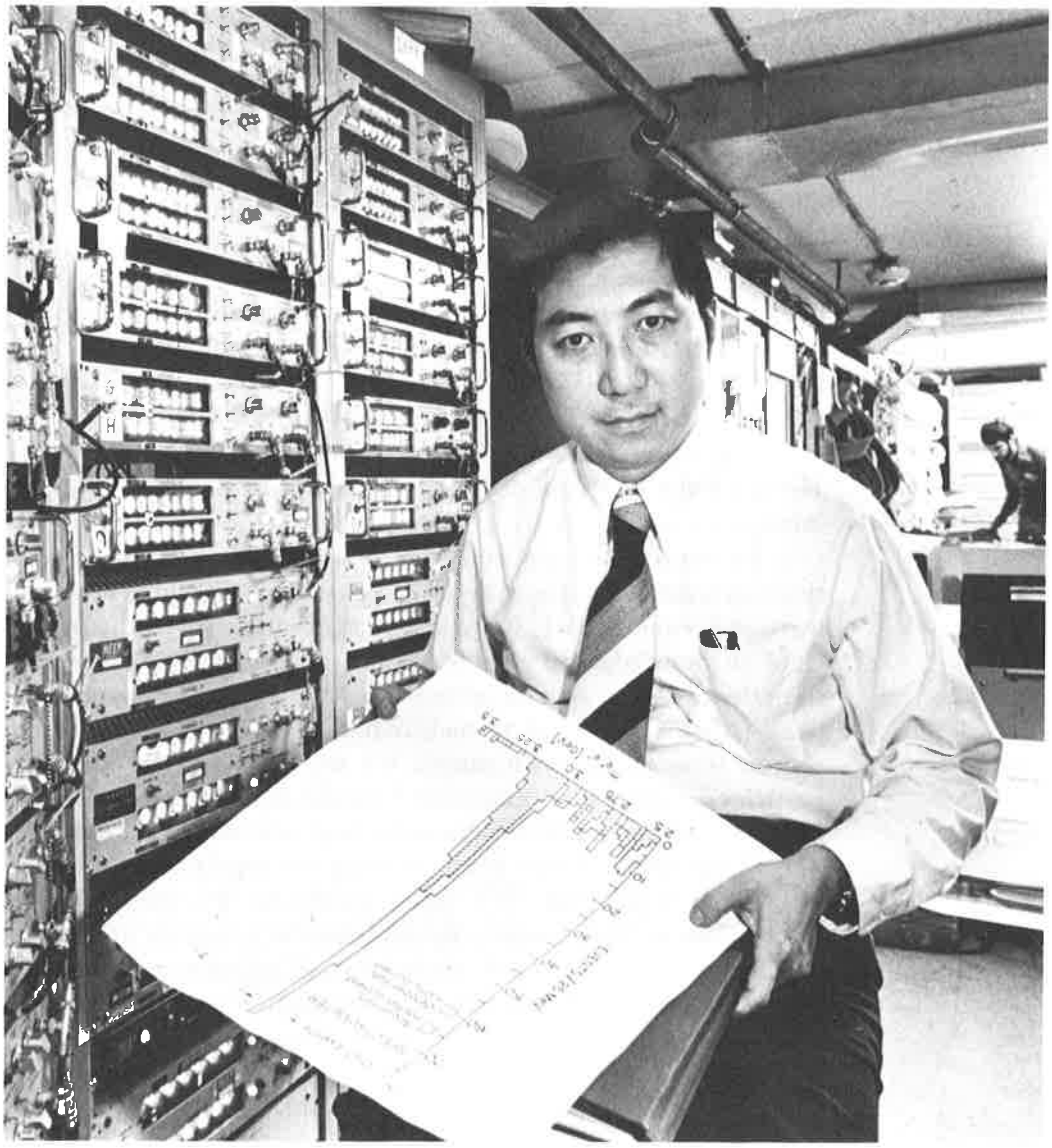
DISCOVERY STORY



Discovery Story author Samuel Ting in the experimental trailer on the floor of the Brookhaven AGS where the discovery of the J particle was made. To the left are racks of equipment used to record incoming data. Ting holds a histogram clearly indicating the existence of the J (November, 1974). [Photo: Brookhaven National Laboratory.]

