Interviewee: Ralph Nelson By: David Zierler

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ZIERLER: Okay. This is David Zierler, oral historian for the American Institute of Physics. It is July 3rd, 2020. It's my great pleasure to be here with Doctor Walter Ralph Nelson. Ralph, thank you so much for being with me today.

NELSON: My pleasure.

ZIERLER: Okay, so to start, please tell me your title and institutional affiliation.

NELSON: My title? Well, I was three months short of being 40 years at SLAC. I came there in 1964 in April and I had a Masters degree in physics at that point. And I had done something that was interesting to SLAC, at the Lawrence Berkeley Laboratory, which turned out to be very important, so they made me a job offer I couldn't refuse. I took it and ultimately SLAC paid for my Ph.D. It was Burt Richter who was my thesis advisor. You know who he was, of course.

ZIERLER: Oh yeah.

NELSON: Yeah. And so I went on to work at SLAC in both physics and radiation physics. According to Panofsky and others, I was told to expect to spend half my time doing physics, the other half helping to create a radiation protection group, mostly doing shielding calculations. So, I took that job and it was a smart thing to do. In hindsight, there are three things I that I have done in my life that were very smart (or lucky): The first was to marry my wife, Kay, we will have been married 60 years this month. The second was to take the job at SLAC and the third, of course, was to buy a house in Palo Alto.

ZIERLER: Right. (laughs) Very smart real estate decision.

NELSON: So I got a Ph.D. and it was sponsored to some extent by the Director of SLAC, Panofsky, since he also was on the Stanford Graduate Special Programs committee. I have been told that he said to that committee, "This is a very interesting profession. There aren't many accelerator health physicists in the world." A couple at Berkeley and a couple in CERN. And he said, "It's a profession in which you should want to get a Ph.D." So I did it in several departments at the same time: Nuclear Engineering, Radiology (Medical School) and Physics. And stayed working at SLAC almost 40 years and had a really good time. I worked with a lot of those people you currently are interviewing.

ZIERLER: Ralph, let's take it right back to the beginning. Tell me a little bit about your childhood in St. Paul. Are your parents from Minnesota?

NELSON: Yes, both of my parents were born in St. Paul, Minnesota. When I was four or five years old, I got polio and spent a year in the Shriners Hospital in St. Paul, along the Mississippi River. When World War II started my mother stayed in St. Paul, while my father went into the Sea Bees, eventually transferring into the Navy. My mother and I moved to San Diego after the war because my dad said, "I'm never coming back to Minnesota, to all those flies and everything in the summer, and it's just too cold in the winter." And so we moved to San Diego and I went to high school there and eventually went to Cal Berkeley. I also took a one year study at the University of Washington for a Masters degree in Physics before moving to Stanford.

ZIERLER: Now, did you go to public schools growing up?

NELSON: Yes. I did.

ZIERLER: And did you think you were going to major in physics even before you got to college? Was that the plan?

NELSON: Not at all. Actually, I thought I might become a musician. I played clarinet reasonably good and I thought about doing that for a vocation. I had a couple scholarship offers towards the end of my senior year in high school, although they were not for music. One of them was to Cal so I took it. By then I could see myself walking up and down a street directing a high school band, and I didn't want to do that. After I enrolled at Cal I went through a lot of testing and the counselors said, "Well, we noticed that you have an aptitude for math and science." I said, "Really? I've only had elementary algebra and no trigonometry." And before I knew it, I was taking every math course there was, and towards the end of the academic year it looked like I might end up with a math degree (jokingly). Let me put it this way, during my sophomore year at Berkeley I was even a teacher's assistant (TA) in the math courses I had taken. Shortly after that I decided to major in physics, and that's where I've been ever since.

ZIERLER: Berkeley's a pretty good place to just sort of fall into a physics program.

NELSON: It is, yeah. Berkeley's a great school, especially in physics.

ZIERLER: At what point did you realize that you wanted to pursue physics as a graduate student? Was that sort of right away once you got comfortable in the department at Berkeley?

NELSON: I would say, yes, because I realized right away how much fun it was studying physics. I suppose I also suddenly realized, "I'm not going to have to direct a high school band marching up and down some street in a parade." A physics career seemed to be so much more fun, and challenging, than music. I suspect that many of the other people you will be interviewing will say the same thing, that is, "Physics is not just a vocation; it's challenging and fun to do." By the way, at 83 I am still employed part time doing physics work every day.

ZIERLER: Right.

NELSON: Before the today is over, I will have put in several hours for a company called Lyncean Technology, a SLAC spinoff by a former SLAC professor and one of his grad students. I have worked a lot for that startup. We recently got a large grant to go ahead and build an electron storage ring in Bucharest, Romania, and for a while this year I was putting in 15 hours a week consulting. Why? I have been retired from SLAC for 20 years and certainly don't need extra money; my wife and I are perfectly well set up in retirement. We travel a lot in our motorhome to bluegrass events, where I sometimes play standup bass. But I still need something more challenging to do every day.

