



AMERICAN INSTITUTE OF MINING,
METALLURGICAL, AND PETROLEUM ENGINEERS

ORAL HISTORY PROGRAM

Robert Shull: Cutting-Edge Research and Expanding Educational Outreach

PREFACE

The following oral history is the result of a recorded interview with Robert Shull conducted by Matt Willard on July 29th, 2021. This interview is part of the AIME and Its Member Societies: AIST, SME, SPE, and TMS Oral History Project.

ABSTRACT

Robert Shull has been leading cutting-edge scientific advancements his entire career. Growing up amidst national labs and the outdoors in Oak Ridge, Long Island, and near Boston, Shull grew a fascination with the sciences and mathematics. Shull began to explore materials science while studying at the Massachusetts Institute of Technology, later doing his Ph.D. thesis work at the University of Illinois at Urbana-Champaign on micromagnetism or the interactions between clusters of magnetic spins. Understanding metallurgy and magnetism in alloys has been a focus of Shull's career. Shull has led remarkable research, much like his father who received a Nobel Prize and hinted on the phenomenon of spin orbit coupling in his Ph.D. thesis. Shull expanded on his father's thesis, working to switch magnetic poles with electric fields in the absence of a magnetic field. Not only did Shull contribute to discoveries during the high temperature ceramic superconductors craze, but he was also a lead of the United States' nanotechnology initiative and was at the forefront of the nanostructured materials evolution. Through leadership and service in TMS, Shull has shared his passion for the advancement of science by bringing together communities through symposia and initiatives as TMS President. One of Shull's greatest successes has been expanding educational opportunities for students of all ages. Beginning in a grass roots basement organization, Shull has grown the Maryland-based Adventure in Science program to several locations, including the NBS and NIST laboratories. Shull has not only advanced research in the materials science sector but has advanced the minds of young students.

Readers are asked to bear in mind that they are reading a transcript of the spoken word rather than written prose. The following transcript has been reviewed, edited, and approved by the narrator.

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PART 1

00:00:00 Introduction

Willard:

Today is July 29th, 2021. My name is Matt Willard, and I'm a Professor of Materials Science and Engineering at Case Western Reserve University. Today it's my honor to interview Dr. Robert D. Shull, Fellow Emeritus from the National Institute of Standards and Technology and former President of TMS. Bob, we've known each other for a long time, both through technical committees and through outreach, especially Adventure in Science. These are great interactions where we've had lots of conversations, but we don't usually get a chance to talk about you and how you became the well-known scientist that you are today. I think we should begin this interview with a discussion of where you come from. You grew up in the years just after World War II. Can you describe your early life and what inspired you to become a scientist?

00:00:00 Roots in Oak Ridge – Growing Up Amidst the Outdoors and National Labs

Shull:

Certainly, I was born in 1946 in Oak Ridge, Tennessee. My father was a scientist at what would, in the future, become the Oak Ridge National Laboratory. At that time, it was called the Clinton Pile. They had developed this wonderful source of neutrons during the war there. And then, my father joined that laboratory just after the war, not too long before I was born. So, I grew up there for the first 10 years of my life, and it was a wonderful place to do that because it was right next to the Smoky Mountains. My father was quite interested in, with my mother, to take myself and my two brothers (I'm the middle child of three boys) up there quite often. We would go up camping in the mountains and hiking and enjoy water sports at the local lakes, which had been created by damming up the Tennessee River.

When I was about 10, we moved up to New York state for about two years. We lived out on Long Island in a city called East Patchogue while my father worked at Brookhaven National Lab during the time they were building a nuclear reactor up at MIT. My father did not want to spend the two years it was going to take to build this reactor, at which he would work doing nothing. So, he took a position at Brookhaven Lab and worked there for a couple of years. Then, we moved up to outside of the Boston area, where my father became a professor at MIT. At that point, I was near the end of elementary school, but I did spend a couple of years there before going to junior high school.

00:03:33 Tipping a Log Boat to Building a Sailboat – Engineering from An Early Age

Shull:

My life was a reasonably comfortable life. We did a lot of hiking while up in that area, up in the White Mountains of New Hampshire and Vermont. I can remember, in fact, building a sailboat at a house that my parents rented during a couple of weeks in the summertime every now and then; it was right next to a lake. That was a very enjoyable experience because it was a rectangular boat rather than designed like a normal boat with a nice pointed bow and stern. And, in fact, I went online, and, in those days, not online, but in the newspaper. The New York Times had this big advertisement section. I remember buying a parachute, and then my mother and I spent many hours ripping out the cording from that parachute. Then, my mother did some sewing to kind of take out the billowing parts of the sheet, and we used that for making the sail of the sailboat.

Willard:

How big was the boat?

Shull:

The boat was about, maybe, 12 feet long, something like that. So, it was not a real long boat, and it was about maybe three feet wide, something like that. It had a gaff rig on the sail, so it was fairly light. It was kind of a platform boat. So, if it turned over, you did not get water inside it, and, thereby, you could right it while you were in the middle of the water.

Willard:

Did you do this with your siblings?

Shull:

Actually, for this particular project, I did pretty much by myself, with my father, when he was up there. He would kind of commute from MIT, or his laboratory at MIT, and up to this cottage. We would be up there for about three weeks, four weeks, and he would come up on weekends. That's when he would be working with me, and the other time I was doing it myself.

Willard:

So, engineering from an early age.

Shull:

Exactly. So, that's where I really got my start. Actually, I got my start when we were in Long Island, and my two brothers and I decided we were going to build a log raft. And we were going to sail it out to Fire Island. We lashed all these logs together and put a mast with a square rig sail on it; we got about 15 feet from shore, and it kind of tipped over on us. The worst part of that was not the drowning; it was the fact that that whole area was infested with horseshoe crabs. So, there are all of these horseshoe crabs running around the place while we're trying to get out of the water, back up to the beach. So, that was a real failure, but it didn't stop me from trying to build a regular sailboat much later on.

00:07:15 Tinkering with My Father and Brothers – Interests in Mathematics and the Sciences

Willard:

What about getting a little bit older, going into high school? What were your interests?

Shull:

My interests were mathematics and the sciences. At high school, the offerings were typically chemistry and physics. I don't even remember biology being an offered science course. And, I liked them, I was good at them. I figured, later on, I would probably end up majoring in one of those areas and working in that area. It was pretty much accepted, not expected, but accepted by myself and my two brothers that, after high

school, we would go off to college. Obviously, my father had gotten degrees in college, getting a PhD degree. My mother also was someone unusual for her time in that she also went off to college, but more specifically, unusual was she went to graduate school. In fact, when my mother and father met was when both of them were in graduate school in New York City, my mother at Columbia University majoring in history, and my father majoring in physics at NYU.

As I said earlier, I always had the expectation that, after high school, I would go off to college. It was not a question in our mind that we would do this. In fact, I did that, and my two brothers also did so. Things I liked doing when I was growing up were playing with a train set my father helped me and my two brothers build, which turned out to become very elaborate, comprised of two big 4x8 sheets of plywood, intersecting at a right angle, that were at different levels. We built a papier mâché mountain at the intersection, and we would have trains going up and down from one level to the other and built villages with lighting and everything. The thing that I particularly enjoyed was working with my father wiring up all these electronic controls for turning on lights or having a gate come up and down that stops cars from crossing the railroad tracks or for uncoupling cars.

That was really a lot of fun, so I enjoyed doing that and tinkering around with all kinds of electronic things. I never built a radio or anything like that until much later on. I got one of these Heath Kits; in fact, I'm not even certain they even still exist. Heath was the company, and they had kits where you can build amplifiers and radios and other things. So, I built this big amplifier, and it took hours and hours. But, really, it taught me a lot about soldering, for instance, especially how to make good solder joints. That's something, unfortunately, that's missing in today's training of our youth. Children, generally, don't get a chance to experience and learn that sort of technique.

I was also quite active at running; I was very good at running long distances. In fact, I remember there was one year this Boston Marathon runner called Abebe Bikila came to our high school to run with us during practice. He was from Ethiopia, and he was going to run the Boston Marathon a few days later. We lived in Lexington, which is only 10 miles from Boston. And so, Bikila came out to run with us, and his pace was faster than my sprint! That made me realize how slow I was, but I still enjoyed the running competition. This particular marathon runner was well-known for winning the Boston Marathon that year, running without shoes. So, he ran it barefoot and was quite an impressive individual.

00:12:24 Majoring in Materials Science – An Agglomeration of Disciplines and The Right Fit for Me

Willard:

Very interesting. So, you had a lot of different types of activities. These creative things that you were doing, kind of unscripted, and then team sports types of things. I think that shows something about your future career, where you are working in teams and being very creative in your work. So, you get through your high school career, and you said that it was kind of expected that you'd go to college. How did you decide where to go, what to do next?

Shull:

Well, actually, I applied to maybe four different colleges, and they were all reasonably competitive colleges. I remember going to this one interview for Dartmouth, as I recall, and most of the colleges required an in-person interview as part of the application process in those days. I remember going through the interview and the interviewer looking at this list of colleges that I applied to. And, he says, "I don't see a safe school here." I was actually considering Dartmouth to be that safe school. That got me a little concerned. Well, I

didn't need to be concerned because I got in pretty much all the ones I applied to, and I was all set to go to Cornell. In fact, when my parents said, "Well, if you went to MIT, then you could live on-site."

My original feeling was that I would be living at home and commuting if I went to MIT. I really didn't want to do that because I wanted to get away from my parents like most children in their high school years. When they indicated that I would be able to live on campus, I then decided that that's where I would go. But, when I went there, like I said earlier, I was planning on majoring in either mathematics or chemistry or physics because those are topics I was good at, and I enjoyed them, and I wanted to learn more about those topical areas. But, I quickly found out that mathematics really didn't suit me because it was much too abstract. Physics also seemed to be a little abstract in the sense that the sorts of laboratory experiences that I had had in high school in the area, which I had liked, were all kind of handbook-type, unreal laboratories.

I wanted something that had more connection to real things. So, physics didn't suit me as well as I would have liked. Chemistry, I also quickly decided I could not major in or work in the area because I couldn't pronounce all the names of all these chemical compounds or remember their names. I also couldn't keep straight, which is positive and negative, the anode or the cathode. So, I couldn't see myself in that area either, which left me with a quandary: what am I going to major in? I did do something that perhaps was a little stupid at the time because I didn't know any better. In hindsight, actually, it was a very foresighted method I chose. I looked at the departments at MIT which required the fewest number of courses in that discipline because I really wanted to still take courses in math and physics, and chemistry because I still liked them, and I wanted to learn a little bit more in those areas.

With the particular discipline I chose to major in having the fewest required courses in its own discipline, I was then freed to take courses outside their department. The particular discipline was materials science at MIT, which had the fewest number of required courses in it. So, I figured this was the course for me. And so, I subsequently began majoring in materials science.

Willard:

Was it called that at the time, Bob?

Shull:

In fact, at MIT, it was called the Department of Materials Science. It was one of the first big universities to do that. Most other places were metallurgy or mining and metallurgy or chemistry departments. Now, most all materials science departments are materials science departments. At that time, MIT was quite ahead of others in that regard. In hindsight, it really was the best thing for me to major in materials science because materials science includes elements of all three of those disciplines I liked, plus biology and other activities. So, that was really the right place for me.

00:18:20 My Time at MIT – A Scholastic Challenge, Father and Son Duo, and Meeting My Future Wife

Willard:

So, at the time, your father was still a physics professor at MIT. What types of interactions did you have with him when you were an undergraduate? Did you take any of his courses?

Shull:

No, I never took a course from him. I was a little scared, I think. I joined a fraternity called Phi Sigma Kappa when I was a freshman at MIT, which was really a good thing to do. In fact, the fraternity system at MIT is a wonderful system. It's quite different than the fraternity systems at other universities, I think, in the sense that all these individuals, brothers, are working to help the others. MIT being a very competitive place, you really need to work together, get assistance by others. It's a very good atmosphere. Some of the brothers knew that my father was Cliff Shull, a professor at MIT. They took delight in letting me know that the nickname that the students had for him was 'Flunk 'em All Shull.'

So, as a consequence, I wanted to be certain I did not take a course from him, but I would see him on occasion. And, it was useful for him to be there and have a car I could borrow on occasion. So, that was handy. I did go to listen to one of his lectures he was giving to the physics department once. Unfortunately, I had spent the whole night beforehand staying awake, working on a humanities paper that was due that particular next day when I went to listen to this lecture of my father to the physics department. My father made a mistake, as far as I was concerned, in that he would turn off the lights and show some slides to the people during the talk. Then he'd turn the lights back on and talk a little bit more; then he'd turn the lights off again.

I made it through the first time he turned the lights off. I did not make it through the second time he turned off the lights. So, the next thing I knew, people were clapping, and people started to file out. So, I did the same thing. I didn't go up to see my father at that point. But, I saw him the next day, probably in order to get a candy bar from his bottom right drawer of his desk, which I found out quickly is where he hid them. His opening comment to me was, "I hope you don't learn in all your classes through the osmosis of your snoring."

Willard:

So, he did notice?

Shull:

Yes, he did notice, exactly. My suspicion is that most everybody in the lecture noticed. Now, I must admit that, while I was at MIT, I didn't do really well scholastically my first semester there. As a consequence, I did have to take the physics course again (of that first semester). I did that during the summertime after the end of my freshman year. It turned out to have been a very fortunate thing that I was doing that because that's when I met my future wife.

Since I was on campus taking this remedial course, I found out about all these different dances (or mixers, they used to call them) at different places around MIT, where you could meet other people, male and female. My future wife, Mary Carty, happened to attend one of these mixers that I was at, and we got to dancing. We would meet up after that on occasion on dates to go to the beach or see a movie or something. We subsequently got married at the end of my senior year at MIT.

Willard:

Fantastic.

