, to go to a university and go into a professorship line, because that's what I had thought I was going to do in '68 when I got this job offer from University of Illinois. I was really tempted to do that. I was still 24 some years old. I got a job offer to be an Assistant Professor. That was pretty attractive. Then, that night when I had dinner with the Department Chair, he said, "Sig, I'm going to make you this offer now for an Assistant Professorship, but, if you want my advice, fatherly advice, don't take it. He says, go do a postdoc. You could benefit from a postdoc. You wouldn't have to worry about raising money or have to worry about teaching right away, setting up a lab. Then two years later when you're done, I'll still offer you a job." So I said, "You can't beat that." That's why I took the Los Alamos postdoc. So then, in '70, I looked at Columbia University and Stony Brook, they had openings in mechanical metallurgy. That was the time when I decided, maybe what I should do, in terms of giving me the best sense of where I want to wind up, is go into industry. I've had some university exposure by going to school. I've had a national lab exposure at Los Alamos twice - summer and postdoc. Why not go into industry? It turns out, at that time, so this is 1970, General Motors was trying to build up its technical center and particularly its research laboratory. Until that time, the U.S. used to have superb industrial research laboratories. In the metallurgy business, US Steel had the so-called Bain labs.

There were a number of other places. Alcoa had great laboratories. In the auto business, it was Ford that had the best laboratory, called the Ford Scientific Staff. However, by that time, General Motors wanted to build up its capabilities. So I wound up at General Motors. What I did at General Motors then was, and again, they were terrific to me. I got whatever I wanted in terms of equipment. That's when I got the tension-torsion machine. I wanted to study multiaxial plastic deformation still. Ah, but, naturally, if you're going to go to work for General Motors, then you have to say, how do you apply that stuff to the mission of General Motors, which is to build cars? So, I got interested in sheet metal forming. That's when I started to do sheet metal forming research and tried to, again, understand the role of materials properties.

Then I had to expand big time into steels, but that was not all that difficult because Case Tech, when you went to the metallurgy materials department, you had to know the iron-carbon phase diagram. It was all about steels. That was important. So, I went back into steels, but then, also, into aluminum. It was a fascinating time. It was right around the time of the first energy crisis, and the car companies were worried about gas mileage. The gas mileage back in those days, if you had an old Buick Super, maybe was eight miles per gallon. So they figured out the best way to get better gas mileage is to make a lighter car. So it turns out the French had been putting aluminum sheet steel into auto bodies and had developed some pretty decent aluminum alloys.

So I started looking at aluminum alloys. While I was still doing research on multiaxial plastic flow, I was also helping with the stamping of aluminum car bodies to go into the Vega, which was one of their not so successful small automobiles back in the early 1970s. I worked for General Motors, did a combination of still ongoing what I would call pretty basic research in the metallurgical world, as well as very applied research going out to the sheet stamping places, which were, at that time, called Fisher Body, and work with the Fisher Body people. I guess, as I look back in my career, that's where I got my best satisfaction, is a combination of trying to understand things in a pretty fundamental way but then also what do you do with that stuff? How do you apply it? This was a direct way to apply that knowledge to how you make a better automobile body. Also, at that time, the other way to reduce weight was the development of what is called high-strength, low-alloy steels to go into the structural parts of the cars and so forth. Some of the heavier sheet metal parts. I did a bunch of work there at General Motors and sort of switched the interesting and technical pieces. It went from essentially the yield limits of flow, where you have the elastic to plastic transition, to the transition on the other end after lots of plastic flow. At some point, things either go unstable, or they break. And so, then, I started dealing with something that is called forming limits.

While at General Motors, I was developing these ideas for how to determine forming limit curves. A good colleague of mine at National Steel, by the name of Stuart Keeler, had developed this concept of a forming limit curve. While I was at General Motors, I developed some techniques, what I call the simple technique for determining a forming limit curve. In fact, the interesting thing today, since today, of course, one has all of these citation indexes and ResearchGate, which I joined a couple of years ago to find out something that I was looking for. So, my most cited papers today are still those forming limit papers from General Motors, back in 1972 to 1973. I get at least four or five of those a week. People are still interested in that stuff. So, then, the interest at General Motors was in the typical industrial materials, namely aluminum, steel, higher strength steel, and so forth. Then the scientific, technical interest was first plasticity and then the issue of large-scale plasticity.

35:56 Leaving General Motors - The Anatomy of an Organization

Hecker:

Then I left General Motors. The main reason for leaving General Motors was, although they were so good to me, but as I looked at the anatomy of an organization, what I found at General Motors is the people who were really good technically at one time, they typically got promoted up into management. There were just a very few people who sort of got old on the job and were able to stay technically, scientifically really active. There was clearly a push in the management direction. I was, at that time, I was 28, 29 years old. They were trying to show me a path towards management, give me a free car to drive, at that age, and I said, "I don't want to do that." So at Los Alamos, they used to call me every six months and said, "Are you ready to come back yet?" The other thing that helped, my wife, said, "I'm not staying here in the Detroit area." So, it was easy to come back to Los Alamos. I came back at the end of August 1973.

37:13 Transition Back to LANL - Exposure to a Whole List of Other Really Intriguing Things

Nizolek:

You said you came back to LANL in part to avoid the push into management. But of course, we know that you became the director of Los Alamos National Laboratory.

Hecker:

That wasn't by design, Tom.

[Laughs.]

Nizolek:

What did you do for the period of time between coming back to LANL and...

Hecker:

So in 1973, I would say, until I took the sort of first steps into the lower level management position, which was '81, 82 or so - so, almost 10 years. That was probably the most productive time of my life here at Los Alamos. It was just fantastic. So, the areas that had to change, somewhat, but again, Los Alamos, back in those days was still, let's say, our overall funding was such that, that the management had a lot of discretion about what it wanted to fund. So, it was pretty clear when I came back, the management in material science and technology arena, they wanted me to look at plutonium. However, they also realized, that I'm not going to come back just to look at plutonium because the chances of publishing much of anything in that arena is going to be pretty small. So, they essentially said, "We'd like you to look at plutonium and some of our applied problems. But, you go ahead, and you keep doing the things that you enjoy doing."

Hecker:

So I continued to study plasticity, particularly at large plastic flow. I was intrigued by large plastic flow in a variety of different materials, and that's where the interest then comes up with also trying to model plastic deformation, to worry about the onset of anisotropy, and textures and so forth. So that opened another area, sort of in this continuing area of mechanical behavior, plasticity, multiaxial stresses, large plastic strain. So, I would say, that sort of continued this string of things that I was generally interested in. So then, I got exposed to a whole list of other intriguing things, and I'll get to it, but, eventually, the most intriguing of those was plutonium metallurgy.

39:43 Plutonium Metallurgy - Plutonium the Most Challenging System to Work With

Hecker:

Along the way, at Los Alamos, you also had this interest now because of the various applications at Los Alamos. For example, in the nuclear weapons business, if you look at, first of all, the materials that are in a nuclear weapon, obviously are pretty exotic things, and they could include things like beryllium. The maraging steel comes later. That comes in when you come to uranium enrichment, which is a whole different problem. I didn't do that here at Los Alamos. So, you get into exotic materials, such as plutonium, and then you get into situations of extreme behavior of materials. Really extreme. As you might imagine, you take these exotic materials, and you go ahead, and first of all, you implode them with high explosive, and that does lots of things to them. Then, in addition, they wind up blowing up in a nuclear reaction.

Then, if you lay out sort of the space of behavior that you're going to try to map, obviously, you have multiaxial stresses for sure, but you also have high strain rates and shock loading. So, all of a sudden now, those issues come into play as to how the materials behave at high rates of strain and explosive loading. So, I became interested in that part as well. You got high temperatures, and so you worry about high temperatures, high rates of loading. You worry about the effect of radiation on your materials. Again, these set of exotic materials, what does the radiation due to them? Even the intrinsic radiation in something like plutonium or, of course, if they're exposed to external radiation. So, all of a sudden, you define this incredibly complex phase space in which you have to understand materials.

