THE DEPARTMENT OF ENERGY ORAL HISTORY PRESENTATION PROGRAM

OAK RIDGE, TENNESSEE

AN INTERVIEW WITH MICHAEL K. WILKINSON,
FREDERICK W. YOUNG, AND
RALPH M. MOON

FOR THE

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STOW: Today, we're going to be talking to three individuals -- Mike Wilkinson, Fred Young, and Ralph Moon. They have all been very much involved with neutron scattering and materials analysis work here at the Laboratory, starting as early in some cases, as 1950. So, we look forward to a good talk with them and learning more about the history of neutron scattering in the Solid State Division, High Temperature Materials Laboratory (HTML), Advanced Neutron Source (ANS – never built), Spallation Neutron Source (SNS), and other important parts of Oak Ridge National Laboratory and its history.

We've started off our interviews typically by asking how and why you got interested in science or technology. Fred, give us a quick review of what got you into this area to begin with years ago.

YOUNG: It's not an easy answer -- not a short one. I went to graduate school and only there did I begin to realize what science was all about. And, I had a really interesting professor, who had a big influence on my becoming a scientist and deciding to try to compete worldwide in research as opposed to taking a teaching job at a small school or a job in industry, which were common things to do at that time. And, it just happened at about that time that Oak Ridge National Laboratory became open to the public. Oak Ridge Associated Universities, or ORAU, started a program of having college and graduate students and faculty members come for the summer. I asked ORAU if it would be possible for me to come to ORNL for a year. This was about the time I graduated. And, they thought about it a little bit and decided it would be a good thing, so I did that.

STOW: That was about 1950, right?

YOUNG: Yes, 1950.

STOW: Okay.

YOUNG: I spent a year here and then went back to the University of Virginia, which was part of the agreement. I finally came back to the Lab in 1956 and stayed here until I retired.

STOW: Did you have any idea at that time that you'd spend your entire career here?

YOUNG: No, not really. But I had more connection with it than most people when they came because I spent a year here and then I was a consultant in between. So, I was here frequently and I pretty much knew what I was getting into when I came back. So I thought I would stay here a long time, anyway.

STOW: Mike, what got you into science originally?

WILKINSON: Well, I guess I really got interested in science in high school ... taking courses in chemistry and physics and mathematics. And, I really liked them very much. When I went off to college, I actually thought I was going to be a chemist. But a very good professor I had in my first-year physics course -- he was department chairman -- influenced me to change my mind and become a physicist. After I graduated from the Citadel, I immediately went into the Army, which sent me to radar school at Harvard and MIT. I spent most of my time in the Army as a radar officer. At the end of the war, I decided to go back to MIT and pick up where I had left off in radar school. I guess the professors I had there made me very interested in research. Prior to that, I think I'd wanted to be a teacher. But, I decided I wanted to get some research experience first. also wanted to come farther south. About the only place you could do research at that time and come farther south, without being associated with a university, was Oak Ridge. And, my background at that time fit in pretty well with ORNL's Neutron Scattering Program.

STOW: Yes.

WILKINSON: At that time, there was no such thing as "training" in neutron scattering. I mean, it was brand new and you just had to have a background that was somewhat similar, and I had that. I came here and went to work with Ernie Wollan and Cliff Shull.

STOW: Yeah, we're going to talk about them in just a minute. Now, Ralph, you're the new kid on the block here.

MOON: That's right.

STOW: What got you interested in science?

MOON: Well, I think it started in high school. Math and science were my favorite subjects. My father was an engineer, as was my older brother. I just assumed that I would be an engineer so I enrolled in the engineering school of the University of Kansas. And, it was during my freshman year as an engineer that I discovered (laughs) I really didn't want to be an engineer.

STOW: Yes.

MOON: I much preferred a little more freedom to do basic science, so I switched to pursuing a bachelor of arts degree in physics. And, I haven't regretted it. Eventually, I ended up at MIT in graduate school just when Cliff Shull, who had left Oak Ridge to go to MIT, was getting his experimental program started at MIT. So, I was his first Ph.D. student. And, it was sort of natural for me to come to Oak Ridge.

STOW: And, that was in '63, I think.

MOON: That was in '63. And, I just continued what I started under Cliff Shull at MIT.

STOW: Is it safe to say that neutron scattering really had its birth here at Oak Ridge -- with the Graphite Reactor?

MOON: Oh, it did. No question about it.

WILKINSON: Oh, absolutely.

STOW: With Ernie Wollan and Cliff Shull.

WILKINSON: Yes, that's right.

STOW: In retrospect, what was the importance of the work that Wollan started before Shull came?

