

DISCOVERY OF THE PSI PARTICLES: A PERSONAL PERSPECTIVE

By *Burton Richter*

The discovery of the psi particles marks for me a high point in 17 years of scientific work on colliding beams. This work began with the start of construction, in 1958, of the first colliding beam machine: the Princeton-Stanford 500 MeV electron-electron colliding-beam machine that was built in collaboration with W. C. Barber, B. Gittelman, and G. K. O'Neill at the High Energy Physics Laboratory (HEPL) on the Stanford campus. Preliminary design of a large electron-positron storage ring began in 1961 at just about the time that the SLAC project was authorized, and in 1964 the first formal proposal for funds to construct this machine was submitted to government funding agencies.

The effort to gain approval for this project extended through six years, eventually culminating in August 1970 with the start of construction of the SPEAR project. In April of 1972 the machine was ready for its first operation, and the experimental physics program began early in 1973. I have taken a few detours along the way into other kinds of experiments--tests of quantum electrodynamics, high energy photoproduction, pion-proton interactions--but my first love has always been the physics that could be done with colliding beams. I have been led on by a naive picture: positron and electron, particle and antiparticle, annihilating and forming a state of simple quantum numbers and enormous energy density from which all of the elementary particles could be born.

It has been particularly satisfying to have witnessed the birth of a new class of particles, the ψ 's with their completely unexpected properties. Every experimentalist dreams of making the great discovery--a discovery that will change the direction of scientific thought. I don't know yet if the colliding-beam machines and the new particles that we have discovered with them will cause a sharp change in that direction, but surely they have bent it a bit.

The group that collaborated on the experiment which uncovered the ψ particles is a very large one, even by the standards of high-energy physics. It is large because the apparatus used in the experiment is huge. The basic outlines of our detection equipment were set down in the first electron-positron machine proposal in 1964. Advancing technology has since changed the detector in detail but not in concept. It now had a magnetic field volume of over 20 cubic meters and is filled with particle-tracking equipment, trigger devices, and particle-identification devices. In 1970, when the construction of the SPEAR storage ring began, I quickly realized that my group

was too small to handle the construction of both the storage ring and the detector, so I began to look around for some collaborators for the experiment. William Chinowsky, Gerson Goldhaber and George Trilling of the Lawrence Berkeley Laboratory (LBL) and Martin Perl of the Stanford Linear Accelerator Center (SLAC) joined our experiment with their groups, and the large detection apparatus was completed by the time that the storage ring was ready to operate. Our first paper on the ψ particles contains almost two authors for each cubic meter of magnetic field. The dynamics of such a large group would likely be as interesting to a sociologist as the experiments are to a physicist.

Gerson Goldhaber has recorded his impressions of the events surrounding the discovery of the first psi particle in a separate article in this issue of the *Beam Line*. My impressions will be somewhat different from his, since each of us perceives from our own point of view. The things that stick most clearly in my mind about that great Sunday, November 10, 1974, are the huge crowd of people in the SPEAR control room, the smiles on all of the faces, and the general feeling of euphoria that possessed all of us. Never before had so many members of the collaboration been present at any one time. The accelerator physicists who had helped to build SPEAR were also there. Machine operators were there. Experimentalists from other groups at SLAC drifted in during the day, as did many of the theorists. All of us were talking and smiling and watching the experimental events as they were analyzed and reconstructed on the computer display scope.

While many of us felt that we had to find *something* new because of the peculiar behavior of the data (described in Gerson's article), Vera Lüth was so sure of the outcome that, during the previous day, she had started to cool down a magnum of champagne in our refrigerator. That magnum and quite a bit more disappeared as the day wore on.