One Researcher's Personal Account

by Samuel Ting

IN the spring of 1970, after five years of continuous work with my American-German research group at DESY[1], I became exhausted and on the advice of my physician I took a year off.

During that year, Ulrich Becker of my group proposed a new spectrometer which was able to measure effective masses of pairs of any particles with a good resolution of ±5 MeV. This enabled my group to perform a set of beautiful experiments which definitely established the ρ-ω interference, whose observation[2] had been a subject of controversy ever since the discovery of the vector mesons. Both the hadronic decay modes of the ρ and ω, $\rho^0 \to \pi^+\pi^-$

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1. DESY = Deutsches Elektronen Synchrotron, the 7.5 GeV Electron Synchrotron in Hamburg.
2. See Discovery of First Neutral Vector Meson, p. 79.

and $\omega \to \pi^+\pi^-$, and the leptonic decays $\rho \to e^- e^+$ and $\omega \to e^- e^+$, were measured[3].

It was during my year of rest that I had many discussions with friends and time to think over carefully the implications of this and previous work of my group, as well as read others' work in the field to consider what experiments we should do at the new high-energy accelerators that were soon to be completed at that time. The first proton colliding-beam system, the Intersecting Storage Ring (ISR), was just about to become operational at CERN, in Geneva. The construction of the 500 GeV proton accelerator at the Fermi Lab, Batavia was nearing completion; and in Hamburg the $e^+ e^-$ and $e^- e^-$ colliding-beam accelerators were in an advanced stage.

By the end of my sabbatical I had come to the conclusion that the most interesting physics lay in the area in which my group was most experienced. We could contribute most to the present state of knowledge by measuring, in a systematic and precise way, the effective mass spectra of both electron pairs ($e^+ e^-$) and muon pairs ($\mu^+ \mu^-$) produced in hadron-hadron collisions—specifically, proton interactions with nuclei. We would search for long-lived particles decaying into these pairs from the lowest mass reasonable, 1 GeV, to the highest attainable with these new machines, $\sim$50 GeV

Since 1965, I had been working on experiments involving electron (e^-)-positron (e^+) pairs produced by hadron-hadron interactions at high energies. By studying the reactions of proton + any hadron → $e^+ e^-$ + recoil, three kinds of physics can be learned:

(a) If a few-billion-electron-volt photon beam is used, the $e^+ e^-$ production rate can be compared with predictions of quantum electrodynamics at large momentum transfer or small distances (10^{-14} cm).

(b) The $e^+ e^-$ decay modes of photon-like particles, like the ρ, ω, and ϕ, can be studied and the coupling strength between photons and these massive photon-like particles can be measured.

(c) It is possible to search for additional new photon-like particles.

But hadron-hadron collisions are strong interactions, while $e^+ e^-$ and $\mu^+ \mu^-$ pair production are electromagnetic processes. The rate of electromagnetic pair production in strong interactions is extremely low compared to that for the hadron pair production, such as proton-antiproton pairs (see Margin Note).

In order to study the reaction hadron + proton → $e^+ e^-$ + recoil to a 1% accuracy, we needed to design an experiment which satisfied two conditions: (a) to obtain sufficient $e^+ e^-$ rates, a detector had to be designed which could accept a high flux incident beam, typically of 10^{11}-10^{12} protons/sec and (b) to make sure that

Low Mass Region Best Investigated At Low Energies

A low mass region can best be investigated at low incident energies; a high mass region is better examined using higher energies. This is because the electrons and muons from the pion decays mask the electrons and muons from pair production, which is the process that Ting and his group were recording. The pion decays can best be 'rejected'—that is, excluded from examination—in a low mass region by using a lower incident energy beam.

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[3] Alvensleben, H., et al, *Phys. Rev. Lett.* 25, 1373 (1970); *Phys. Rev. Lett.* 27, 888 (1971).

what was measured were lepton pairs, $e^+ e^-$ and $\mu^+ \mu^-$, the detector had to be able to *reject* hadron pairs by a factor of 10^6-10^8 or more.