WILKINSON: Actually, Cliff won the 1994 Nobel Prize in Physics for the [pioneering neutron scattering research at the Graphite Reactor], but there's no doubt that it was a two-person program. Many times Cliff stated that it was just too bad this award wasn't made many years earlier when Wollan was still alive, so he could have been a co-recipient. But, Wollan did start the program at the Graphite Reactor in November of 1945. He actually had sent here a spectrometer that had an X-ray spectrometer that he used at the University of Chicago. He installed it at the Graphite Reactor. Incidentally, this spectrometer is now on exhibit at the American Museum of Science and Energy in Oak Ridge. But, at that particular time, the main interest was in measuring the scattering power of atoms by neutrons. And, I won't go through all of the details of what they went through, but the point is -- unlike X-ray scattering where you can calculate the scattering power -- you can't calculate it for neutrons. You have to measure everything experimentally.

STOW: Yes.

WILKINSON: And, before you could measure the first one -- you had to put the technique on an absolute basis -- a quantitative basis. They went through many trials and tribulations. One of their first successes was the use of powders, instead of single crystals, to scatter the neutrons. This switch eliminated the problem of extinction that you have with large single crystals. I won't go into that detail either, but it's a rather complicated effect that involves scattering within a large crystal such that the scattered intensity [of the neutrons] is quite different from what you should be getting. I think their use of polycrystalline materials was really the key to the success of their whole program. After that, they had to untangle a lot of things, such as the isotopic effects of scattering and the spin incoherent effects of scattering from a nucleus. They had a lot of problems with multiple scattering. But, in any event, they were able to put this technique on a firm foundation and use it for some very important experiments in nuclear physics, crystallography, and magnetism. The crystallography work was primarily associated with hydrogen atom crystallography. You cannot locate hydrogen atoms by X-rays because the X-ray scattering power of hydrogen is very small. So, [hydrogen atom crystallography using neutrons] turned out to be an entirely new field in itself. And, Henri Levy started a program within the Chemistry Division here based strictly on hydrogen atom crystallography to measure the positions of hydrogen atoms in crystals and study hydrogen bonding. Cliff and Ernie focused on magnetism, which is the other very important field. But, the really important thing is that they did all this work here first. If they had not established the foundation for [neutron scattering research using a reactor], it would not have grown up around the world as it has.

STOW: For the laymen out there -- put in simple ... terms -- what's the practical importance of neutron scattering? What does it tell us? I mean, what is the purpose of it?

WILKINSON: Well, the purpose of neutron scattering is that it is one of the best techniques available for studying the properties of materials and characterizing materials. If you want new materials in any technology you need, you've got to actually be able to study and characterize the new materials properly. Neutron scattering is one of the best techniques you can use. It has made a tremendous impact not only in physics and chemistry, but also in biology and various types of engineering polymers and medical problems. It's just a tremendous technique.

STOW: And, Ralph ...

MOON: One interesting bit of history that Mike skipped over is that the first instrument Ernie Wollan brought from the University of Chicago was actually designed by Arthur Compton, who was a Nobel Prize winner and Ernie's thesis advisor. This instrument was used by Compton and Wollan in Chicago and then Wollan brought it here. Some twenty or twenty-five years later, it was in use again on another piece of Nobel Prize-winning research.

STOW: My goodness.

MOON: So, it's really a unique piece of apparatus. And, it still exists at the American Museum of Science and Energy here in Oak Ridge.

STOW: I'll be darned. I didn't realize that. I ought to go over there and take a look at it.

MOON: Well, it's hard to find.

STOW: Is it?

MOON: I mean, it has been on conspicuous display occasionally, but not routinely.

STOW: Well, now, Fred, you were more into materials analysis, weren't you? Tell us a little bit about the work that you've done in materials analysis and radiation effects, and so on.

YOUNG: Yes. Well, the Solid State Division was actually formed to study radiation effects in solids. Neutron scattering was not a part of the Solid State Division in the beginning. The Solid State Division was formed in various steps in the late '40s. And, about 1951, it actually became a division. In the beginning, there was no study of this sort going on at Oak Ridge National Laboratory at all. But, it was realized early on that energetic particles striking solids would cause disorder in the lattice of these solids. Most of the materials we have been interested in are crystalline materials. A crystal is defined as an ordered arrangement of atoms.

STOW: Sure.

YOUNG: And, where the arrangement is disordered, a defect exists. If an atom is out of place, that's a defect. Or, it could be a group of atoms out of place. There are various kinds of defects. This was a subject itself that was just in its infancy at the same time that the study of radiation effects was in its infancy. So, the two were used in complementary ways and "grew up together," so to speak. I was fortunate enough to be at the right place at the right time and participate in that. And, so we investigated here in the division, all sorts of materials. It was important practically to know what happened. We had to build reactors, and the materials the reactors were built of were exposed to fast energetic particles, such as neutrons, alpha particles, protons, electrons, and gamma rays.