To me, that was always then the fascinating thing. So, how do you do that? How do you get back to the basics, as to how do dislocations, for example, in materials care as to whether they are being hit with a shockwave or whether it's high rate or low rate. One of the other aspects of applications, which I found really, really fascinating, and I'd never played with before, and that is the application of nuclear batteries. What are nuclear batteries? Well in nuclear batteries, you, essentially, you take something that's a heat source, and, in this case, that would be plutonium. Because of the radiation of plutonium, the alpha radiation, it heats itself. Ordinary plutonium that we would call ordinary, the isotope 239, which is the weapons-grade plutonium, it doesn't heat itself very much.

So, you wouldn't be able to do much with trying to convert that heat to electricity. So instead, you use the isotope 238 plutonium, and 238 plutonium is basically 300 times as alpha active as plutonium 239. It produces an enormous amount of heat by its radiation and its radioactive decay. So, what one does is you try to take the plutonium 238, use that as a heat source and then convert that heat to electricity with thermocouples. Those become what we call radioisotopic thermoelectric generators. These are the nuclear batteries that we sent out into space. So, all of the photos you've ever seen of the Saturn rings, of Jupiter, of Pluto, all of those, the electricity was provided from these radioisotopic thermoelectric generators. The source of that is plutonium 238. Well, it turns out if you're going to make that big enough, of plutonium 238 to get significant heat that provides the electricity, the plutonium metal would melt itself. Because the melting point of plutonium is 641 degrees Celsius, it would melt itself. You have to turn it into an oxide, plutonium oxide, so it becomes a refractory material. A melting point 2,400 C or so instead of 600 C and so now you make plutonium oxide. You mentioned the Ceramics Society. That's how I got into the Ceramics Society because, all of a sudden, you had to understand the behavior of ceramics. And, in this case, we're working with plutonium 238, which is pretty hot stuff. For Plutonium 239, most of the public doesn't appreciate that it is quite easy to handle because it's alpha active. The only reason you'd really have to worry about is you don't want to breathe it, and you don't want to ingest it. But, the alpha radiation is such that it is stopped by your skin. Of course, you don't want to take that chance, so you put it in glove boxes, so it's pretty easy. However, the 238 is 300 times as radioactive. That's much more difficult to contain. It's also, therefore, much more of a problem if it does escape in a glove box. What this laboratory did, we did all of the 238 related work and, then, the safety-related work to be able to convince NASA and the regulators that if we put these heat sources, something about this big, with a lot of plutonium 238 capsules inside about an inch and a half high and inch plus diameter, maybe inch and a half in diameter; that, if we put those on a space mission, that in case that rocket blows up as it's going off or in case that rocket fails to sling around the earth and get sent out into outer space and comes into your backyard. How can you assure that you're going to be able to survive whatever that impact is and not spread plutonium 238 all over the countryside? That then defined the set of problems that, to me from the standpoint of my metallurgical materials career, it was just absolutely fascinating because now I had to worry about ceramics.

Then you have to clad this material. You have to put it in a container. Well, they're not many materials that can withstand both the heat and then the chemical potential reactions from the plutonium oxide. So, it turns out the material of choice is iridium. Iridium is element number 77, a nice face-centered cubic material. But, it turns out, it's also a strange, strange material. So, I started doing iridium. Again, because of the nature of Los Alamos, I was able to study the basic fracture behavior of iridium, the grain boundary embrittlement of iridium or the fact that iridium, even though it's face-centered cubic actually likes to cleave, which is very strange for face-centered cubics. But, there are very good reasons related to electronic structure. So, we had to do that and, then, we had [an] excuse for high strain rate because, if these things go and impact, come back from a sort of a mission abort and impact, they're going to come in 285 feet per second at the height, a thermal, a spike by re-entering and then, re-entering in your backyard. My colleagues here at Los Alamos, they were just fantastic. They built a gas gun to be able to go ahead and simulate one of those re-entries. Then, we had to study, the high rate behavior of those materials, and that's when I first went to- it was through a General Motors connection and a person by the name of Sid Green who had left General Motors- and went to Salt Lake City to build Hopkinson bars and other equipment. So, I went up to Salt Lake City, and we bought the first Hopkinson bar. Then, not long after that, we brought in Rusty Gray and a couple of his other colleagues, in order to be able to get them to take a look at high rate deformation. What happens with the material, high rate, high temperature. Eventually, of course, Rusty then built up the best high-rate group. Particularly that has to combine sort of metallurgical structure aspects with the mechanical deformation and fracture. He built the best group anywhere in the world. So, that got us into high rate, into iridium. We had to worry about graphite, graphite composites because you had to build an impact shell to put these things in. So, ceramics, iridium, graphite composites, high rates, high temperatures, all of those things. Then, eventually, plutonium.

49:04 Being Drawn Into Uranium Work

Nizolek:

It's clear you've sought out difficult problems. You started working on steels, which is something that's widely considered to be a very complex phase diagram in industry and worked on things like your various uranium alloys, plutonium, something so complex that you worked with the Russians after the Cold War to try to reconcile the differences between our two phase diagrams for plutonium. Was plutonium the most challenging system that you've worked on as a metallurgist?

Hecker:

Oh, without question. In fact, what I didn't mention specifically, there was yet one other application that I was drawn to that had its own related problems, that drew me into the uranium work. There were certainly the uranium-related challenges also in a general nuclear weapons program. But, the more fascinating uranium challenges were actually with what we call advanced conventional munitions. So, you would ask, "Well what are advanced conventional munitions?" One of them is what is called long-rod penetrators. It turns out some of my colleagues here at Los Alamos had developed the uranium alloys that are best for long-rod penetrators. Then, there were other people here at Los Alamos, because Los Alamos, as you might imagine, is really good with explosives. So, they had developed, a different concept of what is called shape-charge liners.

A shape-charge liner, essentially, is a chunk of metal. And, in this particular case, their innovation was to take a thin-walled hemisphere material, put explosives behind it, a column of explosive, light off the explosive. It takes that thin wall and turns it inside out, essentially extrudes it inside out and forms a penetrating metal jet. That metal jet is the deadliest thing, to penetrate anything in the world. And, they found out that uranium is the best of those liners. Typically, people had used copper, but uranium is better. That's where my sheet metal forming work came in. They came to me and said, "Hey, look, we have this one particular grade of uranium, and it works better than anything that we've ever tried, but we don't know why." So, I went back to my General Motors days and started doing all the testing, that tensile testing.

In the end that led me to understand that it was the texture of that uranium. It was the texture that made it work that well. And, what I mean by penetration is that jet is able to penetrate what we call a rolled homogeneous armor, one meter of rolled homogeneous armor, just right through. So, again, you talk about fascinating material challenges. I'm not sure that I looked them up. They sort of looked me up. So, those challenges, somebody comes and says, "Hey, we don't understand this uranium alloy."

52:13 Working With Russia Post Cold War

Hecker:

So back to plutonium. I started working with the Russians, but not because of the plutonium at all. It's a long, complicated story because, by this time, I was out of the laboratory and then, actually, by the time I did the first real detailed work with the Russians on the plutonium phase diagram, I was not only out of the laboratory, I was also out of the directorship.

Hecker:

I got into the Los Alamos Directorship in 1986. I was Los Alamos Director from January of 1986 through November of 1997. Then I left the directorship and sort of went back to staff. That's when I started then again, going back into the plutonium world. By that time, during my directorship, the Soviet Union collapsed. Russia was reborn. For a variety of other reasons besides metallurgy, I began to work very, very closely, with the Russians, over the years. It was developing a relationship with the Russians related to security issues: security of their nuclear weapons, security of their nuclear materials, security of their nuclear facilities, questions about export and security of their people. Then, eventually, from 1992 when I first went over to Russia until 1998 when I met their senior plutonium metallurgist, a woman by the name of Dr. Lidia Timofeeva. So, it, it was the rest of the work that we did together that allowed us to get sufficient trust and knowledge of each other where we were able to tackle the pretty sensitive issue of plutonium. And, we did.