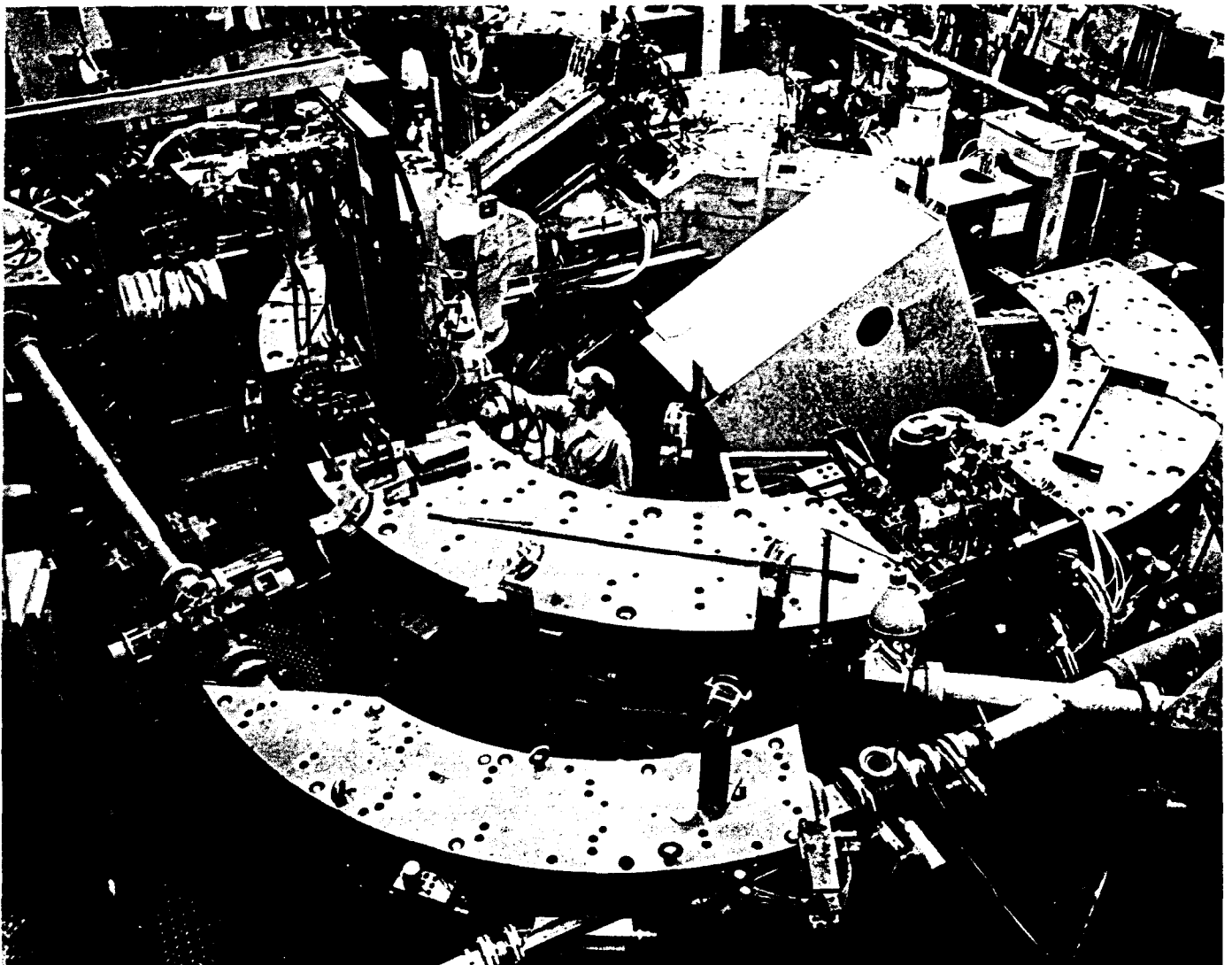
Gerson, Willy and I discussed publication of the results and decided not to wait on further detailed experiments, but rather to send the information out immediately. All of the collaborators who could be reached concurred in this decision. It was clear to all of us that nothing like this new particle had ever been seen before, so it didn't matter a bit at that point what the exact cross section was to the last 20%. We had used the experimental apparatus before, our computer codes had all been carefully debugged, and we had already put in a great deal of work rechecking the whole system when we had earlier found the peculiar experimental data. With no reason to hesitate, we immediately began to write up an "on-line" paper describing the results.