During 1971-1972, Ulrich Becker, Min Chen and I had many discussions on how to proceed. It soon became clear to us that in order to cover the mass region up to 50 GeV we would need to perform large-scale experiments at three different machines:

¶ The Intersecting Storage Ring in Geneva (for masses from 5 GeV to 50 GeV).

¶ The 31 GeV Alternating Gradient Synchrotron (AGS) at Brookhaven National Laboratory, Long Island (for masses from 1.5 to 5.5 GeV).

¶ The 7.5 GeV Electron Synchrotron (DESY) in Hamburg (for masses from 0.5 to 2.0 GeV).

During the same period, we performed a series of Monte Carlo calculations on detailed designs of the spectrometers needed and went over the logistic problems of performing three large experiments in three different countries. We came to the conclusion that in order to perform these experiments carefully, we would try to set them up simultaneously, but could only run one at a time. In this way we would concentrate all our efforts on one experiment, finish it quickly and go on to the next.

In the spring of 1972, we submitted proposals to DESY and Brookhaven; both were approved right away. The third proposal, submitted to ISR, involved enclosing one whole colliding region (intersection region) with a magnetic detector enclosing a solid angle of 4π steradians! It was submitted jointly with the Universities of Pisa, Genoa and Harvard University and was approved in the fall of 1973.

The Spectrometer System

From our early experience at DESY, we felt the best way to build a detector that could handle 2×10^{12} protons per pulse, and at the same time have a large mass acceptance of 2 GeV and mass resolution of 5 MeV, would be to detect electron pairs with a large double-arm spectrometer locating most of the detectors behind the magnet so that they would not 'view' the target directly. To simplify analysis and to obtain better mass resolution, we used the so-called 'orthogonal dispersion' concept[4] in which the magnets bend the particles vertically to measure their momentum, while the angle before bending is measured in the horizontal plane, thus making the measurements of the angles and momenta entirely independent. Other features of our spectrometer are described in the Explanatory Technical Note on the following page.

During the construction of our spectrometers, and indeed

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Monte Carlo Calculations

A high-energy physics experiment typically requires a complex array of expensive electronic and mechanical equipment. To measure properly the desired physical events, a well-designed electronic logic system that correctly accepts appropriate events and rejects all others is necessary.

Thus it is often desirable to make a computer model, or analog, of the experimental apparatus before it is constructed. By inputting known particle and system parameters, events can be randomly generated by the computer and statistics compiled to check the logic circuit. This method can also predict the anticipated spectrum of events. Thus any deviation from the computer prediction is then more likely to be indicative of a new physical phenomenon than an error in the design of the system.

This modeling process is referred to as a Monte Carlo calculation or analysis program since the events are randomly generated just as the numbers are by dice rolls or roulette wheels at casinos in Monte Carlo, Monaco. The Monte Carlo method also provides researchers with evidence that the real system will perform as designed.

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[4] Cross, W., *Rev. Sci. Inst.* 22, 717(1951); Ankenbrandt, C.M. et al, *IEEE Trans. Nucl. Sci.* 12, 113(1955); Brody, H. et al, *Phys. Rev. Lett.* 24, 948(1970).

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the entire experiment, we encountered many criticisms. The problem was that in order to gain a good mass resolution, it was necessary to build a spectrometer that was very expensive. One eminent physicist made the remark that this type of spectrometer is only good for looking for narrow resonances—and there are no narrow resonances. Nevertheless, since I usually do not have much confidence in theoretical arguments, we decided to proceed with our original design.

From the summer of '72 to the summer of '73, we constructed all the detectors, wrote a Monte Carlo analysis program and also began to estimate the soft neutron background which might trigger our proportional chambers and counters, but unfortunately obtained no reliable estimates.

In the fall of '73, Y. Y. Lee of Brookhaven joined us and designed an excellent intense proton beam for the experiment. We began setting up our experiment on the experimental floor and soon realized that we would need 10,000 tons of concrete for shielding. This was solved by borrowing all the shielding from the Cambridge Electron Accelerator which had just closed down due to budget cuts. To reduce the background of slow neutrons, we bought 10,000 lbs of borax soap and placed it around the magnets and Cerenkov counters. Everything went very smoothly until BANG!

