SMB
Richard Mount
Video conference

by David Zierler
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DAVID ZIERLER: This is David Zierler, oral historian for the American Institute of Physics. It is November 9th, 2020. I am delighted to be here with Dr. Richard Philip Mount. Richard, thank you so much for joining me today, and it's good to see you.

RICHARD MOUNT: Good to see you, David.

ZIERLER: To start, would you please tell me your most recent title and institutional affiliation?

MOUNT: Yes. My institutional affiliation is SLAC National Accelerator Laboratory. I retired from SLAC in August 2017, but I've still been connected with SLAC since then, doing quite a number of reviews and helping with some planning, for computing particularly, for the SLAC science program. So if you actually look in the StanfordWho, you'll find I am a casual temporary—

ZIERLER: [laugh]

MOUNT: —employee of SLAC National Accelerator Laboratory. Which means, without any formality, if I do some work, they can pay me for it, and so on. So my most recent title was I think head of EPP computing at SLAC, EPP being Experimental Particle Physics. In history, I've had a lot to do with computing at SLAC. I was hired to become the director of SLAC Computing way back in 1997. And, well, we'll talk about how all of that played out.

ZIERLER: Richard, before the pandemic, even after you retired, were you still going into SLAC on a regular basis, or even by then, you were mostly working remotely?

MOUNT: Well, one of the interesting things about working for SLAC is that I had of course continual friendly discussions with SLAC about money, because it's fantastically expensive to live in the Bay Area.

ZIERLER: [laugh] Yes, indeed.

MOUNT: And after retirement, it was clear from long ago that we would be as poor as church mice if we stayed in our home in the Bay Area, and we would be very comfortable if we went almost anywhere else in the universe. So now we live 475 miles away from SLAC, just a sort of not-too-stressful day's drive away. Before the pandemic, my wife and I were making quite frequent visits, because she had been working for Stanford, I had been working for SLAC, and we had lots of things to do in the Bay Area. So I was frequently present at SLAC in spite of living 475 miles away. Since the start of the pandemic, we, as two 72-year-old people, have been living in almost total isolation in our modest home here, which is of course vastly bigger than our home in the Bay Area. [laugh]

ZIERLER: [laugh]

MOUNT: Vastly less expensive. And we really have had remarkable little contact with anybody, except over Zoom.

ZIERLER: What's your wife's field? Is she affiliated with SLAC as well?

MOUNT: No, she was in the medical school at Stanford University, essentially doing work on teaching physicians to teach better. [laugh] Which is an interesting challenge, especially with surgeons. [laugh] She retired a few months after I did, of course prompted by the fact that we had to more or less synchronize our retirements. But then she continued to also be a casual temporary employee of Stanford University, to tie up loose ends. We have both largely stopped actually formally working for SLAC or Stanford. And as you probably know, all SLAC employees are Stanford employees, which is very interesting in many respects. We stopped that, but I certainly am asked to do various things by various people around the world and in the U.S., and generally I'm doing these for nothing, but maybe for expenses, of course. It's interesting to keep some relationship with the worlds we used to live in.

ZIERLER: Richard, let's take it all the way back to the beginning. I'd like to hear first about your parents. Tell me a little bit about them and where they're from.

MOUNT: Okay, so I am—you probably guessed—English. My wife is English, too. My parents were—one from the south of England, in London, and one from the West of England, Bristol. My father's family was made up of a Lancashireman, as my grandfather on that side, and an Australian, who was actually Irish-German-Australian, on my grandmother's side. I know my grandfather worked as a furniture designer, so that was some artistic capability in the family on that side. My father grew up around London, went to a boarding school in the south of London, and went into various forms of business. By the time I was born, he was largely in charge of sales for what is essentially an auto parts manufacturer in the north of England. Made conveyor belts and auto parts and all sorts of things, a lot of them made from asbestos, which was interesting. I remember as a child playing with lumps of asbestos brake materials. [laugh]

ZIERLER: [laugh]

MOUNT: There's a lot of resin in them, and they don't shed any asbestos—or not much, anyway. You know. [laugh] This was a long time ago, when these things were not understood to be as dangerous as they are. My mother, like I say, grew up in Bristol, of Yorkshire parents who had moved to Bristol for work reasons. Her mother died when she was quite young, but she was a seamstress. She made clothing. Her father was a millwright, the engineer that kept factories running, if you like. Her father was quite a person of strong and persistent views about everything, and I think my mother was quite glad to move back to Yorkshire and pursue a nursing career in Yorkshire. She delivered babies during the bombing of the Yorkshire industrial towns, remembering that you could take a taxi to go to the home where the baby was to be delivered; you had to walk back.

ZIERLER: [laugh] Richard, where did you grew up? Where did you spend your formative years?

MOUNT: I grew up in Yorkshire. This is largely because the company that my father ended up working for, for all the time since before my birth, existed in Yorkshire, in the industrial part of Yorkshire. I grew up in the rural part of Yorkshire, because my father preferred to drive for an hour and a bit each way, every day, rather than have his family grow up in industrial Yorkshire. Which you could understand if you knew what industrial Yorkshire was like at the time—dark, satanic mills, and so on. Very much an unhealthy place to live.

ZIERLER: And your home was more of an urban environment, or a suburban environment?

MOUNT: A rural environment, almost. I grew up in a village of a few hundred people. Starting off in a town of about 2,000 people, so hardly suburban. I remember our telephone number was Wetherby-4-2.

ZIERLER: [laugh]

MOUNT: You see how many people had phones?

ZIERLER: There you go! [laugh]

MOUNT: When I was quite young, they had to add some digits, and it turned out to be Wetherby-2-0-3-2 after that.

ZIERLER: [laugh] Richard, of course the British class system is poorly understood by Americans, and it's a rather class-conscious society. Growing up, how did you understand your family's class status?

MOUNT: I think I understood us to be in what would definitely be called the 5%. Probably not the 1%. That my mother came from a background of highly educated working-class people. They would consider themselves working class, like the millwright, but regarding education and knowledge as absolutely vital, and trying to be more educated than the higher classes, if at all possible. My father came from a clearly middle to upper middleclass background. His mother from Australia came from a family that owned sheep farms and so on and considered themselves perhaps higher class than I am comfortable to see them. I think my father just considered himself a businessman. I don't think he considered he had much class. But I remember he didn't really like to go to movie theatres, for fear of catching fleas.

ZIERLER: [laugh] Richard, was your family's economic position, did you feel that it influenced the kinds of schools that you went to, as a child?

MOUNT: In a sense, yes, and in a sense, no. My grandmother, on my father's side, her religion was Roman Catholic, and that percolated down to my father and to our family. My mother was a Methodist, so they had an interesting sort of intellectual relationship about religion. But from an early stage, I first of all went to the local primary school, walking distance from my home. And I remember having the girls help me tie my shoelaces and learning to read difficult words like "and" and so on, in primary. But I was from, I think before I was born, put down for a Catholic boarding school to go to, as a high school, secondary school. When I was approaching the time to start that, my father and mother decided that I should also do the state examinations that would enable you to go to the upper tier of state schools, the grammar schools. That was called the eleven-plus examination. It was a sort of intelligence test, rather than anything else, probably quite SAT-like, but to be done for 11-year-old children to segregate them into the smart ones and the dumb ones, if you like. The smart ones to go to grammar schools, and the dumb ones to go to secondary modern schools where they might learn to be woodworkers or metalworkers or something like that. It was an intellectually snobbish system, in a way. For some reason, my mother and father discussed the fact that I was taking this examination with the headmaster of Ampleforth College, the Catholic school, and his reaction to that was so negative they decided, "We're not sure we want our child to go to such a school." And so I went to the semi-local grammar school. It was selective enough that I had a half-hour bus ride each day, because I was in rural Yorkshire, so the population density wasn't very high. But I went to Tadcaster Grammar School, very proud to have been founded in 1557, I think, and known officially as Bishop Oglethorpe's Grammar School, Tadcaster. And there, they had some nongrammar school kids, and in each year, they segregated children into eight streams—A, B, C, D, E, F, and so on—where A was the top, and H was the bottom. And whether that's right or not, that worked quite well for me. I was in stream A and prospered quite well with the fairly rigorous education offered by this school. My mathematics education, my geography education, my English education, was all done by very serious teachers who I think were comfortable teaching able students. Maybe they were also comfortable teaching others. But it was interesting. And I succeeded—the school supported me in applying to go to Oxford University at the end of my schooling, which involved a separate set of examinations beyond the normal English state examinations that you take at the end of your secondary education, the O levels and the A levels. I was pointed in that direction partly because the headmaster of the school had been to Oxford University, and so he encouraged that. I did well enough to be offered a scholarship to go to Oxford University. The scholarship was a little bit of money, largely meaningless, but it came with some extra status when you got there. And as the English system is, I decided to study physics. You don't get to study a mixture of things; you just choose what you're going to do. And physics involved of course mathematics and engineering, and a fairly broad curriculum for at least the first year or so. But I was quite happy. That was something that I had been interested in since I was a young child, quite interestingly, spurred on by my father, who was very fascinated by science even though he hadn't had a scientific education.

ZIERLER: And why, of all the scientific disciplines, physics?

MOUNT: It's the closest, if you like, to fundamental truth. Chemistry is chemistry. It builds on physics. Not in a practical sense, often, but certainly in a logical foundation sense, you need all of physics before you can have chemistry. And of course, biology is almost like social science, if you like, to a physicist. It was definitely the way I thought I was going to get the best understanding of the fundamental way the world works, and that was what fascinated me. Mathematics was fun, but it didn't have the same fundamental relationship to the actual world we live in. And I did know at least one of my predecessors at Tadcaster Grammar School who went to Oxford and shocked me by saying it was really the beauty of the theory that mattered, not whether it had any relevance to the real world. [laugh] I couldn't get over that. He was a mathematically-oriented type.

ZIERLER: Richard, of course in the English system, when you're thinking about college, you're committing to a field of study right from the beginning. There's none of this general education and then you settle on a major.

MOUNT: Yeah.

ZIERLER: So this is to say that when you were considering Oxford, you were specifically considering pursuing a degree in physics at Oxford.

MOUNT: Yes, yes. Now, in the English system, you do get a year or so—about a year extra, in high school. I was doing mathematics that was quite advanced calculus and so on for my final examinations in school, and following a reasonably broad curriculum in school. Although for the last two years, it had narrowed down to essentially maths, physics, chemistry, and some small amounts of English and so on, just to make sure you could pass tests in English if you were given them, and things like that. So even from the last two years of high school, the focus narrowed down. Things like geography, history, and so on, which I had up to the two years before going to university, they all vanished. So for my first set of state examinations, the O levels, I did 11 subjects I think, so 11 examinations and results. For my final high school examinations, the A levels, I did four, with two additional special papers at a higher level in mathematics and physics.

But essentially four subjects. Well, in fact three, because [laugh] the subjects were maths, further maths, physics, and chemistry. And further maths was considered a separate subject, because emphasis on being very mathematically competent before you escaped from high school.