ZIERLER: Right. Keep busy.

NELSON: Yeah, and you know, I routinely get together with my former SLAC colleagues—there are about 75 of us who were employees of SLAC. Until this virus thing hit, we were getting together once a month for dinner, engineers and physicists, and we would discuss our retirement lives. One of the things that most of us agree with is that we're all rather bored. We dearly want something to do. Frankly, I'm one of the few of us that has some science work to do, so I am lucky in that regard.

ZIERLER: Who were some of the professors at Berkeley you remember in the physics department?

NELSON: Oh, ooh. (laughs). Bert Moyer stands out. But that was because he was one of the people who was interested in health physics from the beginning. Emilio Segrè, I knew him a little bit along with Owen Chamberlain. Most of the ones that I knew resulted from my part-time student work as a "nuclear emulsion scanner" in the Walter Barkas group at the Lawrence Berkeley Laboratory. LBL has always been a famous place for physics and at that time (1962) SLAC was just starting up, and so... I can tell you more about that, and SLAC's relationship to LBL a little later as we go along here.

ZIERLER: Were you aware of SLAC starting up when you were an undergraduate?

NELSON: Yeah. Because you see what happened was, after graduating from Cal I went off to get a Masters degree in physics at the University of Washington in Seattle. Unfortunately, I didn't have a research or teaching assistantship as a source of part-time income. But my wife had a good job with Boeing, which barely paid our bills and allowed me to study physics full-time at UW. However, a devastating thing happened--I still call it Vatican Roulette--what happened was she got pregnant and she was so sick that she couldn't work anymore. So we came back, after three months at UW, to the Bay area to live with her parents, where I took a full-time job at the Lawrence Berkeley Laboratory (LBL, “up on the hill”). Interestingly, the money for my salary came from a new laboratory being built in Palo Alto, called the Stanford Linear Accelerator Center, SLAC. It had just been approved by Congress during the Eisenhower administration.

At that time one of the things that was sort of ambiguous in accelerator radiation protection was how high-energy neutrons penetrate very thick shielding, either earth or steel. What happens is that these neutrons enter the shield where they either scatter or get absorbed. But we didn't know what the overall attenuation really looked like qualitatively. We knew that an electron beam eventually deposits its energy in matter in a process called an electromagnetic cascade shower---namely, the electrons make bremsstrahlung x-rays, and these photons, in turn, make more electrons and positrons, back and forth. At the same time some of these photons can also create neutrons. They can also make pi mesons and other types of hadrons and, as a result of this, you get what is called a hadronic cascade---and that's the thing that ultimately dominates in the thick shielding of all high-energy accelerators. What SLAC wanted to do was to join up with some physicists who were making measurements of the hadronic cascade through steel and earth surrounding the Proton Synchrotron at CERN. And they needed somebody back home in Palo Alto or Berkeley to analyze these high-energy interactions in the nuclear emulsions that were going to be exposed. That is to “scan” the nuclear emulsions, make measurements o where interactions occur, and then analyze the neutron attenuation. And so they hired me as a junior physicist and I got to hire two technicians to work under me. We worked for a year on the project and we were able to very nicely characterize the attenuation of high-energy neutrons in both steel and earth. This turned out to be important for SLAC. The lab had a physicist, named Hobey DeStaebler, who had written an important document called SLAC Report-9, where the thickness of earth was designated for covering up the two-mile accelerator trench. Panofsky, himself, had originally predicted we would need to put 40 feet of earth over this trench. But DeStaebler said we did not need to make it that thick, we only needed to use 30 feet. Well, 10 feet of dirt over two miles turns out to be a lot, not only from the point of view of the cost of moving that much dirt, but because the longer a construction project lasts, the more chance of having serious accidents. Turns out several people were killed during the construction of SLAC, and one of them was when a large earth-moving rig rolled over while transporting earth for the trench. So being able to reduce the earth shielding by 10 feet was a significant step forward, provided it was true, and our CERN experiment provided evidence that it was.

ZIERLER: And Ralph, what was the experiment that showed you this data that you didn't need to go to 40 feet. What were the experiments that gave you this data?

NELSON: Okay. So I'm looking on my iPhone at my list of publications. It was my very first paper as a physicist, “Shielding Studies in Steel with 10 GEV and 20 GEV Protons: Part II”. It was published in a journal called Nuclear Instruments and Methods (NIM 32, 1965, p.48), and it was with a bunch of European physicists, myself, and a woman named Dr. Marian Whitehead, who had just started the Health Physics Department in the Research Division at SLAC.

ZIERLER: Yeah, so my question was, what was the experiment exactly that made you conclude that you didn't need to go down to 40 feet?