Late in December, 1973, I went to DESY to discuss with Dr. Rohde the progress of his spectrometer. On the night before my return to Brookhaven, we had a traditional Christmas party in my office. Just as we were about to sit down to eat, I received an overseas call from J. J. Aubert, a very gifted French physicist from Orsay who was spending a year with us. He quickly informed me that there had been an implosion of the mylar window on one of the large Cerenkov counters during the process of testing. The force of the implosion was so strong that all the mirrors were broken to pieces of about one square centimeter, and the implosion was heard over the entire AGS floor. It was fortunate that at the time no one was near the counter and there were no serious injuries to any personnel.

Following the implosion we made an investigation but could not find the reason it happened, nor could we repeat the implosion under identical conditions. Nevertheless, it was decided to remachine all the contact surfaces between the window and mylar foil and install shutters around these Cerenkov counters during the pumping-down process.

Radiation Problems

In April 1974, we finished the setup of the experiment and started bringing an intense proton beam into the area. We soon found that the radiation level in our counting room was 0.2 roentgen/hour. This implied that our physicists would receive the maximum allowable yearly dose in 24 hours! We searched very hard for a period of two to three weeks looking for the reason and became extremely worried whether we could proceed with the experiment at all.

One day Becker was walking around with a Geiger counter when he suddenly noticed that most of the radiation was coming from one particular place in the mountains of shielding. Upon close investigation we found out that even though we had 10,000 tons of shielding and blocks with concrete, the most important region—the top of the beam stopper—was not shielded at all. After this correction, radiation levels went down to a minimum and we were able to proceed with the experiment.

From April to August, we did the routine tune-ups and found the detectors performing as designed. We were able to use 10^{12} protons per pulse and took some data in the high mass region of 4 GeV–5GeV. The small pair spectrometer also functioned beautifully and enabled us to calibrate the detector with a pure electron beam. However, analysis of the data showed very few real electron-positron pairs.

The Peak!

By the end of August we had started again. This time we tuned the magnets to accept an effective mass of 2.5-4.0 GeV. Immediately, we saw real electron pairs which *all peaked* narrowly about one effective-mass value: 3.1 GeV (Fig. 1). A more detailed analysis shows the width of the particle (Fig. 2, p. 124) to be less than 5 MeV! Before we could investigate the nature of this peak, we ran out of accelerator time and had to stop to allow the next scheduled experimental team, led by Mel Schwartz, to use the proton beam.

During the week of October 13, 1974, we had discussions with a few people informing them of our results (for example, T. D. Lee of Columbia and Lee Grodzins of MIT). Additionally, in order to ensure that we be given priority over Schwartz in using the accelerator, 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.

We discussed the name of the new particle for some time. Someone pointed out to me that the really exciting stable particles are designated by Roman characters—like the postulated W°, the intermediate vector boson, the Z°, etc.—whereas the "classical" particles have Greek designations like ρ, ω, etc. This, combined with the fact that our work in the last decade has been concentrated on the electromagnetic current, $j_\mu(x)$, and that J is the symbol used to denote internal rotations or spins, gave us the idea

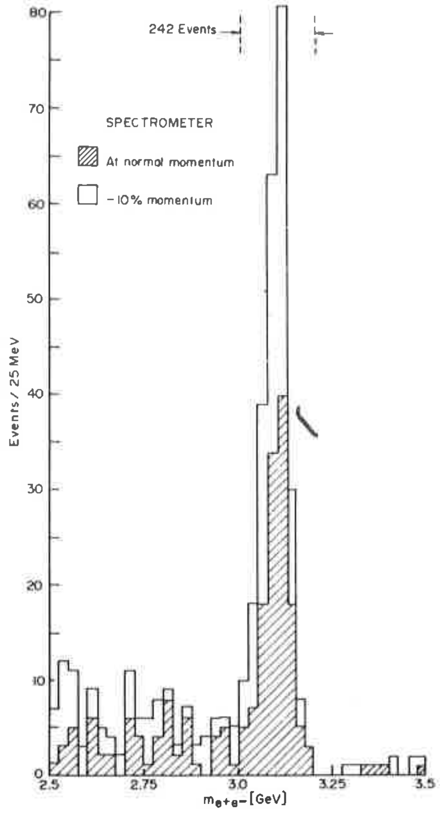

Fig. 1.