ZIERLER: On the social side of things, the late 1960s, at least in the United States, was a quite interesting time to be an undergraduate. What were your experiences in Oxford with regard to global movements like civil rights and racial rights and anti-Vietnam War sentiment?

MOUNT: For me, I suppose maybe I had my head in the sand, but not much engagement in the global reaction to the Vietnam war and so on. I was a member of the Oxford Union Society and remember interesting debates like the Reverend Ian Paisley defending the way he approached politics in Northern Ireland, for example. And I think Clinton was there for some of the time that I was there. But I wasn't really very engaged—I would say not at all engaged in anything that you would call political. And I didn't feel there was a big current of political awareness and involvement around me at the time.

ZIERLER: Who were some of the luminaries, at that time, in the physics department?

MOUNT: Well, I suppose one of them was my tutor and the guy I nearly went to work with to do my graduate studies, John Theodore Houghton. You probably haven't heard of him.

ZIERLER: No.

MOUNT: He was an atmospheric physicist. Did become—I can't remember—he went to almost essentially the top of the U.K. scientific establishment after I had known him, ran a very successful department of atmospheric physics, and it was very tempting to join him. There were people like Rudolf Peierls in physics theory, Don Perkins in particle physics. Have you ever heard of Don Perkins?

ZIERLER: I have. Yes.

MOUNT: He had a book. He wrote a book.

ZIERLER: Yes, yes.

MOUNT: Makes people remember you. [laugh] He was a great character. Very good to hear lecturing and also good just to have coffee with, and so on. In a way, it wasn't an astoundingly notable time in physics at Oxford, at least in the parts of the physics department that I can remember. There was also a strong low-temperature physics part of the physics department, and I'm struggling to remember the names there, some of them quite well-known.

ZIERLER: Richard, was the culture of the department such that as an undergraduate, you can develop meaningful relationships with your professors?

MOUNT: Not terribly. Because the point at which you could actually get involved in research was only after you had done this fairly intense three-year degree, which didn't leave much time for doing anything other than books and the compulsory lab work and so on, but not actually getting involved in research. And one of the things that put me off about Oxford was that the Oxford nuclear physics department, which encompassed particle physics and nuclear physics—there was a Van de Graaff accelerator at Oxford doing nuclear physics—they wouldn't allow you to sign on [as a graduate student] with any fixed plan of what you were going to do or who you were going to work with. If you signed on there, you signed on, and then they would tell you—presumably with some interaction with you, but it wasn't clear—they would tell you what you were going to do. And it might be particle physics, it might be nuclear physics. It wasn't quite what I was comfortable with. I would have liked a better personal understanding of who I might work with. And towards the end of my undergraduate career, I did look around the country for various places to go, and many of the places with less illustrious sort of names than Oxford, like Bristol, had people who would really engage with you and say, "Really would love for you to come and work with me, and this is what we would do" and so on. That was very attractive. In the end, I ended up going to do a PhD in Cambridge, which isn't quite—isn't really a step down, but the Cambridge particle physics department was, in a sense, a step down from the Oxford one, at the time.

ZIERLER: Generally, or in the field that you were interested in pursuing?

MOUNT: Particle physics, I would say generally, yes. Yes. Not a large step down, but a slight step down. And the reason that worked for me was that I had met my [future] wife at Oxford. She was doing a zoology degree. And she had been also doing some work—actually, one thing you have, if you went to Oxford or Cambridge, you normally had a year after taking the entrance examinations—most of a year before you actually took up your place. And during that year, you could do all sorts of things. I worked for a computer company in industrial Yorkshire, when computer companies barely existed. But Jane went and worked with particularly people in Cambridge on the behavior of birds and things like this. And by the time she had finished [her undergraduate degree], she had an offer to go to Cambridge, and wanted to go there. And I said, "Fine, I can go to Cambridge, too." And so I did.

ZIERLER: By the time you completed your undergraduate study, how well-defined was your identity as a physicist? In other words, did you enter graduate school knowing if you wanted to pursue experimentation or theory? And if it was particle physics that you were absolutely set on at that point?

MOUNT: I knew I wanted to pursue experimental particle physics. Particle physics I think because of some notion that this is the most fundamental end of experimental physics that you can do. And the fundamental nature of it appealed to me. And experimental because I—there was a sort of saying at the time, which is of course untrue now, that any experimental physicist ought to be able to fix his own car.

ZIERLER: [laugh]

MOUNT: Of course, you can't fix your own car now, because they're full of computers. [laugh] But I was somebody who liked to fix his own car. And I wasn't comfortable stepping away from hands-on, greasy fingers and so on relationship with the science.

ZIERLER: Plus, it's in your family.

MOUNT: Yes.

ZIERLER: Was there a professor in particular you wanted to work with at Cambridge?

MOUNT: I wouldn't say particularly. People there included John Rushbrooke, who was the head of department, and Janet Carter, who was a good person to work with.

ZIERLER: What was the mix between lab work and course work as a graduate student?

MOUNT: It was I'd say about 50% course work for the first year, certainly, then trailing off. And the course work was fairly broad. I remember frankly struggling through lectures on general relativity at the Department of Applied Mathematics and Theoretical Physics in Cambridge.

Those were given by true mathematical theoretical physicists whose brains seemed to at least work on the blackboard faster than mine. [laugh] At least in those subjects. So most of the coursework was highly theoretical physics, in fact, to counterbalance the more hands-on work being done in the lab. The group I was with in Cambridge was focused mainly on bubble chamber physics, meaning the lab work was to get ladies of various ages to measure things on film. And the skills required were motivational as much as they were physics. This is enormously boring work for people to do. Far too boring for graduate students; they wouldn't do it right at all. So the measurement of the film or the tracks on the film that gave the physics results was essentially almost all done by hired help, and they were almost all married ladies who could afford to work for the small amount of money that the physics department would pay them to sit for eight hours a day using semiautomated machines to measure tracks on bubble chamber film. During that time, one of the stars of the group in Cambridge was Otto Frisch, who was a wonderfully irreverent guy. He helped design a machine that would automate the measurement of bubble chamber film. It was at the time when there was a way to do it called a Hough-Powell Device, which essentially would digitize the whole thing, everything on the film, and then use sledgehammer computing methods to find the tracks in it. And because computing was not what it is today, that was a very expensive and inefficient way [laugh] to find the tracks in bubble chamber film in those days. The Frisch department designed a thing called Sweepnik, after Sputnik, that used a laser optically changed into a rotating line that would rotate about its end and show blips as it passed over tracks, on the film, and then using steerable mirrors could very rapidly follow along each track and measure them to a few microns, very precisely on the film. Part of what I did was using measurements on this device, but still a lot were done on machines with handles and Moiré fringes to measure where the handle was, or how much it had moved the film carriage, and then projecting it onto a table with a sort of cross on it, so that you could move along a track and press a button every time you wanted to record a measurement. Now I did go to CERN and take bubble chamber film for the experiment that I was doing.

ZIERLER: This is before you defended your thesis, you mean?

MOUNT: Oh, yes, this is before. Almost within a few months of arriving in Cambridge, the whole group went off to CERN to do a weird and wonderful experiment, where using the two-meter bubble chamber at CERN, we exposed a copper coil of a magnet just near to the bubble chamber to an enormous blast of protons, and then used a pulsed magnet, discharging a huge capacitor bank into a few-turn coil of copper to create a very high field—I can't remember how many Tesla—but to sweep away all the charged particles created in this collision, and sweep them away just 50 centimeters away from the bubble chamber. And then into the bubble chamber have a primarily neutral beam, which means you could study the collisions of lambdas and kaons [with the protons in the bubble chamber], which you couldn't do before in any sensible way, because they didn't live long enough to have this happen. So the aim of this was to study lambda-proton interactions. This was essentially my thesis topic. The film was mine, if you like, which was quite interesting. I owned it right from being there in the [user] control room next to the bubble chamber control room, and staying up at night and getting this film in the can, then having it shipped back, and then getting people to measure everything on it, and designing ways to get the information out. Because of course you have nothing [visible] coming in, and then things happening in the picture. A lot of the data came from collisions of neutrons with protons and then making lambdas in the chamber, and then the lambda interacts further on. Getting those reliably out, because there's a lot of neutral tracks in there, and measuring them, is a little difficult. Getting those reliably out was interesting, and involved a multistep process of measuring things and then going back to see what they could have been pointing to, and so on. And it involved a lot of interaction with the people who were doing the measurement.

ZIERLER: Richard, I'm curious—of course the early, mid-1970s was a fantastically exciting time for experimental particle physics, generally. I'm curious the extent to which you were aware of what was going on at places like Brookhaven or SLAC in the United States.

MOUNT: I would say I became aware during that period. Especially at the end of my PhD research, when I went to CERN as a CERN fellow, I became very aware that I had been living, at least my brain, in an isolated part of the universe. That I was not aware of the nucleon structure information coming out of SLAC. This was before the J/psi information. But I remember as a CERN fellow, early on going to a CERN summer school and learning about all this new stuff, and being quite embarrassed about how ignorant I had been. [laugh]

ZIERLER: [laugh] Is your sense looking back that in some ways Cambridge might have been a bit parochial in its physics purview?

MOUNT: I think so, yes. Yes. I don't want to badmouth them. I mean, in terms of getting me a PhD, it worked just fine. In terms of bringing me fully up to date of what was going on in the world of particle physics, it wasn't so good, yes.

ZIERLER: Given your research already at this point at CERN, was that a very natural progression for your postdoc?

MOUNT: It was fairly natural for an experimental particle physicist to try for a CERN fellowship. Even then it was very clear that the other facilities available to me in the U.K. were either no longer available—they had been shut down—or were not very capable, and that CERN had all the experimental capability that was really interesting in particle physics. So it was something I definitely wanted to do. And I did say that I went to Cambridge because of Jane, who later became my wife while we were in Cambridge. That was the last time that she got priority, I'm afraid. After that, she [pause] followed me around with a sort of high-class gypsy existence of an experimental particle physics physicist. Not always badly paid. The CERN fellowship in those days was a large multiple of what a graduate student got in the U.K., very large multiple—and even though the cost of living was high, it was a transformative experience to actually earn some real money. Somebody gave you some real money!

ZIERLER: Was the plan at CERN—was this an opportunity, as you saw, to expand and improve on your thesis research, or were you looking to take on new projects?

MOUNT: Oh, I was looking to find out new things and do new things. Yes. And as a CERN fellow in those days—and I think even still these days—you get a license to tour around CERN and have people try to make you want to work with them. That's really nice.

ZIERLER: So you didn't enter with a specific group to work with.