NELSON: Okay, the experiment was to use nuclear emulsions. At that time, nuclear emulsions were a very good technique for measuring particle flux, because you could look into these plates of threedimensional film and see the tracks of charged hadrons, protons and mesons, as well as their interactions with atomic nuclei---in the form we called a “star”. When a hadron would come in and strike a proton or neutron in the atomic nucleus at high energies, that nucleus would break apart and particles would fly all over the place. And I would count and analyze those stars, and I would plot how many I had as a function of the thickness. Because these stars were tiny little things located in emulsion plates placed throughout the soil (or steel) shielding, we were able to observe the hadron attenuation. Neutrons have no charge so they left no incoming track, but we could see where they interacted, you'd see the star, but you wouldn't see any incoming track. So you knew those were the neutrons. And the ones with an incoming dotted or dashed track we interpreted to be protons or mesons. So here was the technique. First you would place the black-tape covered emulsion plates inside the soil or steel shielding, then you exposed everything to the accelerator radiation field, then you'd take the plates out and develop them. Finally, back in the lab, you would look for the stars using a microscope. It was tedious work, but most of the techniques at that time involved measuring particles like this with bubble chambers, cloud chambers, nuclear emulsions and various other kinds of detectors. At the time this actually was a quick way to take all of the data all at once by having all these little detectors placed around this thickness of earth or steel. My team spent a year doing the scanning of the plates. And then I plotted up the data and analyzed it, trying to figure out what this really meant, and as a result, we were able to “film” the neutrons that are produced inside an accelerator, whether it's a proton accelerator or an electron accelerator, and travel out through the shield.

The attenuation distance, as you plotted it on a piece of log paper, was a typical straight line, but the slope we observed was different than what people had imagined. In fact, it was what changed Panofsky's estimate of 40 feet of earth to only 30 feet of earth. It made sense too, because the cross-section, or the interaction probability for this attenuation, turned out to be as if the nuclei that the neutrons were striking were presenting themselves as simple geometric area. If you knew the radius of the nucleus you were hitting, then you could say pi times the radius squared was the area, and that was what was called a cross-section. This turned out to be quite important, because if you can attenuate these neutrons faster, then it would take less earth to shield a two-mile long accelerator, and there was a highway that was going to be built very soon over the top of SLAC---Highway 280. And you know, we had to protect those people who would be driving, back and forth, over the top of SLAC every day. And then of course, when we went to measure for radiation, after we got the accelerator up and running, everything agreed with DeStaebler's physics predictions. So that was an important thing. And that got me started at SLAC in the sense that, you know, they made me an offer I couldn't refuse. "Come here and we'll pay for your Ph.D. And we'll get somebody to sponsor you." Which turned out to be Burton Richter, who shortly after I received my Ph.D., won the Nobel Prize for co-discovering the J/Psi particle (or charm).

ZIERLER: Ralph, when did you get interested in health physics? Was it as an undergraduate first, or that came later?

NELSON: Not as an undergraduate, but shortly after leaving the University of Washington and taking a job at the Lawrence Berkeley Laboratory. As a "junior physicist" with a Bachelor degree from UC Berkeley, I started working on the neutron-attenuation experiment for one year, before returning to the UW for my Masters degree. I finished that degree very quickly, in less than a year total, because I was able to use the work I had done the year before at LBL as my research project. I did not have you write a thesis, the published paper was more than adequate.

ZIERLER: Why was LBL brought in on this? How come SLAC could not do this in-house?

NELSON: SLAC was already very busy trying to hire physicists and engineers to fill a lot of open positions. Creating a health physics program at SLAC was certainly on the horizon. Dr. Marion Whitehead was assigned that job and the first thing she did was to get involved with the neutron-attenuation experiment that was going to take place at CERN. But SLAC did not have the nuclear emulsion equipment, the specialized microscopes, nor the expertise to use them. Dr. Whitehead had worked with the nuclear emulsion group at LBL prior to joining SLAC and she knew me as a part-time LBL employee during my senior year at Cal. That was the important connection for me. Also, to be completely fair about this, most physicists at the time were more interested in what one was calling the “frontiers of physics”. Namely, building and participating in experiments at SLAC to look for something new! What I did was tedious. Like that of many others, however, the work I did was necessary in order to make a machine that was safe to be around since it was well-shielded. Three physicists went on to win separate Nobel prizes at SLAC: Richter, Taylor and Perl. So it truly was a frontiers of physics place, and I had 40 years of fun working with the big guys of physics.

ZIERLER: Ralph, can you talk a little bit about what the scene was like when you first got to SLAC in 1964? First of all, how much was built by the time you had arrived?

NELSON: Well, when I arrived, they had basically dug the entire two-mile trench and they had poured some of the concrete enclosure that would constitute a tunnel. But they hadn't put in any of the accelerator structure. The construction of offices, laboratories and related infrastructure was partially completed, but many of us were working in facilities located on the Stanford on campus---in temporary buildings. There was a building called M1 and another called M2. The M designation stood for "monster", because that was our name for the accelerator we were creating. And 75% or more of the SLAC employees were located on campus during the early construction phase. Pief Panofsky and Sid Drell were in the M1 building. I worked in the Health Physics Department in the M2 building for almost two years before moving into a nicer facility up at SLAC’s new campus, where the group eventually split into two departments: Health Physics and Radiation Physics.