PART 2

00:52 Director of Los Alamos National - Going From Scientist to Director of LANL

Nizolek:

I want to go back to discuss the time that you spent as director of the laboratory and ask you about non-metallurgical problems. You became the Director at a very young age, 42, I believe.

Hecker:

Right.

Nizolek:

What was it like to go from a scientist to Director of the laboratory three years before the fall of the Berlin Wall? Surely, there had to be a lot of non-metallurgical problems that you had to deal with. What was the on the job training like for that?

Hecker:

It was like being thrown into the ocean, sort of jumping in. I would say all those other metallurgical materials, scientific problems, were pretty simple compared to the directorship problems. First of all, I would say from my standpoint, there was a good part of that job that I was totally unprepared for because, I really had not dealt internationally in the policy arena with other countries. I certainly had dealt internationally with the UK and then, some with the French, in terms of materials, weapons-related programs, and things of that nature. We had good interactions on materials with the French. Of course, the U.S. had a very intense collaboration with the UK. I worked on that for many years, but I was not involved in the policy, diplomacy, any of those things. In fact, I had mentioned before, I really had hoped that I'd eventually become a university professor. So, this business of laboratory directorship was not on my screen. I never wanted to be the Director. I never even thought of being the Director of this laboratory. I had done internally, within the laboratory, as I'd mentioned before in '81 or so, was the first time I started taking some management responsibilities. The new Director that came in in 1980, Don Kerr, he set up a number of working groups and one of them was a material science working group. For whatever reasons, I was selected to chair that working group.

So I chaired that working group and tried to understand the importance of materials to the laboratory and the importance of materials to the whole nuclear complex and what it meant. That was my introduction of reaching out beyond my own interest into the laboratory, and I really enjoyed that. I enjoyed the challenge of trying to put the pieces together. I've always enjoyed that. Take the pieces, put them together and then try to lay out a sensible path forward. One of the paths forward from that was actually to try to rejuvenate material science at this laboratory. The technology is throughout all of the laboratory programs and essentially there's nothing you can do without materials, of course, whether it's the weapons program, nuclear energy program, the space power program, or fusion program for example. This laboratory has worked on thermonuclear fusion, civilian thermonuclear fusion. All of those have enormous materials challenges, and we still had that. But, we had sort of lost the fundamental base. So, then we wound up setting up the Center for Material Science in 1981 time frame. That helped to rejuvenate it.

04:32 Division Leader of the Materials Science Technology Division

Hecker:

I had become, through a couple of steps, the leader of what was initially called CMB division, changed the name, the CMB stood for chemistry, metallurgy, Baker. Baker was the division leader, and he was retiring. I eventually wound up as division leader, called it the Material Science and Technology Division. At that time, it had all of the major materials activities at the laboratory, all the plutonium activities, all the uranium activities, and all of the non-nuclear related activities. So, I was running a division of about 700 people. So, this was now 19-, became a division in 1983. So, I left General Motors in 1973 because I didn't want to become a manager. So ,10 years later, I'm here running a 700 person division, but still very, very interested in the technical aspects of all of that and trying to keep my hands in the technical aspect. This is just to lead up to how in the world did I get into the directorship? I know I'm not answering your question directly, but I think it's important. So, here I am at that point, and I was like 41 years old. I said, I don't want to run this 700-person division. I had some colleagues there. I said, they can run this division. I want to go back to the research world.

06:16 Heading up the Center for Material Science

Hecker:

I then headed up the Center for Material Science. So, instead of having 700 people, you've got a dozen people, lots of visitors, lots of excitement, go do your own research again. So, that was July 1st of 1985. Well in, late September, I had a visitor from the University of California because, our then Director, Don Kerr, decided to leave. He went out into industry, and, so, they were looking for a new Director. They came and interviewed me for what should we look for in a Director. So, I gave him a long list which said, this is what you need for a Director. Then, I got a call, maybe a month or a little later, and they asked me if I would be interested in being the Director.

I said, “No,” and they said, “Why?” I said, “Well, I don't meet my own qualifications. I just set all of this up for you, and I've never been around the world and the diplomatic world. I've never done much with Washington. Clearly, that's not who you want for the Director.” And, the University of California essentially said, “Why don't you let us decide that?” So I said, “Well, that's reasonable.” So, at any rate, I wound up being Director, never wanting to be the Director. So, that transition, that was immense. Not immense in terms of internal at the laboratory. I think there I actually had a leg up because I really knew how this laboratory functioned. So, this is 1986 by the time I became the director, and I first came here, as I said, a student, in 1965. Came back in a postdoc and then as a staff member in 1973. So, I'd been here, I knew how the laboratory worked, but the interaction with Washington and then, which you asked, as the Soviet Union starts falling apart.

08:11 Stepping up as the Director of LANL - January 15, 1986 - A Year Fraught with Disaster

Hecker:

So that was an enormous challenge. It was quite clear by that time that something dramatic was happening in the Soviet Union. So, I became the Director January 15th of 1986. And, just to give you an idea of the first year, late in January, the shuttle Challenger blew up, and that gave you a jolt, saying that some high-tech systems can fail. In April, Chernobyl blew up. So, you had a nuclear reactor blow up. In between, the FBI came to see me to tell me you have a Chinese spy at your laboratory, which then turned out to be a whole other story. So, and then, in addition, President Reagan had been building up the defense budget and then they had the first sequestration in April of 1986. So, now I'm faced with the fact we're going to get a budget cut. By October of 1986, Reagan and Gorbachev get together and say, “Why don't we get rid of nuclear weapons?” And I said, “How's that for job security? I've been on the job for one year, and these guys are talking about getting rid of nuclear weapons.” So, they didn't get rid of nuclear weapons. But, eventually, that led to getting rid of the Soviet Union –it collapsed. And so, then, my life revolved around these basic changes that were happening. The biggest change by far was how do you adjust to the fact that more or less what you would consider the main reason for your existence, nuclear weapons to deter the Soviet Union, and that other side has just gone belly up. How do you make that adjustment? That adjustment over the years, and we're still essentially in that adjustment period as to what do you do? How do you redefine? What do you now need from the laboratory? What's important?

My major job was then how do we make sure that our laboratory is prepared to do whatever it takes in whatever is going to come down the road. Even though you say I was young, I didn't think I was young then. As I look back now, I say, yeah, I was young. My kids are older than that, for heaven sakes. As I look back now, I would actually say it was a good thing I was young because I didn't know what can't be done. And so, we had these challenges. We had the Soviet Union going away. The country was thinking it's going to get a peace dividend not realizing that the peace dividend was going to be peace. It wasn't going to be a whole bunch of money. So, we had to make that adjustment and, out of that, came this whole concept of stockpile stewardship. The question was indeed why- I went to congressional hearings, why do we still- from the senators, congressmen, why do we still need you, the laboratory building nuclear weapons? Our world has changed. So, we then packaged that into saying our job is stockpile stewardship. We've got to make sure, as long as the country's policies are to need nuclear weapons for whatever reason, we're going to make sure they're safe, secure, and reliable. That's our job. Then what President Clinton added, you have to do that without nuclear testing. Nuclear testing had been our bread and butter, for all of those years. Because as much as we could do with computers as much as we did with theory, with modeling, simulation, in the end, it was always the experimental manifestation that informed us best. So, we defined a new challenge, and I thought that went pretty well. I would say we did that quite well.