MOUNT: I didn't enter with a specific group, no. I think I had by that time decided that probably bubble chambers weren't the best thing to go forward with. But I ended up working with a sort of medium-sized group called the CERN-Munich group, let by Bernard Hyams, not a big name. This was a group of various nationalities, but with a lot of Germans from Munich. And Germans from Munich are nice people. [laugh] Let's say—they're relaxed people. [laugh] They enjoy what they do, and they enjoy life, and they don't pretend to be like Hamburg Germans. I did work later on in Hamburg for a while, and it was great, but they're different. [The Munich people made it] just a really attractive group and welcoming group to work with. They were doing spark and proportional chamber measurements—again, rather related to the physics of lambdas. [laugh] So it was the last gasp of the hadronic zoo physics if you like, I would say very much just about the last gasp of that. But it was a good group to work with, and I learned a lot more about detector technologies and applying computing to physics than I had known before. Of course bubble chamber physics involves quite a lot of computing, but it's mainly running programs that somebody else wrote quite a long time ago. The reconstruction programs and so on. The so-called counter experiments, [using] spark chamber, proportional chambers and scintillation counters—generally, they had software that was written for the experiment itself. I got to write quite a lot of this, and was able to use some mathematical techniques that I picked up from a mathematician working with the group, a guy called Vincent Chabaud, and was able to pull out results that nobody thought lay in the data. Because they hadn't been able to use mathematical techniques to extract all the information from the data. And you found peaks coming out that nobody had thought you could possibly measure in this experiment. That was great fun. It wasn't great fundamental physics, but it was great fun, and in itself a very interesting and I think good training, not only in the techniques of computing and the techniques of experimental physics, but also working in a group of people, on a medium-sized experiment.

ZIERLER: Now, the postdoc was a set two-year fellowship, or you could have stayed on for longer?

MOUNT: It was a two-year fellowship. You could extend it for a few months, but it was essentially a two-year fellowship.

ZIERLER: What opportunities did you have at that point, when you were wrapping up the postdoc?

MOUNT: I was interested in staying on for a CERN staff physics position, because it was quite nice living in Geneva, it turned out. Our first son was born in London, six months before we left the U.K. for Geneva. Our second son was born in Geneva. So when that happens, you put down some at least tentative roots. A mistake as a physicist, but you put them down. And it seemed attractive to stay. But the CERN staff positions, they're pretty competitive, and they don't last for [laugh] all that long, anyway. CERN, by construction, is not allowed to employ many tenured physicists. Because they're a service to the universities; they're not there to do the work themselves. I cannot remember what applications I did, but the one that was successful was to apply to go back to Oxford, or at least to join an Oxford group that was working on what was called the European Muon Collaboration [EMC].

ZIERLER: When did that collaboration start? Was it just getting underway at that point?

MOUNT: It was essentially just getting underway, yes. I don't remember the history, but it had been probably formed a year or so before. [*Mount noted later:* Actually, EMC was proposed in 1974 and I joined in 1978]. But they proposed—the new accelerator that CERN was building, the superproton synchrotron, that they could use an extracted proton beam to create a muon beam. And part of the whole ingenuity here was to design how to make a muon beam from the proton beam. This was a huge mile-long-or-so transport system inside an underground tunnel that focused, defocused the muons, and gradually made this spray of fairly uncorrelated muons coming out of an initial collision with a target get focused down to a 280 GeV muon beam of quite significant intensity—ten to the seven muons pulse or so. The experiment was—first you designed the beam to make the muons, and then you design an experiment to do scattering of the muons. So this was definitely leaping into the SLAC, if you like, era of physics, because this was using muons to study nucleon structure as opposed to electrons to study nucleon structure, and muons at a far higher energy than the electrons had been. So getting to much lower *x*, much more detail of the structure than could be done at SLAC. By that time, I think I had latched onto the fact that this was really interesting and pretty fundamental stuff, and this is something that I was excited to be doing. It was the largest physics collaboration in the world—99 PhD physicists.

ZIERLER: What was your title? Are you a faculty member for this?

MOUNT: For this, I was what Oxford University called a research officer, meaning I was a postdoc.

ZIERLER: A glorified postdoc, it sounds like.

MOUNT: No, just a postdoc, right? [laugh] They didn't call them postdocs; they called them research officers. [laugh]

ZIERLER: And what was your specific role in this collaboration? What did you add to the project?

MOUNT: Well, my group leader at Oxford, a guy called Bill Williams, he had a responsibility for designing and constructing the veto wall. So you've got a beam of muons coming down reasonably on the axis of the experiment, but you also had a whole shower of junk because of the initial collisions of the protons a mile or so away and all the things that happened as neutrons and so on collided with everything. Mainly a shower of neutrons [and muons] coming down as well, that would illuminate the whole detector, if you didn't have some way to say, "Take no notice of these particles." So there was a wall which was about the size of a two-story house, all plastic scintillation counters, that was there to say, "No, take no notice of this, because it's just junk coming down at this time." So that was fairly simple, but it involved testing and commissioning about 100 scintillation counters and their photomultipliers. All the photomultipliers were secondhand, because of course you don't throw away a photomultiplier when you use it. So they were all of variable quality and it took extensive measurements and so on to commission the photomultipliers. The scintillation counters were of variable quality, and so we took them all to a beam at the CERN proton synchrotron to calibrate them and to make sure that they would be adequately efficient. We even published papers on highly efficient scintillation counters, which was interesting in those days. I'm not sure that it is now. These scintillation counters were, I think, a meter by 50 centimeters each. So then you built them up into a wall—and we also had a to build a wall, so we had to build a support structure for this. The initial design didn't have quite thick enough or high-quality aluminum alloy, and [laugh] started to sag. So we had various senior theoretical physicists helping build this, one of whom I dropped a large aluminum scaffolding buckle onto the head of, where he wasn't wearing a hard hat. He didn't seem to suffer much beyond a painful lump on his head, but that scared me a lot. It made me realize that that could have been a very serious accident.

ZIERLER: Given the size of the collaboration, I wonder, to zoom out for a second, what were some of the major conclusions of this endeavor, and how did they relate to what was going on more generally in the field of experimental particle physics at this time?

MOUNT: The measurements were that of the structure function, mainly F_2, of protons and neutrons, which is the thing SLAC had been measuring as well. Later on after I was there, polarized beams were also used to get F_3 and so on. But this was F_2, essentially the distribution of components of a nucleon in momentum space. We used a number of different targets. There was hydrogen, helium—sorry, hydrogen and deuterium. And iron. One of the first targets used was iron, because it was thought when the beam wasn't as intense as it would be later, an iron target would be very good, and it could be made active with scintillators in it and so on. So one of the things that we were excited to discover—I don't know how much it excited the world—the F_2 measurements were fundamental, showing that indeed the nucleons were made up of infinitesimally small quarks, and they were much more infinitesimally small than they had been shown to be at SLAC. But it was really fascinating that everybody believed that iron would be about 28 times deuterium, and what actually happened was when we started measuring, we saw that the results from iron were quite different from those from deuterium.

ZIERLER: Which tells you what?

MOUNT: Which tells you that the approximation that iron is 56 independent [nucleons]—that the nucleons in iron are bound in a way that doesn't really change very much their internal structure is completely untrue. And that was the basic back-of-the-envelope model of the structure of a nucleus, was it was [made up of] fairly independent protons and nucleons. They sort of stuck together with a bit of gluon effect and so on, but they weren't markedly changed in structure. But we found that the iron structure was really quite different. This was a good introduction for me to systematic errors. Because the beam was so intense, and the backgrounds were so intense in this muon experiment that the proportional chambers started to die. All sorts of things happened to them and they got whiskers growing on the wires and so on. So it was very difficult to take out the systematic effects of these proportional chambers dying, and so it was very tempting not to accept the results that the iron run and the deuterium run were producing very different results. So this was sat on for nearly two years of intense study, essentially trying to get rid of the anomalous results. Finally, not succeeding in getting rid of the anomalous results within anything that we could understand were the systematic errors—the considerable systematic errors of the experiment, and so finally publishing them. Which was clearly the right thing to do. I went through the whole of the angst of the experiment about this, of the collaboration. I don't think the whole world was looking on, because this wasn't an earth-shattering result, but we had to believe either we just didn't know how to do this experiment, or that we had found something that we didn't expect and would produce a minor revolution in nuclear physics, if you like. Which didn't interest me at all, but [laugh] that was fine. It did indicate how difficult it is to really be scientific about your systematic errors. It gave me a strong focus—after that, I went on to do an experiment at LEP, and I—there was a joke going around which I definitely resonated with, that there were five LEP experiments, because one of them is the Standard Model, and they all have to agree. If you don't agree with the Standard Model, you don't publish.

ZIERLER: [laugh]

MOUNT: [laugh] And clearly, that was wrong.

ZIERLER: Your involvement with LEP was still as a postdoc at Oxford, or this was a separate position?

MOUNT: My involvement with LEP was a separate position. In fact, before going to LEP, I spent some time in DESY at Hamburg. Towards the end of my postdoc position, which was a fixed-term position, at Oxford, I looked around, and most of the opportunities seemed to be in the United States. It wasn't a good time—I'm a Baby Boomer, and there were far too many people in the U.K. looking for a very small number of jobs. And the way the U.K. worked, you either had to get an essentially permanent job right off the bat, or not at all. It's not as bad as Italy or France, where they have a problem that you can't be accepted as a graduate student unless you have a career path until the end of your life. But it was fairly bad. So I looked around, and I applied to quite a lot of places. And I got offers actually from Notre Dame, which I visited, and they were very keen to give me a job as an assistant professor, and from Caltech, where Harvey Newman, forming a new group there, was very keen to give me a job as just another postdoc.

ZIERLER: This is after DESY, or before DESY, when you were applying for these positions?

MOUNT: This was before DESY. To cut a long story short, I found the dynamism of the Caltech Newman and actually Ting environment that Harvey was involved with to be exciting enough to outweigh the precariousness of the job, at that stage. So decided to jump on that, but not to go to Pasadena, but to go to DESY.

ZIERLER: To work with Sam Ting?

MOUNT: To work with Sam Ting.

ZIERLER: Had you met him before this point?

MOUNT: Well, I met him before I took the job [laugh]—you can be sure of that—where he made his classical comment, "Everything they say about me is true." [laugh]

ZIERLER: [laugh] "Watch out." [laugh]

MOUNT: And—yeah. It was not scary working for Sam Ting, but it was strange, in many ways.

ZIERLER: Intense, I imagine.

MOUNT: It was intense. And Sam could be very nasty to people. But one thing I particularly liked—he was more nasty to senior tenured physicists than he was to graduate students. In general, much more nasty to the senior tenured physicists.

ZIERLER: Among his peers, he would not suffer fools.

MOUNT: No, he would not suffer fools. And, you know, there were plenty of them around, [laugh] in general.

ZIERLER: What was he doing at that point, at DESY? What was the research then?