STOW: Sure.

YOUNG: First, we had to put [target materials] into the Graphite Reactor and see what happened to them. And, we had to develop methods of measurement to make quantitative statements about a material. There was some theory, but not much. All of this developed sort of simultaneously, and we were part of it. We looked at metals, semiconductors, ionic crystals, polymers, and elemental materials like graphite, for example. It turned out that Doug Billington was one of the early people here in [research on materials]. He actually came to the Oak Ridge School of Reactor Technology first after the war was over. And, then he became head of the Solid State Division. He did some of the first experiments and saw clearly that [studies of radiation effects on materials] should be expanded. He led that program for twenty-five years approximately.

STOW: Of course, all three of you fellows had been the head of the Solid State Division either in acting capacity or a longer-term capacity. Solid State Division is an interesting name.

MOON: Well, you know, it's been changed ...

STOW: It's been changed around, I know. [It became part of the Materials Science and Technology Division at ORNL.]

YOUNG: The term solid-state physics became the name of a division of the American Physical Society, in 1949, if I'm not mistaken.

STOW: Okay.

YOUNG: I think that's correct. And, it was called solid-state physics. But, there's also a field called solid-state chemistry.

STOW: All right.

YOUNG: And then, there were the terms metallurgy, ceramics, and polymer science. All of these fields dealt with solids. We had a group of people investigating various solids who were trained in chemistry, physics, or ceramics sciences.

STOW: Yes.

YOUNG: And so, rather than call it any one of them, they started just calling it "Solid State." That was a discussion that went on at the time that the division was first named.

WILKINSON: I think a better name probably would have been the Solid State Sciences Division.

YOUNG: Yes, I think it would have. But, the managers did not name it that.

STOW: Well, I've always been curious about where the name came from. I mean, it could have been the Liquid State or Gaseous State Division. (laughter) As I was reading about the background of the division and some of your backgrounds, I learned there was a Research Materials Information Center established in the 1960s in the division. Who can tell me something about that?

WILKINSON: Well, I'm sure that Fred or I, or Ralph could, but let me just start.

STOW: Okay.

WILKINSON: As Fred said, most of the very early work in the division involved studying the production of defects [in crystalline materials] and annealing these defects by raising the temperature of the sample. Well, if you start off with a sample that already has a large number of defects in it, then you actually end up "masking" the whole situation that you're studying.

STOW: All right.

WILKINSON: So it was pretty apparent at the time that very nice, perfect crystals that were defect free and of high purity were needed in these research investigations. The Materials Science Division of the Atomic Energy Commission at that time decided it would be nice to start a program that would actually concentrate on developing techniques for producing very-high-quality materials and using the materials, not only in Oak Ridge, but also at other laboratories sponsored by the Atomic Energy Commission.

YOUNG: And also worldwide.

WILKINSON: At roughly the same time, an information center was set up for keeping track of where these materials were being prepared, who was preparing them, what their characteristics were, how to go about getting them. And, an up-to-date list was published periodically and sent both to the research users of the materials, and to the producers. The users needed to know where they could get the materials, and sending the list to the producers kept them from duplicating what was already being done someplace else. It turned out to be a small program. But, at the time, we didn't have information centers all over the place like we have now. We didn't have a lot of people concentrating on growing large single crystal specimens, so it was a very important program.

STOW: What ever happened to that information center?

YOUNG: It lived its life.

WILKINSON: That's right. The other information centers in material sciences were developing throughout the country under the sponsorship of other agencies. There was a big overlap in the programs. The ORNL center was run by a research program, not by a DOE administrative program. The Lab decided it was better to spend the money for research because the information center duplicated the efforts of other organizations.

STOW: Okay. Let me ask about one other historical aspect. Fred, maybe you can reflect on this best. In 1962, some pioneering work was done on ion channeling. Can you explain the importance of that?

YOUNG: Yes. In general, atoms are aligned in the lattices of crystals. If you make a model of this lattice, you can look through it in one direction. You can see many more holes all the way through than if you look in a random direction.

STOW: Yes.