ZIERLER: And what was the scene socially in those early years? Was it chaotic? Was everybody running around? Did Panofsky sort of run an orderly ship? How did that all work in those early years?

NELSON: Well, as I continue to look back at it, it ran pretty good. People seemed to be happy. We were excited by the project. I remember one time, a helicopter came in with a large copper accelerator wave guide that was to be lowered down into this bunker near the M1 building. About 50 of us, including Panofsky, were standing outside watching this process take place. Now you may have heard, that Pief, as all of us called him, was very short. Well, when the helicopter work was finished and the spectators were returning to their offices, our electrical engineer said, "You know, that little short guy standing out there is really smart." He was referring to Panofsky, of course, and the two of them had been debating the best way of getting the wave guide from the helicopter into the bunker. Panofsky was not only really very smart, he was a good guy, very personable and everybody liked him. Everybody, of course, said that nobody could run SLAC like Pief; nobody could be as he was. But, that wasn't true. We had Richter, as our second director, and he was also great. And all the other ones too, all the way up through Persis Drell worked very hard on behalf of SLAC and the employees to produced excellent physics.

As you probably know, Pief came from Berkeley. I think he was also involved in the Manhattan Project before then, while at Caltech. At Berkeley around 1950 there was a loyalty oath they were trying to get employees to sign. Panofsky refused and that's when he came to Stanford. Robert Hofstadter was at Stanford at that time, and he had used a 1-GeV electron linac in order to discover how electrons scatter from nuclei. Hofstadter won a Nobel prize for this work. And the Stanford physics staff said, 'Well, we can make one that's 20 times bigger". So they referred to it as The Monster and it was also called Project M. A key group of people would meet at Panofsky's house in the evenings. Engineers and physicists would sit around, discuss best way to make such a big machine, and how they were going to get money from the federal government to do so. Seemed kind of informal in a sense. That was quite a few years before I got there.

ZIERLER: Now, was the plan for you to pursue a PhD from the beginning? That you would be part time taking courses and part time working at SLAC?

NELSON: No, the deal was, "You come to work at SLAC and help is create a health physics group, and we will let you take courses on the Stanford campus. We have the capability of paying for this through the AEC." The Atomic Energy Commission (AEC) had several names over the years, finally ending up as the Department of Energy. It provided money from Congress that they could be passed on to SLAC for employee education. So they said, "You can take advantage of that if you can get into a school at Stanford, or elsewhere." And I realized that I wouldn't be able to get into the Stanford Physics Department, because you had to be full time student to do that. But I did find out that I could in an engineering department, and I immediately started taking graduate courses in both nuclear engineering and physics. And then Panofsky was able to help me change to an inter-department Ph.D., instead, so I did that. It was actually three departments: Nuclear Engineering, Radiology, and Physics.

ZIERLER: That's a lot of people that you had to please.

NELSON: It was. That's true. And I mean, I had to study a lot of weird things. I had to study biology. However, I did it and I got to the point where I had to study dosimetry. And that involves all radiation, whether it's electrons, neutrons, or photons, or whatever. How it goes through matter, deposits energy, and what that means in terms of something we didn't really understand in those days, called DNA. And so I had to not only study courses with this, I had to also study mathematics for the dosimetry, and I got to the point where I said, "Well, why don't I teach other people in my group at the same time?" And so I did this with another guy named Ken Kase, and it was so good that the next thing you know, we had some 30 MDs and PhDs attending this course each week down on campus in the Medical School. And we wrote a book on it. And it became popular. So I wrote a book on dosimetry as a graduate student at Stanford.

ZIERLER: Was there a radiation physics group at SLAC right from the beginning, or this group developed later on?

NELSON: No, we had what was called a health physics group. And the health physics group, whether it was hid idea or not, certainly had Panofsky's support from the beginning. He wanted a group that was in the research division. He wanted the health physics group to be able to do research, because he had the attitude that the people who are doing the radiation protection ought to be able to work at the level of the physicists they're protecting with their shielding and analysis. So he wanted smart people. He wanted people with Ph.D.s, if he could get them. There weren't very many there that wanted to do that. They wanted to do particle physics. So he got some of us who were like a master degree-level people, and who were willing to go and do research, and maybe get Ph.D.s along the way. And we did that, several of us did that. Another one who did that is Kenneth Kase, who I managed to steal from another lab. I hired him basically out of the Lawrence Livermore Laboratory, and he later was so good that he got his Ph.D. and then went to Harvard for a number of years. He came back and became not only a group leader, but eventually Associate Director of the Environmental Safety and Health division at SLAC. So, you know, back in the early days of SLAC, we had a health physics department in the Research Division. And so therefore, we got money to do research and to travel and to do these things. Invent things. And invent detectors and new ways of doing things for radiation protection. And that came about because of the 100% support from Panofsky.