MOUNT: Well, he was doing the MARK-J experiment, J being the [particle discovered by his] experiment at Brookhaven, and then he moved to DESY to discover top. Built an experiment called MARK-J at the—now I'm trying to remember the name of the DESY accelerator, which has somehow gone out of my—but anyway, the DESY accelerator which would eventually get up to about 40 or so GeV center of mass. He designed this experiment, got a lot of electronics, which turned out be very flaky, from Walter Lecroy. You're probably too young to remember Walter Lecroy, but—

ZIERLER: I don't know him, no.

MOUNT: Counting rooms at the time were full of blue NIM modules with Lecroy written on them. A very successful specialist builder of modules for high-energy physics. We had a lot of ADCs that were designed and built by Lecroy [for Sam's experiment]. Unfortunately, it turned out they would only work reliably at about 40 degrees Fahrenheit, or something like that, in the counting room. So the counting room had to be cool, such that in the middle of a night shift, however many clothes you owned, you couldn't wear enough to stop feeling cold.

ZIERLER: [laugh] And this was the experiment that you joined Sam on?

MOUNT: Yeah, I joined on with Sam. Worked there for two years. We took quite a lot of data. We didn't of course discover anything. It was doing physics in the desert. And essentially none of the experiments there discovered anything other than just measuring the hadron production cross-section at an e plus e minus collider. But it was interesting. Towards the end of that, I became much more involved in Sam Ting's efforts to have an experiment at the LEP accelerator. So seeing nothing coming out of it, he was losing interest in the DESY experiment, and certainly I was losing interest in the DESY experiment. So I spent quite a lot of time living in Hamburg but commuting to CERN to begin the process of the L3 experiment at CERN.

ZIERLER: I know that throughout his career, Sam has worked closely with theorists. So I wonder both at DESY and at LEP if you had a sense of the ways in which theoretical advances in particle physics might have influenced some of the suppositions or some of the hopes in terms of finding new physics in these experiments.

MOUNT: I was aware of relations with theorists, but I would say my impression of the driving force behind Sam was that conviction that measuring muons would reveal anything that was interesting. That was his approach to some extent in MARK-J, and certainly his approach in L3, which was dominated by its muon-measuring apparatus. The magnet that did that is now the ALICE experiment's magnet. But, huge amount of money and cubic meters went into the muon measuring apparatus at L3.

ZIERLER: Is your sense that if DESY had been more successful, that L3 never would have gotten off the ground?

MOUNT: Could have been, yes. I wouldn't say it’s a definite sense, but if DESY had become a top factory, for instance—and of course as an e plus e minus machine, with what we knew at the time, that was quite possible—why go away from that?

ZIERLER: Right, right.

MOUNT: I think if it had been doing that then, I doubt that even Sam would have had the multitasking abilities to lead an experiment at CERN at the same time. But maybe he would!

ZIERLER: What was your sense of the prospects once L3 was up and running?

MOUNT: Well, it seemed very clear that the UA1 experiment had discovered Ws and Zs. It was very clear that we ought to be able to do detailed Z boson physics. So the path was fairly clear. Would we find anything else? Would we see top? Would we see the Higgs, or anything like this? That was a bit of icing on the cake. But there was a lot of very solid physics to be done—because [since] an e plus e minus collider can actually measure the produced states like Zs vastly better than you can do it at a hadron collider, it seemed a very, very sound way forward to do good physics.

ZIERLER: I come back to your humorous comment about the life of a traveling gypsy as a particle physicist. So you're living at DESY, you're commuting to Geneva. What is your status at this point? Who is funding you? What’s your title?

MOUNT: Well, I am a research associate at the California Institute of Technology. That is a sort of postdoc position, but it’s a faculty position. So not a tenured faculty position, but it has more status than just a hired hand or a postdoc.

ZIERLER: Are you physically in Pasadena at all, at this time?

MOUNT: Yes, but maybe for only six weeks of the year or so. Enough time to try to allay the career problems of not being seen at Caltech very much.

ZIERLER: [laugh]

MOUNT: But not entirely. Universities are not quite as kind to the people who aren't there, especially if they're fairly junior, as you would like them to be. And naturally so. If you don't see somebody, how can you possibly know if they're doing anything? Unless their name is all over *The New York Times*.

ZIERLER: And how long did you stay involved with L3?

MOUNT: I stayed involved with L3 until 1997.

ZIERLER: Oh, wow.

MOUNT: That was a really long time. I was with L3 all through the construction. And as you probably gathered, just going back to EMC, one of the things that I did in EMC that I was slightly proud of was to solve the problem of how to measure the beam. This was a fairly intense beam of ten to the seven muons in a reasonably short burst, and if you stick a scintillation counter in that, it gives you some number, but it's not closely related to how many muons went through, because there are just so many. And funnily enough, the people who designed the experiment had designed and built some scintillation hodoscopes. Lots of very thin fingers of scintillator in various orientations that would act like proportional chambers, if you like, but able to measure the incident muon beam, and with enough granularity that you could actually get the trajectory of an incoming muon from these spaced-out beam horoscopes. And I remember going to one of the EMC weekly meetings where the collaboration leader—I think it was still Gabathuler at that time—was bemoaning the fact—"We still haven't got any good way to measure the incident beam normalization." And for a structure function experiment, this is quite important. That feeds directly into the precision of what you can publish at the end. The beam was fairly intense, and nothing that they had seemed to be able to measure that without a lot of systematic errors. So I went away and thought and decided that they had actually built and constructed the thing to do this, which was the things [Beam Hodoscopes] meant to measure the beam trajectory. And the way they did this was—each [plane of the beam hodoscopes] was made of many scintillator fingers and each finger was connected to a TDC, a time-to-digital converter, and you would look for the time coincidence in the space fingers to actually get the trajectory of the particles. So what this produced finally was a time sequence of incoming particles, incoming muons. And if muons were in that time sequence, they were inherently capable of being reconstructed by the reconstruction program, as incoming muons. If they were overlapping or messed up or not detected in that time sequence, they were not. So the idea was this was a device that could measure the flux of reconstructible muons. In other words, muons that could be related to the reconstructed collision products from the muons. And the insight was that it wasn't important what the muon flux was; it was important how many you could reconstruct. How many events you could reconstruct in principle, so that by looking in a time window at these incoming muons in the TDCs, you could measure the flux of reconstructible muons. This relied on being able to measure the linearity of the time reconstruction of the TDCs, because this fed directly into the normalization or at least the constant of time to digitization. It required a way to capture this information. Because normally these hodoscopes were triggered only when the downstream equipment said there was a wide-angle [scattered] muon coming through the detector. So it required having another trigger that randomly triggered the beam detection system to measure just what was coming through, and what could therefore in principle be causing collisions that could be reconstructed. This involved creating specialized hardware using an americium source giving off alphas through radioactive decay, scintillators [to detect the alphas], placing that well away from the detector [and the beam], and then transmitting that through the SPS tunnels and through the twisted pairs already installed to run the machine control system, down to a place where you could trigger the readout of these beam TDCs in order to capture a few beam particles, and record these on tape at a much higher rate than the recording of the actual collisions we were keeping.

ZIERLER: Why is this useful?

MOUNT: Well, from these, you could calculate the number of reconstructible muons that were coming down the pike, which is what you want to know. You're not interested in how many muons there were; it's just how many can you use for physics after that. If they're not reconstructible, you haven't got an event. So this was the number that had to go into actually normalizing the structure function plots. And to do it, we had to record maybe 50 times as many events as the experiment had intended to record. They started off recording events using one tape block per event on the tape. Then an inter-record gap, and another event, and an inter-record gap. The true events were quite big, because it's a big detector, so that wasn't a problem—a 10% waste of tape space. When they started recording my random triggered events, which only had the beam hodoscopes in them, then they're running out of tape in no time. So they said, "No, of course you can't do this, because we'll run out of tape." So that triggered a whole lot of computing effort that I had to do in order to be allowed to record this trigger.

ZIERLER: And Richard, is this your first substantive work with computers in physics?

MOUNT: As I said, before going to Oxford, I worked for a computer company and wrote some software, in COBOL, to print out the sheets that rent collectors would use, going around to collect rent from tenants in industrial Yorkshire.

ZIERLER: But I'm asking in terms of utilizing computers for physics experiments.

MOUNT: I'd say that it was gradual. In bubble chamber [experiment], you don't do a lot of computing that is original. And as I said, in the CERN Munich group, I did write software that did constrained fitting on the data and it extracted more information than anybody thought was there. And so I was labeled as a sort of computer person by that. And at EMC, although I didn't come in as a computer person at all, I ended up designing and implementing essentially the whole data handling system for EMC. Because it had to be able to handle these stupid small events without getting in any way confused. And all of that is trivial conceptually, but it involved a very large amount of work. Because having put these into the data stream, they all had to be pulled out and collected and analyzed to provide the normalization. That in itself meant that I found that there was a limit in the CERN computer system. When you tried to mount tape number 17 in a job, it would throw you off. Because to do this, I had to read [typically] 100 tapes, and extract one tape full of these very small normalization events. So there were all sorts of things like this. I had tasks that did this data extraction—they would optimally run for 12 hours. This was more than the mean time between failure of the machines, so they all had to be restartable and check-pointable and all this sort of thing. And of course, in addition to extracting the normalization events, I naturally had to go on and extract all the physics streams for everybody that wanted physics streams, because that turned out to be a good idea by that time.

So it became a system not just for taking my events, but making streams of events for everybody. Very tape intensive. And tapes are horrible. You can’t mount 16 tapes with a high probability of any success, still less 64 tapes, with a high probability of there not being an error of some type. So that put me well into the position of being one of the computing gurus of the EMC experiment. One time, I was even asked if I wanted to be the computing coordinator of the experiment. But it was the wrong time. That didn't work out. Later when I said, “Yeah, I might be interested,” they said, “No, no.” It was EMC that pushed me into computing, and also told me about the importance of systematic errors, so when I did go to SLAC—we can talk about how that happened—I was really delighted with the blinding of data, and the way in which modern experimentation in BaBar was done, where you weren’t competing too much with the standard model in doing your physics analyses; you were trying to do it right and then seeing what happened at the end. After being through EMC with its two years of misery trying to get rid of an effect, I definitely felt that every psychological technique you can use to stop people trying to get the answer right is absolutely vital.

ZIERLER: When did you get involved in CMS?

MOUNT: Let me take a break for a moment.

ZIERLER: Certainly,

[pause]

MOUNT: Okay, back again! CMS. That went in a slightly strange way. So Sam Ting of course wanted to get involved in the LHC, and so towards the last year or two of the time I was there in L3, the Caltech group and Sam and all his collaborators were involved in proposing L*, an LHC experiment. Then that was unsuccessful. Sam of course was not pleased. But the Caltech group decided that we did actually want to be involved in the LHC, even if Sam's proposal was unsuccessful. So we started getting involved in the CMS experiment, and started some of the detailed planning particularly for CMS computing and software. I don't know to what extent you know Harvey Newman, but he has always been very involved in computing to an extent that sometimes seemed to obscure his reputation as an excellent physicist.

ZIERLER: What do you mean by that?

