Jean Deken [00:00:00] OK this is Tuesday April 9th, 2019, and this is Jean Deken interviewing Martin Breidenbach. This is the second interview in our series. I wanted to start with where we left off last time which was when you came back from CERN right around 1972 and you were working on the detector, you said?

Martin Breidenbach [00:00:27] Yes, that was with Burton's Group C.

Deken [00:00:34] OK.

Breidenbach [00:00:34] We were working on the detector which was formally called the SLAC- LBL Magnetic Detector. Later it became known as MARK 1. I was mostly doing electronics at that point.

Deken [00:00:57] Electronics? OK.

Breidenbach [00:00:59] And data analysis, once it got going.

Deken [00:01:04] And were you reporting directly to Burton?

Breidenbach [00:01:07] Yes.

Deken [00:01:07] So you were Doctor Breidenbach at this time, you were reporting directly to Burton. What was your title?

Breidenbach [00:01:17] Post-doc, or RA...

Deken [00:01:19] OK.

Breidenbach [00:01:20] In the early days at SLAC I think they meant the same thing. Now there is a distinction.

Deken [00:01:25] How long did that process of getting the electronics...

Breidenbach [00:01:35] From when I came, we started being a useful detector probably sometime in 73.

Deken [00:01:44] Okay.

Breidenbach [00:01:44] It was pretty quick and then life shifted over for the most part to gathering data and being physicists...

Deken [00:02:00] And the SLAC-LBL Magnetic Detector was on SPEAR, right?

Breidenbach [00:02:04] That's correct.

Deken [00:02:05] At that point, were you sharing SPEAR with the synchrotron radiation people, or had that started yet?

Breidenbach [00:02:16] No. The synchrotron radiation people hadn't really started. And then even once they started for a long time, they were truly parasitic. They had a beamline that with a couple of mirrors came out to the top of the shielding and they had a Sears Garden shack on top of the concrete.

Deken [00:02:41] I've seen pictures of the Sears shack.

Breidenbach [00:02:43] That's right. They got what they could get, and they didn't bother us...

Deken [00:02:51] So you didn't really...

Breidenbach [00:02:52] We didn't pay much attention to them. They probably were not getting established until about mid '74.

Deken [00:03:03] Okay.

Breidenbach [00:03:04] But that's stretching my memory.

Deken [00:03:06] Yeah okay. I can I look that up. So, you're going along. And what was sort of the first milestone that you hit with the detector on Spear? What was the first achievement?

Breidenbach [00:03:20] What happened during the November revolution is so overwhelming that it's hard to remember anything that happened before that other than the lead up to it.

Deken [00:03:35]
Breidenbach [00:03:38] So Burton had this theory of the electron having strong interactions which would mean this quantity called R, the ratio of the hadronic cross section to the Mu-pair cross section would continue to rise. And so we were embarked on a long program of doing relatively careful measurements over the energy range of SPEAR with the goal of publishing a paper on the total cross-section, or R, versus the colliding beam energy. In the winter of 73 or the spring of 74, , we took data and didn't look at it very carefully. And then during the summer we noticed -- and we were Vera [Lüth], Roy Schwitters and me here at SLAC, and Gerson Goldhaber at LBL - that some runs taken at the same energy seemed to be inconsistent with each other beyond statistics. No one got excited, and we just thought something was wrong with the analysis. One of the things we did was to check the analysis classification of events. Computing was very, very different than it is now. It was all on the big IBM mainframe and there was a display that you could sign up to use for a limited time. There were no screens on desks! ... Or in your pocket!

Deken [00:05:34] Or in your pocket!

Breidenbach [00:05:35] We looked at these events on the screen in sort of a rudimentary two-dimensional projection display. Our conclusion was that the classification was correct. Then we just counted events by hand, not trusting anyone’s more sophisticated software. We found nothing wrong with the analysis: these runs were simply inconsistent with each other. We decided the problem was more interesting but still thought something must be wrong with the apparatus. We started lobbying to go back, after the summer and fall break, to go back and run again at these energies.

Deken [00:06:31] Um hm.

Breidenbach [00:06:31] Burton was dead set against it. During the break, he had engineered the increase in beam energy of SPEAR: SPEAR One transformed into SPEAR Two. And he didn't want to waste any time fooling around at three and a half GeV or so. Things brewed along and we argued with Burt. The people arguing with Burt were primarily [Roy] Schwitters, Vera [Lüth], and me. I believe I was there when Gerson [Goldhaber] and one of his postdocs at LBL suggested that the data at 3.1 GeV, where there was this inconsistency, had an excess of K shorts. Well, who knew what this could be? All of this finally came to Richter agreeing to let us "waste a weekend"...