ZIERLER: Ralph, in terms of protecting people from radiation in the lab, what are the most dangerous places at SLAC if they're not shielded properly? Where are the places where you're looking at, this is where we need to be most careful, this is where we need the most shielding?

NELSON: Well, obviously in the straight-ahead direction where the beam a heading. You see, the beam that we had at that time was so intense that it could burn through a block of steel in a matter of a minute or two. And we demonstrated that even by doing that---we made a TV film to demonstrate that. And once it burned through, then you can deliver a lethal dose of radiation to people out in that area in the straight-forward direction. You could probably give them a lethal dose, everybody there, in a matter of a few seconds. Everybody would die. I mean, that's really a high-dose. So an important part of our job was to absolutely prevent that from ever happening.

ZIERLER: To give a sense of how high a dose, like a Hiroshima, Nagasaki level dose?

NELSON: Yeah, even. I mean, we're really talking about a high dose of radiation. I mean, we could deliver, I don't know, a million rads in a matter of seconds. Okay, well, it only takes 300-400 rads to kill you. So it was a very serious thing. It told us that we had to be very, very careful about not only shielding people from the target radiation, but we had to make sure that we didn't burn through the target and the shield. And we spent probably more time doing that beam-containment analysis than the normal job of shielding. So beam containment became an issue, and one of the first things we realized was that we had to develop procedures for operators and others to carefully follow. By the way, our group was called Health Physics back in those days, and later we split into Radiation Physics and Health Physics. Health physics was moved into the Engineering Department, basically, and the Radiation Physics people stayed in the Research Division. So one group had all the people with Bachelors and Masters degrees, and the other one had all the Ph.D.s, okay? The Radiation Physicists spent most of their time trying to understand and figure out ways of keeping the beam away from people, and the people away from the beam. Now, you can keep people away from the beam simply by having interlocked doors and all this kind of stuff, but you also had to keep the beam from burning through things. It doesn't matter how many doors you have locked if the beam has burned through it’s containment and is out in the research yard where people are walking around. So we spent a lot of time doing that, and we wrote a paper called the Beam Authorization Sheet as a procedure. And in there were a lot of things, both physical and procedure, for controlling the beam so that you didn't get the situation where it wasn't contained. And of course, physicists don't want to have people messing around with their time using the beam. They want to be able to do their experiments. So that meant that we as radiation physicists had to be able to communicate well with the particle physicists that we were protecting, including the engineers, technicians, and everybody in the experimental area. We had to do that, and we had to have respect. And to get that respect, we had to have people making these decisions and telling them that “this is why”. And that took intelligence, experience and education. You had to be able to tell them, "And here's the reason why you can't be in this building at this time." I mean, this is the reason why we had to put in all this extra money and time into interlock systems and ways of controlling where the beam is going to go after it fails here and there and all of sudden is someplace you don't want it, and what that means and how you detected it and shut off the beam again. And so then we had to be smarter in some sense than they were about electromagnetic and hadronic cascade physics. And that's why, for example, one of the things that I developed was a code called EGS, Electron Gamma Shower, which became a very famous code, still is. And it is still being used in medical physics dosimetry for radiotherapy on human beings. It has become the gold standard in the world, a spin-off of SLAC. Also, because particle physicists need to design new detectors, they were able to aid in their design using EGS. Because of EGS, I wasn't just a person with a radiation meter saying, "Get out of here." They realized, "By God, this guy knows more about electromagnetic cascade physics than I do and he can help me in my detector research." And that was the attitude that Panofsky was trying to foster. And I remember when Burt Richter was the director and he told us that the Radiation Physics Department was going into a different division---we were going to go out of the Research Division---he said, "Don't worry, you'll have your research function. I want you to continue to do like Ralph has done here with the EGS code, develop more things like this because this is the way to get respect from the SLAC staff. This started with Panofsky, but kept going with the other directors we had as well.

ZIERLER: Ralph, as you were developing these protocols, what precedents were there elsewhere? In other words, could you look at other laboratories or other institutions that were using radiation to get a sense of what might work and what might not work at SLAC, or was essentially everything sort of made up from scratch because it was so unique, what was going on at SLAC?

NELSON: Well, it was also about this time that Fermilab came on the scene. Brookhaven and Argonne, Lawrence Berkeley Laboratory, Rutherford Laboratory, DESY, and of course CERN were very active. I would say that the attitude of those laboratories regarding HE health physics was quite good, and I am sure SLAC set a good example for this. And it's still that way today. Particularly with CERN, because they still get lots of money (laughs). CERN is still building new accelerators and they need really good people. A lot of HPs have come to SLAC to work from CERN for extended periods. And they come here and they spend a few years and then they go back to their laboratories and stuff. And the rest of us who have done the same thing, we've gone over there and spent a year or two in each of these other laboratories to see what they're doing.

ZIERLER: Ralph, can you talk a little bit about how you developed your dissertation? I'm curious if it had a basic science component to it, or it was really pretty narrowly-directed towards the needs of SLAC at the time?