MOUNT: I mean that it's a disadvantage in a purely physics tenure situation—it was at that time—to have recognized success in computing. It made people question—nothing underhanded was involved, nobody was dishonest or evil, but it was questioned—if somebody has such a reputation in computing, how can they possibly be that good and that effective in doing physics itself?

ZIERLER: This is a perspective that has probably not aged very well.

MOUNT: Probably not. But it's one I remember quite well from that hopefully bygone era.

ZIERLER: Did you think about how this might affect your own career, this kind of perspective?

MOUNT: Oh, yes. It was clear that this did affect my own career. I [pause]—I suppose I ended up with a lab position at SLAC doing something I was very, very interested in, as opposed to a faculty position at a university, partly because of my perception that it was an uphill battle to get a faculty position at a university that I'd want to be at.

ZIERLER: Although, Richard, just to go back, to what extent was Notre Dame interested in you because you did have this talent with computers?

MOUNT: I don't know. What I think was that I went there and I gave a talk about the EMC experiment, "deep inelastic scattering, the ultimate microscopy", and it was a talk to a fairly general physics audience, not a high-energy physics audience. Then [I] had intense discussions with other faculty members in other branches of physics about all of this. They picked up on a lot of excitement. They really liked it. I wasn't talking much about computing. I'm sure I described the work that I had done to make the F2 measurements from EMC actually normalized, but that wasn't a main focus there. So I think they were interested in the fact that I was really excited about the physics I had been doing and could make them excited about the physics I had been doing. I went and gave the same talk at Caltech later—I think that was after I had been offered the job at Caltech—and I remember Barish being extremely unimpressed. [laugh] Emphasizing maybe—I don't know to what extent he had any thought this way, but it made me feel that Caltech people would definitely look down on the Notre Dame people. Which they probably would. There isn't a much better center of arrogance than Caltech, as far as I know. Yeah. Caltech has the reputation that many of its faculty members would not want their children to go to Caltech.

ZIERLER: [laugh]

MOUNT: Because Caltech burns out and destroys all but the really top people. If you're simply the best person in your state or something like that, you're likely going to be—it's going to be drummed into you at Caltech that you're mediocre.

ZIERLER: [laugh] Richard, how did that first opportunity at SLAC come about for you? What was the connection point there?

MOUNT: The connection point was really the SSC.

ZIERLER: Oh!

MOUNT: So in ’93 or so—before ’93, up to that—I got asked to be on the computing planning committee. I'm not sure exactly what it was called. But the committee to steer what the SSC would do in computing.

ZIERLER: This would have been a component of the Central Design Group?

MOUNT: [Probably]. The committee was commissioned by Roy Schwitters and Joe Ballam, essentially. And it was Joe Ballam who was of course from SLAC, who was the guy who—I do not know who fingered me for the committee. It may well have been Harvey, for all I know. But I got asked to be involved in this, and I made many trips from CERN to Dallas. I actually earned a lot of American Airlines frequent flyer miles. [laugh]

ZIERLER: But Richard, this must have been before 1993. 1993 is when things are sort of falling apart.

MOUNT: Things are falling apart. It was a couple of years before this, yes. I could look back—I think I shredded all my old travel reports [laugh] so I can’t do that, but yes, it fell apart in 1993. But probably it was just over two years before [that] I became involved in this computing planning committee. And I was pretty involved in that, and pretty involved in what it was recommending, which was that the SSC should have a computing division. Definitely it was, with hindsight, interactions with Joe Ballam during this exercise that caused I think then David Leith to come and talk to me at CERN quite extensively about possibilities at SLAC.

ZIERLER: Richard, I want to ask specifically, on the SSC, the extent to which all of the design challenges of operating at that high energy may have translated into computational challenges in terms of planning for that.

MOUNT: I wouldn't say so. In fact, had it been constructed, of course the SSC would have been a much simpler, cleaner machine to do physics at, than the LHC. However, it would have been potentially earlier and so had a bigger computing challenge, because computing wouldn't have evolved enough at that time. I can’t cast my mind back to now compare the LHC computing challenge with the SSC computing challenge. I can’t get it numerically correct in my mind at this time. But it was clearly a very significant computing challenge to do physics at any hadron machine at all. But then the SSC would have been probably more difficult than the LHC just because of its timing, had it gone ahead. Now, another thing that is—before I had any interest expressed by SLAC, I did get offered by Schwitters the job of head of computing at the SSC. And this was in the course of 1993.

ZIERLER: But this is a sinking ship at this point, to take on this—

MOUNT: Well, the ship was already holed; It wasn’t quite sinking. And I just prevaricated or said I would need more time, and while prevaricating, the potted plants, or whatever it was, sank the ship. And so I had no problem anymore. I did go as far as having a full-paid family trip to Dallas, and looking around at the interesting idea of sending your kids to school [laugh] in Texas.

ZIERLER: [laugh]

MOUNT: And [laugh] the amazing amount of house you could buy for any amount of money in Texas, even in the Dallas area. I remember being invited to what seemed to be the palace in which Fred Gilman and his wife were living, in North Dallas. Definitely a different way of life.

ZIERLER: Certainly.

MOUNT: One that as Europeans, we did feel we could probably grit our teeth and adapt to this [laugh] but it definitely emphasized that we felt more European than we did English-speaking.

ZIERLER: [laugh] So from there, did Roy say, “Come to SLAC”? Was it as simple as that?

MOUNT: It wasn’t Roy. After the demise of the SSC, then the main approach was from David Leith.

ZIERLER: Oh, I see. What was David working on at that point?

MOUNT: Well, he was working on the planning of BaBar. And BaBar was going to be the most data-intensive experiment in the universe.

ZIERLER: I see, I see.

MOUNT: And David, although not a computing expert, had good enough judgment to know that they were going to have a big challenge there.

ZIERLER: Right. A computational challenge, you mean.

MOUNT: A computational challenge, yes.

ZIERLER: Just because of the sheer amount of data that would have to be worked through?

MOUNT: Right. And they would be in a competitive situation, so you couldn't do as EMC had done and sit on data for two years. Also, another lesson from EMC is there was essentially zero planning for computing facilities for EMC.

ZIERLER: So this was an opportunity. This was an opportunity to get ahead of the pack.

MOUNT: And that emphasized, "You do need to plan." If you're the largest experiment in the world or the most data-intensive experiment in the world, you probably do need to plan for your computing facilities. Of course, it was easier at SLAC than CERN, because CERN, by construction, was not supposed to provide the computing facilities for its experiments. The member states were supposed to do that in their computer centers or universities. Whereas SLAC of course was supposed to provide the computing facilities for its experiments. But David was fairly sure that SLAC was not ready to do so and would need a big influx of money and a big influx of probably expertise to be able to do this.

ZIERLER: Now, when you visited Pasadena occasionally—six weeks at a time—did you ever go up to SLAC? Were you aware of generally what was going on at SLAC before you officially joined?

MOUNT: A few times more early in my employment by Caltech, I went up to give the odd seminar at SLAC. But I was not really plugged into what SLAC was doing, no. I was aware at that time that the SLAC culture was quite interesting, shall we say, with competing groups—lettered groups at SLAC. But I think I was just peripherally aware of that. And I wasn't really very aware of the SLAC science program at that time. Of course I became more aware, not so much as a Caltech person, but as a LEP person. LEP and SLD of course we—we had a certain amount of schadenfreude about the slow progress on problems of SLD.

ZIERLER: [laugh]

MOUNT: [laugh] Sorry, guys. [laugh]

ZIERLER: But there was a bit of a gap. Because, I mean, between when you met David and when you started—you met David in 1993, but you didn't start officially at SLAC until ’97.

MOUNT: I can’t remember when I first met David, but it was much more towards 1994, 1995, or so. He was a member of the CERN Scientific Policy Committee, so he had excuses to come to CERN and he would look me up most of the time. I wondered why at first, but then it became fairly clear he was—he’d start to say—basically he was interested in having me apply for a job at SLAC. And then it became clear he was interested in having me apply to be the director of computing at SLAC.

ZIERLER: And by the time you get this offer, this is sort of your first—what’s the right word—real job? First permanent kind of position?

MOUNT: Yes. That’s true. There, I'm somewhere in my forties—I think the second half of my forties. By that time, I had promotions at Caltech. I was a senior research associate, supposedly equivalent of an associate professor. But still not tenured.

ZIERLER: And not even tenure line. I mean, there’s no—

MOUNT: Not even tenure line. That’s right. So I clearly needed to make some sort of jump. I had no problem with the interest of the physics and the computing being done at Caltech, but I needed to make some sort of jump. So I started—well, David was interested in having me apply. You could tell, with his strong support, it might be easier than you might expect to get the job.

ZIERLER: What was his title at that point?

MOUNT: He was Director of Research. In those days, Director of Research meant director of high-energy physics research. But the only other thing on the site was the synchrotron radiation laboratory, and they were very strange people, so we didn't talk about them.

ZIERLER: And he would have reported at this point to who? Jonathan Dorfan?

MOUNT: Burt Richter.

ZIERLER: To Burt Richter.

MOUNT: Yes. He was Burt’s second-in-command, effectively. Because he was director of the science program at SLAC. So, I got very interested and decided it would be probably good to respond to David and apply for the job at SLAC, but that it was never good to just do one thing, so I also looked around and found that Brookhaven were also interested in having me apply to be the director of RHIC computing. So again, I sent in applications, and my family and I got a nice all-expenses-paid tour of Long Island and also of the Bay Area. I ended up having a pretty difficult time to decide what to do, because I got a firm offer from Satoshi at Brookhaven, who was the head of the RHIC project, and a firm offer from SLAC.

ZIERLER: And the RHIC project was very exciting, at that point.

MOUNT: It was very exciting. And it was difficult. And I remember as part of my involvement with Brookhaven having a really strange experience of sitting in [a review]—I think as a reviewer of the Brookhaven computing plans for RHIC, and having Dennis Kovar, the DOE guy sponsoring the review, take me aside and saying, “You've got to make these guys ask for more.”

ZIERLER: [laugh] That’s great.

MOUNT: He thought they were being too small-minded about what they would need. They had absorbed the traditional thinking which I remember being told in words of one syllable, when L3 was being planned—“Don’t put significant computing in. You'll never get it approved if you do.” So they were still in this mode—“Don’t say how much computing you need. You'll never get it approved if you do.” And the funding agency guy was saying, “For God’s sake, tell us what you really need!”

ZIERLER: Richard, I wonder if in those early days, your sense even then was that the bang for your buck with computer power was pretty low. In other words, a fairly low amount of computational ability was quite expensive.

MOUNT: It was true. I mean, it was very true at the beginning of LEP, where actually saying that we would require something which turned out to be maybe 30% of what we actually used at LEP, and writing this in an official report to the LEP committee, Harvey and I were excoriated by John Thresher for daring to say anything as ridiculous as this. That was the beginning of—well, no, it was a continuation of a long bad relationship with John Thresher. I don’t know if you know John Thresher.