Deken [00:07:45] [laughs].

Breidenbach [00:07:45] So we got to waste a weekend. During this time, I had a computer science graduate student who was sort of my graduate student, Lenny Shustek-.

Deken [00:08:15] Lenny Shustek! Oh, okay, I've heard of him.

Breidenbach [00:08:15] Lenny had done this marvelous job of hacking into the IBM mainframe; I think it was the Model 91... We hacked into it so that we could use it as a real- time computer to run the analysis. The online computer at SPEAR wasn’t nearly fast enough. During the first part of this around 3.1 GeV, the results were incredibly confusing. The cross-section was bigger, the cross-section was smaller and then the SPEAR vacuum system was failing, and that was all on Friday. There's this wonderful image with the vacuum system failing. SPEAR had an alarm which sounded like a submarine dive alarm to indicate a vacuum problem, and that shut us down. Norm Dean was running the vacuum group at that time. I don't know if you remember how much of a character Norm was?

Deken [00:09:30] I've seen the [memorial] tree that's dedicated to "Poor Old Norman, P.O.N." [1]

Breidenbach [00:09:43] Norman was a real character. He had a reputation as being super tough, but he really liked taking kids who had a horrible start in life and a lousy education and turning them into terrific vacuum techs. And these kids were super loyal to Norman. So, when there was an alarm, Norman and a couple of his acolytes would go and diagnose the problem, and once they figured out what the problem was, Norman decided whether this was a "one case problem" or a "two case problem" where the "cases" were quarts of beer. And he would send Alex Gallego out to what is now Safeway to buy the one or two cases. He would bring them back to the SPEAR control room. Then there would be Norman in the SPEAR control room with an open quart in each hand.

Deken [00:10:52] I've seen this picture, I think. Yes.

Breidenbach [00:10:55] Directing operations.

Deken [00:10:59] Those were the days.

Breidenbach [00:11:00] Can you imagine that today?!

Deken [00:11:03] No.

---

[1] Memorial is actually to Newman Kidd, “Poor Old Newman”; there is a separate memorial to Norman Dean: “In Memory Of Norman R. Dean 1938-1992 Who Dedicated His Life to The Success of SLAC"

Breidenbach [00:11:05] They got it fixed and we went on. It was getting extremely exciting. It was clear that there was some kind of resonance, but we didn't understand it. There was collective, accepted wisdom that it was very unlikely there would be any resonances at all in the e-plus e-minus system, and if there were, they would be very broad and not very high. What we realized is that the least count of the digital to analog converter that set the magnet energy that in turn set the beam energy, we were jumping over and around on this resonance. The machine was never designed to have enough resolution in setting the energy to be able to map out this thing. Someone had the clever idea that you could have a vernier adjustment on the beam energy by changing the RF frequency. It was a very cute idea.

Deken [00:12:08] So what kind of an adjustment did you just say?

Breidenbach [00:12:11] You could adjust the RF frequency over a modest range-- the RF that was keeping the beam going around. By changing that frequency, you could change the time it took the beam to go around SPEAR, and if you change the time, –and since it was going at the speed of light, you changed the orbit length, which meant the radius of the orbit had to change. A larger orbit meant the beam was at higher energy. You could change the energy in a continuous way since it was easy to adjust the frequency. It was a very clever idea and using this on Saturday, during the day, we mapped it out. ... We began to walk systematically up this resonance and began to slowly realize it was a narrow, tall resonance. I was there very late on Saturday night... I went home to get a little sleep, and came back in early in the morning. As I'm coming into SLAC, Ewan Paterson was going out. We stopped on the Sand Hill Road side of the guard shack -- which was indeed a shack then -- and he started telling me the cross-section was micro-barns! I yelled at him that 'he was an accelerator theorist; he didn't understand units -- it couldn't possibly be micro-barns.' Then I got in: he was right, it was micro-barns.

Deken [00:13:43] [Laughs].