YOUNG: So, if you look in specific directions in a model of a crystal, your natural reaction is that holes are in there. But, these models are not realistic either; in fact, the electrons orbiting around the nuclei in all positions overlap and join in these open areas. So, we had begun to think that in a cubic lattice [the movement of atoms through a crystal] was isotropic. For instance, atoms diffusing through the crystal would diffuse in all directions with equal ease, with equal velocity. We had convinced ourselves in a way that holes did not really exist throughout the lattice. But then, in order to explain some scattering results, Mark Robinson became involved in an experimental program. He started doing some calculations in a time when general use of computers was just beginning. He was rather clever at using computing in ways that were somewhat more advanced than what other people had been able to do. He found experimentally and theoretically that when atoms were sent down in a certain direction in a crystal, they penetrated a lot farther in that direction than in other directions. What's more, the atoms moving in that direction were not just the ones sent directly along the lattice, but also the ones that bounced off atoms in the crystal in a way that let them go through it farther. This phenomenon, discovered at ORNL, is called channeling.

STOW: All right.

YOUNG: They decided to name it ion channeling because the lattice "channeled" charged atoms, enabling the projectiles to go through the crystal. And so, at first, we wondered whether this was true or not, you understand. And then, lo and behold, some people at Chalk River Laboratory in Canada who were investigating ion implantation discovered the same effect, which was found to be channeling. And then, the whole world woke up to the new phenomenon, and immediately it began to be a very important subject. Robinson continued his research with Ordean Oen, and Mark and Dean together performed the calculations. They broadened their program and then we began to develop programs here. And, at the present time, ion implantation is used to adjust the concentration of charged carriers in semiconductors.

STOW: Yes.

YOUNG: And, this understanding of channeling is the basis of that whole industry now. Channeling effects must be taken into account properly. You can't do ion implantation exactly along the axis of a silicon crystal. You've got to cock it off just a little bit.

STOW: Yes.

YOUNG: And then, you can determine exactly how far all the atoms will be implanted into it. And, I mean, this was a tremendous achievement. We're in the silicon age, aren't we? This is of tremendous importance to the whole industry. It turned out also that some really nice theoretical and experimental studies were done here, showing you could use this as a way of determining interatomic potentials.

STOW: Okay. So, a lot of that fundamental work that we're benefiting from today in silicon technology came out of the Solid State Division in those years.

YOUNG: That's quite true.

STOW: Neutron scattering -- the Neutron Scattering Program -- has really been one of the longest continuing programs at ORNL stretching back to 1945. How has the program changed as new reactors have come online? We started with the Graphite Reactor, and, of course, have been through thirteen, fourteen, fifteen different reactors here at the Laboratory over the years. How have changes in reactor technology influenced the neutron scattering program and its ability to achieve new objectives? Ralph, would you want to jump in on that?

MOON: Yes, sure. Three reactors have played key roles in the Neutron Scattering Program. The first was the Graphite Reactor, where the flux of neutrons is measured in neutrons per square centimeter per second. That number tells you how many neutrons are flowing into your experiment. At the Graphite Reactor, that number was around 10^{12} neutrons per square centimeter per second. We then went to the ORR, the Oak Ridge Research Reactor, where flux number increased enormously to 3 times 10^{14} -- a factor of 300 larger than the flux at the Graphite Reactor. Finally, we did neutron scattering research at the High Flux Isotope Reactor, where the neutron flux was boosted to 10^{15}. What happened is that the complexity of the experiments followed that increase in flux. When you get more flux, you usually don't just do experiments faster. You do experiments that you couldn't do before because you didn't have enough neutrons. So, at the Graphite Reactor, elastic coherent scattering was the main technique used because the researchers could measure the number of neutrons scattered through a particular angle. Beginning at the ORR, another type of experiment involving inelastic scattering, in which both the number of neutrons scattered through a particular angle and their change in energy could be measured.

So, neutron cross sections were measured as a function of energy and momentum. When we got to the High Flux Isotope Reactor, we created a new sort of experiment, in which we not only measured the scattering angle and energy change, but also the spin of the neutrons in a beam. In a magnetic field, the spin of one half of the neutrons can be either up or down. We'd known for a long time how to produce polarized beams of neutrons, so we could produce a beam where the spins of all the neutrons are up. We also had the technology for flipping those spins from up to down. So, what we did at the HFIR was -- in addition to measuring energy and momentum changes – was to measure how many neutrons in a beam came on the sample up-spin and how many came off the sample up-spin.

STOW: Okay.

MOON: A different problem was to determine how many neutrons came on the sample with down-spin and how many came off the sample with a down-spin. Also, we tried to determine the probability that some neutrons would flip their spin – come into the sample up-spin and come off the sample down-spin. Theory told us that for certain types of systems, all those cross sections (probabilities) would be different. We found new information in each "spin dependent" cross section. We experienced a gradual evolution as experiments became more and more complex as the source flux got bigger.

STOW: So, we've had three reactors that have influenced the program -- the Graphite Reactor, the Oak Ridge Research Reactor, and the HFIR.

MOON: That's right.

STOW: But, Mike, there are sources of neutrons that are not from reactors, right?

WILKINSON: That's right.