Shull:

So, as I said, it was a very fortunate thing that I was not so good scholastically that first semester; luckily, I did much better after that first semester. In fact, this reminds me of something that I tell students all the time when I'm interviewing them for college, which I've done for many years here locally. I was pretty good in high school in the sciences and mathematics, so I didn't actually have to do a whole lot of work doing the homework. I kind of thought going to college would be the same thing, that I wouldn't have to work so hard in these disciplines.

Well, it was a rude awakening for me that first semester to find that everybody else there was equally good, in fact, a lot better than me. I had to do some work, but it took me several weeks to find this out, and by that time, I was in a deep hole because I kept pushing to later work I needed to do now. If I couldn't do a particular problem in a homework assignment for that week, well, okay, I wouldn't do it. And, if I didn't have time to deal with the chemistry topics that were exposed that week, I'd wait until the following week. But, new material was being added all the time that I needed to also learn, so it's like a never-ending spiral. I warn new students about this and caution them to not let things slide.

Willard:

Did you ever get help from your father with your physics homework?

Shull:

On occasion, I would touch base with him and say, "Gee, I don't know how to approach this particular problem," and he would give me some good hints. Usually, I would get together with some of my fraternity brothers, who were in the same class as me, or those who were older than me and had obviously gone through some of these required courses beforehand, for some assistance.

00:26:00 My Father's Remarkable Research – A Nobel Prize in Physics for Neutron Diffraction

Willard:

So, just as you were finishing your bachelor's degree, your father wrote a now quite famous paper in the Physical Review. This is the work I remember that he did on neutron diffraction in manganese oxide. I think I associated it most closely with his Nobel Prize in Physics, that was awarded in 1994. At that time, right around when you're finishing up your bachelor's degree and thinking about graduate school, maybe, did you recognize the significance of that work? Was it during grad school or later that you realized that your father was really doing something remarkable?

Shull:

No, actually, while I was growing up and also a little while at MIT, I really did not appreciate the significance of the work he had been doing up until then. It wasn't until I actually went to graduate school at the University of Illinois in Champaign-Urbana that the significance of some of that work, and specifically this work on the manganese oxide, became apparent. In fact, most of the reason for awarding the Nobel Prize to my father was actually not for his foray into using neutrons for magnetic structure determination. It was for developing the whole science behind using neutron scattering as a structural tool for, in particular, very light elements like hydrogen and carbon, and oxygen. Of course, these are the elements that make up life.

These are the elements where x-ray scattering, x-ray diffraction, was not sensitive for showing you the locations of them. So, neutron diffraction became a tool, not just for the metallic elements but also for

probing biological things and, as he found out, also for magnetic things. Now, in fact, that work on the manganese oxide he did before I was at MIT. He had done this work when he was actually still at Oak Ridge and developed the capability of using neutron scattering for probing magnetic structures. And, of course, this was the very first tool that was available for telling people whether this particular atom has a magnetic spin that's of a certain magnitude and pointing in this particular direction. Up until then, there was no tool that was available for people to make such determinations. This was really the beginning of magnetic structure in general, and that manganese oxide work you mentioned was work that showed that there was the existence of a long-range ordered magnetic state known as antiferromagnetism.

It had been predicted by Louis Néel a number of years before, but there had been no proof to show this. With his manganese oxide work, they were able to show that, at low temperatures, there were additional Bragg scattering peaks, which came about because you had a second lattice, a sublattice, that had magnetic moments ordered in the opposite direction from those which were apparent at high temperatures where it's paramagnetic, not antiferromagnetic. I did not recognize the significance of that work, or any of his work in scattering while growing up because my father was very good at not pushing science on me and my brothers, but he was always there to explain the science behind things and to get you to appreciate that there are reasons for things happening the way they happen; but he didn't push it on us. Even when we would go and visit him at the reactor, the nuclear reactor, either at Oak Ridge or Brookhaven or MIT, he didn't show us all this equipment and explain it was used to do such and such or explain it was a useful probe of such and such else. He was kind of laid back and stayed back in that regard.

I took a course at the University of Illinois in Champaign-Urbana given by my thesis advisor, Paul Beck, on magnetism because, at that point, I was studying magnetic properties of materials. He taught this course, and one of the things he brought up was showing this manganese oxide work. So, I looked up the paper and read it in more detail of what it was all about and began to recognize the significance of some of his work.

00:32:29 Interning at Union-Carbide to Graduate School at the University of Illinois

Willard:

Did you know of Paul Beck from your father, or how did you decide that the University of Illinois was the right thing for graduate school? Was it right after undergraduate work?

Shull:

Yes, I went there right after my undergraduate bachelor's degree. During the summertime, while I was an undergraduate at MIT, the first summer I spent in remedial training but actually not completely. The second semester of that summer, I spent working for a couple of professors in the mechanical engineering department: Egon Orowan and Ali Argon. Now, here again, I did not know the significance of these particular individuals, but both of them were phenomenal developers of the science of dislocations and strengthening of materials. I didn't know anything about dislocations or anything about materials at the beginning. But, after that summer, where I was working with them part-time on torsion experiments and stress-strain relationships, I liked it a lot. It was my first exposure to a fundamental material science topic.

Just before my third year at MIT, during the summer, I got an internship working for Union Carbide Corporation in a place in the sticks called Kokomo, in Indiana. Now, it was exciting to me to go out away from the east coast, with the exception that there was no more ocean to go swimming in. But, I was enthusiastic to go and work for a company, a real company, to find out what working in this field really was like. While I was working in this laboratory on nickel alloys, hastelloys, looking at an over-aging effect

basically is what it turned out to be. That was my summer job to find out why these particular alloys they were making huge ingots out of were not as strong as they should be. I concluded it was because of over-aging at the treatment temperatures and times that they were using. Paul Beck was a consultant for them.

One day, he was coming out to give a talk to people at Union Carbide. So, I went to his talk; unfortunately, I overslept that morning, so I got to his talk late. Instead of going in the main doorway to the conference room, I kind of went in from a backdoor way from the secretary's office. As I opened the door, I almost knocked Paul Beck to the ground as I opened the door because he was standing right by it. So, it was a very inauspicious meeting of him. As I was talking to him afterward, he said he knew my father, and he would be interested in me coming up and visiting and seeing what his university looked like that summer. So, we made plans for me to do that. I also then later arranged to go up to Northwestern University to talk to John Hilliard about the possibility of graduate school there as well.

I ended up applying to both of those schools at the end of my senior year. I was admitted to both of them, and I was about ready to go to Northwestern University when Illinois offered me a lot more money. Being bought, I decided to go to the University of Illinois in Champaign-Urbana instead. Unfortunately, I only stayed there for one semester. This was right in the middle of the Vietnam War period, and I got my draft notice saying to report to a physical up in Chicago on this particular day in February of that year 1969. I figured I would get out of being drafted because of my eyesight being so bad. And, in fact, they didn't measure it as it was worse than they could measure. It's 20 over 600 plus, I remember they said, but they still took me. So, I left graduate school in February of 1969. Then, I came back to the University of Illinois graduate school in September 1971 after spending two years defending our country in Oklahoma. And, you'll notice that I was very successful in what I did because nobody ever successfully invaded Oklahoma while I was there.

00:38:30 Mictomagnetism – Interactions Between Clusters of Magnetic Spins

Willard:

So your thesis work was on mictomagnetism and iron aluminum alloys. Can you describe what mictomagnetism is? Even magnetic-materials people probably don't understand what mictomagnetism is.

Shull:

Well, it comes from the Greek word meaning mixed, micto, as I'm sure you know quite well being married to a Greek lady. At any rate, it was a term coined by my thesis advisor, Paul Beck, and it described the magnetic state of some particular materials, iron aluminum turning out to be one of them, wherein a certain temperature range, its magnetic character looks like it was kind of a combination of anti-ferromagnetic interactions working on the magnetic spins, as well as ferromagnetic interactions working on those spins. It occurred in materials where the magnetic spins in that material were grouped together into clusters. So, it was actually a clustered magnetic state. The interactions between the spins in these clusters and the surrounding magnetic spins that were not in those clusters could be described by particular confirmations of interactions, some of which are positive, in other words ferromagnetic, or negative (or anti-ferromagnetic).

Paul Beck coined this term mictomagnetism, but it describes basically a cluster of magnetic spins, which are acting cooperatively and interacting with the sea of other spins around them, as well as other magnetic clusters. As a consequence, you get very characteristic magnetic hysteresis loops when you cool them down from a high temperature to lower temperatures. Typically, this magnetic state exists at lower temperatures

than room temperature, and you would need to cool them down in a magnetic field in some particular direction. So, you get the signature of it, it's this magnetic anisotropy, where the hysteresis loop would be displaced from the origin along the field axis. But, in the iron aluminum system, it was particularly interesting in the sense that a previous graduate student of Paul Beck's, Hiroaki Okamoto, had measured one of these alloys from room temperature on down, and they found this data point that was outside of the normal systematics that you would expect.

But, Okamoto did not have that many data points in this temperature region. So, Paul Beck suspected it was a real data point, and he wanted to suggestively explore that. And so, I was quite interested in trying to do that. It turns out that it was a real data point. And, if you look at the systematics with much more detail, it was quite unusual. In fact, what we found in this particular system is that the material changed from ferromagnetic, or long-range order, to this cluster magnetic state, or perhaps more people called it, not mictomagnetic, but spin glass magnetic state at low temperatures, or paramagnetic at lower temperatures. So, this is very unusual, of course. All ferromagnets become paramagnetic as you go up in temperature. Here was this material that went paramagnetic from the ferromagnetic state as you cooled it below room temperature.

So, we coined this term, this inverse Curie temperature in this one particular alloy; I think it had 29.5% aluminum in iron. So, it's basically iron and has a very small amount of aluminum in it. One of the things that Paul Beck stressed was for all of his graduate students to build a special apparatus. This had the dual purpose of, first of all, training the student in designing and then having constructed some particular apparatus equipment. Secondly, then the laboratory, in general, had another tool they could use to explore other materials. So, the device that I was then charged to construct was some way to show that, in fact, this material, this iron aluminum alloy, was paramagnetic in this lower temperature region. One way you could do that is to see if there's any remaining magnetic moment on that material after no fields are applied at low temperature.

So, I built this apparatus, which would rotate the sample quickly at any temperature. Then you would have a coil around that sample so, as it rotated, it would induce a potential and, thereby, current in that coil. And, you thereby could deduce whether it had a remaining dipole moment on the sample, and that would tell you whether it was paramagnetic, which would have no dipole moment remaining, If it was still ferromagnetic, it would have a little dipole moment, and you'd pick up that induced potential. So, that was the device I constructed and, sure enough, showed that at room temperature, you've got a nice big pickup in the coil, while at low temperature, there was none.

00:45:43 Understanding the Metallurgy and Magnetism of Iron Aluminum Alloys

Willard:

How did you make your samples, Bob?

Shull:

So, this was another thing that Paul Beck really stressed with his students is to understand, well, the character of the material you're making your measurements on. He was a metallurgist from the very beginning, and he stressed the metallurgy of the materials. So, we made these materials from arc melting iron and aluminum metals into alloys. Then we would cold work it after quenching the material. The cold work builds dislocations into the material, which causes dendrites, which are formed in the solidification process, to break up and get moved all around. Then we would put it under a vacuum enclosure, sealed in

quartz capsules, wrapped in tantalum foil to keep them from oxidizing, and then heated up to very high temperature. Then we would quench them and make certain that we quenched them very quickly into an icy brine solution and smash these capsules as they got into the system.

So, it would quench very quickly, thereby not changing the structure of the material from the high temperature to the low temperature. Then we would cut out pieces, and we would use a grinder, a belt grinder, with a little copper elbow upside down just over that paper on the belt sander with Emery paper inside that little elbow. These little cubes would then go bouncing all over the place and getting ground by the Emery paper inside to form spheres. You want a sphere so that you do not have any shape anisotropy to your measurements that you subsequently make on the magnet, on the magnetic property of the material.

Willard:

So, the treatment was for both microstructure and for crystal structure, getting everything so that you'd have these cluster formations that you need for the interesting magnetic properties. Was it for both of those things?

Shull:

Exactly, and so we would conduct x-ray diffraction measurements on all the samples. We would send it out for analysis of the gas content. We would do microstructure sectioning and observe it under a microscope to get microstructure. So, indeed, we would do a lot of work, or I did a lot of work, during this thesis to probe the metallurgy of the material, in addition to the magnetic measurements. Then we wrote lots of programs for computers. Now, this was the beginning of computers, of course. I would take this set of cards, we punched out cards in those days, and I would physically take these up the street to the computational center and put them in, and it would run a program I had designed and written for it. It was very cumbersome. It was wonderful when I was a graduate student that we got this wonderful calculator made by Wang Corporation, where we didn't actually have to go up the street to the computer. It was basically a microcomputer that the department had gotten and that we could use to do some fittings. So, we wrote programs to try to fit our data to Brillouin functions and all sorts of things. So, those were the early days of computations.

00:50:10 My Postdoc Motivation – Understanding Magnetic Character in the Amorphous State

Willard:

After graduate school, I know you went to CalTech for a postdoc. Did you go directly to Caltech?

Shull:

So, I did. I went directly to Caltech from Illinois. The reason I did that was I went to work as a postdoc for a fellow, Paul Duwez. Now Paul Duwez was essentially the father of rapid solidification or metals which were not crystalline; they were amorphous materials. People didn't think that was possible until he showed this a number of years before I got there, then explored that quite a bit. My interest was from knowing in the iron-aluminum alloys that its magnetic character, including this reverse Curie temperature phenomenon I mentioned we found, was dictated by crystal structure. You can take the iron-aluminum alloys in the composition range I was working in, and you could order them into the cesium chloride FeAl structure for the same composition, or you can order it at a different temperature range into what is called

the DO_3 , or the Fe_3Al structure.