ZIERLER: I know of him.

MOUNT: Yeah, he was the head of the U.K. particle physics enterprise, and he was something important at CERN at the time, and I forget exactly what. Certainly in charge of the computing planning. But I think he already knew I was a troublemaker by that time. [laugh] The LEP experience was one of being regarded, not just by John Thresher but by a large fraction of the community, as semi-crazy, for actually putting down plans that turned out to be maybe a bit better than 10% of what we finally needed and used. But that was because that was the time when the mainframes were fading out and other ways of doing much cheaper computing were coming in. That happened during my years at LEP. We moved from mainframes to at least using much cheaper workstations and computers to do most of the computing. And then for BaBar at SLAC, it was just where SLAC was moving from the mainframe era into lots of Unix boxes. Just thinking about doing it. And it wasn't my idea that they should do it; it was becoming quite obvious that they would do it. But there was a lot of resistance. At SLAC, SLACVM, a big IBM mainframe, had done everything at SLAC, up until just about the time I got there. It would print the paychecks, do the HR and the business systems, and do all the physics. Everything at SLAC was done on one big, time-sharing IBM VM mainframe. And SLAC had improved its software to the point where everybody loved it, and it was just fine. But—

ZIERLER: Now, did you create the computing services department essentially out of whole cloth? How well-formed was it before your arrival?

MOUNT: No, no, no, no, it existed. There was a SLAC Computing Services run by a guy called Charles Dickens—

ZIERLER: Oh!

MOUNT: —who called himself Chuck Dickens. He was American. [laugh]

ZIERLER: [laugh]

MOUNT: Because that would have been too much!

ZIERLER: [laugh]

MOUNT: And he had been very successful in, as far as I know, managing and developing SLAC computing. He had been there quite a long time. And he was ready to I think essentially retire, with a good deal from the lab. And David was ready to help him get a good deal from the lab and wanted to move somebody else in to meet the challenge of BaBar. But the organization was there. It was probably mildly dysfunctional, as all organizations are. It was also struggling with the move to support desktop PCs and Macs and things like this, which were just coming in at this time. Not just terminals to the mainframe. So there were quite a number of struggles to move into the modern world going on, and I moved into the middle of this evolution. And so a lot of what I had to do at first was not just simply have ideas about how they should do computing; it was to provide moral support to the people who were actually trying to move in the right direction, and let them move it. Not always tell people what to do. Because there were people there who knew what to do. They just needed support to overcome a very conservative computing environment. Conservative in the physicists and conservative in the people in computing. And all other users of computing. I suppose that's perfectly normal. But definitely it felt like a more conservative environment than I had left at CERN. Which isn't surprising, because CERN was addressing large computing challenges at the time, and SLAC hadn't really got there, by that time.

ZIERLER: Richard, is your sense that your work would be focused as a service to BaBar? Or you had responsibilities generally through the lab?

MOUNT: I knew I had responsibilities generally throughout the lab. Though definitely the motivation for hiring me was BaBar.

ZIERLER: And it was clear that BaBar was in greatest need of your expertise.

MOUNT: Yes, I think it was. And the lab was BaBar. There was this separate part called the synchrotron radiation laboratory that used the old SPEAR accelerator that had done the Psi discovery, for instance, but this was now in the hands of these photon scientists or materials scientists or whoever they were, and they lived in a different world and didn't talk to anybody. [laugh] And the lab and the lab director, Burt, and the upcoming lab director, Jonathan, were clearly focused on BaBar, and that was what really mattered. At the same time, the lab was moving all its business functions from homemade software on their IBM mainframe to a modern suite of business software. They were moving to PeopleSoft. You know PeopleSoft?

ZIERLER: I vaguely remember PeopleSoft, yeah. This is testing my memory, but yes.

MOUNT: Later bought by Oracle, so it’s essentially Oracle. And this was a huge cultural change, and really much more difficult than I understood when I got there. And it was actually being done much more like a physics experiment than running a business system. Some of the principal developers they had were old physicists and things like this. And it was actually very scary in hindsight [laugh]. The lab could have closed itself down or made huge financial errors, fired everybody overnight—I don’t know. This was ultimately successful, but it was a very large effort on the part of the lab, and actually ended up being quite a large distraction for me.

ZIERLER: Richard, in what ways did your expertise really help BaBar? What were you able to do that allowed BaBar to achieve what it did?

MOUNT: So first of all, I was financially ambitious. I believed what I chose to believe out of the BaBar projections about what they would need, and I inflated what I didn't choose to believe, because I didn't believe it was true. And I was supported in this by David. He essentially never questioned my judgment on these things. And he was able to help ensure that the lab would put resources behind the necessary computing and sort of guarantee those even before it had guaranteed to get the resources from Washington. Not "If we manage to get it, you'll have what you need," but "You'll have what you need, and we'll play catch-up with Washington." Not always said explicitly, but that was really what happened. Washington likes to fund success rather than blah-blah statements about how much you [are going to] need. It did help a lot when PEP-II came on, exceeding its design luminosity in the first year or so, and creating an obvious computing problem with all the data that it was delivering. It's a much better problem to have when you go to a DOE, that wasn't terribly financially crippled at the time, than it would be to say [before you have data], "Oh, we're going to need ten times as much [computing] as anybody said before, and we'll need the money." So in the end, I did succeed in getting unprecedented amounts of money from the lab for the hardware for computing. It was also a time when the lab clearly lacked expertise. The experiment had made the choice of an object-oriented database system, [Objectivity DB], to handle the data, going along with object-oriented code, the C++ code that BaBar had decided to write its software in. This was a brave, one might say foolhardy, decision, and they didn't—they had a certain amount of expertise in the collaboration, particularly the Berkeley group, the LBL group, but they didn't have much expertise at SLAC at all. With the collaboration of the Berkeley group, I was able to find people from CERN that I could hire, young people who had got some grounding in writing object-oriented software, and in object-oriented databases and so on, get these people from CERN and bring them into SLAC with, again, the financial support of David and the lab. This was a key. I was already getting to an age where I recognized that I was better off with Excel than I was with C++.

ZIERLER: [laugh]

MOUNT: But I could still recognize what had to be done in C++, and I could recognize the challenges of it. And we managed to strengthen, really with one key person—a couple of people at SLAC—the group that would work on the data handling software for BaBar. There was one guy already there called Andy Hanushevsky who was an interesting and brilliant designer of highly efficient data handling software. He actually managed to make Objectivity work, at scale, for BaBar—not well, but at least not disastrously, by redesigning the underlying engines as scalable engines, that would be spawned to make objectivity queries actually work properly. That, and an expert—well, a young guy—I hired from CERN called Jacek Becla, who eventually got tired of not being appointed to be in charge of software for LSST, or the Vera Rubin Telescope as it now is, and he went off to Teradata just a little while ago, probably two years ago now. These were people who also managed to work very enthusiastically together. And I would say a lot of the success was not just the lab supporting me to buy the right hardware, but the fact that we had key people to make the software that actually made BaBar work at a data handling level, and a lot of good BaBar physicists who wrote the software to work at a reconstruction and physics level. So this ended up making BaBar fully competitive with Japan, with Belle, in spite of the fact that the accelerator was—well, Belle actually had more data in the end, but their ability to analyze it was not as good as BaBar's. And that was a combination of computing hardware capability and particularly the software capability that we developed.

ZIERLER: Richard, I wonder if this might have been a point when you felt vindicated by those earlier concerns about computers not being so high up on the totem pole in terms of respect in the field.

MOUNT: Slightly, yes. There was a clear acknowledgement that BaBar's success and competitiveness was—a good fraction of that was the success of its computing. Without the success in its computing, there would have been big problems, clearly, but there would have been big problems without some of the really excellent physicists who were doing analysis on BaBar. I had over the time quite good relationships with my counterpart in KEK, Yoshiyuki Watase, partly because we were both involved in the Geant4 simulation software. Do you know about Geant4?

ZIERLER: I've heard of it, yes.

MOUNT: Yeah. So this is the fairly universal now in high-energy physics simulation toolkit—the first object-oriented simulation toolkit in high-energy physics with any success. And that was adopted very early on by BaBar, before it almost existed, as the way they would do their simulation. It started off as a CERN-centric project, while I was at CERN, and I and Joao Varela were the two referees for the LHCC-DRDC about should they approve this project as a CERN project to go ahead and create this object-oriented simulation toolkit. I wouldn't say I was an expert in writing C++, but I had a reputation for good judgment in software development. And I looked at these plans and decided with Joao that this was definitely the way to go. The collaboration, which was international, that had been put together had the best possible abilities to do this. So we gave our advice, and the LHCC approved the project. And then a couple years later, we looked at the first two years of progress and reviewed it again, and gave them the go-ahead. And then later, I moved to SLAC, and SLAC clearly had a vital interest in the success of Geant4. Geant was an acronym for GEometry And Tracking. [The original version] was created by René Brun a Frenchman, so he called it Geant [*Mount noted later:* Géant is also the French word for Giant – it was a big body of software]. His first attempt at this did not meet with much success, and probably his second, but finally he got something that became widely used as a community tool, and it was called Geant3. Geant3 was written in Fortran, so it was natural that with all the interest in object-oriented languages, the next version, Geant4, should be written in C++. SLAC clearly couldn't rely on something being delivered by a CERN-centered collaboration, especially something central to its physics program and something where SLAC was the guinea pig for the world in using this software. So the first thing to do was we had to hire at least one key developer from CERN. I hired a number of developers from CERN over the years. And we also—somebody pointed out, "Well, you could try to hire Makoto Asai. Makoto was the chief architect of Geant4. He was at Hiroshima University in Japan and rumored to be possibly interested in a job in the U.S. So it worked out, and I was able to hire Makoto Asai, essentially the intellectual driving force behind Geant4, get him to SLAC, and SLAC became almost the center of this Geant4 universe. KEK was also involved, because a lot of the architecture of Geant4 was Japanese, to start with. There were a number of very, very able Japanese computing physicists who were involved in this. So in the collaboration board for Geant4, which was supposed to ensure that Geant4 got the resources it needed to move ahead and develop the software, SLAC was a major force, but also Japan and KEK were a major force, and CERN. We had an international collaboration board that met fairly regularly, and in person. It was the time when you had to do this, to steer this. And for a time, I was the chair of this collaboration board. And for most of the time [laugh] I was often referred to as the godfather of

Geant4, because of my role in not writing a line of its code, but in being its initial and next-level referee, making sure it went forward, and then from the SLAC side, providing a lot of support and hiring the right people to make sure that Geant4 was successful. We got a lot of money out of DOE over the years to support this development, because we could show clearly how important this was—it became adopted by the U.S. high-energy physics program, almost in its entirety. Fermilab also became very involved, and quite a lot of money flowed around, until DOE got rather tired, and sort of said, “You must have finished it by now” [laugh] and rather cut down the funding. But that sort of thing happens, as you know.