STOW: Has the Neutron Scattering Program here relied solely on neutrons from nuclear reactors?

WILKINSON: Well, are you thinking in terms of accelerator-type neutrons?

STOW: Yes, I think so.

WILKINSON: Well, there's been very little neutron scattering here done using accelerators. Herb Mook has done some experiments at our accelerator facilities, but, of course, a lot of it is going to be done fairly soon on an accelerator when the Spallation Neutron Source is finished.

STOW: That's right.

WILKINSON: The types of experiments you do at research reactors and accelerator-based neutron sources, such as the SNS, are a little bit different, but, nevertheless, equally as important and equally meaningful, with respect to the information you get. In the pre-reactor days, scientists observed neutron scattering using very small sources, such as radium-beryllium sources, but they didn't learn anything, because there were not high enough intensities to do any meaningful experiments.

STOW: Fred, you were involved in the 1970s with the planning and the establishment of the High Temperature Materials Laboratory. Tell us a little bit about your involvement there.

YOUNG: My involvement really started at the time when the Atomic Energy Commission became the Energy Research and Development Administration and then the Department of Energy. At that time, we were constrained to study materials of interest to the nuclear programs. We weren't funded to look at anything we wanted to.

STOW: Yes.

YOUNG: With the onset of the Department of Energy in 1977, we were asked to investigate materials problems related to all energy technologies. So, this opened up a tremendous new area for us. It became apparent immediately that for many of the energy technologies, including nuclear, high-temperature materials were very important [because fuel is used more efficiently when machines are operated at higher temperatures, but research is needed to identify or create structural materials for such machines that can endure high temperatures for a long time]. We had no programs in that area to amount to anything in the whole country in the atomic energy laboratories. So, the origin of the HTML here began when Don Stevens, head of this program, asked me to meet him in Washington. Then he asked me to go to several other laboratories around the country and determine what was going on in the area of high-temperature materials, and what we should be doing about that. Well, after a long process, I came back and wrote a report in which I stated that some really exciting [research on high-temperature materials] could be done here. I said that it was appropriate for the Department of Energy to sponsor a program of this sort. Well, it took many years to realize [this vision], but ultimately, the High Temperature Materials Laboratory [was designed and built at ORNL]. And, I was involved with it over that whole period of time. It was finally decided to put HTML into the Metals and Ceramics Division, as opposed to the Solid State Division. That was a somewhat arbitrary decision but not an unreasonable one. And, I was proud of my efforts in that respect. I think I had a lot to do with it.

STOW: Well, that's something to be proud of, because the HTML is one of the cornerstones of our materials science work here in Oak Ridge.

YOUNG: Yes, yes. It was created out of whole cloth in a way, because there was almost no [study of high-temperatures materials] here. And, almost none in the whole set of DOE laboratories.

STOW: Well, Fred, you've touched on a couple of things that I want to ask you about, Mike. You were division director for Solid State Division starting in 1972, I believe, and up into the 1980s. So, you were division director during the transition from the Atomic Energy Commission to ERDA to DOE. As manager of the division, did you experience problems with that transition?

WILKINSON: Well, we didn't experience any problems. It sort of opened the door for us. The point is that the research done in the Solid State Division is mission-oriented research. That means that you do research on materials and their properties that are important to the mission of your sponsor. There's one exception to that I might point out, and that's the Neutron Scattering Program. The exception is that, it's acceptable to use the major facilities that have been built by the sponsor here at the Laboratory to study materials of any kind that can't be studied by other techniques.

STOW: All right.

WILKINSON: For the Lab's Neutron Scattering Program in the Solid State Division, the determination of which types of materials to study was not as restrictive as was the case in other parts of the division. But, under the Atomic Energy Commission, of course, we were restricted to work on materials associated with fission and fusion reactors. Under ERDA and then the Department of Energy, materials associated with all energy technologies were of interest. ERDA and DOE's mission [embraced] not only fission and fusion energy, but also fossil energy, geothermal energy, magnetohydrodynamic energy, energy storage, energy conservation, and so forth. Well, all of these [energy technologies] were materials limited. Advances in energy technologies [to increase supply and reduce demand] required new materials. So, it was very interesting from my viewpoint to be [encouraged] to expand our program from a fairly restricted one involving mostly materials radiation and neutron scattering, to almost anything else we wanted to do. And, we got some extra money for starting new programs, but a lot of it was associated with redirecting some of the programs in radiation effects to other areas. As a matter of fact, nowadays relatively little research is going on in the radiation effects studies; most of this type research is tied into other efforts in a very broad program. It was a tremendous opportunity for us to be able to expand our investigations. The Solid State Division has developed into one of the broadest materials programs in the world, and certainly one of the best. It's one of the best organizations in the world for studying different types of problems in the materials sciences. [Since 2009 SSD and the Metals & Ceramics Division became parts of the newly named Materials Science and Technology Division at ORNL.]