12:15 Changing Relationships - Greatest Challenges as Director

Hecker:

The part that was much, much more difficult, is that our relationship, with the public, changed completely, at the same time. And, our relationship with the regulators changed completely. As long as the Soviet Union was there, we had some, I guess you could call it, immunity. And, that is, the country knew that it needed us, and there were certain things that we had to do in the conduct of our business; that was hazardous. Since we lived here and our families lived here, we always thought, of course, the most important thing is to do that and do it safely. But, the whole regulatory business just changed enormously when the Soviet Union went away. And, I would say today, so we're now we're talking 30 years later, we have not yet recovered from that. So, I think one of my greatest challenges as Director was how do you adjust to that new regulatory framework? How do you adjust to the new interface with the public? Los Alamos and the other weapons labs said, “Hey, you can't look in here. The stuff that we do, it's here because of the Soviet Union.” Then, all of a sudden, the doors blew open, and the public didn't particularly like what it saw. So, one of my greatest challenges there then was how do you go out into the community? How do you go through a three-hour hearing at the state legislature or a two-hour public hearing where they want to go get those big bad guys from Los Alamos? I would say, all of that, I had to learn on the job, how well it's done. It's still difficult today.

Nizolek:

You say that stockpile stewardship started under your tenure as Director, that was 26 years ago since the last US atomic test. Do you think stewardship is still a valid approach? Verse remanufacturing? Or is science still able to certify the weapons?

14:40 Last Nuclear Test - Divider - September 23, 1992 - Defining Stockpile Stewardship

Hecker:

So, the last nuclear test was on my watch as Director, September 23rd, 1992, and it was interesting. We had ways of giving the tests names, some of them were cities in west Texas or lots of other things. This one was called Divider. What was interesting, as you look back now, it actually was a divider. It was a divider between the era when we used to be able to do nuclear testing and then this whole new era of stockpile stewardship. So, I would change your question a little bit to say, “Is it a matter of stockpile stewardship or remanufacturing that is stockpile stewardship?” So, stockpile stewardship is the whole thing as to how in the world to make sure that we're able to retain all of the things we need to deter our adversaries, whoever they may be.

Of course, we had a huge nuclear arsenal of tens of thousands of nuclear weapons. That's been drawn way down. So, first of all, we have many fewer nuclear weapons. The second biggest change is that you can't test them anymore. So then, the stewardship challenge was how do we deal with that? I should have added; also, it wasn't just no nuclear testing. George H. W. Bush, in October of 1992, set the policy for the country, which said, “And, we will not develop nuclear weapons with new capabilities.” So, essentially said no new nuclear weapons. Okay. No nuclear weapons with new capabilities and no testing, but you guys take care of this in the meantime. All right. So, the way that I divided the world, in the nuclear stewardship business was you can remanufacture. That was not disallowed, so we could remanufacture the nuclear weapons to the way we had them or, you can extend the lifetimes of the weapons.

Now, the weapons, you have to appreciate, we designed for a lifetime of a decade plus, not that they would fall out of certification. But, we usually replaced them with a new model. So, you never really thought you're going to have to worry about a weapon past a decade, maybe two, at most. All of a sudden now, with life extension, we're talking about weapons that have been in the system for more than 30 years. Well, when we worried about plutonium properties, and what's that plutonium going to do, we didn't worry about 30 year lifetimes at that point. That is an enormous challenge. It's not only that the plutonium is the most important part, but a lot of the rest of the stuff that goes into weapons you can test, and you can replace. The part that you can't test, you can't make it go boom. You can't do the nuclear implosion, and that means the plutonium and all the things associated with the plutonium. So, to extend the lifetime of those things is an enormous challenge. You would think then, okay, so why don't we just go and reproduce? That ought to be simpler right? There are blueprints. Blueprints have dimensions; they list materials, let's just reproduce. What the public doesn't understand, of course, and it's not surprising. We never talked so much about this, is that the plutonium components that go into weapons were produced at a place called Rocky Flats in Golden. Colorado. That was the only place in the United States that could make those plutonium components. The reasons that I just told you for the regulatory world changing all of our lives is they actually changed the lives of the production facilities much faster than the lives of the laboratories. We were still somewhat protected.

19:04 Plutonium Production Facilities - R&D

Hecker:

In the production facilities, now you go to places like Rocky Flats, Hanford, Savannah River, and others that provided uranium enrichment capabilities. For the most part, those started closing one after another. Actually, in June of 1989, Rocky Flats was raided by the FBI never to be opened again. So, all of a sudden, in 1989, we're sitting there not able to make another plutonium component. I was the Director at that time, so I said, “What do we do now?” Well, we [at Los Alamos] have the only other plutonium facility. But, it was built, what we call TA 55 plutonium facility; it was built to be an R&D facility. There were times when we had to draw it into do bigger productions, but not for plutonium components. It was mostly for plutonium, chemical related, processing.

So we said maybe we could take this R&D facility, it wasn't meant to do production, but maybe we can make a few tens or so. So, we started to reconfigure TA 55, seeing what we can do. Then, and this is where the metallurgy comes in, and the training from Case and all the other places. Then, we have this problem that the way that the plutonium components were made at Rocky Flats was such that metallurgically, didn't produce a product that I would be terribly happy with unless you can test them all the time. If you can test them, it's okay, but if you can't test them, there are better ways to do those. The equipment was huge, virtually impossible to take into R&D facilities, so we had to do it differently. And, in doing it differently, metallurgically, the metallurgist always worries about, you have processing, the structure, and the properties and that determines your performance. So, then, we really have to start digging into the metallurgy of plutonium in much greater depth than we had before in order to ask, can we use a different processing technique to provide the proper performance still? Well, on the performance end you can't test anymore by the way, okay? So you're stuck with that. That was a challenge. Then, on the life extension, again, what's generally not appreciated is, of course, everyone knows, that things age. I age, plutonium ages even faster than I age. So, things age, and typically, if you get steels, you worry about rusting, right? So, you rust, and you age from the outside in. So, you have to understand oxidation or the combination of oxidation and hydriding, which are the sort of things we have to worry about in the nuclear arena, oxidation, hydriding. Some steels rust very fast. It turns out that plutonium puts them to shame. I mean, it can rust incredibly fast unless you control the environment. Now, the other thing that plutonium does, that the steels don't do, and most of the other material, certainly none of the other structural materials, it ages from the inside out. That is, it's constantly transforming itself, through radiation. So, this alpha radiation that plutonium undergoes actually transforms the materials. It builds in americium, uranium, helium. It creates helium inside the plutonium itself. So, if you look at that in a simplified fashion, you'll see there's no way plutonium should last. Well, it turns out, it lasts reasonably well from what we can tell. Again, we can't test it, and so we've studied that whole arena of plutonium metallurgy and aging. So, either refabrication, understanding processing-structure-properties, or aging. Understanding radiation, self-induced radiation damage, understanding hydriding, understanding oxidation. Then, these presented one of the biggest challenges to stockpile stewardship.

23:45 Federal Laboratory Partnership - Victor Reis - Assistant Secretary of Defense Programs at the Department of Energy

Hecker:

The second challenge was then since you can't test, can we predict better? Can we model it better? Can we simulate it better? For that, we needed big computers. There we had the federal godfather of stockpile stewardship, this fellow, Victor Reis. He was Assistant Secretary of Defense Programs at the Department of Energy in the early nineties when we were developing this. It was one of the best examples ever of a federal - laboratory partnership. He knew he needed the laboratories. We knew we needed him. He knew how to effectively promote this in Washington to get the support that one needed to do stockpile stewardship. Now, to get to your question, so has it been successful? So far, we've done enormously well in improving the scientific underpinnings of everything that goes into nuclear weapons and nuclear weapons performance. However, we have not demonstrated that all the practical aspects that go with that can be taken care of. So, we continue to do that, and it's a year-by-year situation. Today, this year the answer is the laboratory Directors will sign a certification that goes to the President that says the nuclear weapons that we put into the stockpile, meaning Los Alamos and Lawrence Livermore supported by Sandia, they're safe, they're secure, and they're effective. I signed the first two in 1996 and 1997. They're still signing those today. That's the bottom line. Is Stockpile Stewardship successful? If it were not, they couldn't sign it. They can sign it this year. The challenge is, will they be able to sign it next year? The answer is, you got a lot of people here who are doing a lot of the work that's going to be required to make sure the next director is able to sign that and from there on. That's why you're doing high strain rate work. Rusty Gray is doing a lot of these things in doing plutonium metallurgy work in order to try to understand that.