NELSON: I think both, because it turns out that what I did was to look at how muons are made. A muon is a lepton. It's one of the three leptons that we know. There's electrons and muons, and there's the Tao lepton, which was discovered at SLAC. Muons, once they're produced, travel long distances before they are stopped. For example, a 20 GEV muon, if you could make one, which we could with our accelerator at SLAC, would go through 43 feet of steel before it stops. And that's a lot of shielding, you know? So I said, okay, then I'm going to measure this: that is, understand how they're produced in a target and how they are stopped. Panofsky gave me beam time---I didn't have to get it approved by the laboratory or anything; he did it personally. He gave me a week of beam time, probably a $10,000 electrical bill to SLAC, for all I know. And I created an experiment using iron blocks and nuclear emulsions. I had a big set-up in one of SLAC’s two experimental End Stations. I had a 20 GEV electron beam hit a copper target, where muons were produced along with other particles of radiation. All of the radiation stopped within the 15-feet of iron shield down beam, except for the muons, which I detected in the emulsions as a function of the radial distance away from the centerline of the beam. What I discovered was that I could produce the exact theoretical prediction at zero degrees, but as I got farther away radially, there were too many muons than expected. Later in time, when I was working on a completely separate experiment with the physicist known as BJ, he pointed out that these wide-angle muons could be due to the production of J/psi particle (charm) and their decay. In a sense I was doing a particle physics experiment for my thesis, but it had practical significance in health physics for predicting the production and transport of muons by accelerators. As a result of this research, later on I ended up as an advisor to the site selection committee for the Superconducting Super Collider (SSC) they were planning to build in Texas.

ZIERLER: SSC.

NELSON: SSC, yeah. I was an advisor on a committee for that, because they were going to make lots of muons with such an accelerator and it would take miles of earth to stop them, the energy was so high. Someone sitting in a boat fishing on a lake many miles away might get exposed to the muon radiation from the SSC beam. Even today muons produced in a new machine, such as the Next Linear Collider people have been contemplating, are going to present a I could radiation hazard that must be dealt with. My thesis also turned out to be of interest in predicting background-signals from muons in the various detectors. I've always been lucky with my radiation physics research.

ZIERLER: I've heard a lot about Burt Richter as a physicist and as a lab director. I haven't learned about him as a graduate mentor. Can you talk a little bit about, first how he came to be the director of your thesis, and what it was like to work with him in that setting?

NELSON: Well, somebody talked him into it. It wasn't my idea to pick him. Somebody, probably my boss Dick McCall, had heard my request for an advisor and said, "Well, Ralph needs a thesis advisor. And he's not just doing a health physics type of experiment. He wants to do one that's particle physics. We need a particle physicist to cheer him and help him." And so here I am, doing high energy physics now, and somebody, said, "Well, maybe Burt can do it," and he agreed. And again, I was lucky in this regard. Turns out I was getting a future Nobel prize winner. I remember when he signed my thesis in April of 1973, we had yet to discover the J/psi particle, which is now understood as a combination of quarks. Prior to this lots of work had been done on what was called deep inelastic scattering, and they knew that something crazy was happening---that is, it looked like there was point scattering in the nucleus similar to what Rutherford discovered with alpha-particle scattering from atoms as a whole. Feynman and BJ had suggested that the scattering might be due to particles they called partons, but quarks had yet to be seen experimentally. Everybody was looking for quarks and when Burt's team saw the J/psi particles in November 1974, quarks were established to exist. They named this period of particle physics the November Revolution and it is a large part of what is now called the Standard Model of Physics. Back to my thesis, I handed it to Burt, who said, "How are you explaining these muons you see at wide angles?" And I said, "Well, I ruled out everything, so I'm writing at the end of the published paper that I might be seeing a new particle-physics process." The thesis was in the form of two papers, each about 15 pages long. The first was on the theory of muon production, their scattering, and propagation. The second paper was the measurements from the emulsions. And all I'm going to say is, since I ruled everything out that's possible for these things, then I'm seeing a new source of physics. And that's written in the paper. So maybe I discovered a new source of particle physics, but that has yet to be proven.

ZIERLER: Wow.

NELSON: For producing muons. Okay? I didn't know what the wide-angle source was. I published a paper, but unfortunately in the wrong place. I shouldn't have done it in Nuclear Instruments and Methods, but in Physical Review. My results came out in April 1973 and in November 1974 Burt's group of a dozen people saw the J/psi resonance. And of course, once you've determined that you have produced a quark, a charm quark in the J/psi case, then maybe the other forms exist? The quarks that we now understand to make up the neutrons and protons in matter turned out to be the cause of deep inelastic scattering, so Dick Taylor got a Nobel prize for that work. Did I answer your question?

ZIERLER: Yeah, absolutely. Yeah. Ralph, can you talk a little bit about how the technical challenges of your job have changed over the years at SLAC, as SLAC had moved onto different programs?

NELSON: I guess I don't understand the question.