STOW: Well, in 1971, I think, Alvin Weinberg, Laboratory director, asked you and Sheldon Datz to look into opportunities in nonnuclear research and the basic physical sciences. Is what you've just described an outcome of some of Alvin's foresight?

WILKINSON: Alvin jumped the gun here in that he established committees for looking into [areas of nonnuclear energy research]. We did actually publish a report on it, which the Department of Energy found very useful, once they got involved in all other types of energy. As a matter of fact, the so-called nonnuclear energy technologies and the materials associated with them turned out to be key to their programs.

MOON: You mentioned Alvin's forward-looking thinking. Let me mention one example of that.

STOW: Okay.

MOON: From the beginning, he had recognized that neutron scattering was very important scientifically, but he played a really crucial role when the HFIR was being built. The HFIR was designed to produce transuranic isotopes but the original design had no provision for neutron scattering.

WILKINSON: No beam holes ...

MOON: No beam holes. Alvin Weinberg insisted that they could not build this reactor without putting in beam holes [for neutron beams] so that neutron scattering could be done at the reactor.

STOW: And, they put in four beam holes, I think.

MOON: Four beam holes, yes.

STOW: So, that really was a key part of the Neutron Scattering Program.

MOON: Absolutely, yes.

WILKINSON: Well, the engineers were afraid, because of the type of reactor it is. They said they were afraid that putting in the beam holes would affect the operation of the reactor. That's the reason they compromised on just four beam holes. It's too bad they didn't put in a lot more. But, as it turned out, of course, the beam holes did not affect the operation of the reactor and have really been a tremendous help to the Neutron Scattering Program here at the Laboratory.

STOW: Fred, you took over as division director in 1988, I believe. What were your feelings and thoughts at that point in your career in taking over an administrative role like that?

YOUNG: Well, unfortunately, my time as division director was simultaneous with [the visits of and issues raised by] the so-called Tiger Teams. [They were sent to ORNL by DOE to check on compliance with environmental, safety, and health regulations].

STOW: Okay.

YOUNG: (laughs) So, a lot of my efforts had to go toward dealing with those issues. I didn't enjoy that very much. Doing research is much more fun than being an administrator. But, I think Mike told me this one time: it didn't make so much difference about how many more research papers I published, as it did whether the Solid State Division developed into a powerful institution as it did.

WILKINSON: Well, let me interrupt him for a minute.

STOW: Okay.

WILKINSON: He didn't just become a division director and become involved in the management of the division. Fred had been an associate director for a long time ...

STOW: Sure.

WILKINSON: ... and as associate director, he had also been highly involved in division management.

YOUNG: You know, I was quite familiar with management, and so it wasn't an exceptional chore ...

STOW: I understand.

YOUNG: ... except for this Tiger Team business. That was pretty late in my life here. Unfortunately, it was a time of change in the Department of Energy and its way of funding things. Funds were taken away from research programs to help solve the cleanup problems that we had from earlier handling of radioactive materials here at Oak Ridge.

STOW: Yes.

YOUNG: And, I felt that was unfair. We had not been the ones that had caused the problem, but we had to pay for it. And, we did get a chunk of that. But, nevertheless, it was also an exciting time because we were doing a lot of new and interesting research then. I was proud to be part of it. I did want to make an addition to what was said earlier by Mike and myself on the studies of irradiation effects.

STOW: Okay.

YOUNG: By the end of the 1970s and early '80s, we had, to a large extent, both theoretically and experimentally, categorized the irradiation effects. And, they were reasonably well understood then.

STOW: Yes.

YOUNG: And, so it was natural that we use this expertise to investigate other types of problems. We went from being certainly one of the best-- if not the best -- radiation effects laboratories in the world -- to one of the best general solid-state physics laboratories in the world, over a few years, from the onset of DOE to the time I left. We opened up a lot of new areas of research that utilizes the facilities and people that we had. And, that's what research management is about, and that part is okay. It's just those Tiger Teams ... (laughter)

STOW: I get the impression you didn't care for the Tiger Teams that much. I don't think anybody did, frankly. (laughter) Ralph, you served as an acting director of the division for a period of time, but I want to ask you more specifically how you got involved with the ANS. Tell us a little bit about your involvement there and what happened to ANS in the long run.

MOON: Well, there's a lot of prehistory to the ANS, which is also prehistory to the SNS ...

STOW: Yes.

