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Particle Discovery at Brookhaven

Many stories have appeared in newspapers and science magazines here and abroad about the discovery of the J or psi particle announced jointly last November at the Stanford Linear Accelerator Center (SLAC) and Brookhaven National Laboratory (Research News, 6 Dec. 1974, p. 909). In the hope of eliminating confusion which has arisen from these articles, I present here a record of the Massachusetts Institute of Technology–Brookhaven experiment taken from our log books and records of 1974.

After the initial shakedown of our electron-pair spectrometer at Brookhaven, we started taking data from April to August on the high-mass region 3.5 to 5.5 Gev of $p + A \rightarrow e^- e^+ + x$. We made a 100-hour run and observed very few counts. During this period we measured the known meson decay region and observed genuine electron pairs, which showed the spectrometer was functioning properly.

At the end of August we changed the setting to cover the mass region from 2.5 to 4.0 Gev. The data exhibited a sharp peak at the mass of 3.1 with little background. The original mass plot of this run was presented in *Physical Review Letters* (*1*, figure 2). The measured width of the peak, $\Gamma < 5$ Mev, was presented in *Nuclear Physics B* (*2*). Before we had time for more studies, the accelerator was turned over to M. Schwartz (Stanford–New York University collaboration).

During the week of 13 October, we informed a few people of our results (for example, T. D. Lee of Columbia University), and in order to make sure that we received a priority over Schwartz on the use of the accelerator in the coming weeks, I informed the management of Brookhaven (in particular R. R. Rau, the director of high energy physics) of the existence of a sharp and narrow peak at a mass of 3.1 Gev.

I was considering announcing the results during the retirement ceremony (17–18 October) of V. F. Weisskopf. We postponed the announcement for two reasons: (i) We realized that old measurements by L. W. Smith (*3*) at Brookhaven had shown the direct μ^-/π^- ratio to be 10^{-4}, a mysterious number that seemed to not change from 2000 Gev to 30 Gev. We found that this ratio could not be easily explained by ρ, ω, ϕ, or J alone, indicating something more exciting might be just around the corner, and we decided to make direct measurement of this number by ourselves (however, even to this day, the mystery of $\mu^{-1}/\pi^{-1} = 10^{-4}$ is still not solved). (ii) There were speculations that high mass electron-pair production from proton-proton collisions came from a two-stage process of $p + p \rightarrow \pi + \ldots$ and $\pi + e^- e^+ \ldots$. This can be checked by a target thickness measurement. The yield from a two-stage process would increase quadratically with target thickness, whereas for a one-stage process the yield increases linearly.

During Weisskopf's fest we had quite a few discussions, in which we disclosed our discovery, with physicists who came for the occasion, for example W. Jentschke of CERN. On 22 October, U. Becker of our group gave an open seminar reporting our results to MIT high energy physics groups.

During the day of Becker's seminar, I was at Brookhaven and received a surprise visit from Schwartz, who had returned to Brookhaven to start after we had finished. He immediately wanted "to see the mass plot of the resonance around 3.0 Gev." Not wanting to spread information further and announce our results in this way, I denied his request and bet him $10 that there was no such resonance. I returned to our counting room and posted a memo which said, "I owe M. Schwartz $10." I paid him after the announcements of the discovery of the J particle. One member of our group, S. L. Wu, and I later talked with Schwartz and others and learned that, at the time of betting, not only Schwartz's group knew about the discovery, but many others knew as well.

In the last week of October, both Y. Y. Lee, of our group at Brookhaven, and I received many inquiries about our results. Members of our group working at the MIT Laboratory for Nuclear Science computer were besieged by people interested in seeing our mass plots. I also received a few phone calls from M. Deutsch at MIT suggesting that we should publish our results quickly, as, by then, many people knew about them.

On 6 November, I paid a visit to G. Trigg, editor of *Physical Review Letters*, to find out if the rules of publication without refereeing had been changed, and I wrote a simple draft.

On 10 November I went to SLAC for a program advisory committee meeting. The moment I checked into the hotel, I received a phone call from Deutsch, who mentioned that he had heard there was great excitement at SLAC but he did not know the nature of their results. I traced Rau to Los Alamos and informed him of my decision to announce our results and placed a call to S. Brodsky at SLAC informing him of our results. He was very excited but did not want to tell me about the SLAC results. He told me that he would arrange for me to give a presentation the next day. The next morning I walked into W. K. H. Panofsky's office at Stanford to inform him of our results. He mentioned that similar results had been obtained at SPEAR (the storage ring at SLAC) over the weekend.