Without changing composition, you can have any of those two different orderings. And some of my thesis work examined how that changed the magnetic character of it. Or, you could take the material in the cesium chloride structure, and you can disorder it to the point where it was not the cesium chloride, but it was basically body-centered cubic structure. So, I thought going out to Caltech and working with Paul Duwez, I could work on iron-aluminum alloys, where we made them amorphous to show my naivety. If we made them amorphous, then we eliminate a lot of the structural connectivity to the magnetic character. This would educate me quite a bit at the same time as educating me in this whole new class of materials, which was really a big advent in materials science in those days. This was a completely new class of materials, these metallic glasses that you can form.

00:52:56 Three Years at Caltech – Probing the Heat Capacities of Materials

Shull:

So, that's why I went out there. I quickly realized that one of the big areas where there was a lack of information was the heat capacities of the materials. So, I embarked on a project to build a probe that would enable me to measure heat capacities. So, that's basically what I did for the two to three years there. Most of the time was spent constructing this apparatus, and only in the last month or two was I able to actually make measurements on these materials. But, in the interim time, I learned a lot about metallic glasses. I also came into contact with a fellow who's become very well known in that area, Bill Johnson - William L. Johnson, who came there as a young professor at exactly the same time I came there as a postdoc.

And so, we got to know each other quite well, and I made a trade with him. I told him I would teach him nucleation theory, which, of course, all good materials scientists are taught in college. And, in trade, he would teach me electron screening in a lattice. As an electron goes through a lattice of material, how it gets screened by positive ions in certain locations. So, we each taught the other these other topics. And, of course, you can see what was much more important was learning about nucleation theory. At any rate, I had a good time there. One of the things that I set out to measure was to look for an anomalous linear contribution to the heat capacity when you made the material amorphous. I did this by making a series of metallic glass superconductors.

In superconductors, when they are conducting, there is no electronic contribution or a linear contribution to its heat capacity. This is what I set out to find. In fact, right near the end of my three years at Caltech, I got these measurements in hand, and sure enough, there was a slight linear contribution, but very small. Unfortunately, there was a group at Bell Labs, which had come out using zirconium palladium materials in showing exactly the same thing about six months to a year before me. And, as a consequence, I never published this data. I probably should go back and find it and at least put it in the literature, but it was a little disappointing. Well, it was both disappointing; it was exciting too to measure this sort of thing, especially to be able to measure heat capacity on very small samples, which is what all we had to deal with by splat quenching. We would make these materials amorphous.

Willard:

I was just going to ask how you made those materials. The rapid solidification was still pretty new, and Duwez had his way of doing it. But, there were other ways, right?

Shull:

Right. So, the way that Paul Duwez found most, I wouldn't say effective, but perhaps gave you a little bit more sample to work with, was a piston and anvil technique. His original thing was to shoot all liquid out of a quartz tube with a high-pressure jet onto a slanted piece of copper. As that liquid hit that copper at an angle, it's spread out, and it got quenched quickly by the high heat capacity of the copper, sucking the heat out of that liquid. Well, you ended up with a very irregular material, mostly powders, not a lot of material to work with. Whereas what he devised later was to slightly pressurize this quartz tube from the top, where you would liquefy your alloy inside with an induction heating coil.

You would pulse the pressure so the liquid would drop out of that tube but at a slow rate. And, it would trip a little light sensor which controlled a piston and anvil sitting on the side that would come smashing together, and, hopefully, you would catch that liquid droplet in the middle. In fact, it works nicely, but you miss hitting that liquid droplet most times, or you'd hit it on the edge, or your plates would not be parallel; they'll be at an angle. And so it would quench the droplet at different rates through it. After doing this, I don't know, 50 or 60 times, you would hit it maybe four or five times nicely, and you would get some sample, but not a lot.

Willard:

But that was the way that you did your experiments for the heat capacity, right?

Shull:

Correct, I would make samples this way for making the heat capacity measurements.

00:58:57 Setting Up Rapid Solidification at NIST – Analyzing Properties of Amorphous Structures

Willard:

So, you end your postdoc at Caltech. Did you go directly to NIST from there, or did you have some stops along the way?

Shull:

I took basically a two-week vacation traveling across the country in a Volkswagen Squareback car with my wife and two children: The oldest being four or five and the youngest being two at this point. So, with our worldly goods in this the Volkswagen Squareback, we went up to Yosemite Park, then several national parks as we traveled our way east, and just one step ahead of a big snowstorm which was coming. We had some motivation to keep on moving, but we spent basically two weeks traveling to go to the east coast from Pasadena, California, to Maryland, where I had accepted this position as a postdoc at what was then called the National Bureau of Standards or NBS.

In fact, please pardon me if you hear me switching from NBS to its later name, which they changed the name in, I think, 1988, to the National Institute of Standards and Technology or NIST. So, I took this postdoctoral position at NBS, and my job was basically to set up a capability there for doing rapid solidification. As I say, this was a whole new area of materials science. So, it was quite wide open in terms of what sort of properties these amorphous structures possessed in different systems. But also, at the same time, there was a big activity, led primarily by NBS, but jointly with the American Society of Metals, for

improving on and measuring and determining the binary phase diagrams of all these different systems.

The big books of that time were Hanson's book and then Elliot's and Shunk's compendiums, which would add to them. Most of the phase diagrams had dashed lines indicating that's what it's thought to be, but there had been no measurements or definitive determination. They wanted to redo all these systems, all the binary systems, and some of the ternary systems. So, I joined that effort at the same time at NBS as setting up the capabilities to do rapid solidification.

Willard:

So, when was this, Bob? Was this the end of the 70s or the beginning of the 80s?

Shull:

Yes, it was. I arrived at NBS in November of 1979, so at the beginning of 1980 I began all this work in those two different areas. And due to my experience of not getting very much sample by splat quenching materials, as I described earlier, I decided a much more productive method for doing this rapid cooling of liquid to a solid-state would be by a technique called melt spinning.

01:03:30 Melt Spinning – Designing A More Productive System for Rapid Cooling

Shull:

Here you have a very fast rotating wheel, generally made of copper, but it could be made from other materials depending upon what alloys you're trying to rapidly quench. And, you spit out from a tube an alloy that you have melted in an induction coil and pressurize it briefly, so it shoots out this liquid from a hole in the bottom of its quartz holder onto this fast-rotating wheel. In the best case, you would get these ribbons of material, and you get miles of ribbon for the small amount of liquid that you are injecting down because the thickness of this ribbon would be in the tenths of mm range. I decided that was a much more productive way to do it and set about constructing such a thing. It also became important I realized when I was at Caltech working with Paul Duwez that there are many alloys, like titanium alloys, in particular, which will oxidize very quickly when you heat them up to the temperature that they have to go in order to liquify.

That oxidation, or nitriding from the nitrogen in the atmosphere, will change the system significantly. So, the system I went to construct at NBS had to be able to melt spin under vacuum, or at least be vacuum tight so it could be evacuated from all the air in it and then contain the inert gas we re-introduced into it. That was the design I used, and we came up with initially using a big bell jar with the top cut off and then placing a big rubber seal on the top to a steel cover plate. No, sorry. We actually used a big plexiglass plate on top, through which we had a vacuum feed-through for our little quartz sample holder tube sticking into the induction heater located inside the chamber. So, that's the system that I designed. And one of the first things that we tried melt spinning was for a guest scientist that came to the NBS at that time named Danny Shechtman.

He was interested in doing some rapid solidification of aluminum alloys. One of them was an aluminum manganese alloy. These are primarily aluminum with small amounts of manganese or other elements added to it to melt spin. So, a technician in this laboratory where I had this melt spinner constructed then melt spun this for Danny. Danny, being a wonderful electron microscopist, took these flakes -- they came out with flakes rather than ribbons -- and took some of these flakes and looked at them under the electron microscope and found this unusual five-fold symmetry; in some of the structures he observed. And, of

course, that took a whole life of its own, these so-called quasicrystals that were formed in this material. To show how smart I was, I decided that this was not something I wanted to get involved in, even though I could see how exciting this was since it went completely against all my training at MIT on what sorts of symmetries are possible in crystals. They had to fill space, and therefore five-fold symmetry is completely verboten. I had another focus of probing metallic glasses. So, I kind of stayed out of that arena pretty much. I don't know if that was good or bad. At any rate, I would focus on other things.

Willard:

Well, he did get very famous for it.

Shull:

Of course, Danny Shechtman ultimately received the Nobel Prize for that discovery.

01:18:10 Phase Diagram Clarification of Titanium, Aluminum Binary System at High Temperatures

Willard:

I know. So, famous or not, he did win The Nobel Prize. What was your activity with the titanium alloys? You described a little bit about this. This seems like a pretty practical thing to do, not related to melt spinning per se, I think.

Shull:

Yes, this was in that second project, or focus, of NBS at the time clarifying phase diagrams. One of the big industrially important systems where there were, at the time, five or six conceptually different diagrams for the phase equilibria was the titanium rich end of the titanium-aluminum system. With this system, the useful alloys tend to be quaternary and quinary alloys with other elements like niobium and molybdenum and other things added to it in small amounts. But, the basic system was titanium-aluminum with small amounts of aluminum. And, as I say, this was like a lot of other systems where there were lots of dashed lines demarking the phase boundaries. The big uncertainty was at high temperature; it was well-known that the titanium-aluminum system formed a complete solid solution of aluminum and titanium, forming a nice body-centered cubic structure at high temperatures. At low temperatures, it was also well-known that, at the small aluminum contents, again, a nice solid solution of aluminum in titanium was stable, but now in a hexagonal close-packed structure.

This low-temperature phase was called the alpha phase. That body-centered cubic phase was the beta phase. But, it also was found by people that, at much higher aluminum contents, that is, contents still less than 50% but greater than 30% aluminum in titanium, then the material would form an ordered hexagonal structure called the alpha-2 structure or Ti_3Al , was the prototype of this. But, how you went from the disordered alpha hexagonal phase at low aluminum contents to this ordered alpha-2 structure as you added aluminum was very debatable. And, debatable in addition, was whether or not the two-phase field between each of these two single-phase fields of alpha and alpha-2 actually went up to high enough temperature to intersect the body-centered cubic structure. Most of the diagrams people were using at that time showed there was an intersection of that two-phase field with the beta phase.

That was part of what I set out to clarify. And indeed, what we found is that the miscibility gap between these two hexagonal phases, alpha, and alpha-2, at low temperature, came together at high temperature

to a single point. And, that single point was not at a high enough temperature that it reached the alpha-beta two phase field. So, there basically is a miscibility gap rather than an intersection forming a peritectoid reaction with the beta phase, as most of the diagrams showed. In addition, even at higher temperatures and aluminum contents, we showed that there was a eutectoid reaction between the alpha-2 phase and an even higher aluminum content single phase called the gamma phase, which is also a very industrially important structure because it is stable to very high temperatures before it decomposes into some other multi-phase system.

So, that was basically what I did. It was very good to finally clarify that diagram since it has a profound impact on the morphologies obtained in industrial alloys. The alpha-2 Ti₃Al phase is very brittle. The alpha phase is not brittle. But, if you get an intersection of those phase fields with the beta phase, then on cooling from this multiple-phase field of nucleated beta, you get surface rupturing when you go from the cubic to the hexagonal phase, which then means lots of dislocations are introduced into the material, in particular facets. That can be deleterious to the structural integrity of the alloy.

Willard:

So, this was more a practical project for you.

Shull:

Correct.

Willard:

This is something that was industrially motivated. And, the other side, so you had two very different things that you were working on: something very fundamental on one side in looking at the rapid solidification and something far more practical on the other side.

Shull:

Right. One of the other things that we learned in this whole project was how important it is, if you're doing experimental work on clarifying or determining phase diagram for the system, that you approach a particular phase boundary both from above and below. That is one approach is from high temperatures down, making alloys that you quench from a single-phase field that's above a two-phase field boundary. And the approach from below is via alloys that you quench down to very low temperature, and then you sequentially age it at higher and higher temperature to come to that boundary. It's very important to do both and make certain that it converges. The second thing is to make certain you do a series of compositions that are relatively close to each other in order to definitively determine that boundary. This is something that John Cahn, an extremely well-known materials scientist who was also at NBS at the time and whose office was only two doors down from mine, taught me at the very beginning. That was really important because that helped us to define the phase boundary between a single-phase and a two-phase region definitively. And we could, in addition, determine differences between coherent phase boundaries and incoherent phase boundaries. That is where one of the phases is crystallographically coherent with one of the other phases across that boundary.

Willard:

So very, very interesting.

PART 2

00:00:00 Pivoting from Alloys to Superconductors – Starting a Magnetic Materials Group at NBS

Willard:

So, Bob, by the late 1980s, your work changed from alloys and kind of working with melt spinning and in phase equilibria in titanium alloys. And, it changed in what, on the face of it, seems like a very radical way to looking at high temperature ceramic superconductors. It seems like a real significant pivot. How does that kind of connect to these other topics? How did this work kind of originate for you?

Shull:

It came out of the fact that, after working on these other two projects I described earlier, melt spinning and phase equilibria, one of the things I found at NBS was that there was no magnetism activity per se at this place. And, of course, having been trained in that area, as well as materials science at the University of Illinois Champaign-Urbana, I was interested in, perhaps, starting some activity in that area. There were also two other individuals that I had met in this interim period at NBS who also had a magnetism background but from different directions. One of these people was a fellow named Larry Bennett, Lawrence H. Bennett. He had done a lot of work in nuclear magnetic resonance. In fact, it was his work using an NMR probe of a cancer in a rat's tail that was cited by the individual who ended up receiving the Nobel Prize for developing magnetic resonance imaging.