26:03 What it was Like Working with the Russians - Reykjavik Summit

Nizolek:

Sig, I want to go back to the beginning of your Directorship and ask what it was like to work with the Russians, the Soviets? In 1988, you had them over to measure the yield of one of our nuclear weapons at the Nevada test site. What was it like to work with a group of people, scientists, that many, I'm sure in America, treated as the enemy?

Hecker:

I would say unexpected. I never thought, when I took over the Directorship, or before even, as we were designing the deterrent against the big bad Soviets. I've mentioned, the Reykjavik Summit. Out of the Reykjavik Summit, came this idea that you just mentioned, of developing offsite monitoring techniques for the size of a nuclear explosion underground by going on site and making some direct measurements using techniques that scientists here at Los Alamos and also at Livermore developed. So, the idea that came from the highest diplomats of the country was that we're going to do a nuclear test at each site. The US would do one in Nevada. Put a device downhole, blow it up, and the idea was to see whether, with seismic signals measured at a distance, one can have enough confidence of the yield of that explosion.

27:58 Threshold Test Ban Treaty of 1974 - Russians out at Nevada Test Site for Kearsarge Nuclear Test

Hecker:

The reason is there was something called the Threshold Test Ban Treaty that was signed in 1974 and limited the yield of a nuclear explosion underground to 150 kilotons. There was a previous treaty in 1963 called the Limited Test Ban Treaty that banned nuclear testing anywhere except underground. In other words, in space, atmosphere, water, and so forth. So, this Threshold Test Ban Treaty had been lingering since 1974 through '86 and then '87 and '88 because the two countries didn't trust each other. So, it's sort of classic: we thought the Soviets cheated; they thought we cheated. So, the two presidents, that was Gorbachev and Reagan, they took our ideas and said, “Yeah, hey, we can do this.” So, what happened then, in 1988, was called the Joint Verification Experiment. The first one was the one I just described, put a device downhole in Nevada, measure the yield.

The second one was, do the same thing over in the Soviet Union. It was still the Soviet Union at that time. It was before the collapse. We would have our scientists to go and measure on site. So, what happens then, here we are in the few months running up to August of 1988, and we have to allow the Soviet scientific delegation on our test site. One of our most treasured secret sites that we had. And yet ,the government, the president said, “You've got to do that.” We said, “Okay,” and we did. So, we had the Soviet's there, and that was really, that was the experience that then set the stage for everything we did post-collapse of the Soviet Union. We started working with these guys, and except for the language difference, we found out they're just like us.

They love science; they love instrumentation. They think nuclear weapons are important to deter the other side. They think nuclear testing is important. So here we were, on August 17th of 1988, and I'm sitting down in the control room out at the Nevada test site, a place we called Mercury. And, in that same room are the Soviet scientists, and we're going to set off our nuclear device. It turns out I was there because it was a Los Alamos device that we set off. We called the test Kearsarge because, again, we named them all. So, we called it a Kearsarge. It was there; we're setting this off. I like to tell the story that it was probably one of the most anxious moments of my life. The Soviets were there; we're going to go ahead and push the button so to speak. You know you go ahead and say, okay, you're going to detonate this. So, the first thing that goes through my mind is that I say, “God, I hope it works.” Just imagine, we're the Americans. We're here. We've got this nuclear arsenal. We're going to demonstrate one, and it doesn't work. The second was, I sure hope it's under 150 kilotons. That's what we said it was going to be. It turns out; it worked on both accounts. We set it off, it worked. We then went and celebrated, with the Russians sitting across the big picnic tables out at Mercury, at the steakhouse.

31:53 Americans at Semipalatinsk - Lab to Lab Cooperation - Visiting Sarov

Hecker:

Then, the whole thing was reversed. I didn't go to the Russian test site called Semipalatinsk, but our technical people did, Lawrence Livermore technical people. The DOE folks from headquarters were also there. They then did the whole thing exactly the same. Theirs worked. It was also 150 kilotons; it had to be close. You can't do one at five kilotons instead. So, it worked. That introduced us to each other. It was after that the negotiators got together in Geneva and said, “Okay, now let's iron out the actual protocol that the Senate on our side and the Duma on their side, their congressional body, can ratify this treaty.” It was Paul Robinson, who was a former Los Alamos employee, who became ambassador to the Geneva testing talks. So, he went to work with the Russians. Then one of the key moments was, there was one of these technical discussions in Moscow and our technical people, Los Alamos and Livermore technical people, went there to have this discussion. The leader of their delegation, a guy by the name of Viktor Mikhailov, he later became their first Minister of Atomic Energy of Russia. He then invited these guys to their secret laboratories. Their Los Alamos, their Livermore. So, here we were in 1989, and then into the '90 timeframe. We got our first introductions into these laboratories, and these laboratories were so secret they didn't appear on the Russian maps. Those towns did not appear on the Soviet maps, I should say. So, they let them go in. They came back with proposals to me and my counterpart at Lawrence Livermore, John Nuckolls, for why don't we do joint scientific work together? You guys have interests in these material properties under difficult and extreme conditions. We do also. You guys do computer modeling. We do computer modeling. So, they invited us to join them. The U.S. government was not ready to do that. They were still Soviets at that time. I kept knocking on Washington's door and said, “Hey, look, those guys are ready. We should go over there.” Then, eventually, George H. W. Bush and then-Secretary of Energy, Admiral Watkins, gave us the green light. So John Nuckolls and I went to their Los Alamos, a place called Sarov, in February of 1992. February 23rd is when we arrived. That was surreal. To wind up in this secret non-existing city and be there talking to people who were just like you, had the same interest that you had. We found out how incredibly advanced they were in computer modeling. That was the biggest surprise because we knew their computers were thousands of times less powerful than ours because they're electronic industry never really come along. It all stayed in government. Ours had gone private sector. Our computers were way ahead. When I asked them, for example, I mean this is really kind of cute. I asked the guy giving the presentation about the 3D simulation and modeling. I said, “How can you do this with your computers?” He said, “Well, you guys, you have these fancy computers, you got lazy. We have to think harder than you do, and we do.” And, they did. So from that, once the Soviet Union then dissolved, which was December 25th, 1991, then we were allowed to go over in February of 1992.

36:01 From Confrontation to Cooperation - The Start of 25 Years of Cooperation

Hecker:

That started essentially 25 years of cooperation. When the Soviet Union dissolved, we changed overnight. What we call Confrontation to Cooperation. They made a video in 1993 that was called Confrontation to Cooperation, to try to underscore why we had to work together. What we did- I think this is one of the beautiful examples of where scientists can help to facilitate diplomatic breakthroughs. That is because we were so much alike because we had such great interest in the fundamental science that we started to work together. Our government gave me the go-ahead to say, “Okay, you can talk to them about fundamental science, but you can't talk to them about other things. Just stick to the fundamental science. I said okay, we’ll stick to the fundamental science.” So we did that. We started to build a relationship and then, eventually, we kept pushing, and it allowed us to get into tackling the big issues, and that's 25 years of cooperation. What we called it, was essentially the four loose nuke problems. Loose nukes - the weapons. So, when the Soviet Union dissolved, there was economic chaos, political chaos, every other chaos. And, it was a country that, at one time or another, had 39,000 nuclear weapons. All of a sudden you thrust that country into chaos. Okay? That was a recipe for disaster. Nuclear materials, you know, it takes a few tens of kilograms of fissile materials, highly enriched uranium or less of plutonium, to make a bomb. The Russians had, we're not sure they ever knew how much they had. We didn't know much, but well over a million kilograms of this stuff in literally hundreds of sites and buildings. They had a million people in the nuclear complex, several thousand of them associated with the nuclear weapons complex. So, we were worried about exports. We worried about 1) loose nukes, 2) loose materials, 3) loose people, 4) loose exports. What we realized is that this is a problem for both of us, Americans and Russians. We should work together. So, we did.