News Of The MIT-BNL Experiments

As it happened, a meeting of SLAC's Program Advisory (scheduling) Committee was scheduled for the following morning, November 11. This Committee consists of nine members, six of whom are from other institutions, who provide advice on the scientific merit of proposals for experiments to be done at SLAC. One of the Committee members was Samuel Ting of MIT, who was the leader of a joint MIT-Brookhaven group that had been conducting an experiment at Brookhaven National Laboratory. When I met Sam early that morning, he said to me, "Burt, I have some interesting physics to tell you about." My response was, "Sam, I have some interesting physics to tell you about!"

What this conversation lacked in sparkle it more than made up for in astonishing coincidence, for it soon became clear that Ting's group had discovered the very same particle in their experiment at Brookhaven, and in fact they too were in the process of writing up their results for publication! It will come as no surprise that the scheduling committee did not get very much of their business attended to that day.

Since the new results were much too important to sit on, the word was passed that special seminars would be presented that day at SLAC and at LBL to announce the findings to all who were interested. Starting at about noon at SLAC,



This photo shows the figure-8-shaped 500 MeV electron-electron storage rings that were built as a collaborative Stanford-Princeton project at the High Energy Physics Lab at Stanford. The orientation of this double-ring machine in the photo is about like this: \odot . The odd-shaped magnet in the foreground is a part of the beam-injection system. This was the first colliding-beam machine to carry out a successful program of physics experiments, its first results being obtained in 1965. Burt Richter was one of four principal collaborators in the construction and research use of these rings. (Stanford News & Publications photo.)

a seminar was given in the auditorium to an audience that filled to overflowing the room's 350-seat capacity. Sam Ting described the work of his group, in which the production of muon pairs was observed in proton-proton collisions. In a sense Ting's experiment is the inverse of the SPEAR experiment, in which electron-positron collisions lead to the production of strongly interacting particles. Roy Schwitters of SLAC presented the SPEAR results, and he did a remarkable job of adapting his presentation on the spot to the nature of what must have been the most unusual audience ever to attend a physics seminar at SLAC. For every experimental or theoretical physicist present there must have been two or three other persons--secretaries, technicians, administrators, engineers, craftsmen, librarians, clerical workers. . . . I don't know how much of what Roy was saying was understandable in detail, but my impression is that nearly every one shared in the enormous sense of excitement and enthusiasm that pervaded the room.

The Search For More New Particles

Once the initial furor had died down a bit, our experimental group faced the decision of what to do next. Clearly there was a great deal to be learned about our new ψ particle, but we also wanted to find out whether there were any more such remarkable states lurking about. Ewan Paterson, Robert Melen and I considered how we might effectively hunt for such additional states, and we decided to try to make SPEAR operate in a "scanning" mode, where the energy of the machine could be increased in steps of about 1 MeV every minute or two of operation. Ewan and Bob were able to make the necessary control system modifications for such a scan mode.

Our next decision was where to begin the search. At that time the machine could run at center-of-mass energies between about 3 and 6 GeV. This meant that, at a scan speed that would provide a suitable signal-to-noise ratio, it would take a couple of weeks of running to cover the full energy range. Opinions were solicited, and it turned out that only one member of the collaboration had a strong prejudice concerning the energy region that should be looked at first. This was Martin Breidenbach of SLAC, who had put together a theoretical model similar to that of the hydrogen atom which predicted the energy at which the next ψ -like state should occur. One version of this model used the 3.1 GeV mass of the ψ as the $n=1$ state of "hydrogen" and 4 GeV as the unbounded mass, which implied a mass of 3.78 GeV for the $n=2$ state. The other version of Breidenbach's model used the photon with zero mass as the $n=1$ state and the 3.1 GeV ψ mass as the $n=2$ state, which implied an $n=3$ state of 3.67 GeV. Since Marty had a model, and since he had also arranged the use of SLAC's big computers in real time to give in-



This was the SPEAR site in June of 1970, just after the initial foundation work had begun. From the left, Bill Davies-White, Bob Gould and John Rees check things out.

stantaneous feedback for our data analysis, we decided to begin the scan at a center-of-mass energy of 3.6 GeV.