A plot of data taken on-line at the Brookhaven National Laboratory in August and October, 1974. The J particle can be clearly seen in this data. From these events and assuming that

$$\frac{d\sigma}{dp_\perp} \propto e^{-6p_\perp}$$

the production cross section is calculated to be

$$\frac{\Gamma_{J\to ee}}{\Gamma_{J\to all}}\,\sigma \cong 10^{-34}\,cm^2$$

for J (3100 MeV.). The cross section for production of J (3700 MeV) is calculated to be ≤1% that of the J (3100).

A view of the double-arm spectrometer which was used in the detection of the J particle, Brookhaven Alternating Gradient Sychrotron, 1974.

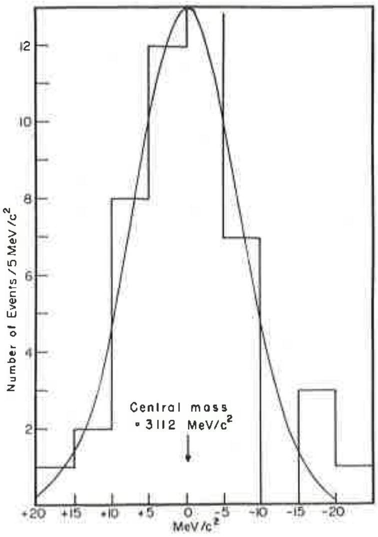

Fig. 2.

Measurement of the width of the J particle by Ting's group, October 1974. The width is shown to be less than 5 MeV. Subsequent measurements by other groups have set the width's upper limit at 100 KeV.

to call this particle the J particle.

I was considering announcing our results during the retirement ceremony, called the "Vikifest," for V. F. Weisskopf which was to be held October 17,18. We postponed the announcement for two reasons. First, we realized that there were earlier Brookhaven measurements[5] of direct production of muons and pions in nucleon-nucleon collisions which gave the μ^-/π^+ ratio as 10^{-4}, a mysterious ratio that seemed not to change from 2000 GeV down to 30 GeV. This value was an order of magnitude larger than theoretically expected in terms of the three known vector mesons, ρ, ω and ϕ which, at that time, were the only possible 'intermediaries' between the strong and electromagnetic interactions. We then added the J meson to the three and found that the linear combination of the four vector mesons could not explain the μ^-/π^- ratio either (it increased the expected ratio by a small amount). This we took as an indication that something more exciting might be just around the corner, so we decided to make a direct measurement of this number ourselves. Second, there were speculations on high mass e^+e^- pair-production from proton-proton collisions as coming from a two-step process: $p + p \rightarrow \pi + \ldots$ where the pion undergoes a second collision $\pi + p \rightarrow e^+e^- \ldots$ This could be checked by a measurement based on target thickness. The yield from a two-step process would increase quadratically with target thickness, whereas for a one-step process the yield increases linearly.

During Weisskopf's fest, members of our group had discussions with visiting physicists on our experiments. I made no announcement, but some members of my group spoke privately about our result. For example, S. L. Wu told W. K. Jentschke of CERN of the existence of the peak in the e^+e^- spectra. On October 22, Ulrich Becker (who had gone through a complicated operation during the summer and had not been at BNL during the data-taking) presented a seminar to the high-energy physics groups at MIT. The other members of our group were all working at BNL during Becker's presentation.

During that same day, I was at Brookhaven and received a surprise visit from Mel Schwartz who had returned to resume his experiment after we had finished. He immediately wanted "to see the mass plot of the resonance around 3 GeV." Not wanting to spread information further or 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 other

5. Leipuner, L. B., et al., *Phys. Rev. Lett.*, 34, 103 (1975).

physicists and learned that at the time of betting not only Schwartz's group knew about the discovery but many others as well.

