DISCOVERY STORY

Author of the Discovery Story in 1962 (Photo: CERN).

One Researcher's Personal Account

by
Bogdan Maglich

ON a sunny July day in 1959, I reported my arrival at Lawrence Berkeley Laboratory as a new 'postdoc' with the Alvarez group. As I picked up my pass at the gate, I had no premonition that at midnight that night I would begin the work which, two years later, would result in the discovery of the first 'heavy photon'—the ω meson.

Because of the shortage of Ph.D.'s to run the midnight-to-noon shifts, I was asked to postpone my vacation plans and immediately join the Antiproton Experiment then in progress. This experiment had been proposed by Lynn Stevenson and Philippe Eberhard to search for strange particles and antiparticles produced in antiproton-proton annihilations; vector mesons were not its aim. New types of events had already been observed; consequently, the experiment had been granted so many extensions at the Bevatron that they had begun to conflict with the physicists' summer schedules. Stevenson was leaving for the U.S.S.R. to attend an international conference; and two other physicists were leaving Berkeley. There was also a feeling that the cream had been skimmed from the experiment, and some physicists were contemplating new experiments. The only full-time Ph.D.'s left were Philippe Eberhard who took charge of the experimental floor, Janice Button, George Kalbfleisch and Joe Lannutti. Gerry Lynch and Nguyen Xuong subsequently joined our team.

Super Experiment

This was the first physics experiment to use the giant 72-inch hydrogen bubble chamber, conceived and built by Luis Alvarez against odds and opinions that a large liquid-hydrogen device could be made a reliable physics instrument. By the time I arrived, the chamber had produced 20,000 clear, beautiful pictures of sharp particle tracks, measurable with a precision unparalleled by any other bubble chamber.

Stevenson, Kalbfleisch, Eberhard and Morris Pripstein designed and built the first 'separated antiproton beam' applying the beam-separation technique which had earlier been used to build the separated beam of K mesons[1]. But the real strength of the $\bar{p}$ experiment lay in another big 'first': the extensive use of automation and large digital computers in a combination known today as the bubble-chamber 'system.' The 'production line,' as Alvarez called the organizational setup of the experiment, ran from the operation of the antiproton-beam transport system, to the bubble-chamber operation, to the prescanning of pictures, to the scanning and semi-automatic measuring of the particle tracks, to the computer 'fitting' of events to the hypotheses as conceived by Frank Solmitz and Art Rosenfeld together with Horace Taft of Yale University. Alvarez's group had about 25 Ph.D.'s, 25 graduate students, 12 engineers and about 100 scanners (most of the latter were part-time). This system, which I saw begin in 1959 and then implemented in my own searches, has since been copied by all bubble-chamber labs. From it I learned how 'big leaps' in science can be made by utilizing the 'firsts' in technology and instrumentation, and engaging a large number of highly capable and ambitious young scientists, together with a few very experienced ones.

At about the time of my arrival, the bubble chamber recorded the first observation of pair production of hadronic matter and antimatter[2]. This dramatic demonstration of the symmetry of matter and antimatter in the Universe (see photo p. 83) created much excitement in the lab and drew the attention of the press.

Contributors to the Antiproton Experiment

Both the production and operation of the antiproton beam encountered great difficulty. John Poirier, Sherwood Parker, and graduate students of the Moyer group developed and operated the electronic equipment that was essential to the beam's operation.

Under the direction of Donald Gow and Robert Watt, the crew of the 72-inch bubble chamber not only ran the chamber but helped with the vacuum system of the beam and the operation of the high-voltage parallel plate separators. The scanning personnel, under Hugh Bradner and Margaret Alston, were kept extremely busy assisting with the beam development and operation as well as with the scanning.