Breidenbach [00:13:43] By Sunday, we had mapped this thing out, and we understood that it was indeed a resonance. The phones were going crazy. This was pre-email, and you could see this spreading east. It first got to the east coast, and a bit later jumped the Atlantic. Everyone was going happily nuts. I think Burton and Gerson and maybe George Trilling threw together a first draft. On Monday morning, Roy [Schwitters] and Vera [Lüth] and I sat in what was then the Group C Conference Room and worked on rewriting the paper. During Sunday night the information clearly got to M.I.T. Sam Ting was flying to SLAC on Sunday for an SPC meeting. His M.I.T. colleagues got a hold of him in some motel around here. They went collectively nuts because they had something they had been sitting on with their experiment at Brookhaven, which was really the same thing. But we knew nothing about it. Now there's controversy about whether we knew anything about it, but I was right in the thick of it, and I personally can guarantee you: we didn't know anything: Roy didn't know anything; Vera didn't know anything... I don't believe it's possible that Burt knew anything. There is no way he would have resisted that much if he knew anything. Ting realized that we had discovered the Psi and we had effectively instantly published by telephone. He had been sitting on this thing, worried about being careful and checking everything... For us, it was the equivalent of, once you knew where to look, of being beaten over the head with a great big sledgehammer. Statistics and significance were irrelevant. And he was doing a subtle experiment which needed to be checked and rechecked and triple checked. At any rate, we were working on this paper somewhere between ten and eleven.

Deken [00:16:46] This is on Monday?

Breidenbach [00:16:47] On Monday. [SLAC Director] Pief Panofsky calls and asks Roy, who was spokesman at that time, to come up to his office, and Roy was away for half an hour. When he returned, he was as white as a sheet and told Vera and me: "You're not gonna believe this: Sam has the same thing." So, we had an "emergency seminar" that day with Roy talking about the Psi and Sam [Ting] talking about the J. And three papers went to Phys Rev Letters, at effectively the same time: the paper from us; the paper from Sam and his M.I.T. collaborators; and the Italians at Frascati who had gone nuts trying to reach 3.1 GeV and they succeeded. The Frascati machine was figuratively smoking, but they found it too. They quickly wrote a manuscript and sent it over care of an Alitalia pilot who hand-carried it to the PRL editor's office in New York.

Deken [00:18:04] Oh my gosh.

Breidenbach [00:18:05] Great fun.

Deken [00:18:06] Yeah.

Breidenbach [00:18:07] That was the Psi, and it made the front page of The [NY] Times above the fold. Burton acquired instant amnesia. He totally forgot that he didn't want to do this, and we only had three days to do it.

Deken [00:18:29] Interesting.

Breidenbach [00:18:30] That caused a massive hullabaloo of all kinds of speculation in the field about... “What was the Psi?”

Deken [00:18:52] What was it?

Breidenbach [00:18:55] So there was all kinds of speculation. Terry Goldman, who was a theorist at SLAC then, and is now at Los Alamos, and I made the simplest model we could imagine of what was going on: that it was a new quark. We just calculated as if it were positronium and predicted where the next resonance would be. Then, in a week, Bob Melon changed the magnet controls on SPEAR so it could step in tiny little steps.

Bob Melon was a super engineer, and we also worked together later on the SLC Control System design. But he died very young of a heart attack while he was doing a medical stress test. Bob had changed SPEAR, and I changed the detector controls, so that we could automatically tell SPEAR to step the energy. In these 1-minute mini-runs, we got results back from the IBM 360, and plotted them on an old mechanical plotter. We tested just a little bit, and late one evening it was ready to go. We started a little bit below our estimate of where the second resonance would be, and I left Chuck Morehouse running it and went home for a little sleep. At 2:00 in the morning, Chuck called and says he’s got it.

Deken [00:21:10] Where you predicted it would be?

Breidenbach [00:21:11] Yes, the Psi prime. I said 'Well, that's very nice but I'm utterly exhausted.' I tried to go back to sleep. That was useless. I got up and tried to decide whether it was smart to go look for the next resonance. I calculated where the next one would be. But then I decided it's probably better and wiser that we go over this a few times and make it solid. I went in and we did exactly that. It was maybe four in the morning now, and it’s there, no question about it. I was annoyed at Burton for hogging much too much credit for the Psi discovery, and so I didn't call him and tell him that we had the Psi prime, but I did call Pief [Panofsky]. Adele [Panofsky, his wife] answers the phone -- remember, I'm a Postdoc.

Deken [00:22:17] Yeah.

Breidenbach [00:22:18] So Adele answers the phone. Maybe this is 5:00 in the morning; and says 'Pief is in the bathtub, shall I get him out?'.

Deken [00:22:25] Mm hmm.

Breidenbach [00:22:28] And I go, 'um... um… um… Yes.'.

Deken [00:22:29] [laughs].

Breidenbach [00:22:29] Pief was enormously happy and said he'd be right in. I didn't call Burt. Since we were scanning and using the IBM 360 mainframe, a problem was coming since the machine was normally taken down on that morning for maintenance. I called Chuck Dickens, who was running the computer center. I was quite friendly with Chuck and ~~said~~said, 'Hey Chuck, we’ve got a new particle - you can't take down the 91 - we need it.' He said 'OK.'