MOON: It started when both the High Flux Isotope Reactor here, and the High Flux Beam Reactor at Brookhaven National Laboratory, were built and came online in the mid-1960s. Shortly after that, the neutron community, not only the scattering community, but other scientists that used reactors, got worried that DOE -- or I should say at that time, the AEC – was not thinking ahead about the next-generation neutron source.

STOW: Yes.

MOON: There was a workshop in '73 called "The Workshop for Intense Neutron Sources," held at Brookhaven. From this Lab, Mike, Alvin Weinberg, and Wally Koehler went to it. Their recommendation was that the AEC should really start thinking about the next-generation neutron source. At that time, Argonne National Laboratory had already started thinking about "pulsed sources," or spallation sources of neutrons.

STOW: Okay.

MOON: ... so that was in 1973. And in '77, a National Research Council group did a study of neutron sources, with Cliff Shull as the chairman. The group recommended that people in charge should start thinking about new neutron sources. In 1980, a DOE panel headed by Bill Brinkman made the same recommendation: start thinking about new neutron sources. In '83, there was a local competition. Alex Zucker and Herman Postma had decided that it was time for the Laboratory to go after a big new facility and that it was going to be a neutron source.

STOW: Okay.

MOON: So, the next decision to be made was whether to go for a reactor or for an accelerator-based spallation source. And, I was the guy in charge of making arguments for the reactor. Dave

Olsen was in charge of making arguments for the spallation source. So, we had a big management meeting in which we both presented cases, and the Laboratory decided to go for a new reactor. Well, the decision was made at that time to upgrade the HFIR. Postma and Zucker didn't want to go for a new reactor. They wanted to upgrade the HFIR. We got money starting in '84 from the internal Laboratory Directed Research and Development (LDRD) Program to begin a small project about planning upgrades to the HFIR. The same day we got that money, we received an invitation to appear before another National Research Council meeting in March. It was January 1984 that we got the money, and we had to appear in March before the National Research Council group, whose mission was to decide which major facilities DOE would fund. This was the Seitz-Eastman committee. They decided that DOE's first priority should be to build an X-ray synchrotron source, which became the Advanced Photon Source, the APS [which was built at Argonne National Laboratory]. The Advanced Neutron Source (ANS) reactor was the second choice. Argonne representatives were there touting an [accelerator-based] spallation source, which was the fourth choice. At that point, the decision was made to go for a new reactor. This committee didn't want an upgraded HFIR. They wanted a brand new facility. So, the ANS took off from that point. When it came to funding the ANS, the federal government found it was a lot more expensive than people thought, and the timing was wrong. Our country was having these massive deficits every year. So the decision was made to not fund the construction of the ANS, but the scientific case for a new neutron source had been made.

STOW: Yes.

MOON: And, the decision had been made to put a neutron source in Oak Ridge. So, the decision to try for a spallation source was natural, and the timing was right. When the conceptual design report was completed and DOE wanted to ask for construction money, we had this magic moment when there was a surplus in the federal budget. And, that was very important, I think, in getting funding for the spallation source. But, the whole prehistory -- the scientific case for a new neutron source was made beginning in 1973 and going up to now -- and when the arguments came about whether a spallation neutron source should be built in Oak Ridge -- the scientific case had already been made.

STOW: That's an interesting history. I didn't realize all that. And, we'll be talking to Al Trivelpiece about his role in the SNS. So, I appreciate the background there. Mike, let me ask you one quick question here.

WILKINSON: Could I insert something? I've just been sitting here thinking about our conversations involving, particularly, the neutron scattering and materials research, and in all of this, Wally Koehler's name has not been mentioned.

MOON: I was going to mention him ...

WILKINSON: I wanted to make sure that we recognize that Shull and Wollan started the program, and Wally Koehler joined their group in 1949 ...

STOW: Yes.

WILKINSON: ... I became the fourth member in 1950, and Wally stayed here and performed some beautiful neutron-scattering research for many years. As a matter of fact, he led the classic work done here on interpreting the magnetic scattering from rare-earth metals and alloys, and both he and the Laboratory got a lot of credit for it. I just want to make sure that this oral history mentions his name.

STOW: Good. I'm glad you did. And, let me follow up on that. I'm going to ask all three of you the same question. In your career here at ORNL, has there been any particular individual who has influenced you in a positive fashion, been your mentor, or influenced the direction in which your research has gone? Mike, do you want to answer that?

WILKINSON: Well, there's no question in my mind that Cliff Shull was a very strong influence on what I did here. When Ralph said he was Cliff's first graduate student, I wanted to say, "Well, I was Cliff's first postdoctoral student." I really didn't come here in a postdoctoral position, but a postdoc is what I was. I worked with him very closely for five years and learned a tremendous amount from him. So, there's no question that as far as my research was concerned, he was the main influence.