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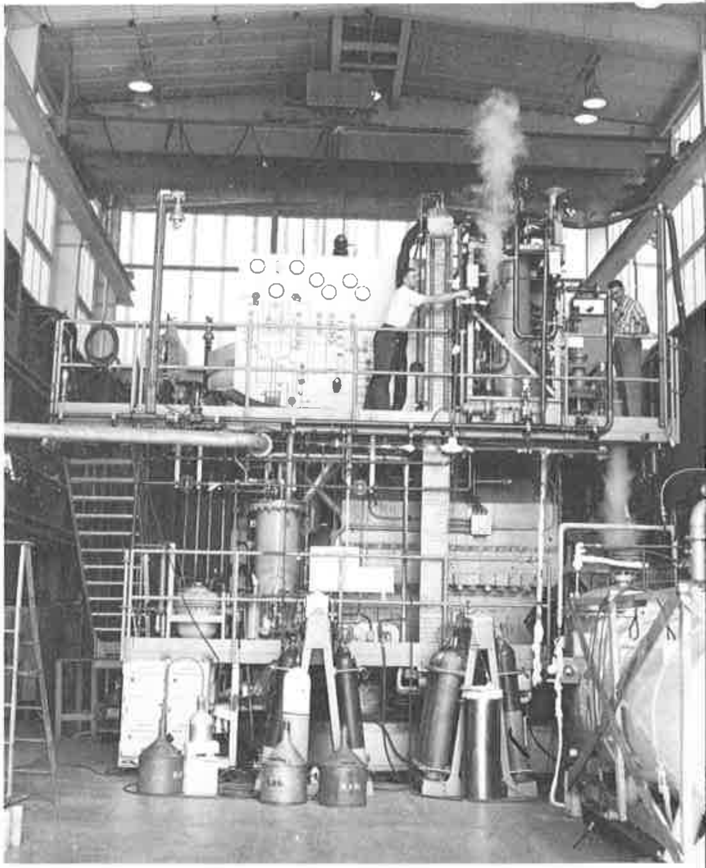
1. Eberhard, P., Good, M.L. and Ticho, H., *UCRL-8878*(1959).
2. Button, J., Eberhard, P., Kalbfleisch, G., Lannutti, J., Lynch, G., Maglich, B., Stevenson, M.L., and Xuong, N., *Phys. Rev.* 121, 1788 (1961).

Separated Antiproton Beam

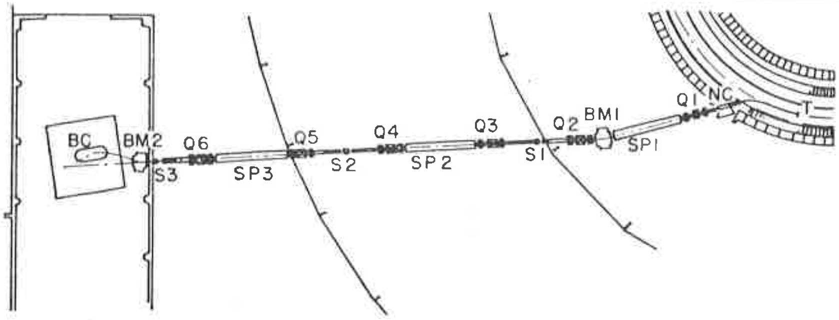
For each $\bar{p}$ produced at the Bevatron target, 17,000 π^-'s are produced in the same direction and at the same momentum. After the passage through the separator system (below), the $\bar{p}/\pi^-$ ratio is reversed to 3:1, implying a π^- rejection of 5×10^4. Undesired π's and μ's (from π decays) are deflected out of the horizontal plane by three parallel-plate velocity spectrometers which utilize crossed electric (E) and magnetic (H) fields. Particles with velocity $\beta_0 = E/H$ traverse the spectrometer undeflected, whereas particles with other velocities are deflected by an angle $\theta = (eV/pc)(L/d)\ \Delta\ (1/\beta)$ radians, where p = momentum in eV/c, V = voltage on the spectrometer plates, d = plate separation, L = plate length and $\Delta\ (1/\beta)$ is the difference in $1/\beta$ for the two velocities in question.