A second person was Lydon Swartzendruber. He was an electrical engineer by training but had done a lot of Mossbauer spectroscopy. And so, the three of us went to the laboratory director at that time of our laboratory, the materials science and engineering laboratory, and suggested we start a group probing magnetic structure and the magnetic properties of materials. This laboratory director was Lyle Schwartz. He had come to NBS from Northwestern University, where he was a Mossbauer spectroscopist. So, he appreciated the significance of being able to measure magnetic properties of materials. And so, he was very receptive to this idea. He said he would talk to our division chief about seeing how they could do this, and they subsequently freed up a little bit of money for us to get started.

I, therefore, left those other two activities and started this new group, magnetic materials [with Larry and Lydon]. There was only enough money to purchase one piece of equipment, so we purchased a magnet, an electromagnet, and a vibrating sample magnetometer made after the original design of Simon Foner at MIT. So, this was the only tool we had at our disposal, but it was sufficient for us to get started.

00:04:17 High Temperature Superconductivity in Ceramics – Stunning the Physics Community

Shull:

I started out making magnetic measurements on fine particle [materials, especially such materials made as thin films] by sputtering. In fact, I started co-sputtering two different species together in order to make a uniform mixture of materials, [one of which was ferromagnetic] in a thin film form. Well, shortly after, like a year or so after we had started this new group in measuring magnetic properties, there was a discovery by two scientists in Switzerland, Georg Bednorz and Alex Müller.

They had discovered high temperature superconductivity that was available in some ceramics. This was not

the first ceramic, of course, that showed this, but it was the first ceramic that showed superconducting transition temperatures as high as 40 degrees Kelvin. A lanthanum strontium copper oxide was one of the first ones they came out with. And then, later on, Paul Chu discovered superconductivity occurring up as high as 90 degrees or 92 degrees Kelvin in a yttrium barium copper oxide.

Well, these discoveries stunned the world. The whole scientific community was astonished by these discoveries. Most of us stopped what we were doing and went into the superconductivity area to see what we could do to help explore how such a phenomenon could occur in these unusual oxides. Up till then, [superconductivity had] primarily [been found only] in metals [and their] alloys, so this was kind of a new class. So, that's how I moved to that area, and everybody was working full blast. I mean, it was a terribly exciting time in the physics and materials community for those of us working on electronic or magnetic properties of the materials.

Willard:

There was a very famous APS March meeting where the announcement came out and went deep into the night. Were you at that meeting?

Shull:

Indeed, I was there. It was held in New York City. This was at the well-known March meeting of the American Physical Society in March of 1988. I was present at this meeting, presenting work on my co-sputtering of iron and silica, as I recall, at this meeting. It was like the "Woodstock" of physics. I mean, everybody was excited. They held a special session in the evening of this one day; I think it was the second day of the meeting. The session was going to start at like 6:00 in the evening. By 5:00 in the evening, in this whole hotel convention center, all the space, the meeting rooms, and everything were completely filled up by people. People were sitting on the stairwells in order to be able to view several closed-screen TVs set up outside of the meeting room.

You couldn't get into the meeting room, which housed a thousand people easily. You couldn't get in there because it was so packed. It was just terribly exciting. Well, the meeting went on until, I don't know, 3:00 or 4:00 in the morning. I remember I was able to finally get into the actual room, the actual meeting room, at about 1:00 in the morning, and I lasted for another hour nearly, and I decided I had to go to bed. It was amazing; I've never seen anything before like that in the scientific community or even afterward. So this kind of showed the excitement and enthusiasm of the whole physics community around the world.

00:09:10 Making Superconducting Ceramic Thin Films by Laser Ablation

Willard:

So, was it an influence on what you did for your contributions to this area? Did you learn some things that kind of gave you an idea, a spark, for your contribution?

Shull:

Yes, indeed. I think most of us then went to using the tools that we had at our disposal and our expertise to probing these new materials. Well, the one thing I was doing, as I mentioned, was doing this co-sputtering of thin films. And I remembered a paper by William McLean. I think he was at Princeton, but I may be wrong there. [In that paper, he described his work] probing the superconductivity in aluminum and

aluminum films. And, when you made particulate aluminum films, he found the superconducting transition temperature rose in aluminum. It went up to, I think, maybe 4.4 degrees Kelvin from 4.1 degrees Kelvin, something like that. It wasn't a monumental increase, but it increased by having this film comprised of aluminum particles [rather than solid aluminum]. So, I thought that's what we can do here, [to increase the superconducting transition temperature further in these new ceramics.]

My feeling was that this high-temperature superconductivity probably comes from [the particulate nature of] these yttrium barium copper oxides. So, we [started co-sputtering them to make even smaller particles]. If you make very small particles of the yttrium barium copper oxide and also of the lanthanum strontium copper oxide, we'll get transition temperatures that would go up to room temperature. So, this was my thought, which shows how naive I was, of course. So, that's what we embarked on doing. I was collaborating at the time to do this co-sputtering with a group at the Applied Physics Lab of the Johns Hopkins University Kishin Morjani was the professor [at APL in charge of that facility.] So, we tried sputtering, and it didn't work at all. We did this like two days after this APS Woodstock in New York City, and it didn't work. [We got no superconducting material at all.]

Then, we decided to try using another tool that Kishin Morjani had in his laboratory. I'd never utilized it before. It is called a laser ablation system for making thin films. Here you send a laser hitting a target of the material you want to deposit into a thin film. It blasts the material away from this target; it then deposits on some sort of substrate, making your thin film. So, we tried that, and it worked. We say it worked because, at the same time, [we used a new tool developed by] Kishin Morjani [to probe for superconductivity. Kishin] also was an NMR specialist, and he had this system for doing NMR measurements. But he came up with this idea of modulating a magnetic field on top of the sample at a slow rate [while running the NMR equipment]. And, you look at the absorption of the microwave in this system that is in-phase with this low field modulation. The idea here is that when a superconductor changes from superconducting to normal, it will do that [at a modified temperature] as you apply a field to it. So, its transition temperature will modulate in-phase with the field modulation.

So if we measured the microwave absorption in phase with that, we could detect the superconducting transition. That's what we applied to these thin films. And, sure enough, we could see a nice big signature at what we thought was a transition temperature, which was right around that possessed by yttrium barium copper oxide and, similarly, by lanthanum strontium copper oxide. So, that's how we got into it. We wrote up the paper, and by the end of that week [(i.e., a week after the APS "Woodstock" meeting)], we had submitted this paper. We got it through the review process at NBS, which is unheard of in that short time, and sent it to the Physical Review Letters for publishing. That got held up for a couple of months before it got published. And in the interim time, there were other announcements of other discoveries of making thin films by laser ablation. That turned out to be the industrial method of choice after that.

00:14:25 Magnetic Levitation of Superconductors – Understanding Their Hysteresis and Magnetic Flux

Shull:

That got us started in the area. But in the meantime, I started doing some other related things. In particular, I came into contact with a friend of one of the co-discoverers of the yttrium barium copper oxide superconductor. That co-discoverer was a fellow named M.K. Wu, who had been a student of Paul Chu's at the University of Houston. He was, at this time now, an assistant professor at the University of Alabama Huntsville. He had come up with a variation of this yttrium barium copper oxide material. Basically, he added some silver to it, making it kind of a composite, kind of like what I was doing before, but not with those materials. And he gave some of this material to a friend of his named Chou Yen Wong. He was a

member of the scientific staff at Lockheed Corporation in California. This fellow, Chou Yen Wong, brought this sample to our lab and said, "Look, this has an unusual effect: you can attract this superconductor with a magnet."

Of course, we told him to go away. We had many more important things to be doing than probing these materials [because that attraction effect is] not possible. We all know that a superconductor repels a magnet because of the flux exclusion of the magnetic field by the superconductor. [Such flux exclusion occurs because] supercurrents are created in the superconductor so as to create an image of the magnet that's outside of it. So, you, therefore, have two similar magnetic poles close to each other, and that results in a repulsion. You can't attract them. So, we told him to go away. Well, he didn't go away. Instead, he came and demonstrated [the phenomenon in front of us]. He had a little magnet, a bar magnet; he put it close to this superconductor (immersed in liquid nitrogen) that had been made in M.K. Wu's lab in Alabama. And he pulled this magnet away. Sure enough, the superconductor came with it. But, interestingly, the superconductor wasn't touching the magnet; it stayed stably located below it. We couldn't believe it.

So, we got very interested [in the phenomenon and how it could possibly exist]. We, consequently, put some of this material into the vibrating sample magnetometer that we had, and we measured a hysteresis loop, magnetization versus field, of it [at liquid nitrogen temperature. We] found it was a typical type two superconductor, which means it had a very broad open loop. We realized that, indeed, in the operating region where you have a positive field and positive magnetization, as the superconductor gets further away from the magnet, the field decreases, but the attractive force increases. This is because in a type two superconductor, you've got some magnetic flux trapped in the superconductor, and it's like a permanent magnet at the same time as having a superconductor. So, you have both repulsive and attractive forces, and, in this particular case, you could get stable levitation. So, it was really a learning experience, also quite fun. We then published in the form of all sorts of demonstrations that our general concept of attraction and repulsion in some superconductors was not complete.

And this showed that even the old type two superconductors that were well known in very low temperatures should exhibit exactly the same phenomena. But, of course, nobody ever tried to measure that before because you had to do this under more difficult liquid helium conditions, not the liquid nitrogen conditions we could use for these high-temperature superconductors. And so, later, people would check that out [and verify that result.]

00:18:55 Field Ion Microscope Imaging of Yttrium Barium Copper Oxide

Shull:

And, a third [area I got involved in was through a collaboration with] another scientist at NBS, a fellow named Alan Melmed. He had been a post-doc with a scientist named Erwin Mueller. Erwin Mueller had pioneered the development of something called a field ion microscope, and Alan Melmed had become a very expert field ion microscopist and had set up a system at NBS.

He came to me to see if we could provide some samples of this yttrium barium copper oxide to him. I gave him some of the samples which had been prepared by another scientist at NBS, a fellow named C.K. Chiang. C.K. Chiang, by the way, is very well known for having made the very first conducting polymer. He did that with Alan Heeger at the University of Pennsylvania well before C.K. Chiang came to NBS. Of course, Alan Heeger got the Nobel Prize for pioneering the development of conductive polymers. At any rate, C.K. Chiang had made the superconductor.

I gave pieces of that to Alan Melmed but also went with him to try to make sharp points [on the samples]. You need a very sharp point to get any image in a field ion microscope. So, you had to either electropolish materials to make very, very sharp points or find some other method. Alan was a real expert at doing this. Well, it turns out he wasn't very expert at making sharp points for these materials because, [at the beginning of this study,] he would think something was sharp enough and [only find out much later they were not sharp enough to get an image. It was common for us to find this out] in his laboratory late at night; 2:00 and 3:00 in the morning was not unusual because of the excitement in the area. 50 to 80 times, Alan would put the tip into the microscope, pump out the vacuum, impose this very large voltage to ionize the gas that's around the tip, and thereby propel it to a phosphorus screen to get an image of the location of these atoms, and, the tip would fly apart. So, we'd start from scratch again, find another piece and try it out, [only to see it again get] destroyed.

Finally, we got an image, and the image would just be amazing. For reference, this is a tool that will usually give you information on crystal structure. So, you'll usually see a series of circles showing where there's a pole of some crystallographic orientation. Here, we didn't see that. Over the whole field of view [when the sample was at the low temperature of 90 degrees Kelvin,] we saw basically parallel lines, a very unusual image; we had not seen them in any material before. So, we concluded from this that only some of the atoms were being imaged in the structure of this yttrium barium copper oxide. And so, we got together with this fellow in our mathematics laboratory who was very good at developing computational images. We calculated what sort of image we would get if we assumed only certain atoms did the imaging. Our results subsequently appeared on the cover of Science Magazine. That picture is a composite picture, showing that we only got imaging from these particular planes in the crystal structure of the yttrium barium copper oxide, which were planes formed of only copper and oxygen [atoms]. So, that [showed the superconducting state only existed in a small region of this crystal, along those planes.]

So, that's how I got into superconductivity and the sorts of things that we quickly did. Then, once we had done quickly that and a bunch of other things that weren't quite as striking, but certainly very important in terms of flux pinning and where the flux goes and how it comes out of the lattice, [I moved on to other areas. Specifically, I] decided to change after a couple of years working on these sorts of things to kind of go back to what I was working on beforehand. This was kind of like other people in the community; after they had also done the things that they could do quickly, they started meandering back to the things that they were working on before.

Willard:

Fantastic.

00:24:27 Discoveries of the Superconductor Craze – Layering Effects on Electrical Resistivity

Willard:

So, Bob, your work on high-temperature superconductors had some significant outcomes. At what point did you get back to that thin film work you were talking about before? Did you find this as a small segue from what you were doing, or was this a major thing that you continued to do for many years?

Shull:

I think, like most other people around the world, I stopped pretty much everything I was doing on those

materials discovered in 1987. I worked on superconductors using the equipment we had at our disposal and our expertise. But, after a couple of years, we had pretty much done the quick things that we could do and recognized that further exploring of the area would take a lot longer, that is, to answer some of the really important questions regarding keeping the current carrying capacity very high in some of these high-temperature superconductors. That was going to require a much more lengthy process. And, we weren't, particularly expert, I wouldn't say, in that area. So, we all kind of went back to what we were working on before, which for me was basically on thin film materials. Specifically, particulate materials that we would make by thin film processes like sputtering or a co-sputtering, in our case, primarily of a ferromagnetic material and a nonferromagnetic material like iron and silica, is a good example of systems I was looking at.

So, this is like the 1989-1990 timeframe. Interestingly enough, during the high temperature superconductor craze of everybody somewhat hidden were a couple of papers that were published respectively by Albert Fert in France and Peter Grünberg in Germany. And both of them had found an unusual effect in some thin films and, specifically, in layered material, where they had alternating layers on top of one another. Specifically, if there were three layers, where the two outer thin film layers were a ferromagnet, and the in-between layer, sandwiched between that, was a nonferromagnetic material, [they found had interestingly new electrical properties]. They first looked at iron and chromium around room temperature, where chromium is essentially paramagnetic so, therefore, not ferromagnetic. Obviously, the iron is ferromagnetic. When the thickness of that chromium layer was very thin, in the nanometer regime, they found an unusual effect on the resistance, electrical resistance, that is, of that material.