ZIERLER: Richard, how did your tenure as the head of ATLAS Computing come about?

MOUNT: Well, I was head of SLAC Computing for 11 years. Then in 2008, I was strongly pressured by the SLAC management to get heavily involved in ATLAS Computing with actually the idea of trying to take it over in the U.S. That of course didn't pan out. But also at the same time, the DOE program manager responsible for LHC computing in the U.S. was continuously pulling me aside at reviews and other meetings saying, “I don’t think these Brookhaven guys can do it. I don’t think they have the competence. I think SLAC should step up and try to take this over.”

ZIERLER: Was there a competitive spirit to that as well?

MOUNT: Yes, there was a competitive spirit. Now, I was somewhat torn in this, because I knew the Brookhaven guys. Initially, Brookhaven was—well, they were almost reluctant [laugh] in their approach to RHIC computing. And when I didn't take that job, they appointed Bruce Gibbard, who was quite reluctant to [laugh] take the job of being responsible for it. And that went on into the ATLAS era as well. So there was a certain amount by which it was true that

Brookhaven was not even an enthusiastic participant in ATLAS Computing, to start with. But then they hired Michael Ernst, in particular, who was at DESY, then at Fermilab, and then at Brookhaven. A sort of old colleague from Europe, who was a very capable guy. And I just refused to [laugh] badmouth them in front of the DOE people. I was fairly convinced that with Michael there, if he got the support that he needed, that there would be no useful prospect in trying to undercut Brookhaven. But there was a good prospect—this was before the LHC blew up—since the LHC was due to come online very early, and it was due to kill us with data so that we would need a second major computing center in the U.S. Not trying to destroy Brookhaven, but to work collaboratively with Brookhaven. And SLAC at that time was the biggest high-energy physics computing center in a way, because of all the demands of BaBar. We had better network connections than anybody else because DOE-ASCR had had to provide us with the funding for that because they could see we were the most data-intensive activity in the Office of Science. So we were well positioned—if we needed to step in and bail out LHC computing, ATLAS computing in the U.S., then we were well positioned for that. So that was the environment in which I was persuaded that I should step down from running SLAC Computing, and just join ATLAS. I wasn't a member of ATLAS before that. And sort of said, "Here I am. What can I do?" [laugh] Not, "I'm here to take over everything," or anything like that. But to get involved and to try to get involved in a positive way.

ZIERLER: And what were some of the key challenges right at the beginning?

MOUNT: Well, the key challenge was really—if you plotted things on the usual log plots, and you put on the luminosity with which LHC was supposed to come on, and the time it was supposed to come on, and the rate—because Moore's Law was still in effect at that time—the rate at which technology would be solving your problems—you'd be going into a new era of stress due to technology having not advanced enough for you to be able to afford your computing properly. It was likely to be about as bad as BaBar had been because of the good performance of the accelerator in the first year or so. That was a terribly stressful time. People were [laugh] very unhappy, because everything was falling apart, due to the amount of data. For a little while. So there was a very good possibility that LHC Physics would be completely constrained by its computing for a year or two, and so there would be a lot of career stress on the people responsible for computing in LHC as a result. So it was potentially a very exciting time. We did manage to—it was slightly before I moved into ATLAS, but we proposed the only non-university tier two site for ATLAS to be at SLAC, and got this approved and funded. Obviously, a good jumping-off point for a much bigger role in ATLAS if necessary. But then within months of my having started to travel to CERN a lot, and starting to get involved in ATLAS Computing discussions and looking in a non-elbowy way for a role in ATLAS Computing, the machine blew up, as they were starting to commission it, because of burned joints in the magnets, as was later found out. Too much resistance in the joints of the superconducting wire. So essentially this produced a period of detailed investigation, a long down time, and a reworking of all of this, all around the LHC. I can't remember the exact delay, but it was about two years or so, as a result of this. And that pushed us into two years of—Moore's Law doubling every 18 months or so—quite a useful fillip. And the start was still not too fast after that, so it pushed LHC Computing into a regime where it was possible for Brookhaven to provide the principal U.S. resources for ATLAS Computing. I didn't feel unhappy supporting this. SLAC had a significant role in providing one of the major distributed computing centers for ATLAS, and also a significant role in developing distributed computing software for ATLAS, going on from the software that had worked for scaling up the objectivity database for BaBar, we had the software that was running a lot of the distributed access to data for ATLAS as well. So I think we had a good position there. But SLAC and I were interested in my having a more rewarding and influential role in ATLAS Computing. So with the lab's support, I tried to become as involved as I could in all the regular meetings, which are largely open participation, trying to decide what would be done, particularly in ATLAS distributed computing, and to a certain extent in the ATLAS physics software development. And I'm trying to remember exactly the point, but ATLAS has this process by which the leaders of all its major groups—so trigger, data acquisition, computing coordinator, and so on, and the coordinators of physics groups—they get elected. This is of course no way to run a railroad, [laugh] as we've just discovered. [laugh] But there it is. They get elected. They get elected by the Collaboration Board, essentially the professors who do no physics [laugh], but not by the active members of the collaboration. But in general, it works out sort of all right. Leaders actually get elected for a two-year term, one as deputy, and one year as "it," as the person in charge of the activity. And then they disappear again or go back to doing whatever else they were doing. So I let it be known that I would be happy if the U.S. put forward my name as being a candidate for computing coordinator. And I don't remember; I think this was a fairly natural process. I had a certain deserved or undeserved reputation, I don't know, and I was proposed—I think it was in late 2012—got elected to start in 2013 and go through to essentially the first half of 2015 with deputy and then computing coordinator. This did require moving to CERN for—it should have been two years, but it was more like a year and a quarter with a lot of commuting in between. It was a time when I tried to make quite a number of changes in a positive direction for ATLAS Computing. I was successful in some of them. It was a high-inertia environment. And I thought my inertia evaluating skills were very good, but I have to confess that they were not perfect, and some of the inertia actually ended up stymying some of the ideas I had. One of the ideas was that we should do a proper review of ATLAS Computing *à la* U.S. style. That was one of the things—I'd been working for Caltech since 1982, so I was well aware of "which week is it, therefore which review are we being subjected to at the moment." Having four or five major agency reviews in a year, some of them quite aggressive, was just bread and butter. You just did it, and normally you came out of it quite well. I had had a little experience of proposing and executing a review of the Geant4 collaboration, where it was actually very useful and produced quite a lot of progress forward. But I can remember how deeply insulted many of the European collaborators in Geant4 were, all of whom were of course collaborators [with their own funding] who were not getting any resources from Geant4 at all, but being reviewed and told what they could improve. This was absolutely foreign to them, that anyone would come in and tell them that, "You're not doing this so well. It would be a lot better if—why don't you think of doing it this way?" I think the net effect was positive, but there was still a lot of people to calm down. After that we did subsequent reviews, and I think the culture had sort of caught on in Geant4 that actually this is a group of people that *we* hire to come in and tell us things that will help us. And even if they're saying we're doing it wrong, they're trying to help us. So that was good. But ATLAS didn't seem to have the ability to accept this. And finally, due to a combination of inertia from the trenches and opposition from the spokesperson, I was unable to organize a useful review of ATLAS Computing. I had reviewers lined up who I knew would do a good job, who were much more capable than I was to evaluate all the details. But—

ZIERLER: Richard, in what ways was this more of a scientific problem and not just an administrative problem? Not being able to do the review.

MOUNT: It's not so much an administrative problem. It is a scientific issue, that although it does take you quite a lot of time and effort out of your day job to do all the things you need to provide information for a valuable review—and if you get it done every other week by the DOE you do begin to wonder whether it's a good idea—these things have great value for seeing whether you need course corrections. For helping the bits of the culture that are not really working for the experiment correctly, to be reassessed and reevaluated. The fact that the experiment couldn't accept this I think lowers the scientific value of the software. It's particularly software that I was concerned about, not so much are you buying the right computers. A number of people agreed with me. My successor as computer coordinator, Eric Lançon, a Frenchman with a more European attitude towards these things, who you might have expected to think that reviews were a bad idea, I think he had been infected enough by American diseases that he—he was firmly on my side. But we couldn't sway the management to do this. It has to be, to a certain extent, friendly but aggressive, or else it is not of value. And I didn't want to conduct one that was not of value. So it's a different way, trying to get things right without external criticism and advice. That was one of the failures. A greater success was having a reevaluation of how to handle the potential mismatch between the flood of data and the resources that we could possibly have for managing this data. I introduced a fairly automated way to handle data life cycles, and particularly the lifetime of data sets. There's no way in an environment like the LHC experiments that you can simply store, on tape, everything relevant to the physics that you produce. You have to discard most of it, because you can't afford to store all of this. So you need to have a systematic way—not a way involving acrimonious meetings all the time or wasting a lot of human brainpower—you have to have an automatic way to assign lifetimes to data, and to adjust them as necessary, but to have data often get removed, and keep the system fluid and functional, rather than have, as had been happening, every few months there would be *a crisis* where we have to go out and delete some stuff in crisis mode, therefore without adequate planning and thought, in order to survive the next few weeks of processing. So I think we moved from that to having a more systematic approach, and that was something I proposed and it was somewhat ridiculed and resisted by some people in the management. I won't mention the names. But it did succeed and is now functioning inside ATLAS. A small thing, but—

ZIERLER: What has ATLAS revealed, as a result of having a specific computing coordinator? In other words, because the computational aspect was bureaucratically built out specifically, in what ways was that decision indicative of what ATLAS would later go on to find?

MOUNT: [pause] I find it difficult to say that there was any way in which we could have done computing, any reasonable way in which we could have done computing badly enough so that the Higgs discovery wouldn't have happened, for instance. I think that the level of prioritization for that sort of physics is such that that was not really influenced by me—and of course, a lot of what I changed in data handling was only taking effect around that time anyway. It would be nice to say that there was something fundamental in allowing physics results to come out in what was done in computing, but I think there are a lot of competent people in ATLAS, a huge number of people in ATLAS, and there are so many ways to do things. Some are efficient and reasonably elegant. Some involve midnight meetings and panics and people running around like headless chickens and so on. But I think the damage to ATLAS physics from a less efficient approach to computing would not have been too great. It's particularly the sort of down-ballot topics that do get shortchanged if you don't have a good approach to computing. So the key things—the CP violation, the Higgs discovery—you're going to get these even if your computing system is a shambles, unless you're very, very unlucky. But you do narrow the focus of the physics that can actually be done if you only have to focus on the highest-priority things and you really can't spend much time on a broader range of physics. So there's a lot of less exciting physics coming out of ATLAS, but still important physics, and I think that is empowered by keeping the computing system efficient and effective, and the way the software handles data efficient and effective, more than anything else. Best I can do.

ZIERLER: When did you become head of EPP computing?