This angular separation θ was transformed into a spatial separation of 0.5 inch (see Ref. 2 for details).

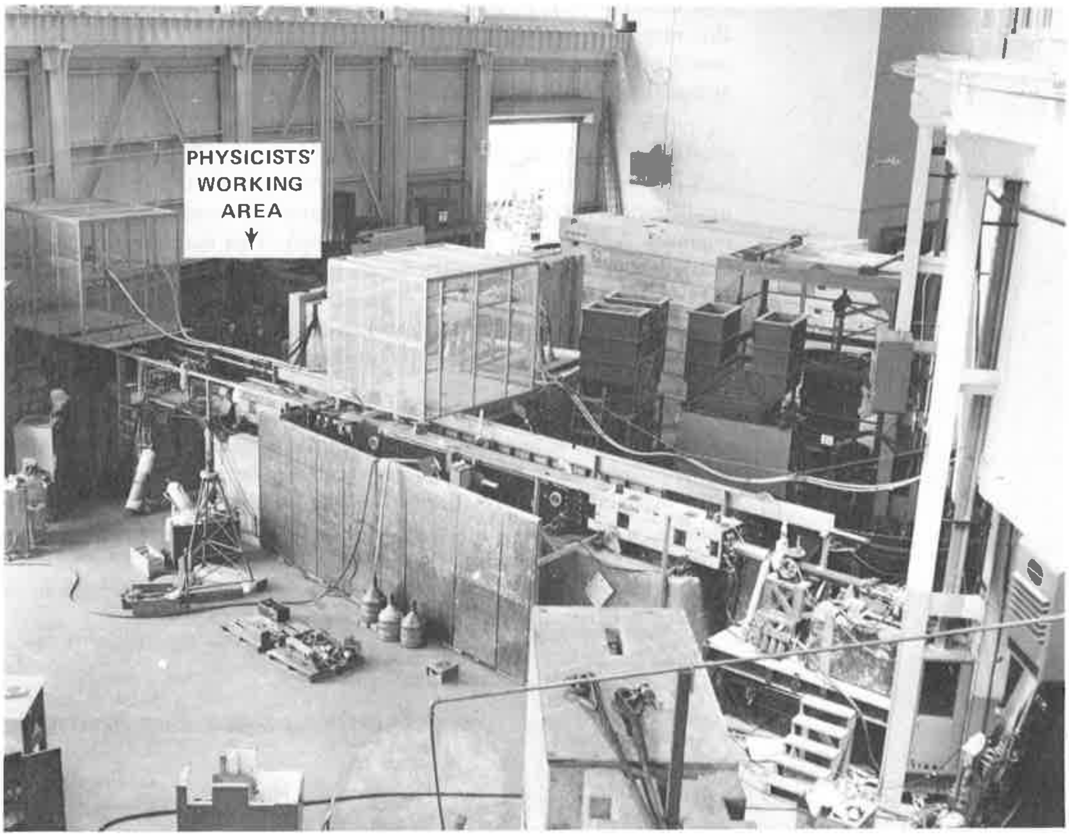
Interactions occurred for about half of the total of 58,000 $\bar{p}$'s of momenta 1.6 and 2 GeV/c which entered the bubble chamber in the course of the 5-month run, and 86,000 pictures were taken.