In fact, the electrical resistivity of that sandwich structure was quite high. And, when you applied a magnetic field, you got an even higher resistance, and it was quite a large increase in resistance. So, this well-known magnetoresistance that was created in this particular nanostructure was on the order of perhaps 30 to 80%, depending on what temperature people were measuring it at. Now, to put this into context, the largest magnetoresistance of any material known at that time was in Permalloy. This is an iron-nickel alloy, and there it's on the order of 3%. So, getting almost two orders of magnitude larger resistance by applying a magnetic field was quite exciting. But, most people missed this because they were all focused on the high-temperature superconductors, including [me and my collaborators].

00:29:15 Getting back to Thin Film Co-sputtering – Nanostructure Effects on Ferromagnetism

Shull:

It wasn't until we went back to what we were doing before, in particular, focused on making these particulate magnetic films for recording media, high-density recording media, [that I heard about that magnetoresistance effect. I was focused on how] you could make the ferromagnetic regions down in the nanometer size range, so then you could create many more of these in a certain square area of your hard disk or your tape where you are, magnetically storing the information as ones and zeros, binary coding. If you make that little region or those particles very small that you're storing this information in, then you also need to have a more sensitive device to detect these magnetizations of ups and downs or rights and lefts, depending upon how you're magnetizing those local regions. And this discovery of this so-called giant magnetoresistance effect, by Fert and Grünberg independently, showed a direction for making a very sensitive detector because that magnetoresistance would drop relatively quickly as you apply the magnetic field to it.

You could also use that [magnetoresistance] signature to tell you in which direction the magnetization was directed on this layered material because it was so sensitive. As a consequence, basically, people started to work on making [the read heads on hard disks] with a magnetoresistance detector. Now, you could read

these smaller ferromagnetic regions on the tape or hard disk. Well, that required, as I've mentioned, a very nanostructured region [of the type I was investigating how to make]. And, about the same time, there was also a nice paper by another scientist in Germany, Giselher Herzer, who showed that, with very fine particulate material, or materials which had grain sizes in them that were in the nanometer regime, that you could get very soft ferromagnetic character in them. Now, that's quite unusual. Normally, as you'll make things with a grain size smaller and smaller, the grain boundaries in the material impede the motion of domain walls, and, therefore, the material gets what we call magnetically harder.

As you get smaller and smaller grain size, you have more and more grain boundaries to do this domain pinning. Well, Herzer showed that if you can make your crystalline grains very small, now into the nanometer regime, then, in fact, the magnetic coercivity, that is the signature of whether a material is soft or hard, would drop. You want a very low coercivity for a nice soft ferromagnet like one would use for a transformer core. And, [Herzer showed the coercivity would] drop very quickly as a function of grain size, below a critical value. So, this opened up a new direction for making very good ferromagnets for things like transformer cores or that sort of thing. And, [it is exactly in transformer cores] where there is an awful lot of energy wasted throughout the world. So, this was another exciting area where, again, a nanometer size is required, in this case, grain size.

Then, around the same time, I guess it was 1991, there was also another important paper published, a theoretical paper by a couple scientists also in Germany, E.F. Kneller, and R. Hawig. They showed that one way to make a better hard ferromagnet, that is, make the coercivities larger, was to use something they coined as an exchange spring type of material, where you take a very hard ferromagnet, something like a rare earth-transition metal magnet so that you have some very big coercivity, and sandwich in the middle of that a very thin portion of a very soft ferromagnet. So, again, create a layered material with the thicknesses of these layers, again, being very thin so that the magnetic exchange interaction between spins in one type of material are interacting strongly with those in the other type of material.

Then, you could get actually materials with a larger coercivity and a higher saturation magnetization than you would if you just had a regular single hard ferromagnet. And, again, this pointed a direction for material development to make much better hard ferromagnets, again requiring a nanometer kind of structure. So, this got me kind of involved in doing more work in this nanometer regime. I started organizing lots of conferences in this area, primarily at the TMS meetings but also at numerous other meetings with and without the Federation of European Materials Society meetings. This brought me into contact with other people in other disciplines, other than the magnetic regime, where it became obvious that there were interesting new properties of other types, including optical, thermal, and structural in materials which have this nanometer dimensionality built into the structure.

00:35:46 Nanotechnology Revolution – A Leading Scientist in the US Research Initiative

Willard:

So, it sounds a lot like the early days of what became nanoscience and nanotechnology. I mean, this is the early 90s, and people weren't talking about that too much until maybe the mid-90s as its own kind of area. I know that you got involved in what became the kind of build-up to the national nanotechnology initiative. Is this the beginning of that?

Shull:

Exactly. So, after I got involved in organizing all these meetings and meeting all these different people from

different disciplines of my own, it became obvious to me that this was an area of materials science that really had not been explored in the past. First of all, because people didn't really have the capabilities to controllably make these sorts of materials until recently. But, now, that capability was available for all the scientists all around the world. In fact, the *Acta Materialia* journal recognized this as well. And so, in 1990, they held this meeting on the topic, what was called The Conference on Ultra-fine Microstructure Materials. This was in Atlantic City, and it was a relatively small meeting. But, it brought together all these people from these very disparate fields to interact and to talk about the unusual things that were being discovered.

That was really, I would say, the beginning of this whole unified activity on materials with some nanometer-scale structure because that early meeting then prompted a sequel meeting, which happened a couple years later. That meeting was held in Cancun, Mexico, organized by a scientist Miguel Yacaman, who subsequently became the head of the Mexican National Science Foundation later on. At any rate, Miguel organized the sequel meeting, and that started a whole series of international conferences on nanostructured materials, as we now called them. That is, materials that have some nanometer dimensionality or sizes in their structure. Well, that then prompted some of the government funding agencies in the US to be thinking, well, maybe we should be putting some money into supporting this sort of activity, especially when some of us, through our connections with people in these agencies, indicated that much of this technology is being developed outside of the US.

And unless the US starts putting a fair amount of money into exploring this area, the US was going to lose out on the technology and its applications. It was more the latter aspect that really made an impact on these funding agencies. And so, Mihail (Mike) Roco, at the National Science Foundation, whom I had met in one of my organizations of meetings, was talking to me one day about this, or vice versa, and suggested that we try to put together a major funding initiative in this area. So, I helped him form this committee called the Inter-agency Working Group on Nanotechnology (IWGN), which became a sub-committee of the Office of Science and Technology Policy out of the White House Committee on Technology. And basically, using the people I had come across in all these different agencies that have some interest in this area, I suggested a number of names to Mike Roco, especially DOE people and people of the defense establishment. These names were added to many others Mike already knew of, including people from additional NSF divisions, and we put together this committee and started meeting.

The first thing we decided to do was to get some money to form a research exploratory panel to assess the state of knowledge in this whole area around the world. And so, we did that. We had Dick Siegel, from RPI, as one of the chairs of this panel. Evelyn Hu from the University of California at Santa Barbara, who is in the electronics area, was a co-chair. For the next couple of years, they were going around to different countries, holding meetings with scientists who are doing work in these areas, and laying the groundwork for putting together an initiative.

After this review was performed, then those of us on this IWGN committee then began writing up a major initiative that was submitted to Congress in 1999, near the end of it. President Clinton, at that time, saw its potential and backed it, showing his initial backing with an announcement at Caltech in January of 2000. Then, of course, much of our behind-the-scenes activity was also devoted towards selling this whole idea to the members of Congress. Now, I can't sell it to them because that is against the law for a federal employee to do that, but I could provide information to them. So, when they would come to me, I could provide, or help put together, the reports that others were supplying to members of Congress.

There was a very important person, Tom Kalil, on this committee. He was a consultant for the White House; he knew the political scene quite well. Mike Roco was also particularly important as the chairman of this

committee because he really knew who to go to at other agencies and selling NSF on it. And, in addition to Tom Kalil, it probably would not have gone as well as it did. For example, Tom came up with the important idea that we needed to have several grand challenges. What are we going to solve with this whole focus? So, we came up with these 10 grand challenges, which could excite the imagination of the general public and, thereby, members of Congress to vote for this. And so, indeed, the initiative did go through in 2000, and it's still continuing to this day. An interesting thing about this whole activity was that it was very frustrating at times because it was either on at one time and then it was off. And then, it was on again, and then it was off again, depending upon what sort of hearing was being held, who was backing and who was not.

But, there was one interesting thing I never even thought about during this process. I probably should have if I was smart enough. That was after the announcement had come out from the US government that it was putting a lot of money behind researching nanotechnology. Outside the US, a lot of other countries subsequently did the same thing. The reason was these other countries are always all looking at the US. If they see the US put a lot of backing and money into one area, they figure it must be a good area. And so therefore, they put together their own initiatives within their own countries to support research in their own countries so they don't lose out later on. So much more money was put into this whole area around the world than was put into it in the US, National Nanotechnology Initiative (NNI).

So, that was kind of an interesting finding that, as I say, I didn't know before. This was how the initiative got started. I basically told Mike Roco that my position on this committee was not because I was knowledgeable in the political scene but I would be the practicing scientist to bounce things off of to assess whether something is a viable thought or not. That was kind of the role I played.

Willard:

All the way through the whole process, you were playing that role?

Shull:

Yes, I served on the committee in that capacity all the way through the initiative process and then for a number of years after the first initiative. It was renewed several more times up until now. So, I kind of bowed out of this committee probably in 2005. Actually, it was in 2010, probably when I stopped [participating because it was taking too much of my time, and my sanity check was no longer really needed. The committee] would hold meetings in the old executive office building pretty much every month, once a month. And, all of us would get together and decide what's the next step, et cetera. And then, we began assigning, putting together focus groups, each requiring additional work. Obviously, being an employee at NIST, where our focus is on metrology or measuring things, I was very active in the committee on establishing facilities around the US that could address and make measurements on materials with this size scale.

00:47:13 Enhancing the Magnetocaloric Effect by Utilizing Superparamagnetism

Willard:

Yeah. You know it's right around this time. So, you probably don't remember this, but I remember the first time I met you, Bob. It was during this kind of mid-to late-90s period. You came over to the University of Pittsburgh, and you gave a seminar.

Shull:

I remember that.

Willard:

I attended that seminar, and the topic of that seminar was nanoscience and nanotechnology. Some of the things you just talked about now were topics in there. I was working on that for my graduate work. But you also had a connection to the magnetocaloric effect in there. I think it was right around that time that you were starting to work on something that I had never heard of before. I'm not sure if you're ready to talk about this yet, but I know this is a topic that you worked on well into the 21st century. How did you get started on that? What was the connection to what you were doing before, and are you ready to talk about that?

Shull:

Certainly, I remember giving that talk, and I hope I didn't yell at you for creating a scene at the lecture. At any rate, around the same time that I'm helping put together this national initiative, as I mentioned, I'm still very active in my own research, specifically magnetic properties. That, of course, being my expertise, I was looking at how they could be affected by this nanometer structure. It just so happened that I was talking to another scientist at NIST one day. Actually, he was more of an administrator, and he had just joined one of the other divisions. And, he had been working at the Oak Ridge National Laboratory beforehand and had left there, obviously, and came to NIST. I was talking to him about what sort of activities were going on there, and he, knowing that I was a magnetics person, mentioned this work on magnetic cooling.

I had not heard about this topic before. So, this would have been probably around 1990 time period. I started thinking about that, did a little bit of reading about the magnetocaloric effect upon which magnetic cooling was based. And, I realized that, well, up to that time, there were two different types of materials people were looking into, researching, for obtaining large magnetocaloric effects in order to have the large cooling capabilities. Those two types of materials were ferromagnets near their Curie temperature and paramagnets, especially for very low-temperature cooling. This was where it was already being applied for low-temperature cooling materials in order to get to the lowest temperature possible. That was the whole idea of adiabatic demagnetization for which a Nobel Prize was awarded back in the 1940s, I think, where you cooled down paramagnetic materials as cool as you can.

You get it down to liquid helium temperature and then pump on the liquid helium and go down to about 1.6 degrees Kelvin. In order to get down even further in temperature, then what you would do is do this cooling in a magnetic field in the presence of one of these paramagnetic salts, in this case. Then you take the field off, and that would cause it to cool down into the millidegree temperature regime. I realized that if you looked at some other types of magnetic materials, specifically materials which are called superparamagnetic, then you actually get some advantage in this cooling capability at low magnetic fields. The other problem, it always seemed to me, that was problematic with this technology is you'd have to use a very large magnet. Typically, a big superconducting magnet would be required to get a large enough field to effect significant cooling capabilities. So, to become a viable technology, you'd need to reduce the field somehow.

Willard:

Can you describe what a superparamagnetic material is? Is it a super magnet?

Shull:

No, [it is not a super magnet]. A superparamagnetic material is a material kind of like what I was working on making at this time. These composites of materials where you have regions of ferromagnetic clusters that are dispersed in your material, and they're independent of each other. But, within any one magnetic cluster, all the magnetic spins are all lined up in one direction. So, now you'd come around with a big magnetic field from outside, and you can rotate the direction of magnetization of these clusters to try to align with the field. And, since the magnetic moment of this cluster is the magnetic moment on a single atom times the number of such atoms [in the cluster,] the magnetic moment is very much larger than that of a single spin.

Willard:

How big are these clusters, Bob?

Shull:

They could be hundreds to thousands of spins. So, they can be quite large, depending upon the size of the little cluster region you make. But when you take a magnetic field off of this sort of system, then the spins of these big clusters, the overall total spins, randomize in random directions, just like a paramagnet does with individual atomic spins with these superparamagnets. You have these clusters of spins, which orient at random with no magnetic field around them. And so, there's no remaining magnetization in this material when the field is off. But, when you apply a field, you'll get much larger magnetization change because so many of these spins from the clusters reorient right away. So, I put together a theory [with Lydon Swartzendruber and Larry Bennett] that you could get this very much what we called enhanced magnetocaloric effect in a nanocomposite because there are nanometer-size clusters in kind of a sea of material, which was not ferromagnetic at all.