The Second Psi Particle

On Wednesday, November 20, we tested the system by making a calibration scan run over the ψ particle at 3.1 GeV. At 12:30 AM on Thursday, November 21, we started our first actual scan. At 2:30 AM, a promising bump appeared in the data. The run was stopped and the machine energy was reduced to 3.69 GeV. The run was then restarted at a slower scan speed. At 3:20 AM, with Robert Stege operating the storage ring and Charles Morehouse and Alan Litke running the experiment, the second ψ particle was discovered. The three of them phoned Breidenbach and then sat collecting and admiring the data until I arrived at about 5:30 AM.

After recovering from the shock of finding the second ψ particle on our first scan, I called my wife, Laurose, to tell her the news. Since it was her birthday, I offered her ψ_2 as a present and told her that a more conventional birthday celebration would have to be postponed for about a week. I also called some of our other collaborators and asked them to inform the remaining members of the group. We knew that with a group as large as ours it would be impossible to keep this second discovery quiet for very long, but we did want to get in another day's worth of data collection before we made our announcement to the world.

I had to phone the head of SLAC's computation center, Charles Dickens, because we needed the big computers for real-time data analysis and they were unfortunately scheduled to be shut down for maintenance at 7:30 AM. I asked him to postpone the maintenance, told him the reason,

and swore him to secrecy. Dickens, in turn, then asked the computation center staff to keep going and told them why. At the scheduled shutdown time of 7:30 AM, a message went out from the computer to our teletype machine, to all other teletypes at SLAC, and to all the other teletype machines at universities that happened to be tied in to the SLAC computer at that particular time:

DUE TO NEW PARTICLE DISCOVERY...SYSTEM
WAS NOT TAKEN DOWN THIS MORNING

The news of our four-hour-old particle was no longer exclusively ours.

Once again the control room began to fill with members of our experimental group, members of the SPEAR group, other theorists, other experimentalists and--so it seemed--everyone else in the laboratory. After 9 AM or so the day is a complete fog in my memory. Nothing in particular stands out--only the repeated very strong sense of excitement in uncovering the second of these remarkable, unexpected and rather exotic new particles. On the following day, Marty Briedenbach gave another short seminar in the SLAC auditorium to the same kind of large and varied audience that Roy Schwitters had address-

ed 10 days earlier. Gerson Goldhaber and I did our on-line paper-writing act again, and most of Friday was spent in answering the phone and trying to get the data well enough organized to send it out for publication. On Saturday, the finished version of the paper was whisked off to *Physical Review Letters*.

Those two weeks in November 1974 were certainly the most exhilarating in my career as a physicist. I have never worked at such a fever pitch before, and I loved every minute of it. Since that time, our joint SLAC-LBL group has gone on to delineate the properties of these new particles and to discover a number of other related states that now make up a dozen or so members of the "psion" family. We have observed certain new states that are most easily interpreted as "charmed" particles, and we have also accumulated a sizable number of curious events that may result from the creation of a new class of "heavy leptons." The entire SLAC-LBL group has been enjoying a marvelous experience in our efforts to understand more deeply the workings of Nature. We hope to continue the learning, for that will automatically continue the enjoyment.

--Burt Richter



Burt and Laurose Richter are shown here with UC-Santa Cruz physicist Stan Flatté during the Nobel Prize celebration at SLAC on October 20. (Stanford News & Publications photo.)