The 72-inch LBL hydrogen bubble chamber. →



← **Separated Antiproton Beam Layout.** *The antiprotons were produced in the target (T) by the 6.2-Bev proton beam of the Bevatron and were directed over a 200-foot path to the 72-inch liquid hydrogen bubble chamber (BC). The beam channel consisted of a "nose cone" magnetic shield (NC), six triplet 8-inch quadrupoles (Q1. . .Q6), two bending magnets (BM1 and BM2), three parallel-plate velocity spectrometers (SP1,SP2,SP3), and three slits (S1,S2,S3). Photograph of the area extending from S1 to S3 is shown below.*



Separated $\bar{p}$ Beam ←

Into 72-inch chamber

Unseparated Negative Beam ←

From Bevatron Target (π^-, K^-, $\bar{p}$, μ^-)

I began the 12-hour midnight-to-noon shifts on the experimental floor with little enthusiasm for the purpose of the experiment: hunting for new types of strange-particle events. Strange particles are produced in only 10% of annihilations. My interest remained attached to the most abundant particles produced in annihilations: the π mesons, then the only non-strange mesons known and considered to be responsible for nuclear forces. The nature of stong interactions had fascinated me ever since my first contact with physics and I will never forget how moved I was when I saw an event in which antiproton and proton annihilated into 8 π mesons (photo opposite).

I felt that the hundreds of thousands of pictures to be taken in the world's only large hydrogen bubble chamber, and at the world's only accelerator with an antiproton beam, would yield a unique treasury of pion and nucleon events worth my running the 12-hour graveyard shifts, just to have access to that treasury when the run was over. I was told I would.

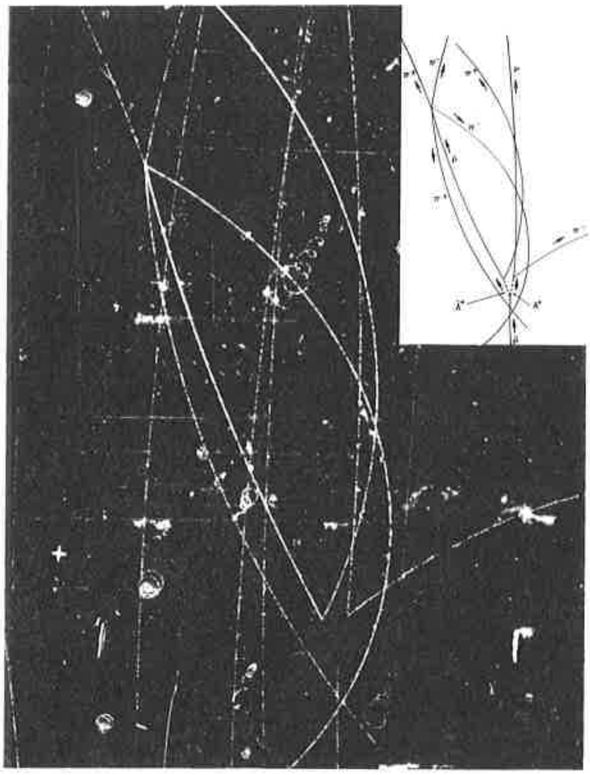
My First Bubble-Chamber Measurement

In mid-October 1959, the $\bar{p}$ run was over, and I began my first bubble-chamber measurement, that of the polarization and magnetic moment of the antiproton. This was my first experience in learning the ropes of how an individual physicist within a large bubble-chamber group can proceed in transforming his ideas into 'his' experiment.