It showed that for certain low fields in particular, indeed, the magnetocaloric effect you would get from such a system was much larger than that of a normal paramagnetic material. Then, we subsequently showed this [experimentally] in a garnet, specifically gadolinium gallium garnet. And, by the way, gadolinium gallium garnet was one of the best magnetic refrigerants at that time. It's a paramagnetic material, the best refrigerant from about 15 degrees Kelvin and down to lower temperatures. That's why we chose this material. We then measured it directly by measuring how much [temperature change occurred for a 1 tesla field change at different ambient temperatures using] a thermocouple embedded in a sample, going back to my days at Caltech, where I was designing a system for measuring heat capacity. It's the same sort of system. We also then measured it by magnetization measurements and showed that [the two measurements] were consistent with each other.

And showed, in fact, if you took a little bit of iron, this is our contribution; we put a little bit of iron into this gadolinium gallium garnet. Then the iron magnetic spins would couple to neighboring gadolinium spins so that, if you change the orientation of one of those gadolinium spins, the iron atom would feel a torque rotating it. And that would exert another force on another neighboring gadolinium spin, causing it to also rotate with it, thereby forming a magnetic cluster. We showed that indeed made the material superparamagnetic, and we got this enhanced magnetocaloric effect in the system; that kind of opened up a new area. So, this was in the early 1990s, the same time period I was involved in the NNI activity.

00:57:38 Magnetic Field Induced Crystal Structure Changes and the Magnetocaloric Effect

Shull:

Around 1997, I think 1998, there was a paper published by the people at Ames, Iowa, Ames Research lab, showing what they call a giant magnetocaloric effect in a gadolinium germanium silicide. One of the things we showed right away is this material gets its large effect because it has a field-assisted crystallographic change. So, if you apply a magnetic field to it, it causes the crystal structure to change. And, when it changes, its magnetic character changes to become more ferromagnetic at that point. And so, you've got a big enhanced effect here. What we then subsequently considered, again going back to our earlier activity, what happens if we put a little bit of iron into it, just like we had done with the garnet materials.

One thing we found is that, in fact, the peak effect [is reduced]. At this time, that was the figure of merit people were using to determine how much entropy you get by aligning the spins. This magnetocaloric effect comes about from basically aligning magnetic spins in a particular field, thereby ordering them in some order. Whereas beforehand, they were somewhat randomly oriented, and there was no magnetic order. So, if you could order them, then this means that you can reduce their entropy. That is the spin entropy in the system. Entropy, of course, being a measure of disorder. And so, if you can order these spins, then you can decrease their entropy, and that can be converted to a temperature change depending upon the heat capacity of that material. This amount of entropy varies with the particular temperature over which the material happens to be at.

So, what we found is that for this gadolinium germanium silicide with a little bit of iron in it, that peak entropy term actually dropped by a factor of about [two]-thirds that if you had no iron in it at all. So, using the existing metric for deciding whether this material is good or not, you would automatically think, "Well, this new material with the iron is terrible. You don't want to use this at all." But, at the same time as it dropped, this peak term, it also had no magnetic hysteresis in it. If you went through a field cycle, which is what you're going to do in any magnetic refrigerator, you have to go through a field cycle of the refrigerator. If that material has some sort of magnetic hysteresis, that's the amount of energy it costs to go through this field cycle.

So, you want that to be as small as possible. Well, it just so happens that the gadolinium germanium silicide has very large hysteretic effects, but with the iron, one had very small hysteresis effects. So, now you take the peak entropy and integrate that over the total temperature range of the refrigerator, coming up with something called the refrigerant capacity. If you then subtract out of that the amount of work to go through a field cycle, then we come up with this modified refrigerant capacity because you're taking out the major magnetic work. We compared that for the two materials and find that the material with the iron added is so much better, by a factor of two, over this gadolinium germanium silicide with no iron. At any rate, those are the two major things I think we did in that area.

I think we came out with our paper on that around the beginning part of the 2000s, maybe 2003 or 2004 - somewhere around there. Since then, we've been looking at working with some other materials, and nothing startling yet has come out of that. There are some fundamental questions in this whole arena that still need addressing. But, I must admit, during this same time, we were also working on other projects, which turned out, in some respects, as exciting as this magnetic cooling area that occupied my thinking.

01:02:58 Imaging Magnetic Flux Penetration – Developing the MOIF Technique

Shull:

Again, in the 1990s, we had this guest scientist come to our lab from Russia. Now, this is during the time [just after] the USSR was broken up, and there were all these scientists in Russia who weren't getting paid. There was no money to support their activities. And so this one scientist, Valerian Nikitenko, came to our laboratory and had this idea for imaging magnetic flux penetration through superconductors. Of course, this is that same time period when the high TC superconductor activity was going on, although it was starting to die away a little bit. The idea was to use a thin film of a material that would be put on top of the sample, and then he would apply a magnetic field perpendicular to that sample. This film had the property, on top, that it was in-plane magnetized. Now, when the field went through the sample, say one of these high-temperature superconductors, [it would go] through the film on top as long as it was not superconducting below. If it was superconducting and the flux gets expelled outside of it, it would not come right through [the film].

So if [the flux] came through [the film], then the magnetization in that film would rotate perpendicularly. And now, if you came down with a light beam from the top that was polarized, so its electric field vector is in one direction, then it would see this perpendicular magnetization region if flux actually penetrated the superconductor underneath it, and it would rotate the polarization of this light beam. So, when it got reflected back up to your microscope or eye, you could see a color change in that local region. Therefore, basically, it would become a really nice technique for imaging, getting images of flux lines which have penetrated the superconductor. It was very useful in showing how flux came in, and where it got stuck, and how it subsequently came out of the sample. But, when [Valerian] came to me, a little bit after that initial work, he suggested that we might want to modify that technique and try to put this thin film on a ferromagnet, recognizing that especially thin film ferromagnets will tend to be magnetized in the plane of that film.

If any of those magnetic spins in that sample rotated upwards, and if you had this imaging film on top, you could detect that. That was this idea of Valerian Nikitenko, and we decided we would put together a system and test it. We did that, and it turned out to work wonderfully. One place where you get magnet spins coming out of the sample plane is when you have a domain wall in your sample. So, half of it is magnetized this way; the other half of it is magnetized this other way, still being in the plane. But, at the boundary, you'll get some magnetic flux lines coming out of the plane. And, if you had this imaging film on top, then it would rotate its magnetization vector perpendicular, and you'll be able to see it and get actual pictures of it. We realized that this was a pretty important technique for detecting domain walls and seeing how they move; we could [also actually] measure their dynamics. So, we applied it to some interesting nanomaterials.

01:09:25 Effect of Dislocations in the Antiferromagnet on the Neighboring Spins in the Ferromagnet

Specifically, materials which were kind of like the materials I did my PhD thesis work on, where we're looking at, if you recall, these mictomagnets, which were a clustered material, but where there are both ferromagnetic and antiferromagnetic interactions working on the individual spins. Well, one of the signatures of that material is you get a hysteresis loop which is displaced along the field axis. And, in fact, if you just take a thin film of a ferromagnet and put an antiferromagnet on top or vice versa, then you'll get the same sort of magnetic hysteresis loop. If you cool the material down from above the Néel point of the antiferromagnet to below it and then measure it down there, you'll see this displaced hysteresis loop.

So, my thought was, well, let's use this imaging technique of ours to explore that. And, in fact, that's what we did. We got this nice thin film of a nickel oxide, an antiferromagnet, with Permalloy, a nice ferromagnet at room temperature on top of it, and we did some imaging [after heating it and cooling it in a magnetic field to room temperature]. First of all, we measured the hysteresis loop of it, and sure enough, it's nicely

displaced. We showed it was, in fact, in going from positive fields towards the side of negative fields, shifted towards negative fields. And the switching from positive magnetization to negative magnetization occurred [first] by the nucleation of local regions, which were magnetized opposite most of the material, which is a normal method for a reversal. But, they would nucleate on the edges of the sample where there are surface perturbations. That's the normal process, and they propagate through the rest of the material.

Shull:

But, what was really interesting, or unusual, was when you came back with a field. When you reduced it from the negative direction to zero and then slightly positive, then indeed you got nucleation of reversed direction domains. But, they were located not on the edge of the sample, but they were located inside the sample, kind of along vertical and 45-degree lines. Those sites were along screw dislocations and edge dislocations [respectively] in the material. And, that's where we realized that it's so much better to be lucky than smart because these samples [had been prepared at] the Lawrence Livermore National Laboratory on magnesium oxide single crystals [which had unknowingly edge and screw dislocations introduced into them].

In order to get more samples for different types of measurements, [our collaborator, Alison Chaiken], took this magnesium oxide before doing the deposition and cleaved it into smaller pieces. Well, interestingly enough, when you cleave the magnesium oxide, you actually put dislocations into the magnesium oxide, and that's what we were seeing. So, when she then grew these films on top, the dislocations in the magnesium oxide propagated into the nickel oxide first and then into the Permalloy on top. [Imagine] a dislocation in this antiferromagnet: initially, the spins are alternating in the antiferromagnet. After you put a dislocation into it, you're going to have spins along that dislocation which are now lined up parallel. Now, imagine the effect of that on a neighboring ferromagnetic spin in the ferromagnet on top.

It doesn't know which way to align. So, it's easiest for those spins to begin turning [in response to the exchange field created when they originally rotated away from their equilibrium orientations by the external field]. That basically [enabled us to understand] one of the big uncertainties in this whole area: how come it took so many smaller fields to get this reversal in such systems than had been predicted. It's all because of dislocations, which people were not controlling in their material. And that's normal because it's very hard to control them. So, at any rate, that result showed the real power of this technique.

And then, we applied it to an exchange spring of the type I mentioned that Kneller and Hawig had predicted for getting very good hard ferromagnets. And, here, we showed that, in a film, in this particular case, it was samarium cobalt and iron [composite], as I recall, that had been prepared at Argonne Laboratory by Sam Bader. We had a thin film of samarium cobalt and then a thin layer of iron on top of it. We looked at the magnetization reversal [using] this imaging tool which is called the magneto-optic indicator film technique, or MOIF, M-O-I-F, by its acronym.

And, what we showed is that, first of all, when we tried imaging it, we didn't see anything. We couldn't see any domains. That's because the domain walls are not on the top; they're on the edges because you will have basically a hard ferromagnet and a soft ferromagnet [as prepared on top of each other]. The domain wall is between them. So, we wouldn't see anything. In order to decide how reversal occurred, we drilled a little hole in our sample, and, from the inside edge of that hole, one half of it would be white-colored, and the other half would be dark-colored. That mimics the direction of magnetization in the region surrounding that hole. We, thereby, showed that if the field was slightly off the easy axis of magnetization, the spins in the soft ferromagnet rotated in one direction. And, if the field was slightly off in the other direction for the easy axis, the spins rotated in the opposite direction, thereby confirming the theory directly. So, that has

turned out to be a very powerful tool, and we've applied it to lots of other things since then; it's an extremely valuable technique.

Willard:

Very interesting, Bob. This is a great tool, not only the science that's coming out of it, but the tool is important, too.

Shull:

Exactly, exactly.

PART 3

00:00:00 Creating a Method to Switch Magnetic Poles and Provide 3D Storage

Willard:

Until recently, the way magnetic materials have been switched was when you changed the north and south pole orientation, you apply a magnetic field. I understand that recently you've been working on a method to switch magnetic poles without using a magnetic field as traditional. Could you tell us something about that, Bob?

Shull:

Certainly, yes. That's really been - well, all these have been - rather fun activities; but this, in particular, was rather unexpected. So, one of the areas that have high interest right now, certainly in the recording industry, is how to continue to get higher and higher storage disks. One problem with doing that is, as you try to locally magnetize your disk, the local regions want to stay perpendicular to it; that would be your one in binary coding [when magnetized in one direction] or zero in binary coding [if magnetized oppositely]. In order to do that switching, you had to apply a magnetic field in the direction you wanted to switch it to. So, if that local region's magnetized up, and you wanted to switch it down, you'd have to apply a [negative] field perpendicular, which would cause it to switch [down].

Well, imagine the effect of that magnetic field on the region below where you have it magnetized. Well, it's going to cause those regions, if it's magnetizable, also to switch. And, that's one of the reasons you can't, or up until recently, you could not get three-dimensional storage in your material. It was all two-dimensional. It was limited to putting [stored] things on the plane of your hard disk or tape or whatever [medium you were using], but you couldn't put things underneath it easily that you could cause to switch with a magnetic field up and down, because if you use the magnetic field from outside, it would switch both the top and the bottom [regions at] the same time.

So, there's always been an interest in somehow being able to switch this magnetization from up to down without using a magnetic field. One way that people have come up with in recent times was to use the fact that there's a spin orbit coupling. That is, there's a coupling between the trajectory an electron takes and the magnetic spin on that electron. It's what's responsible for the Hall effect; for instance, it creates voltages.

If you apply a magnetic field perpendicular to the direction of an electric current, then you can impose an

electric potential on the other mutually orthogonal direction. So, that comes about from this spin orbit coupling. One thought was to try to use that somehow to switch the magnetic spins in the material up to down. And, it's been largely unsuccessful by taking advantage of this spin orbit coupling. But, what was predicted here a while back, is [that things change] if you had an anisotropy in your material. If you, for instance, had a heavy metal on top of your ferromagnet; the heavy metal has lots of spin orbit coupling occurring in it and whereas less so in the ferromagnet. Well, if you put an electric current along this sandwich, or not sandwich but this bi-layer, then the coupling of [the moving current] electrons at the interface could exert some sort of torque on the magnetization of the [neighboring] spins in the ferromagnet.