STOW: Ralph, what about you?

MOON: Yeah. Well, certainly Cliff influenced me when I was a graduate student, but when I came here, I started working closely with Wally Koehler ...

STOW: Yes.

MOON: And, we collaborated closely for probably, sixteen, seventeen years after I came here in '63. I certainly learned a lot from Wally and benefited from his experience and his choice of scientific experiments to go after.

STOW: Fred, do you have any insights on this?

YOUNG: Well, I didn't have the same type of experience. There was not a world-famous scientist in my area that I learned from. I was on my own more from the very beginning. But, one thing that helped the Solid State Division, in my view, was the interaction we had with other laboratories and universities in this country and around the world. And, in particular, I would like to mention a theorist in solid-state physics named Gunter Liebfried, a German located at the University of Aachen. He had learned his solid-state physics as the field grew up in Germany prior to World War II when it was put on a firm theoretical quantum-mechanical basis.

STOW: Yes.

YOUNG: He spent one year here and then, after that, a couple times [during each of the next few years] he would come to ORNL and stay six weeks and work with theorists primarily. But he also worked with me a lot.

STOW: Okay.

YOUNG: And, I learned an awful lot from him. I got just a basic sort of understanding of solid-state physics, because I was not trained in that area at all, you understand. And, he had a big influence on me. Other than that, everybody influenced me. I was getting all the help I could at all times, but I didn't have a mentor in that sense.

STOW: All right. Fred, look back on your career and tell me if you have one research accomplishment that you're most proud of. I want to ask all three of you to quickly reflect on that.

YOUNG: I developed techniques for looking at defects in metals that scientists have used since.

STOW: Good.

YOUNG: I think that was the way I became recognized scientifically in this country and around the world. I was able to prepare samples of copper and then do experiments with them to show that I knew what I was talking about. This was new entirely for metals, in particular, at that time. And, I gained some national and international recognition for this work.

STOW: Mike, what would you be most proud of as a contribution to science?

WILKINSON: Well, actually, as far as research is concerned, the work that I did with Cliff Shull was, I think, extremely important. But, I continued studies of magnetic materials after he left, and I think I did some very important work there. But, I would like to think that I would be remembered as much as anything else for the developments that took place in the Solid State Division. Fred and I worked very hard on this and it became one of the best organizations in the world for doing research in materials sciences.

STOW: Good.

WILKINSON: I think that both of us would like to be remembered as much as anything else for our contributions in making it happen.

STOW: Well, we'll make sure you are. (laughter)

YOUNG: I think that we both realized at sometime relatively early on that that was a role that we were best suited for, given the circumstances here and the people here. We gave up our treasured experiences as experimental scientists in order to help everybody else do a better job.

STOW: Well, it takes all sorts of people to make this place operate. And we've interviewed some people who acknowledge that. They have said something like "Hey, I was not a good scientist necessarily, but I was a better administrator." And Ralph, what would you say your greatest contribution has been?

MOON: Well, it was one of the early experiments at the HFIR, where we developed the polarization analysis technique. It came about in almost an accidental way. A fellow named Tormod Riste, who was an outstanding scientist from Norway, came to Oak Ridge. I was going to work with him on doing some other experiment at the HFIR, one of the first experiments to be done out there. We actually started that experiment when the HFIR went down because of a problem, and [the shutdown] lasted six weeks. Tormod was sharing an office with me, so we often talked about neutron scattering and the theory of neutron scattering. It came to us that we already had the equipment to do some very interesting experiments related to measuring whether the neutron had flipped its spin [from up to down, or vice versa] or not when it scattered from a sample. So, we just forgot our plans to do this original experiment and started thinking about polarization analysis experiments. When Wally Koehler heard our conversations, he wanted to become a part of that. So, that was it. The three of us did this kind of experiment. Everyday, it was a different thing. We were trying something brand new that nobody had ever done before. And, we had lots of opportunities for brand new experiments. It was just really great fun.

STOW: Good. Where would you guys say the center for neutron scattering is now worldwide? Is it in Europe?

MOON: Yes. Probably at the reactor at the Institut Laue-Langevin in Grenoble, France.

WILKINSON: That has been for many years probably the world's single largest center for neutron-scattering research. The ILL reactor was built strictly for doing neutron scattering research. It is a high-flux reactor with many beam holes. I think that the world's center for neutron scattering research is going to be taken away from ILL when the Spallation Neutron Source (SNS) is operating and the modifications of HFIR are made.

STOW: Well, that's a good way to leave the interview then -- looking toward the future when Oak Ridge will return as the world's center of neutron scattering. Thanks very much, fellows.

MOON: Thank you.

WILKINSON: Thank you.

--END OF INTERVIEW---