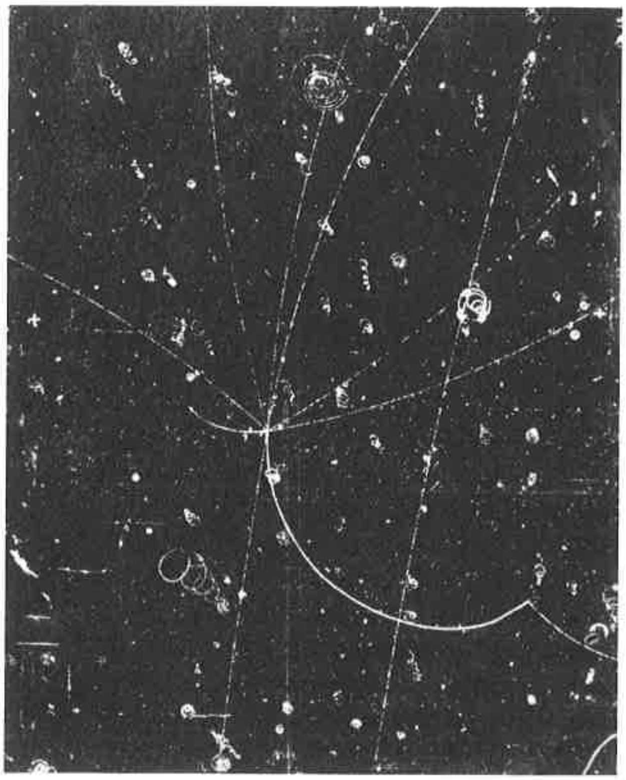
While looking at the bubble-chamber pictures during one of my shifts, I spotted a double elastic scattering of an antiproton with a proton. I realized that if such events showed that the direction of the second scattering depended on that of the first, this would imply that the antiprotons were polarized. I had recently read a review article in which Emilio Segrè had suggested that the magnetic moment of the antiproton could be measured if antiprotons could be polarized in scattering with protons. However, it was not known if antiprotons could be polarized.

First, I looked at 2,000 pictures marked as candidates for double-scattering events on the scanning table, selected 500 doublescattering events and marked them for measurements. Next, I tried to obtain measuring time on "Franckenstein," the first semi-automatic measuring device (named after engineer Jack Franck of Alvarez's group). This machine was in operation 24 hours a day, was constantly being repaired and, in parallel, was undergoing further development by the engineers at the same time measurements were being made for the physicists, and operators were being trained. It was almost impossible to get one's events measured on it unless they had a priority assigned by Alvarez.

To obtain Alvarez's approval, the researcher had to present his proposal in the form of a seminar at a regular Monday evening gathering at Alvarez's home. These Monday meetings were inspiring. Only new ideas and results were allowed to be presented, or what Alvarez called 'exposed' before 'going public,' that is, out of the group. In the private atmosphere of his home, where beer and pretzels were served, all inhibitions seemed to disappear and exchanges were direct. Privacy is essential for deliberations in which new ideas or preliminary experimental data are scrutinized.



Production and decay of neutral lambda and anti-lambda hyperons.



In my proposal, I made the point that once the polarization of $\bar{p}$ was established, another experiment should be launched to precess antiprotons between two scatterings in the magnetic field and to determine the magnetic moment. Alvarez said: "But you already have the magnetic field of the bubble chamber between the two scatterings. Can't you measure the magnetic moment of the antiproton now?" This sparked an animated discussion.

Following the suggestions made at one such meeting, I wrote a proposal to measure simultaneously the polarization *and* magnetic moment of the antiproton using the existing photographs. At the same time, Jan Button independently wrote her proposal to do the same, using a different technique. The measurement was forthrightly approved; and the results were eventually presented in a joint paper[3] describing our two different methods by which the same effect was observed. Although there were large statistical error-bars, we could demonstrate that antiprotons were polarized and that the magnetic moment indeed had a negative sign.

My work on the $\bar{p}$ magnetic moment brought me for the first time into close day-to-day contact with Luis Alvarez, who had become personally interested in my measurement.

Alvarez would either question and sharply criticize my steps and conclusions; or praise them. There was no midway. He constantly suggested new procedures or checks. When my likelihood function showed peaks in both projections, asymmetry and magnetic moment, implying the existence of polarization and negative sign for the magnetic moment, Alvarez suggested that the function itself, rather than the measured data put into it, may have been responsible for the peak. "Feed random numbers into your function, and only if they do not produce peaks will I believe your result." I replaced all the measured quantities of each event with numbers from Rand's Table of Random Numbers (simulating the measurements in the number of digits, etc.)—a tedious procedure when done by hand—and observed no such peaks. Alvarez urged me to give the RPM (research progress meeting) talk, and write a report. This checking procedure turned out to be useful in my search for vector mesons, as will be seen later.