MOUNT: Well, that was more or less when I came back from CERN. I stayed a little bit beyond the actual term as computing coordinator, because the terms of computing coordinator are ridiculously short, and I wanted to try and make sure that there was a little bit of continuity. To give you one idea how short, the computing coordinator has responsibility for quite a lot of common fund budget, and to understand where all the bodies are buried in this takes several years and quite a lot of social skills. So in general I think computing coordinators fly blind in that sort of thing, because they're only around there for two years. A lot of [financial] things happen in some obscurity, for example how the Italian contribution to the common fund budget comes in funny money, which then gets recycled and goes back and does various other things for Italians. And you know, all sorts of things like this. So [laugh] you think you have a budget of this—oh, no, you don't. There are very few parts of this that you can touch without the whole house of cards falling down. And so on. So it was important to spend more time together with Eric Lançon so we could form a team with some continuity. So then I came back—I was still involved in ATLAS, still getting all the ATLAS emails, traveling regularly to CERN and helping in the discussions of the computing directions and the regular meetings, and lots of remote meetings by the various technologies. I was immediately coming back from CERN because of my responsibility for the Geant4 people at SLAC. I was head of simulation for particle physics at SLAC. And then at some time I can't quite remember, that was translated into head of EPP computing, which, to be honest, was more of an oversight role than giving me a lot of people on the chess board I could move around and do things with.

ZIERLER: But your portfolio was broader at this point.

MOUNT: It was broader than ATLAS, yes. There is other computing going on in experimental particle physics, plus there's also computing going on funded by particle physics in the accelerator department, a lot of simulation work there. And quite a lot of that involving proposals to the funding agencies that I was involved in, sometimes simply in an advisory role, sometimes more directly. So it was partly the relationship between particle physics computing and the funding agency, managing part of the particle physics computing explicitly, and having a titular oversight role for the bits that I didn't manage explicitly. It was a much less important job than that of director of SLAC Computing back in the BaBar days, but by that time, of course, SLAC Computing was somewhat different.

ZIERLER: I wonder, Richard, in some ways, if this was a recognition generally at the lab that what you were doing for BaBar previously could be done on a more general scale.

MOUNT: I'm not sure. We talked about the frustrations of people seeking tenure if they had been doing computing and so on. One of the frustrations I had was that I thought that as a result of what had been done in computing, and particularly data-intensive computing for BaBar, SLAC was well positioned to build on that and become a major center for more general data-intensive computing. And Jonathan Dorfan thought that as well, so we resonated on that. This was before I joined ATLAS, even, and of course before Jonathan suffered from arc flash problems and was no longer director. I don't know if you know the details of that, or anything about that, but if you're studying SLAC, no doubt you know about arc flashes.

ZIERLER: Yes.

MOUNT: Jonathan was supportive of that, but not terribly clued up on how [laugh] to go about doing anything about it. His successors were not sold. Persis, not very much. Chi-Chang Kao actually, when he was not SLAC director, was very, very clued up about how important simulation, for example, should be in SLAC science and in the LCLS science, and so on, and was trying to create all sorts of scurrilous projects to subvert the LCLS management and get them involved in doing proper simulation of their machine and its experiments. Then he became director, and he finally had a day job and could no longer do this, so his support became more distant. However, the reality is that I spent some time while SLAC was studying its navel and trying to decide where it should go, especially after the arc flash incident, trying to persuade the lab that it should make data-intensive computing—not supercomputing, but data-intensive computing—what they were calling at the time, in the SLAC-speak or business-speak, a line of business. Something that we do scientifically and seek funds for scientifically. That was not successful. It was considered, but I think not very seriously, and it was decided they would not go to the funding agency and seek to get explicit funding for a line of scientific business in data-intensive computing, and to the university and seek a few faculty billets in data-intensive computing. I know they didn't do what I thought would have been good and what I wanted. I have no idea whether that was the right decision for SLAC or not. I mean, SLAC remains a small lab, and it better not have too many lines of business or it just becomes a collection of people pursuing individual ambitions and not talking to each other. So perhaps SLAC was not big enough to do this—but I thought SLAC had the leading position in [data-intensive] science in the U.S. in the last years of BaBar, and it could have built on it.

ZIERLER: Wow.

MOUNT: And at one time, Jonathan was sort of assuring me that I would have my institute. [laugh]

ZIERLER: What was your response to that?

MOUNT: Well, you know—

ZIERLER: "Very nice."

MOUNT: [laugh] "Very nice." Yes. But I think even by that time, it was "In spite of all the indications to the contrary, you will." And [laugh]—so the center of computing gravity in the Bay Area moved back to Berkeley, where it was at one time—I had a lot of conversations and interactions with Horst Simon at LBL, and I could tell he was very envious of the dynamism of the SLAC work in data-intensive computing, and of our good relationship with the computer science funding agency in DOE, which was looking pretty favorably on us and was at that time being very nasty to Berkeley, for no reasons that I understand. There was some personality breakdown between Germantown and LBL. Itself—LBL—not the university. So I remember being quite amused that this computing leader from a much bigger lab was appearing envious of my position. [laugh] But it didn't last.

ZIERLER: [laugh] Richard, when did you start to think about retiring? I guess what I'm really asking is, how secure were you at this point that what you had been building for so long in your career had essentially established its own momentum? That the things that were important to you were important institutionally across the board.

MOUNT: Well, I have to say, I was a little disappointed that while—it was very clear, and in my subsequent work for the lab in planning for the LCLS era, it has become very clear that computing is a major gating factor on success for the experimental science that the lab does, be it particle physics or the non-experimental science of astrophysics and cosmology, or the LCLS science, the natural evolution of science and instrumentation is taking this in a direction where computing is of rising importance. I think that is recognized. And it's not so much that I built something that the lab is building on, but I can see that the momentum there is growing. Unfortunately, I could also see that the particle physics role of SLAC is shrinking. It was much bemoaned by all the old guard in particle physics. David Leith was so unhappy about even the idea to construct the LCLS, the idea to bring in the particle astrophysics and cosmologists, because of its dilution of this very focused lab that had been very successful in doing focused particle physics. I didn't agree with these fears because you know, you have to move forward, and if you're not going to construct the world's largest accelerators on site, you'd better find some other way to do good science and use the intellectual capabilities you have. So I fully endorsed Jonathan's decision to go into large-scale photon science, to build up the particle astrophysics and cosmology. No way did that matter to Jonathan, but I thought it was a good idea. The inevitable result—there was one sort of very short period in which particle physics went from being the largest recipient of funding at the lab to being one of the smallest, without anything changing. What happened was the accelerator complex, which had been all funded by particle physics, became sold to Basic Energy Sciences, so they started funding that. Nobody lost their job. Everybody was doing in approximation the same thing. But the particle physics budget went down to a third of what it had been. That has a big psychological effect on people.

ZIERLER: Certainly.

MOUNT: You are not the dominant force in the lab. You are an embattled constituency at the lab. And all that without anybody changing what they were doing, on that day.

ZIERLER: Richard, on that point, I want to ask, I think, since we're right up to the present now—I want to ask, for my last question, the extent to which—obviously the—downfall is perhaps too strenuous of a term—but the diminishing of particle physics at SLAC and in the United States and in physics generally, that’s one part of the story. But I wonder if you can reflect generally on the influence of your work as it relates to particle physics specifically and physics generally. In other words, there’s a duality to your career, right? There’s the specificity of the way that you have harnessed your efforts toward particle physics. And yet there’s a larger story for which your career overlaps and which you're a part of, in which computational power is relevant for areas far beyond particle physics. So I wonder if you could sort of reflect on that, both in terms of your recent past, and where you think things are headed.

MOUNT: There are not many long time-constants in computing, as you know. If you talk about people, there are somewhat longer time-constants. So I think I have definitely had a significant, perhaps even major, positive influence on the state of software for doing simulation in particle physics. I say I didn't write a line of this, but several critical times throughout the life of this Geant4 collaboration, I have I think saved it from disaster. Because there are always disasters waiting to overwhelm you, some of them financial, but many of them caused by infighting and factions and things like that. Some of them being purely scientific and technical. But I think I had help to give and maybe played a pivotal role in giving particle physics its core simulation software. I would say Makoto Asai has done more than I have, but we've done different things, and I think we both agree that we were both very important in this. I point out that this software consumes more computing cycles than everything else in particle physics. It’s of course not so data-intensive, but in terms of actual computing cycles, this is the dominant thing that we spend computing cycles on. In terms of data-intensive science, I've helped encourage some of the right ideas and some of the people who are now leading things in the right directions. I don’t think any particular things that I have done have a very large persistency, because that’s how it is in computing, but trying to take a scientific and good engineering approach to the policies of managing data is something that I think turns out to be very important, when handling of data is often the largest cost and the largest constraint on science that you actually have in these very data-intensive experiments. Influence on the physics itself—it’s probably mainly on physics that I haven't followed too carefully. Diffraction physics. The boring physics that some people choose to study that you couldn't possibly do unless you had capabilities left over from doing the high-priority physics. Even top physics at the LHC now becomes a boring and second-class topic. So all of these, the way in which the LHC can be maintained as a broad program, and physics undertakings like it can be maintained as a broad program, which is vital to their social success. Because if all the LHC did with its thousands of PhD physicists and actually thousands of graduate students was to produce one Higgs result every five years or so, you couldn't maintain this as an intellectual activity with many people working on it. There needs to be a broad program of interesting physics. And this is made possible by taking the data analysis seriously and making sure that it has the software, the policies, and the computing resources so that it will actually happen. So in a sense, we couldn't do science on this scale, I think, if it wasn’t for some of the “boring” things that are—boring in quotes—that are made possible by doing the computing right. So maybe we have to leave it there. I think you've gathered that in a sense I don’t feel I left SLAC where everything I wanted to achieve had been achieved. I had decided since I have children, grandchildren, and I have a life that I would like to live [laugh] before I die, that I would retire by 2018 at the latest. And then I saw financial difficulties coming down from the Department of Energy that would have required that some of the people that I brought to SLAC were let go, and I decided, particularly because of the way in which SLAC and Stanford can work together to make things easy for departing people, that it would help the Lab and it would actually help me if I retired a few months early. So that's why I retired in August of 2017 instead of being around Easter of 2018, which was the original plan. And then having retired, I was brought back nearly full-time to work for the deputy lab director on the LCLS computing issues, which they might have made me do instead of retiring, if only the lab management had been talking to each other.

ZIERLER: Maybe they realized their decision and regretted it soon enough.

MOUNT: No, it was clearly not enough communication among the lab management, or else they—it worked out very well for me financially, because I ended up essentially being paid twice for the [laugh] same amount of time. [laugh]

ZIERLER: [laugh] Well, Richard, on that note, I want to thank you for spending this time with me. It's an absolutely riveting history that you've been able to share, with so many insights across collaborations and laboratories and countries. The SLAC archivists are going to be just delighted that we're going to be able to include this perspective for historical preservation. And I'm just so glad we were able to connect, so thank you so much.

MOUNT: Okay, well I look forward, with some trepidation, to see what you actually decide to write down.

ZIERLER: [laugh]

[End]