Monday morning, 11 November, Wu called G. Bellettini, director of the Frascati laboratory in Italy, informing him of our results. On very short notice, the ADONE (storage ring) group succeeded in pushing the energy above its normal limit (2×1.5 Gev), set up a special 1 Mev per step searching program, and began their search on 13 November. Since they knew approximately where to look, after only 2 days, on 15 November, Bellettini informed us that a clear J signal had been observed at Frascati.

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I would like to add my recollections—aided by notes made at the time—to Ting's letter.

For several years, considerable resources of the MIT Laboratory for Nuclear Science (LNS) have been devoted to his systematic search for new vector mesons. My first knowledge of the discovery of the J particle came on 22 October 1974, when U. Becker presented a preliminary evaluation of the data to a laboratory seminar. The presentation was so cautious that the full significance of the data did not become clear to most participants. My own understanding was largely based on a private discussion following the seminar. At this point, the Ting group was obviously caught between the contradictory desires of communicating the discovery to other friends and avoiding premature dissemination of specific quantitative results which might still be subject to last-minute corrections. In accordance with their explicit request, I restricted my discussions with outsiders

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borne and his associates showed that the track structure in the emulsion was consistent with the monopole explanation, but not with the superheavy nucleus.

While this evidence appears convincing, researchers will remain skeptical until more data and hopefully more monopoles are available. For example, implausible though it may seem, an electrically charged particle with a Z of about 70 and a mass of 10,000 protons could also have caused the observed track. Some physicists, including Owen Chamberlain of the University of California's Lawrence Berkeley Laboratory who has had a chance to examine the experimental results, believe that there is a small but nonnegligible chance that a less massive charged particle could have caused the observed detector response. If the particle suffered one or more collisions that caused it to lose some of its charge as it passed through the Lexan, the damage would approximate the uniform damage expected for a monopole.

Other scientists, such as Luis Alvarez of the Lawrence Berkeley Laboratory who was involved in the search for monopoles in the moon rocks, want to know why previous attempts to find monopoles failed. More than one unsuccessful monopole hunter suggested that monopoles with the charge, mass, and velocity reported by Price and his colleagues ought also to have been detected in their experiments. It is important that this question have a satisfactory answer because the effective collecting power (measured in square meter-years) of the other experiments exceeds that of the balloon experiments by about a factor of 10^5. It is to be noted, however, that this large collecting power is based on the assumption of certain properties of monopoles that have not been verified.

The best answer would be to catch a monopole or at least obtain more monopole tracks. Price, Osborne, and their associates are looking into the possibility of an expanded balloon experiment embodying perhaps 50 balloons with 40 square meter detectors. Antarctica might be a good location for the search, they suggest, because the continuous sunshine in the summer there would enable balloons to be kept aloft for long periods.

Regardless of whether the present report of a magnetic monopole is confirmed by future experiments, it might be wise to recall what Dirac noted in his original paper: since the possibility of the existence of monopoles is not precluded by quantum mechanics, it would be surprising if nature did not make use of this possibility.

—ARTHUR L. ROBINSON

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LETTERS

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and even with colleagues at MIT to vague hints that an interesting structure had been observed in the electron pair spectrum. Some colleagues interpreted my remarks as important news, others did not. B. Richter (a member of the SLAC experimental team), who was in Cambridge to give the Loeb lectures at Harvard, did not seem particularly impressed by my story—told at a cocktail party at the end of October. I now regret having been so ambiguous in my remarks and I apologize to him and others for not being more explicit.

In any case, it became obvious that the news was spreading through the physics community. On 4 or 5 November, a technician working for a different MIT–LNS group at Fermilab remarked in a telephone call that the Ting group at Brookhaven was preparing a champagne party to celebrate their discovery of a new particle. I repeatedly urged members of the Ting group to end this state of "secret publication." The first news of the beautiful SPEAR experiments reached me on 10 November, when D. Frisch relayed the gist of a telephone call he had received from SLAC, and I, in turn, alerted Ting, who was on his way there.

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Island Sanctuary

The system of primary wildlife reserves which A. L. Sullivan and M. L. Shaffer examine in their article (4 July, p. 13) is an essential system for the ensuring of a diversity of plant and animal species in the future. They rightly point out the need for a hierarchy in developing such reserves.

I should like to offer a reserve, an established sanctuary, a coral atoll in the South Pacific which is already dedicated to scientific research. This atoll is 5 kilometers in diameter, 700 meters from outer reef to lagoon, and 5 meters above sea level. Two hundred years ago it served as the center of a Polynesian kingdom.

For those who wish to work in an island biogeography environment, the sanctuary provides a unique opportunity. Scientists interested in working on the atoll are cordially invited to respond. No grants are available, but the committee will help in other ways and housing will be provided.

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