At the end of October, I'm going to go back to Russia for my 56th trip to Russia since that first one in February of 1992, working with the Russians to try to make sure that nothing goes astray during that time. So far, the record has been great. Unfortunately, our governments, which more or less got along during the Clinton presidency and the Yeltsin presidency, after Gorbachev was out, after the dissolution of the Soviet Union. Then, things started to get pretty difficult again, after 2001 or 2002, and then really terrible after 2014 and the Russian invasion of eastern Ukraine and also, of course, the annexation of Crimea. So, today we call this cooperation, lab to lab, scientists to scientists, that's pretty much at a standstill. So, now I work with the Russian universities and our universities. Now that I'm at Stanford working with their universities, still trying to take a look because these nuclear problems never go away.

Whenever you think you've got them licked, that's when you're going to be vulnerable. Since we are the two countries with, by far, the greatest number of nuclear weapons, the number of nuclear people, nuclear materials, nuclear facilities, we have a special responsibility. So, what we're doing now is biding time trying to get the younger generation to understand that this is an interesting and important problem. To keep the temperature warm until our governments come to their senses and say, ”Look, we really ought to continue to cooperate in the nuclear arena.”

40:25 Benefits of Professional Society Membership - Platform for Presenting Papers and Networking

Nizolek:

Sig, I want to go on to talk a little bit about professional society membership and the benefits of that. I know that you have attended many, many conferences. Switching gears from international collaboration to maybe collaboration within the U.S. or external, what is the role that TMS has played in your professional networking?

Hecker:

Well, for me, personally, it was most important early on, for the young. I mentioned I went to Case undergraduate school in '61 through '65. I became a TMS member in '64, so, right after I switched from nuclear physics to metallurgy and materials. There was TMS; there was ASM. TMS, particularly, was viewed as the society run by the scientists themselves, the metallurgists, the engineers and so forth. It was the one that was aimed at more of the research end of things, so it was particularly important. I also joined ASM. ASM had, at that time, greater links into the industrial world, and, so, I joined that as well. But, I joined TMS. To me, that was big time. Here, I was a junior in school, and I was able to join as a student member of The Metallurgical Society.

When it became the most important, during the formative time of my professional career, is once I got into graduate school. My advisor, not only allowed us, but he encouraged us to go to the professional society meetings. I remember going to TMS and ASM meetings starting in about 1967. He encouraged us. He said, “Go, present your work.” So, I presented my work. Then, as I finished my Ph.D., and then, also as I did the postdoctoral work, because here at Los Alamos the sense was the same. You do your research, you need to publish it, and you need to present it. I still remember the TMS meetings had this ability. They had the best people. They had the people that I considered my heroes, sort of my idols. I still remember this one meeting, the TMS meeting in Las Vegas in 1970. I went there to present my work on corners in yield surfaces. We had people like Fred Kocks, who was one of my all-time idols, and he was there in the audience. Sure as can be, Fred always had a way to make sure he asked these questions that just went right through everything. So, Fred got up and asked me the big question about corners in the yield surface.

I attended them through the early part of my professional career. I always made it a point to go to the TMS fall and annual meetings. I was associated with committees for TMS, and then, also for ASM. Then, I joined the other professional societies. I'm a member of the American Physical Society, and things like that. I joined those much later on. So, the early formative years, that's where TMS really was important to me. First of all, I was so proud to be a member. Then second, it provided a platform where you could present your work and then you could mingle with the greatest minds in your field.

44:25 Advice about TMS Membership - University Student Chapters

Nizolek:

So, you see the benefits for young people today to join a professional society in that they can present their work and that they can network. Is that a fair assessment? Those are the two things you think are the most important?

Hecker:

Yes, absolutely. That's the benefit. My advice would be, join it as early as possible. In addition, what I've seen since that time, and I don't think we had as much of that, back in the early years, let's say the 1970s or so, is student chapters at universities. So, I've seen some of the programs that have been put on by student chapters from different universities, and those are superb. That really allows you to be in an environment that's not quite as confrontational as when you go and present your work to the leaders in the field. It gives you a chance to work with others, work with your mentors from the universities and get a somewhat more gentle introduction. Then, also, to make your voice be heard in the profession. The whole business has changed. If you look at the publication business, right? If you're going to counsel somebody today as to what you do, what journals do you publish, what do you do with your work? It's changed so dramatically. My feeling is, when I get to that today out at Stanford, I just asked my students. They know a lot better than I do.

46:01 No Regrets - I Don't Think I Would do it Differently

Nizolek:

Sig, your career has been filled with so many successes and major accomplishments, yet having interacted with so many people from members of US Congress to North Korean scientists, surely there have been some disappointments. Is there any one disappointment that sticks out in your mind? Something that, perhaps, you wish you had a second shot at?

Hecker:

Let's see; as I look back in my scientific and technical career, I don't think I would do it differently. I don't see any disappointments. I see it all as I had enormous opportunities, and I was sufficiently fast on my feet to go into the places that I found to be both challenging and interesting. In the directorship, there are probably more than I can possibly name. Once you get into having to manage people and having to manage organizations and having to interface with the government, having to interface with the public. As I look back, as I mentioned, and you mentioned it, I was quite young for a directorship at age 42. I didn't necessarily have the background and experience as to how to deal in Washington. So as I look back on those days, I could say, yeah, I think I would do it differently today. For example, I'd go and give congressional testimony. Being a science type, I'd go there with viewgraphs, and somebody told me that you don't do viewgraphs in congressional testimony. I said, "Well, that's how I best tell my story. I'm going to do the viewgraphs." So, I used to bring the viewgraphs in, and they went through all sorts of machinations to get a projector and get a screen, and I would go and do this. You know that was still viewgraph days. There was no PowerPoint. I watched the members; they were either scribbling things for themselves and not paying much attention. The staff was paying attention. So, I learned. Those are the things that you had to learn, and, eventually, you learn. I look back at my congressional testimony, and I had people here at Los Alamos who would help me, and I watched others. You, basically, go in, and you say, "Mr. Chairman, Madam Chairman, and members, I've got three things I want to tell you." That took a few years before you really figured out how you got to focus on those things that they can relate to but still get your message across. I could say that it took me a little while to get there. I might do it differently.

49:08 The Human Genome - A Long Way From Nuclear Weapons

Hecker:

On the other hand, I did things during the directorship, because I didn't have the experience, that I would say that I wouldn't have done later in life. I was better off having done them early. Today, my view of youth is, at 42, I didn't know most of the stuff that I know today. All of the things that I've learned in my interactions internationally, nationally, with Congress, the administration, and so forth. But, I think I was a better director at age 42 than I would have been at 62. Because, at 42, I didn't know what was not possible. For example, one of the most exciting things that we did at Los Alamos, we essentially built the case for the mapping and sequencing of the human genome. The human genome is a long way away from nuclear weapons. However, we built that case, and it was our scientists here. We had good bioscientists because, if you're going to deal with nuclear stuff, you have to understand the effect of radiation on human beings. So, we had very good people here. We also had people in physics and computers that were intrigued by the bioscience world and said, “Hey, these bioscientists, they are living back in the last century. They're not using modern tools, techniques, and computers.” They said we can do this, we can do it, and we, at Los Alamos, we'd push it because if you're going to map and sequence a human genome, you've got to have computers.

Los Alamos and Lawrence Livermore had the best, biggest computers in the world and knew how to use them. Second, we had instrumentation capabilities. We did laser-based cell sorting, flow cytometry. That was invented here at Los Alamos. I was young, second year in my directorship, and these guys came and said, “Hey, you've got to go to the government and tell them that we've got to map and sequence the human genome.” Well, I went there. The bioscientists mostly opposed that idea. So, the one person who made this go was Senator Pete Domenici from New Mexico. There was good reason as to why we called him Saint Pete. He actually paid attention to what we told him. He cared about New Mexico. Particularly in today's political environment, if you look at that, he cared about the country, and, in the end, he made the decisions that were most important for the country.