Deken [00:23:20] OK.

Breidenbach [00:23:22] This must have been around 7:00 AM and a few minutes later there's a broadcast announcement from the computer center to everyone who had a connection: "Due to discovery of a new particle at SPEAR the 360-91 will not be taken down for maintenance today."

Deken [00:23:44] [laughter] Blew your cover!

Breidenbach [00:23:48] Yes, totally.

Deken [00:23:51] Yeah.

Breidenbach [00:23:51] And then Burton comes waltzing in, and he is pissed. I should've known better. He was particularly pissed because Pief knew before him. Oh well. We had a plot of the cross section and not much else. And we had another emergency seminar that day...

Deken [00:24:20] That day?

Breidenbach [00:24:20] Which I gave, probably with the smallest number of slides of any talk I've ever given by far, like five. This was now getting into stuff that Sam Ting couldn't touch. They barely could see the Psi. This was the beginning. It did not take long for Charm to be accepted. It was the beginning of a huge program of finding out everything about the Psi, which was extraordinarily rich. We were publishing what felt like a paper a week, but it was probably only a paper every two weeks. This was just marvelous stuff on all the states of charmonium and charmonium decays. It opened up a new industry.

Breidenbach [00:25:34] I want to tell you a Harvey Lynch story. Pief brought Murray Gell-Mann around, who had his Nobel Prize and was accustomed to being treated with great respect. Harvey was always the best organized of us and had gotten very annoyed at all the visitors to our Control Room: it was barely controlled chaos.

Deken [00:25:54] Yeah.

Breidenbach [00:25:55] Pief shows up with MGM and Harvey had set up a "slave" of the event display in in the little kitchen that we had at SPEAR. Harvey literally pushed MGM and Pief into the kitchen and said 'go watch there we're too busy here. Don't bother us.' Pief was amused and Gell-Mann was shocked.

Deken [00:26:30] So Harvey was a post doc like you at that point?

Breidenbach [00:26:33] I suspect he was he was actually an assistant professor.

Deken [00:26:38] Oh, OK.

Breidenbach [00:26:40]

Deken [00:26:41] Generating a paper every two weeks, how long did that go on?

Breidenbach [00:26:49] It went on probably for another half year to a year before we had mined out what you could do with SPEAR. We also took advantage of SPEAR-2 to go to higher energy. But nestled right into this, a little bit independent, but nestled right in, was Martin Perl and Gary Feldman discovering the Tau.

Deken [00:27:28] Doing a separate experiment?

Breidenbach [00:27:32] No. They were looking at the data differently.

Deken [00:27:35] OK.

Breidenbach [00:27:36] This was the beginning of the τ discovery when we started taking data just a little bit above the ψ.

Deken [00:27:44] OK.

Breidenbach [00:28:27] In late '74, I believe, after the ψ and ψ', events where you could identify an e and a μ with nothing else, began to show up. Martin and Gary did a very careful analysis. No jumping up and down to claim anything. But by August 75 they wrote a PRL, "Evidence for anomalous Leptons in e-plus e-minus annihilation." It was 'evidence for', they didn't claim the discovery, and they were working very slowly and carefully. Gary led a modification to put in better muon identification on top of the detector. It didn't cover the whole solid angle at all, but it was experimentally the right thing to do... Here we were busy acing out DESY in Germany. By '78, this was in very good shape. The τ was real, and Perl eventually got a Nobel Prize for this. The total cross-section with the two resonances and the τ made sense. It was nice: the standard model was modified, there was the new quark charm and the τ and everything fit together. At this time, there was a decision, largely by Burt, to build a more sophisticated detector, MARK-II. MARK-II began life late at SPEAR. There was nothing as dramatic as any of this. It then went off to a second life at PEP, later called PEP-I. Again, there was nothing approaching the November Revolution, as the Charm discoveries were called. Then it went off to SLC, which starts a whole new major chapter.

Deken [00:31:20] what was the timing of SLC? Was it starting in parallel to the work that was being done on the Mark I?

Breidenbach [00:31:52] I don't know the date that it started. The idea for a linear collider had been kicking around for a while and Richter gets the credit for dreaming up a way of doing it with the SLAC accelerator, almost as it was (the almost is a big "almost").

Deken [00:32:16] Yeah.

Breidenbach [00:32:17] It was the one accelerator accelerating both the electrons and positrons and then bringing them around these large arcs shaped roughly like a tennis racket, and then colliding them at the apex of the tennis racket. That became SLC. There's a long story but the germ of that idea was Burton’s, and he gets credit for it.