In fact, people found that if you apply the magnetic field in the same direction as the electron current, in some cases, if you pulse it, you could get switching if you had the material perpendicularly magnetized from up to down. This switching occurs because with a field this way, then the electron current spins, which are up and going to precess around this field. And, if you pulse only so long enough that the spin is now down when you take that field off, it will stay there. That was the idea, but that still requires a field.

00:05:09 Spin Orbit Coupling – Demonstrated in My Father's PhD Thesis

Shull:

The effectiveness of this spin orbit coupling is proportional to what is known as the spin Hall angle. This is a quantum mechanical effect, but it tells you which way spins are going to be moving. So, if you actually have an electron current going [in the plane of the film] down through [both] this ferromagnet and a heavy metal on top, as I described before, then what happens perpendicular to the direction of motion of the electron and the direction of spin of the electron? The coupling will cause a motion of those electrons in that third mutually perpendicular direction. So, if the current is going along the plane of this film, then electrons, which have spins in the plane but perpendicular to the direction of motion, will cause electrons with that spin state to actually move up or down, depending upon whether the current is going this way or the other way.

So, it causes a large number of electrons to move up in this sample up to the boundary separating the heavy metal from the ferromagnet. In fact, if you have a large enough number of spins in this one direction, it can cause a large torque on the ferromagnetic spins and cause them to switch over and similarly back. Well, if you just had a single heavy metal, it apparently doesn't work really well because you can bring the spins down, but you can't continue to reverse them back to the other direction. Chia-Ling Chien, from Johns Hopkins University, and a couple of his students came to us and wanted to know if we could use our imaging, our domain imaging tool, to look for perpendicular magnetization; to see if we get switching definitively as we put an electric current through a sample. So, of course, we felt this would be a good tool to use because then you can tell definitively whether it switched from up or down and vice versa.

They did something a little different, however. They made a sample which had a nice heavy metal like platinum on top of the ferromagnet. In this case, the ferromagnet was an alloy of cobalt, iron, and boron, which is known for the thickness of it to give perpendicularly magnetized material. By putting this platinum or heavy metal on top, you would get this spin orbit coupling in scattering. It would cause electrons [in the current] with a certain spin state to migrate up and the other opposite [spin electrons] going down, thereby causing a torque [on the ferromagnet spins]. But then, they took a second heavy metal, Tungsten, which does the same thing. It causes some current electrons to move up and some electrons to move down, but it causes those electrons to move in opposite directions than the platinum does.

The idea was, if you set up basically a spin current up with a similar spin current going down in these two heavy metals, they should cancel each other, and we would not see any switching at all. But, it will be of use to examine if there's any effect on the magnetization of that cobalt iron boron. So, that's what we set out to do. And, lo and behold, we found that, if we set up that sort of dual spin current, then, in fact, we can cause switching. We have a nice video [of our magneto-optic imaging], which we may show later on, which shows the switching: where it goes from a dark region to a brighter region when we pulse the current in one direction. And then, when we come back and pulse it in the opposite direction, it changes from a very light region to a dark region, showing that we've gotten magnetization reversal from up to down and then back up at will.

This is without using any magnetic fields at all. So, this was a rather exciting finding. I have to confess; we still don't really understand why it's happening. But it's a real effect, and we can control this at will. So, there are a lot of people now looking further at this effect. I actually have reduced my activity in that particular area, primarily because just after we published our results, I decided that it was time actually for me to retire officially from government service. And so, I don't really have as much access to the equipment as I used to have. But, it's really an exciting area that I fully expect we will see some outcomes in commercial products in the future.

What's also kind of interesting about this whole area is this whole area of spin orbit coupling is something that my father showed in his PhD thesis actually existed. Up until then, there had been theories that there was an interaction between the spin of an electron and the trajectory it took or its orbit, but nobody had really shown outside of experimental error that it actually existed. That was my father's PhD thesis. They didn't call it spin orbit coupling in those days, but that's exactly what it was. It showed that way outside experimental error that such a phenomenon exists, never thinking about how it was going to be applied to present-day electronics. So, an interesting insight.

Willard:

You've been doing cutting-edge work your whole entire career. This is really right at the edge where we still don't understand what the effect is, but the experiment shows that something is there to look at. So, just amazing.

Shull:

I'm going to leave it up to people, smarter people like yourself, Matt, and others, to come up with explanations of some of these things. I've always thought it's better to be lucky than really smart.

00:12:28 Sharing My Passion for the Advancement of Science – Involvement in TMS

Willard:

I think that's a good time to change topics here. Your technical career has certainly shown your passion for the advancement of science. But, I know that you've had other passions, and two of these are really not doing science yourself; it's service through professional societies and science and engineering education. And, these were kind of sharing your experience with others and trying to make a difference in other ways. Why don't we start with the first one? When did you get involved in TMS, which I know is an organization you've spent a lot of time interacting with over the years? And, why was it important when you started out doing service with TMS that you did at that time?

Shull:

I got involved or became a member of TMS when I was actually an undergraduate student at MIT. At that time, you could join both TMS as well as the American Society of Metals for the same student fee. I did so partially because it was a way for me to meet my professors as well as other students because the department at MIT would hold these informal gatherings - typically on a Friday evening or late afternoon - in the Foundry, where they had big furnaces for melting and casting things. A number of the faculty would be there, as well as graduate students of them, in addition to the undergraduates. We were all invited, and it was a good way to kind of meet other people - to talk to your professors as well as other members of the faculty informally without worrying about a problem set or something scholastic.

So, that's why I joined. But, I really didn't know much about the organizations at that time. Then, later on [I stayed involved] when I went to the University of Illinois in Champaign-Urbana (UIUC), again in the Materials Science Department. There it was actually called the Metallurgy and Mining Department because they had still a mining activity that was a very large part of the department many, many years beforehand, but it had gotten much smaller by then. I again stayed a member of this student chapter they had there, although it was more of a materials student chapter there, specifically ASM or TMS, at that time. For a very small fee as a student, I was a member, but I really didn't think much of participation in the society activities themselves other than as a way to get to meet other faculty members and students at UIUC.

It wasn't until I had come back east and joined the National Bureau of Standards that I started looking at it more seriously. Partially because one of the members of the group that I helped form, Larry Bennett, said, "You really should get active in your professional society." Now, Larry was a member of the American Physical Society, primarily, and he knew from his own activities as a student how beneficial joining and becoming active in your professional society was in terms of primarily networking. So, I did that. I kind of looked at the different committees in TMS, and it seemed to me that the one that fit me best, on the basis of my interest, was the Chemistry and Physics of Materials Committee. This was a committee that transcends divisions in the TMS structure.

It was part of both the Structural Materials Division (SMD) and also the Electronic, Magnetic, and Photonic Materials Division (EMPMD), as it was called those days. Now, it's called the Functional Materials Division (FMD). But, at any rate, this committee transcended the TMS structure, and that made sense to me because it had applications to different TMS divisions. So, I applied to the chairman of this committee about joining. After they looked through my CV and interest statement, they voted to have me join them. That turned out to be very beneficial because it, again, brought me into contact with a different community of people than I had met beforehand, many of whom were working in areas I was not working in. That enabled me to rely upon my previous education at MIT and the University of Illinois in terms of following what they were doing and recognizing the significance of what they were saying and accomplishing.

00:18:50 Organizing Symposia – Bringing Science Communities Together

Shull:

I began to organize symposia and, through doing that, I'd meet more people [in my area of activity]. In fact, my advice to students or young professionals, in general, is indeed to get involved with your professional society. Whether it's TMS, ACoRS, ASM, IEEE, APS, or whatever, get involved with that society as a way of networking and meeting other people. What I found is once I started to organize symposia, I would send out invitations to some of the people who had done some significant work that I had read about in papers from them beforehand and invite them to come to the symposia that I was putting together. When they

would come to the symposia, I would have a good topic to break the ice and talk to them about, and that one thing led to the next. But getting to know some of these people in more depth and vice versa was enabled. While you're meeting them and hearing what they're doing, I realized they're also hearing about what you're doing!

Then, of course, they will talk about your work to other people. You'll talk about their work to other people, and it's a good way to spread the information on the experimental results of yours, as well as that of others. I also then got involved in being the publication representative for the EMPMD division. Then, I got involved in the Programming Committee. The Program Committee, in particular, I think, is one of the most important committees in TMS because you're involved in putting together the program of each of the annual meetings. I always thought that was really a lot of fun and, of course, a key output of being part of a professional society because it gives you this venue for presenting your results and vice versa.

I recall sitting down for lunch once with the present chairman of the EMPMD division. That was Bruce Wessels at the time. This was probably in the middle 90s. And, also another fellow, Darrel Frear, who was from Sandia, I think, at that time, although he went into the semiconductor industry around that same period as well. Both of them were saying, "You know, these symposia you're putting together are pretty interesting. You should think about taking a higher office in TMS", an administrative thing. I was not so interested at this point because I had spent my whole career until I couldn't say no, staying out of management. I've always felt if I got into management, then that would be curtains for my research, and that was what I was most interested in.

I, first of all, told Bruce and Darrel that's not really of high interest to me. And, they said, "Well, you know, people are looking at you and thinking you would be a good person to fill some of these positions." I guess that gave me confidence that perhaps I could be useful in that regard. So, later on, when they came back to me and asked me to run for election as the Division chair a few years later, then I was a little bit more receptive. And, I said, "Okay, that will be fine." As a consequence, I became the Division Chair for three years. In that capacity, I was sitting on the Board of Directors (BOD) of TMS.

That was interesting from the completely different viewpoint that there are people from all the divisions on the BOD. These are real experts in their particular fields. We had to work together for some common purpose. Sometimes there wasn't an apparent common purpose, but we could always find it. At the same time, we would think about what is best for TMS in this activity. So, that was a lot of fun and important. I'm doing this simultaneous with my normal research activity and then with my involvement with the [National Nanotechnology Initiative] (NNI) for a while; in addition to my outreach-type activities, we'll get to a later. My wife told me many times, "You don't know how to say no. You keep on taking on these new activities, but you've got to give up some sooner or later." Many years later, I did take her advice, and I did start to give up a few things. But I do still have that problem.

00:24:52 President of TMS – A Successful Leadership Experience

Willard:

You eventually became president of TMS, and that must be a really big job. What was that like?

Shull:

I became voted as a nominee and then voted in as President of the TMS in 2007. I was voted in as vice president the year before then. I actually sat on the Board of Directors a year before that. In order for me

to serve in that sort of position, being a government employee, I had to get permission from my management to do that. Before I agreed, [I queried] other people previously nominated for that position. They all told me, especially previous presidents, without exception, "Oh, it's a piece of cake. There's not any more work involved in this at all." Well, they lied to me, and, in fact, I think I probably lied to others after my presidency because if you told the truth, they would not accept the nomination, I'm convinced.

As a government employee at that time, I was not allowed to serve in a leadership position, like president of the society, during government time. With one exception, I could be in an administrative position in the American [National] Standards [Institute (ANSI) as it is] a metrology entity. I would be allowed to serve in that position on government time, but any other society I belong to, especially in the administrative position, I could not. So, I talked to my [NIST] administrative superiors at this point and told them I would be happy to [serve as a TMS Officer] on personal time. That is, I would take leave anytime I went to a meeting specifically for TMS or was spending time focused only on TMS activities. So, I figured I could work on the TMS presidential duties, perhaps one day a week, and the rest of the time, I would do other things.

Well, things like these presidential duties aren't all coming to you on that one day a week. They're coming at you all the time. So, basically, I would take leave for approximately the same amount of time in a week, but I would be having constantly to pick up a phone and call somebody or receive a call from somebody or answer an email, put together a letter, or whatever else, quite a bit of time. And then, I could go back to my research. So, I managed to do that. But, [before embarking on this venture, I first got] the okay of my management for me to be nominated for this position on the basis that I would use my personal leave for the times I was focusing only on TMS activities. And, in fact, I used up pretty much all of my leave, which I had been accumulating for years beforehand, during that time as TMS President. But it was worthwhile and very enjoyable.

00:29:05 Establishing Interaction Between Societies and Expanding Youth's Science Education

Shull:

When I look back on that period, there are two things that I really look back on as being successes. One is I was really interested in getting more interaction between the various professional societies. So, one of the things that I was pushing, and luckily was successful in was to join into a partnership with the Materials Research Society, MRS, in sponsoring a government fellowship. A fellowship for a member of our societies to spend the year down as an intern on Capitol Hill, working with some senator, or congressperson, or a major committee down there. And, the stipend for supporting this intern will be coming from combined half and half, TMS and MRS.

I was really happy to see this congressional fellowship get started. And secondly, through the establishment of this interaction with the MRS, I was hoping it would blossom out into more and more interactions. I specifically tried to get joint student chapters established between the societies. That, unfortunately, never happened, or it turned out to have more problems than we could solve at that time. So, that never happened. The second thing I got started, and it's only partially [begun, was to get TMS active in outreach activities.] Since I had been very active in stem activities for many years before this time, I realized that if we could partnership a professional society with other organizations around the country which have a focus on educating our youth, we could expand the amount of science education to our nation's youth.

Specifically, I found that the 4-H organization is the educational arm of the agricultural extension service of the U.S. Department of Agriculture. They obviously are focused on education. But, they don't have the expertise in educating youth in the sciences other than animal science and food science. Well, if we could

partner them with a professional, scientific society like TMS, then, in principle, we would be able to help start stem activities throughout the nation since there are 4-H agents in every county of every state of the US. That only worked partially. We got something really going in Maryland between the 4-H organization and the materials chapter at the University of Maryland [College Park] specifically. [I was already part of a 4-H linked stem program in my local area near the University of Maryland, and through the connection with the Materials Science Department students, started a program at UMD]

So indeed, I think that getting involved in a professional society is one thing a person should really do, and the payoff will be wonderful. [In this example, it was improvement of outreach activities.]