Of course, he hoped that it would help in New Mexico and the lab as well. So, it was really Senator Domenici that pushed through the funding for the Human Genome Project. As people look back today, of all the government projects that have ever been done, that's considered to be the most successful project, particularly in terms of spinoffs and what it's done to change the world. Those are the things I look back now, and to some extent, I would say, I could've been better prepared. I could have been this or that differently.

You always come into our laboratory, and your tendency is you're going to reorganize it because you don't particularly care for the way it was organized before. That's a temptation. So then, you talk to consultants, and they say, “Well, Sig, you got to understand, you can't organize your way out of a problem that you behaved yourself into. Think about other ways of doing these things.”

So, as I look back at the directorship, there are lots of different things I could have changed, but, so it was. We tried to rise to the challenge in the technical world and the interface with the other countries. The things we did with Russia, I'm immensely proud of because I think, probably in the last 20 or 30 years, it was one of the most important national security challenges. Together with the Russians, we made sure that nothing terrible happened in the Russian nuclear complex and, therefore, to the world. I started working with the Chinese. I'm proud of that, but that was much, much more difficult. In the end, it's related to this problem that I mentioned early on just briefly: when the FBI came to tell me that we had a Chinese spy in our midst, in the laboratory. Things started very well with the Chinese, went pretty badly, with the so-called Cox report. So then, we've been trying to build that back up again in the last number of years. Then once I went out to Stanford, I looked much more broadly at the whole nuclear landscape. I run a project out there that I call Nuclear Risk Reduction to essentially deal with all of the nuclear countries.

54:05 Non-proliferation International Diplomacy - 2 Minutes to Midnight

Nizolek:

I want to ask you about that current work. Generally, though, how successful have scientists, or the nation, been at non-proliferation international diplomacy? I can't help but notice that the Bulletin of the Atomic Scientists currently puts the doomsday clock at two minutes to midnight, it's worst ratings since 1953, after both the U.S. and the Soviets detonated their thermonuclear weapons. Under your tenure in 1995, it was set at 14 minutes to midnight. Is that an unfair metric? Are there really reasons to be more optimistic today?

Hecker:

Well, like all things of that nature where you try to capture something as complicated as the fate of the world with one number, like how many minutes to midnight, it also has its problems. But, I think what's fair is, if you look at that, since the inception of the doomsday clock, it gives you a general sense of where are we today and how close are we to terrible things happening. So, the reason it was at 14 minutes to midnight was precisely because of the dissolution of the Soviet Union. That changed the world. The hopes were enormous that things are going to turn out very, very differently. So you look at, when I went over there in 1992, it was clear that the world had changed and we still had nuclear weapons. We still had them literally pointed at each other.

55:54 Los Alamos 50th Anniversary - From Russia With Love

Hecker:

One of the interesting things, on the 50th anniversary of this laboratory, which was in April 1993. I invited the Russians to come to our 50th anniversary. Could you imagine that five years prior or 10 years prior? So, when you invite the Russians to your anniversary, you can turn that doomsday clock back and say, “Hey, we're not sitting right there at doomsday.” And, speaking of missiles, one of the gifts brought to me by the Russians was a piece of an SS 11 missile that used to be pointed at us, remachined into a little replica. It was mounted on a piece of Serpentine, which is the favorite mineral out in the Urals where their "Lawrence Livermore" laboratory is. So, he handed that to me, explained it was a piece of an SS 11 missile that used to be pointed at us and the inscription on it said, "From Russia with Love." That's the point when I knew the cold war was over; so, the clock captured that spirit.

By that time, even though President Kennedy predicted early on in his presidency that by 1970 that we probably would have like 25 countries that will have gone nuclear, they weren't. So, you look at that, by the early 1990s, it certainly was much less than 10 countries that had gone nuclear. That was then, we worried about Iran. Iraq had been pretty much taken care of in terms of nuclear weapons. We knew North Korea was up to something. We didn't know exactly how far along they were. We knew that India and Pakistan both had nuclear weapons programs, but they had decided not to display them. However, in 1998, they tested: India first, followed almost immediately by Pakistan.

Then over the next five years or so, is where North Korea came into play. It turns out, one of my Stanford colleagues invited me to go to North Korea in 2004. By 2006, North Korea had done its first nuclear test. So you could see that the clock would go backward, because of those developments. Now, if you forwarded to when it was last set, there you have the problem that, particularly since I've followed North Korea very, very closely. I first went in January of 2004; I was still here at Los Alamos as a senior fellow at the time. Then I went seven years in a row to North Korea, and I visited their nuclear sites. Not each time, but four of the seven times I visited, their nuclear sites. North Korea, at that particular time, I watched how they built it up. I've recently developed what I call a comprehensive history of North Korean nuclear program that we have on our Stanford website. In 2017, things were really, really bad. So that was a time to put that clock, very, very close to midnight. My feeling was we were right at the precipice of the potential of a nuclear war.

We had a young man in North Korea about whom we knew nothing, basically nothing, and we had a president in the United States, that no one also knew what to predict in those circumstances. The two of them just continued to raise the ante during 2017. So, by the November 2017 timeframe, I would say, that clock captured the spirit, particularly from a nuclear standpoint. However, hopefully, when they reset the clock this year, my own feeling is we've walked back from the precipice. I do not believe right now we're at the precipice of a nuclear war with North Korea. For better or worse, President Trump and Kim Jong Un got together, they shook hands, and they lowered the temperature. What they didn't do was solve the problem. That's why I continue to work on that. To try to bring some sense to what are the real issues. It took North Korea 50 years, particularly the last 25, to build up everything that they needed for a nuclear weapons program. It's immense. Let me say they don't have a whole bunch of nuclear weapons, but they have enough. They have the entire capabilities from plutonium production, to highly enriched uranium, to making a warhead. They've tested them. They've done six nuclear tests, and that really demonstrates that they know how to build a bomb. Can they deliver one? Well, I don't think today to the United States of America. So, that's why I've worked with the North Koreans. I've also worked in India, Pakistan, Iran, and other places. I continue to work in China and the United States.

01:01:21 Recommended Qualities for Young Engineers - Be the Best

Nizolek:

Having been in both industry, national laboratories, and, now, academia, if you had advice for today's young engineers, what qualities would you recommend that they have?

Hecker:

Curiosity and the desire to marry the fundamental understanding of something to an application. That's the part that I have found most fascinating. So, I would say that's a good way to look at the world: be curious, and then, try to be good. Try to be the best at whatever you choose to do. Try to do it best. Look up the best. Go to TMS meetings and keep on pushing and always feel that there's nothing out there that's impossible. Also, if you can get the opportunity to, work in different sectors. I really valued the opportunity of being in all three of those sectors. I learned things in each one of those.

01:02:52 Lessons from Academe, Industry, and the National Laboratories - Don't Ever Sell Them Short

Hecker:

I thought I knew academe reasonably well because I was at Case. I was a student, and I went all the way through my Ph.D. However, in the end, I didn't learn academe the way I've done at Stanford, and that is the real benefit that you get by interacting with students; by working with students, then by appreciating just how smart students can be. Do you know what I found? Don't ever, don't ever sell them short. Don't ever think that you're giving them a problem that they can't tackle. Just keep on pushing them, and keep on interacting with them. So, I know academe much better today.

Then the industrial world, for example, what I learned at General Motors is economic metallurgy. They told me, “Look Sig, if you can save 50 cents a bumper at General Motors, you'll be a hero in this company.” It wasn't just a matter of whether I could find some treatment or some slightly different material for a bumper or a car body. The point was that it also has to be economically feasible. So, I learned the economy and learned a whole different set of structures and issues that you have in the commercial world.

Then in the national laboratories, at a place like Los Alamos, for all these years, it was just you could do anything, You could do anything; any field of science. There was somebody here who could help you out. You could span that whole range from the fundamental to the applied.

01:04:33 Metallurgy and Diplomacy in Russia and North Korea

Nizolek:

Through your interactions with the Russian scientists, Chinese, North Korean, has your background in metallurgy helped you with diplomacy?