Photo (Left):

First observation of the production of lambda Λ° and antilambda $\bar{\Lambda}^\circ$ hyperons. A $\bar{p}$ enters the chamber from the bottom and annihilates with a p in liquid hydrogen (end of the track). The Λ° and $\bar{\Lambda}^\circ$, being neutral, are invisible before their decays: $\Lambda^\circ \rightarrow p + \pi^-$ ("V" on the right) and $\bar{\Lambda}^\circ \rightarrow \bar{p} + \pi^+$ ("V" on the left). The $\bar{p}$ from the $\bar{\Lambda}^\circ$ decay subsequently annihilates $\bar{p} + p \rightarrow 2\pi^+ + 2\pi^-$, which convincingly proves that the event is antilambda.

Photo (Right):

Antiproton-proton annihilation into 8 charged pions, $\bar{p} + p \rightarrow 4\pi^+ + 4\pi^-$; one $\pi^+ \rightarrow \mu^+$ decay is seen.

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[3] Button, J. and Maglich, B., *Phys. Rev.* 127, 1297(1962); see also: Maglich, B., *UCRL-9336*(1960). The result was first publicly presented by Alvarez at the 1960 Int. Conf. on High-Energy Phys. at Rochester, (*Interscience, 1960*) p. 159.

First Search For Neutral Vector Meson—A Near Miss

Our first search for the neutral vector meson began the day after the appearance of Geoffrey Chew's paper "Three Pion Resonance Or A Bound State" in *Physical Review Letters,* February 1960. Although Chew's office was above ours, I first read of this idea from the journal rather than from the preprint. From his paper I learned that a neutral vector meson had been needed and postulated, predicted and looked for, since 1955. At last I had found the challenging goal I had been seeking! The neutral vector meson was being proposed by many theorists for different reasons[4]. Johnson, Teller and Dürr introduced it to explain nucleon-nucleon and nucleon-antinucleon interactions; Nambu, in 1957, predicted it to explain an electromagnetic structure (he called it ρ^0) which resulted in experimental searches for its decay into $\pi^0 + \gamma$; Fujii and Gregory Breit used it to explain uniquely nuclear and spin-orbit forces. Such a particle was also expected in the vector meson theory of Sakurai. As a member of an octet of mesons, it was predicted by the Japanese school. Now Chew pointed out that the neutral vector meson should exist on dynamic grounds as a 3-pion resonance or a bound state of 3π; he called it B^0 (B for *bound*).

I walked into our postdocs' office with the journal in hand and showed Chew's article to Joe Lannutti: "Let's look for this in the $\bar{p}$ experiment!" Lannutti was spending his sabbatical from Florida State University with our group and was sharing an office room, subdivided into glass boxes, with Jan Button, Gerry Lynch and myself.

Joe was terribly interested; he pushed aside whatever he was doing, and we started discussing how to proceed. By the end of the day we had the method that could result in a quick measurement: we would select the $\bar{p}p$ annihilations in which two charged pions, π^+ and π^-, would make visible tracks and two or more neutrals would be 'missing.' The term *missing mass* was coined then. Following this we went to see Lynn Stevenson to seek his support for the measurement. Lynn told us that the previous year an Italian group had reported at a conference in Kiev a negative result in their search[5] for the Nambu particle (this is how everyone referred to the predicted neutral vector meson). Nevertheless, he thought the search using bubble-chamber pictures should be done.

Within days Lannutti and I had written our "Proposal for the Search for B Meson in $\bar{p}p$ Annihilations"[6]. In late March, Lannutti presented our case at the Monday evening seminar at Alvarez's home, but our request for the measuring time was not approved. It was felt that the most important point of the $\bar{p}$ experiment was

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[4] *Theories Predicting Neutral Vector Mesons at the Time of Maglich's First Search (1960).*

Johnson, M. and Teller, E., *Phys. Rev.* **98**, 783(1955); Durr, H. and Teller, E., *Phys. Rev.* **101**, 494(1956); Durr, H., *Phys. Rev.* **103**, 469(1956).