In the last week of October, Y. Y. Lee and I received many inquiries about our results. Members of our group working at the MIT-LNS computer were besieged by people interested in seeing our mass plots. I also received a few phone calls from Martin Deutsch suggesting that we should publish our results quickly, as he had had discussions with people and by now many people knew about our findings. Deutsch later described these events, as he recalled them, in a letter published in *Science*[6].

Search for More Particles

Although we were absolutely sure about the existence of the J particle for some time, we did not decide to announce it because we suspected that the anomalous μ^-/π^- ratio implied new phenomena which had to be looked for. Since we could not measure the μ^-/π^- ratio with our spectrometer, we decided to look into the possibility of investigating the e^-/π^- ratio.

We began various test runs to understand the problems involved in doing the e/π experiment. The most important tests were runs of different e^- momenta as a function of incident proton intensities to check the single-arm backgrounds and the data-recording capability of the computer.

On Thursday, November 7, we made a major change in the spectrometer (see Fig. 3, p. 130) to start the new experiment to search for more particles. We began by measuring the mysterious e/π ourselves. We changed the electronic logic, the target, and reduced the incident proton-beam intensity by almost two orders of magnitude. To identify the e^- background due to the decay of π° mesons, we inserted thin aluminum converters in front of the spectrometer to increase the $\gamma \rightarrow e^-e^+$ conversion. This, together with the C_B counter (see Explanatory Physics Note p.119) which measures the $\pi \rightarrow \gamma e^- e^+$ directly, enabled us to control the major e^- background contribution.

We had planned to follow this e/π measurement with another change in the spectrometer by installing new high-pressure Cerenkov counters and measuring systematically hadron pairs (K^+K^-, $\pi^+\pi^-$, $\bar{p}p$, etc.) to find out how many other particles exist that do not decay to e^+e^- but into hadrons.

Decision to Publish

In the meantime, since the end of October, Chen, Becker and others in the group had been insisting that we quickly publish our results. I was very much puzzled by the $\mu/\pi = 10^{-4}$ ratio and wanted to know how many particles existed. Chen, however, pointed out

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[6] *An excerpt of Martin Deutsch's letter to SCIENCE (5 Sept. 1975) has been selected by the Editor and is reproduced below:*

> 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 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 to others for not being more explicit.

that "a bird in the hand is worth two in the bush." Under pressure, I finally decided to publish our results of J alone.

However, on November 6 I had paid a visit to George Trigg, Editor of *Physical Review Letters*, to find out if the rules for publication without refereeing had been changed. Following that visit, I had written a simple draft in the style of our quantum electrodynamics paper of 1967. The paper emphasized only the experimental effects of J—without mention of our future plans.

It took a few days for the draft to be sent to MIT to be typed and to ink the final graphs. The paper was then returned to us at Brookhaven and submitted to Dr. Trigg's office November 12.

Results from Other Laboratories

On November 10, I went to the Stanford Linear Accelerator Center for a Program Advisory Committee meeting. The moment I checked into the hotel I received a phone call from Martin Deutsch who mentioned that 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 quickly sent out a preprint. I then placed a call to Stan Brodsky, informing him of our results. Stan 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. I then called various laboratories like CERN and DESY to tell them of our discovery. The next morning when I walked into W. K. H. Panofsky's office to show him our results, he informed me that similar results had been obtained by SLAC and Lawrence Berkeley Laboratory over the weekend.

Monday morning, November 11, 1974, S. L. Wu called Giorgio Bellettini, Director of the Frascati Laboratory, informing him of our results. On extremely short notice, the Adone group succeeded in pushing the energy of their Storage Rings above the normal limit of the facility (2 x 1.5 GeV) and set up a special 1-MeV-per-step searching program, which began its search for the new particle on November 13. Since they knew approximately where to look, we were not surprised when Bellettini called on November 15 to tell us that a clear J signal had been observed in Adone. The Adone physicists wrote their paper immediately and had it delivered to the editor's office of *Physical Review Letters* by November 18. Thus it occurred that three papers on the discovery of the new particle appeared in the December 2 issue of *Physical Review Letters*. ☐