Hecker:

Yeah, actually it's come in, somewhat unexpected ways at times, but very, very handy. The Russian one is very straightforward, because when I did come back here to Los Alamos in 1973, and I started to work seriously on plutonium and trying to understand plutonium. Going back to the question that you asked about plutonium and its complexity. What plutonium does, it essentially combines the complexities of all of the materials into one material. Whether you want to learn about phase transitions, or you want to learn about the electronic structure of plutonium, or you want to learn about the peculiar mechanical property, it has everything. So, one of the great puzzles of plutonium was that the basics of plutonium and, of course, for any metallurgist, is the phase diagram. For the Manhattan project, we hear a lot about the great physicists that were here. It turns out it also had the U.S.’s best metallurgist, Cyril Stanley Smith, who came from Chase Brass and Copper Company, then went on to MIT after the Manhattan project. He's the guy who figured out that the pure plutonium, of which you essentially can't make anything because it's a monoclinic structure, it's brittle as can be. It's unforgiving. If you alloyed it with a little bit of different alloying elements, it actually behaves more like aluminum. But, that basic phase diagram, either plutonium-aluminum or plutonium-gallium phase diagram was in question. When the Russians first started to roll out those phase diagrams, it was after President Eisenhower's Atoms for Peace speech and the Geneva International Conferences. We found out that we had a totally different phase diagram. In 1975, I went to a meeting in Baden-Baden Germany. The Russians presented theirs, and it was different than ours. I asked them questions. There was no way to get an answer. Finally, after we had worked together, going back to your earlier question of working with the Russians, by 1998 we were ready to talk turkey and talk plutonium phase diagrams. So, in the end, I started working very closely with Lidia Timofeeva. It took us about two years, and we published a paper together that's called the “Tale of Two Diagrams”, where we, in essence, showed that, for the fundamental phase diagram itself, the Russians were right. From a practical standpoint, we were right enough, but then we laid it out together. So, that was one case where, from any diplomatic standpoint, if you didn't understand plutonium, you didn't have much of a chance.

The second one, which was totally unexpected, was actually in North Korea. So, my first visit to North Korea in January 2004, the diplomatic backdrop to that is intensely complicated. But, in essence, the Clinton administration had struck a deal. The Bush administration said, this is the worst deal ever made, which sounds kind of familiar in today's world, and they walked away from the deal. The North Koreans walked away then also, restarted their reactor, withdrew from the Non-proliferation Treaty, built the bomb, and nobody seemed to care.

That's how I got to North Korea. My Stanford colleague, John Lewis, was invited. He'd been there before, and they brought him back. When they heard it was me that was going to come along with John Lewis, they decided to show us the plutonium complex. So, I went to their reactor, the spent fuel pool, the reprocessing facility. They showed us all of that. The coup de gras of that, and where plutonium metallurgy really came in handy, is after they took us through those facilities; they sat us down in a conference room.

They said, “Well, Dr. Hecker, we've now shown you our deterrent,” because they wanted the American government to understand they actually had the bomb. I said, “Well, you didn't really show us your deterrent. You know, a deterrent takes three things. You've got to have the bomb, fuel, plutonium. You've got to be able to weaponize. You've got to be able to design, build, and test, that's weaponize. Then you have to deliver. All I saw is that you have a reactor that can make plutonium. You have a reprocessing facility. They're going to reprocess it. You know everything about those processes. You answered all my questions, but I'm not even sure that you actually have the plutonium.”

So, the Director turns to me, he says, “Would you like to see our product?” We're sitting in a conference room, and I said, “You mean your plutonium?” He said, “Yes.” I said, “Well, sure, bring it in. I mean, I've handled plutonium since 1965 when I was a student. Bring it in.” So, lo and behold, they bring in this red metal box and open it up. Inside was a white wooden box with a slide off top. They slide the top off; there sit two glass jars. One of them, “They say this is plutonium oxalate.” That would be one of the precursors to making plutonium metal. The other one was a glass jar, marmalade jar, and they said, ”That's our product. That's the plutonium.”

It turns out it was a cone-shaped piece of plutonium with a very thin wall. So, I looked at it, and the surface, so, again, you have to know something about plutonium, and you've got to have seen plutonium, the surface was a dark gray color, while plutonium freshly machined is a silvery metal. As soon as you leave it out, because it oxidizes fast, it gets this dark gray layer. It hadn't yet turn greenish or got plutonium oxide on it, so it was not a very old piece of plutonium. But, the most important aspect of that plutonium was it was this thin-walled funnel shape. I said, “How'd you get that?” He said, ”Well, that's the scrap from our casting, it's upside down. So, it was actually a sprue that looks like this.” [Motions with hands.]

I knew as soon as I looked at it that it had to be delta phase plutonium alloyed with gallium because there's no way you could make that out of alpha plutonium. So, here's the Director and I having this discussion, and I said, “Okay, what am I going to do? I've got to try to find out what that is, but I don't want to ask him directly, Did they alloy it? What did they use?" So I asked them, "What density is it?" That's where the plutonium metallurgy comes in. Pure plutonium has a density close to 20 grams per cc. If you alloyed this stuff, it's less than 16, 15.8 plus or minus a little bit depending on how much gallium or aluminum you use. So I said, "Well, Director Lee, what's the density?" He was so clever, I still think back and that, he was so incredibly clever. So, he said, "Well, Dr. Hecker, it's between 15 and 16." That is such an incredible, clever answer because it means it's delta phase plutonium because there's no way you can get down to 15 to 16 without being delta phase. Yet, he didn't tell me that it was 15.6, 15.9, 15.8 because, if he does, I immediately know how much gallium is in it. But, he tells me this very clever answer between 15 and 16. I said, "Oh, Director Lee, it's alloyed." He said, "Well, yes it is." I said, "So, Director Lee, what do you alloy it with?" And, he says, "Well, Dr. Hecker, I'm not authorized to tell you that, but you know something about plutonium. It's the same stuff you use." So, there you are. That's plutonium and diplomacy. They were sending the diplomatic message to our government, "Look, we've got the bomb and you ought to care." They did it in such a clever fashion by being able to explain everything about the whole plutonium chain from the reactors to the reprocessing to the metal and to its density.

01:13:11 What Keeps You Going - My Grandfather Saves the World

Nizolek:

Sig, you're in your seventies now and far from being retired. You teach at Stanford; you're active in the areas of international studies and nuclear security. And, I couldn't help but notice you're even on Twitter. What keeps you going when you could have been, I'm sure, retired years ago. Why do you continue to work?

Hecker:

So when I first came back from North Korea, my grandson at that time was eight years old. And, he was in school, and somehow the issue came up: they were asked to talk about their grandparents. He was asked, "So, what do your grandparents do? I had just come back from North Korea. And, he said, "My grandfather, saves the world." So, that's really stuck with me.

I think if I had stuck just to the technical world, I would still be dabbling away with the latest and greatest things in the metallurgical materials world. For example, the one that would capture my imagination today is advanced manufacturing. Where people say, you can make anything. As a metallurgist I say, "Well, you can make anything, but it's processing-structure-properties. We have to understand those things. You can make different structures. You still have to understand the performance." I think I would still do some just because I love my work. But, particularly right now, I mean, the most important thing that keeps me going is that the directorship at Los Alamos really changed my life. What I found, especially after directorship, is that there was life after directorship. There's real life after directorship. The way it's changed my life. What I've found is whether I go to Russia, whether I go to China, whether I go to North Korea, to India, to Pakistan, being there as the former Director of Los Alamos National Laboratory opens doors. It is so amazing. I mean, Los Alamos is considered the Mecca of the nuclear world. You have instant respect. You have an instant opening of the doors, and I can't see that I'm not going to walk through the doors to try to make the world a safer place. So that's what keeps me going.

Nizolek:

Sig, it has been truly a pleasure to spend this time here with you today. What a fascinating career and life you have. Thank you very much again for your willingness to share your story with AIME.

Hecker:

Well, Tom, it's been my great pleasure, and thanks for your interest, and I also hope the AIME members will at least find something of interest. Thank you very much.