ZIERLER: Well, the work that you did, obviously it had to be tailored to the different kinds of research projects that were going on at SLAC. So I'm curious how the work that you did changed in coordination with those different research projects?

NELSON: SLAC was a particle-physics laboratory and the health physics group I was in needed to be aware of unusual radiation fields, muons being one form. The particle physicists were also doing experiments where they tried different kinds of new detectors and stuff, a lot them were huge and costly. At the same time new accelerator ideas were being designed and Richter and a couple others were very into rings of colliding beams. They realized that they could take an electron beam and a positron beam and put them both in the same ring, and then collide them. The center of mass energy was equivalent to putting a SLAC accelerator around the earth four times. Somehow Panofsky and the DOE scrounged up enough money from existing funds to build the SPEAR ring, which led to the discover of the J/psi, and that changed the whole way the physics was going to be done from then on. The beams were delivered to into a storage ring and were made to interact in a couple different places, where you located your detector. And those kind of experiments became the way of doing things. SLAC was one of the laboratories that pushed this idea and, in order to design a complex 4-pi detector system surrounding the interaction point, the particle physicists needed a Monte Carlo code like EGS. With EGS you could design virtually all of the experimental apparatus: detector response, background noise, radiation damage, and the safety shields and facilities. Quite soon almost all the accelerator facilities in the world were building colliding beam machines and detectors. At some point in time I gave out hundreds of copies of EGS. And not too long after that the medical-physics community realized that they could study electron-photon radiation dosimetry to unprecedented levels of understanding. I have been told that EGS has been cited over 10,000 times. It definitely is considered to be a spinoff of SLAC technology and has been a great "ticket" for me. Now there are groups all over the world that have produced these kinds of Monte Carlo simulations, very similar to EGS and for hadron beams as well as for electrons and photons.

ZIERLER: In what ways, Ralph, has the research that you've been pursuing at SLAC, has been adopted at other laboratories? In other words, do you see what you were doing at SLAC as sort of setting the tone for other laboratories and what they would do?

NELSON: Well, yeah, because... this is interesting, because on my very first paper that I mentioned to you, where I did those measurements with nuclear emulsions, I knew nothing about Monte Carlo techniques, but one of the coauthors on that series of studies did. His name was Johannes Ranft and he was a professor of theoretical physics at Karl Marx University in Leipzig, East Germany. At the time, many years before EGS, Ranft was developing a Monte Carlo program, called FLUKA, for the transport of hadronic particles in matter. It still exists today and remains as a leader in the field. Around 1975, Alberto Fasso (CERN) coupled EGS with FLUKA, so that hadronic cascades could simulate electromagnetic cascades, and in 1987 Ranft and I made it possible for FLUKA to run in the opposite direction: electromagnetic cascades could lead to hadronic cascades. Since then FLUKA has been completely revamped and appears to be the computer code of choice for the study of all radiation transport physics.

ZIERLER: So we were talking about how other national laboratories and labs around the world have taken cues from the kinds of programs and research projects that you developed at SLAC.

NELSON: Yeah, right. Ranft was the most important developer of the hadronic cascades codes, the transport of neutrons, protons, and mesons through matter. Now, so as this is happening, I got to working on electromagnetic cascade Monte Carlo methods. That's the kind of beams that we have at SLAC. The next thing you know, everybody's doing it. Today, there are a lot of EM-cascade codes that are very well-written, and they're applied to not only physics of high energy, but for other things in medicine and engineering. Particle transport using Monte Carlo did start to become popular partly because of EGS at SLAC, and with Ranft, who became sort of a visiting scientist at CERN. Here’s an idea of some of the radiation-transport codes that have now been developed around the world: EGSnrc (Canada), Penelope

(Barcelona, Spain), FLUKA (CERN), MCNPX (Los Alamos), Geant4 (CERN), and most recently PHITS (Japan), just to mention the most well-known ones. I am proud to say that EGS influenced them all.

ZIERLER: Ralph, I wonder if you can talk a little bit about Panofsky's vision with regard to health physics. What did he see? How was he ahead of the curve in his emphasis on health physics, and what might that tell us about his larger vision for how he wanted to develop SLAC?

NELSON: Well, first of all, Panofsky was a very smart guy and I understand he had been associated with the Manhattan Project too, so he knew about safety problems associated with radiation. SLAC was going to be so new in concept that radiation protection should not be left for beginners. You needed somebody who has got some experience, but also with a good physics education. SLAC couldn't just hire people who knew how to make measurements with existing health physics instruments. It needed people who could develop new instruments. We had a unique problem here at SLAC because of the short pulse width. We planned to run our machine at 360 pulses per second and, at the time, one microsecond pulse widths. Geiger counters were standard for use reactors and medical facilities, but they were useless for the prompt gamma-ray fields emanating our two-mile accelerator. Geiger counters were too slow and would only tell you that you were seeing 360 pulses each second! And measuring the neutron radiation, especially the high-energy ones, was even more of a challenge.