Willard:

So, Bob, I really enjoyed the times that I came out and worked with you on the Adventure In Science program, that 4-H education activity, for a whole decade when I was living in the DC area; that was a really great program. What other types of things have you been working on that are kind of outreach activities to try and engage students in science and technology?

00:33:52 Outreach Activities and Basement Organizations – Engaging Students in STEM

Shull:

I've been involved in a lot of outreach activities, probably starting in early 1980. Shortly after arriving in the Washington DC area and joining the National Bureau of Standards (NBS), I noticed that my children -- actually, it was before my children had gotten into school — but I noticed that there was not a large emphasis on science education. By chance, I met up with a fellow named Ralph R. Nash, a retired NASA scientist. In fact, he was a materials scientist, interestingly enough. He went by his middle name, Robert or Bob. And, he had noticed that, with his children, who were in the public school system here in Montgomery County, which was supposedly one of the more advanced public educational areas, his daughter, in particular, was not getting a very good introduction to the sciences.

And so, he started something in his basement. This was in 1973, so well before I got to the area. And, this was the program you were mentioning that you participated in for a number of years, wonderfully, I might add. As an aside, your sessions on magnetism were a favorite of a lot of the students as well as of mine because I knew what you were doing before you did it. You did a wonderful job putting together a nice sequence of educational things for the kids to do. So, in 1973, Bob Nash recognized there was a need to do something outside of the school system. He, therefore, started this program called Adventure In Science, that you alluded to, in his basement on Saturday mornings for local neighborhood children. I think in the first group, there were like eight children.

They would meet in his basement for a couple of hours, and he would go through some sort of scientific activity. Being from NASA, he had a particular focus on astronomy. He would lead hands-on type activities for these children. T was kind of a key element of his. And, I would say it was very foresighted of him to recognize there was a need to develop a program like this so long ago before it was widely recognized that our nation was going to have a problem with a poor scientifically-educated population. After a while, he outgrew his basement [and expanded to other locations.] After reading an article in the local newspaper, - that's how I found out about this program — I called up Bob Nash and talked to him for a while about it. Even though my children were too young at this time to participate in it, I was interested in, perhaps, helping out.

And, of course, he was always looking for volunteers. So, I started to lead some sessions on metals, and I think on friction. Heat conductivity was another topic that I led, initially in his basement. By the time I had arrived in Maryland in 1980, a fellow named Tom Charlton had started a meeting site for this program at NBS, where I was working. So, in fact, I met Tom, and then much of my teaching was actually at the NBS site. Then, I think, like in 1985, Tom got a job elsewhere, up in Connecticut. And so, when he left NBS, and the future of this program on-site at NBS was really up in the air, Bob Nash called the parents of participating children for help.

By this time, my son was involved in the program. So, Bob called a meeting of the parents of the children that were in the program who worked at NBS to see if anyone would be willing to take it over and help lead it. I agreed to do that with three other people, so not by myself. I [ended up taking a lead role in continuing the program after Tom Charlton left and] continued to lead the program up until, I think, maybe, 2000, or maybe it was 2001, or something like that. After 2001, I continued helping lead the NBS program site, but somebody else became the principal person, as I became the President of the whole organization. It was at that time that Bob Nash decided to retire from the area and moved up to Maine to be closer to where his daughter was living at the time.

00:40:11 Leading an Exploratory Program for Our Youth – Applying the Scientific Method

Shull:

And so, this AIS program grew. When I joined it, there were probably 30 to 40 children involved, maybe 50 children total. We started sites at the Hewlett Packard Corporation, Lockheed Martin Corporation, Bechtel Corporation, and IBM at different times. Then, over time, one would be closed down because the company was moving out of the area or the people who were leading the site were leaving the area or whatever. We were constantly moving from one place to another, but we always had a number of different sites running for providing these hands-on activities. These activities would run from October of one year - every Saturday morning pretty much except for the holiday periods - until March the following year. And by 1995, we would typically have 150 to 200 children. Now, for the last ten years, I think we've had well over 300 children involved in the program each year.

So, we've had a lot of children come through the program, and, by and large, they had positive experiences. That was one of the objectives of this program: to try to address children early. So, we would start at age eight and go up to age 14 — therefore, pre-high school. After they got to high school, then generally, there were science-related activities available in the high school for them to get involved in. In some cases, some of them would still stay with us and help act as teachers or as an additional pair of hands. The first thing that we wanted to do was to teach the scientific method to children in general, not for the purpose of them becoming scientists, necessarily, but to enable them to know at least how to answer questions of a scientific nature themselves. They don't need to go to Matt Willard or Bob Shull to give them the answers to the questions they will pose but to give them the tools to come up with the answer or figure out the answer on their own.

But also the second objective was to expose these children to as many different topical areas as possible. Each Saturday morning, the children at the NBS or NIST site, for example, would choose one of maybe five topical areas where there'll be somebody like myself or Matt leading that topical area. They would spend two hours learning basic concepts in those areas. Then the following Saturday, they would be presented with another five different topic areas they could choose from to participate in and learn. So, in the 20-21 weeks that we were working with them each year, children would learn lots of different topical areas with the hope that they would find an area that really excited them: areas they might want to go into much

more depth in the future, learning much, much more about that area, perhaps even ending up working in those areas.

That was the second objective. The third objective was to make these teaching sessions fun by having the children actively participating in them. They were hands-on-type activities, so the children were not passively just seeing and listening to somebody talk. Instead, they were actively doing something, performing some experiment, whatever, while we would be walking around lecturing to them without them realizing. Of course, when they would measure something or come up with some result, we go off on some tangent, "Oh, well, you know what that means?" We'd go off on some tangent, bringing into recognition what they had just done to their real-world of things that they know. Lastly, we wanted to teach them that science is not that hard as long as you know the basic concepts. And then, you can start using the scientific method to probe answering questions that you're presented in the future.

00:44:50 Providing Internships and Laboratory Experience for Undergraduate Students

Shull:

So, that was the whole purpose of the AIS program. And indeed, the local program did join with the 4-H organization in a partnership back in the year 2000 when a local leader of the 4-H organization saw this program we had and realized this is one way they could help expand the topical areas that they would address in educating their participants. We saw the 4-H organization as a way of potentially expanding our local program. That's the major activity I've been involved in since 1980. So, that's what now, almost 40 or 50 years that I've been doing this every Saturday morning for basically six months each year?

And then the second area I got involved in was to provide internships for older children, specifically, those who are now in college as undergraduates, to provide an internship in materials science at NBS, actually NIST by this time when I got it started: 1998.

This came about from the fact that a program called the SURF program, or Student Undergraduate Research Fellowship program, was started at NIST by a scientist in our Physics Laboratory. He had applied for some money from the National Science Foundation for helping support this for two or three years - I guess three years [actually, it was five years]. And, during that time, he would come to me knowing that I, many times, would bring students in from my Adventure In Science program to actually work in my laboratory in the summertime. This fellow from our Physics Laboratory would come to me saying, "Well, you know, I've got a student who wants to do a materials project where he wants to work on materials things." Whereas all the projects they had were physics-based rather than materials-based.

So, he wanted to know if I would be interested in having them work in my laboratory. And I would usually say yes. At a meeting, a local meeting of not TMS but the Materials Research Society, I met a young lady, Terrell Vanderah, who was a ceramicist, at an outreach lecture during lunchtime once. It turned out that she was going to be moving to Maryland, and she was going to be joining the National Institute of Standards and Technology in our ceramics division. So, of course, I became really interested in getting to know her better because she was interested in stem-type things. Therefore, not long after she came to NIST, we decided that we would go to the National Science Foundation ourselves and see if we could get a grant to expand this SURF program beyond just this one laboratory at NIST, making it into a bigger program.

We were successful in getting that grant. We then joined with the Physics Laboratory at NIST and began running this joint SURF program. After a couple years, we began contacting the laboratory directors of the other five laboratories at NIST to see if we could get the program started there. And so, one by one, Terrell

and I, along with the two people who were leading the Physics Laboratory part, managed to bring all the laboratories at NIST onboard to having their own activity, kind of jointly with it. That has become a very successful program. We started out with, I think, ten students in our materials science program that joined the ten students in the Physics Lab program, and which had been started a couple of years before ours, as I mentioned. And now, there's like, I think, 200 students who come to NIST and spend the summer there. It is interesting to hear management now refer to this program as one of their best programs that they have going on at NIST. They say that because it creates all these goodwill ambassadors for NIST who, after the summer there, go out and advertise what a great place NIST is and about the great things that NIST scientists are working on, et. cetera. So, it's been a great public relations activity for NIST, which, of course, nobody thought about initially when this grassroots activity got started.

00:50:28 Father and Son – Making Scientific Advancements

Willard:

Thank you for sharing your stories about the outreach efforts. Quite remarkable, a lot of growth and in a really important area really needed nationwide, but it's good that it's taken root very deeply in Maryland. I wanted to as we get close to the end of the interview, I was hoping that maybe you could share your father's Nobel Prize. I know you have the Nobel Prize someplace because I've seen it before. Do you have it with you now?

Shull:

Yes, I have not lost it, and I actually do have it here. So, I'd be very happy to share it with you.

Willard:

Can you show it to the camera here?

Shull:

Do you want me to show it to everybody else? Oh, I thought you'd just want to see it personally. Okay. Yes, of course. So, here it is. This is the front of the medal. My father received it in physics with Bert Brockhouse for pioneering the area of neutron scattering, specifically. The front of it has the relief of Alfred Nobel, who started this prize. On the back, there are a couple of [Greek] muses, in this case [the Goddess of Nature, Isis, and the shorter Genius of Science figure]. Each discipline, by the way of the Nobel Prize, has a different arrangement on the back of the medal. On the very bottom, my father's name is inscribed along with the year in Roman numerals that he received this prize.

Where there is no relief of these muses, the other areas are quite flat and smooth. Similarly, on the front side, similarly, on these areas where the relief is not present, it's nice and smooth. That's quite different from the Nobel Peace Prize medal; those latter areas are very rough. In fact, that's how you can distinguish between the medals from the Nobel Peace Prize from these medals, which are awarded and handed out in Sweden in the sciences and literature.

Willard:

That's fascinating. Thank you for showing that. I'd like to ask you maybe a more personal question about your relationship with your father. I hope you don't mind me asking. But, I've always wondered if you felt

any pressure to match your father's scientific achievements and maybe get your own Nobel Prize to match the one that you just showed. Did you ever feel that type of pressure?

Shull:

No, I did not. I think it's common for a child of somebody who has achieved some real significant thing for them to feel pressure to match such accomplishments. In my particular case, I did not. The reason was twofold. One is that by the time my father was awarded this prize, it was already 1994. I was already well-established, as we've described in this video before now, in my own fields. Those fields were somewhat linked to the field of my father, in the sense that I did use neutron scattering as a tool for some of my work, but different enough that I wasn't competing or in the same area. So, that was one reason I did not feel so much pressure.

The other thing is that I [was too old to change things. I] figured it took 50 years from the time, [1946], that my father did his very formative work – the same year I was born - to when my father went down Oak Ridge, Tennessee to 1994 when the prize was awarded. So, if it takes 50 years for that prize to be awarded, that means that, for me, I would have had to have already done the significant work well before then.

And so, there was no pressure for me to match it to get my own Nobel Prize. Although I did note that the number of people who received the Nobel Prize since 1900 when they first started awarding it, whose parents also had received the Nobel Prize, although small, was not insignificant. Looking at the proportion there, I noticed that my chances of receiving the prize went from absolute zero beforehand, before 1994, to a whopping two and a half percent afterward. So, maybe there has been an oversight that my name hasn't been announced since 1994, but I'm sure it'll be corrected sometime in the future.

00:57:11 Advice for Young Professionals – Get Involved in Professional Societies

Willard:

I'm glad those odds are on your side, Bob. Do you have further advice for students and young Professionals that you would like to give?

Shull:

Well, I'm not sure if I have any further advice now, but the one thing that I've said before, and I'll reiterate it here in closing, is get involved in your professional society. I've benefited significantly by having been a member, especially of TMS, but I'm also a member of IEEE. In fact, I'm a Fellow of IEEE. I helped actually put together the nanotechnology council for IEEE. But I'm also a member of the American Physical Society (APS). I'm also a member of the American Society of Metals (ASM) and sometimes a member of the Materials Research Society (MRS). The society I've spent most of my efforts and time with has been TMS. The reason for that is that the structure of TMS, with respect to its parent society, AIME, which is, of course, sponsoring this oral history, is really the proper relationship.

That is, the founder society AIME made a decision many years ago that the member societies, which came out of that founder society, TMS being one of them, should be the stronger entity. That is, membership dues go to the member society, and then some of those will go to AIME. Of course, the member societies are closest to the members, so they have a much more direct connection with the membership, I think. If you compare that to a society like IEEE, it being the original society and out of it spawned many member societies, like the Magnetics Society, Electronic Society, et cetera. IEEE made the decision years and years

ago that they would remain the stronger entity and that the member societies would be the weaker entity. That structure, I think, is the wrong format because then once membership dues go to IEEE, which they then partially disperse to the member societies, IEEE (the parent society) can make decisions which are not necessarily for the best interest of the members.

My advice is to get involved in a professional society, and it makes no difference which one it is really. Even though I see differences between these different societies and the volunteers in them, the payoff will be manyfold to you as a consequence of membership.

Willard:

That has certainly been the case for me, too. So thank you for saying that, and thank you, Bob, for an enlightening conversation. And I really look forward to seeing you in person sometime soon instead of just on the computer.

Shull:

Hopefully, this pandemic will end soon, and we'll be able to actually get together in person at meetings, etcetera. Thank you, Matt, for putting together these questions to probe my career, and I thank AIME for hosting this, making it possible to share some of this information with the rest of the membership.