So Panofsky brought in Richard McCall, who was an MIT graduate and a specialist in dosimetry. He was especially noted for the new field called thermo-luminescent dosimetry, or TLD. I had already joined SLAC a few months before Dick arrived to be our group leader, a job he held for something like 25+ years and was a terrific boss. He also had a great deputy named Ted Jenkins and I wrote a lot of papers and a book with him. Panofsky’s first Director of Research, Joe Ballam, told me when I interviewed for the SLAC job, "We're putting an apple out in front of you. We can't pay you a lot of money, you get more money by working for or somebody else. But what we have here are interesting problems. And they haven't been solved yet in the field, in your field". All of us in the HP group were attracted by this, witness the fact that all of us remained at SLAC until we retired.

ZIERLER: Ralph, it's clear that SLAC from the beginning placed the highest priority on being cautious and being conservative with regard to safety protocol. I wonder, though, given the uncharted territory that SLAC was in, if there were any safety concerns that only became apparent after the fact. In other words, were you able to assess all of the various kinds of problems that would come up? Or were there surprises and some maybe even potentially dangerous circumstances that came up over the years?

NELSON: Well, we had some situations like when the beam came on one time, after being off for three or four months for maintenance and repair, we discovered the hard way that someone had forgot to turn the water on to the beam dump---a relatively cool spot where the beam was to terminate before ending up going through an immense amount of concrete and earth. What little water that remained in the dump turned into steam and blew the sides out of the cylindrical container. This was immediately detected by back-up systems in place and everything was shut down. Of course, that led to an internal investigation and also one by the DOE. Over the years we had some of those kinds of situations, but for the most part, you know, nobody got hurt by beam and radiation. And the reason was because we had so many extra safety things to keep people from being near in the first place.

I remember one time when, in the middle of the night, I personally shut down accelerator operations before they got going because I saw a major flaw in the beam containment design, one that already had been approved by the Radiation Safety Committee. I got a phone call at 6 AM from Richter, the SLAC Director at the time, and I had to explain why I did this to a group of physicists. Richter backed me up and we were kept turned off for several weeks until the problem was properly fixed. So that kind of stuff happened, but not often during my almost 40 years at SLAC. All of SLAC's Director's supported us in this way, thanks to Panofsky setting the culture and tone for safety first.

There was something else in your question, I was going to answer, do you remember what the question was again?

ZIERLER: No, I mean that the tremendous amount of foresight that Panofsky put into the question of radiation exposure. What do you think that that might say about his larger vision for how he put SLAC together in general?

NELSON: Well, I'd say it's just very typical of the guy. Anybody you talk to, these people that you've been talking to and the ones you're going to talk to, are going to tell you that this guy was something tremendous, they're going to tell you that. He had real foresight and not just on stuff that I was doing, everything. He could do these calculations I did in his head somehow. He would sit in a meeting in which we'd have maybe 40 or 50 people, and somebody would be making a presentation. Pief would sit in the back of the room with this little smile on his face, and he'd be nodding his head just a little bit in agreement with the presenter. All of a sudden his smile would disappear and he would shake his head, because he would be listening carefully, understood everything that was being said, and didn't agree.

ZIERLER: Ralph, for my last question, I want to ask you... It's not so much a technical question, it's more like a sociological question, given your long tenure with SLAC and you being there really almost right at the creation. And that is, in what ways has SLAC over the decades stayed true to Panofsky's and then Richter's initial vision, and in what ways has it changed? Both sort of culturally and scientifically, in your view?

NELSON: Well, I think SLAC's original culture has followed the science. I think the attitudes after Panofsky remained the same, with Burton Richter, Jonathan Dorfan, and Persis Drell as directors. They recognized what Panofsky was trying to do there when he established our group, and other safety things that needed to get done, the laser safety and all of things like that. What happened, of course, was that SLAC had contributed very significantly to our current understanding of the Standard Model of Physics, with three Nobel Prizes. In my opinion, there was not too much more we could do in high-energy physics with the Two-Mile Accelerator. However, the Accelerator Physics Department at SLAC had made great contributions to a then growing field of research using synchrotron light, with its low-energy, not high-energy, experimental beam lines. Again, SLAC was a pioneer by turning the SPEAR storage ring, where the J/si and the Tau lepton were discovered, into a famous place for utilizing synchrotron radiation called the SSRL (Stanford Synchrotron Radiation Lightsource). In hindsight, it seemed obvious to me, that biological and material science would be important for the future of SLAC. Synchrotron light from a storage ring was originally considered to be “junk radiation”, but the SSRL proved differently. And that is what has happened in the 20 years since I retired from SLAC. They still design new beam lines at the facility, but no longer for high-energy physics.

I only can hope that the original laboratory culture invoked by Pief Panofsky still remains with the new, modern SLAC.

ZIERLER: Well, Ralph, I really appreciate you sharing your perspective with me. It's been great talking with you, and this is going to be a really important perspective to add to this project for SLAC. So, I want to thank you very much for your time.

NELSON: Glad to have chatted with you.