Nambu, Y., *Phys. Rev.* **106**, 1366(1957).

Fujii, Y., *Prog. Theo. Phys. (Kyoto)* 21, 232(1959).

Breit, G., *Proc. Natl. Acad. Sci. (U.S.)* **46**, 746(1960); *Phys. Rev.* **120**, 287(1960).

Sakurai, J., *Ann. Phys.* **11**, 1(1960); Ikeda, M., Ogawa, S., and Ohnuki, Y., *Progr. Theor. Phys. (Kyoto)* **22**, 715(1959); Yamaguchi, Y., *Progr. Theor. Phys. (Kyoto)* Suppl. No. **11** (1959); Sawada, S. and Yonezawa, M., *Progr. Theor. Phys.* 22, 610(1959).

[5] Alberigi, A. *et al., 9th Annual Int. Conf. on High-Energy Phys.* Kiev, 1959 (Acad. Sci. USSR, Moscow, 1960).

[6] Lannutti, J., Maglich, B. and Stevenson, M.L., *UCID-1124* (1960).

the strange-particle events.

To our surprise, Chew gave only a qualified endorsement to our search. He believed the neutral vector meson would be easier to observe in π-p collisions than in annihilations. Annihilations would be "too violent" a process to preserve the physical correlation between three pions, even if the 3π resonance was produced in the first place, Chew told me. Jan Button, whose measuring experience and organization would be essential to the project, strongly believed in Chew's teaching but nevertheless was encouraging to our project.

Since we could not get our measurements done on Franckenstein in the regular way, by the trained operators, Lannutti and I started using the machine ourselves during periods when it was idle. This happened to be between 4 A.M. and 8 A.M. Jan then joined the effort and proved to be extremely helpful.

In April, theorist John Sakurai wrote to us suggesting that we also look for the meson by measuring the effective mass of 3 pions, $\pi^+\pi^-\pi^0$. We then included into our sample the annihilations into 4 visible prongs. To save on the measuring effort, we measured only two visible prongs in these events, π^+ and π^-, and the missing mass of these 2 prongs was expected to give us the 3π effective mass. In May 1960, we had a 2-prong missing-mass spectrum totaling 300 events. There was a broad but clear peak extending from a mass of 700 MeV to 1,000 MeV (Fig. 1) in the

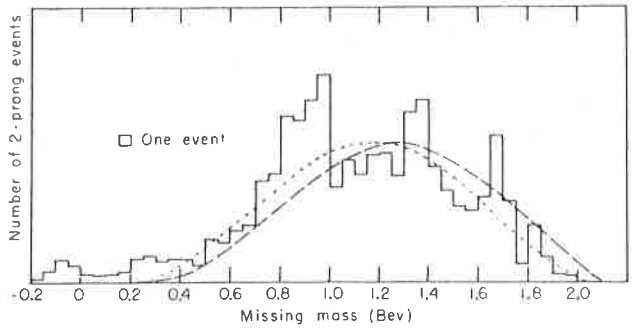

Fig. 1. *Missing-mass spectrum for annihilation events with two visible prongs $\bar{p}p \to \pi^+ + \pi^- + MM^0$. The histogram represents a total of 311 events. The dashed curve is the estimate of the spectrum for $\bar{p} + p \to \pi^- - \pi^+ + \eta\pi^0$ with the assumption of the equipartition of energy, etc. The dotted curve is the result of phase-space calculations with massless particles (except at the extremes of the curve). Events with one neutral pion should constitute only a few per cent of the 2 prongs. The reactions $\bar{p} + p \to K^+ + \pi^- + K^0$ or $K^- + \pi^+ + K^0$ very likely contribute to distortions in the region of 800 MeV. [From Ref. 7.]*

Missing Mass

The term "missing mass" was first introduced by Lannutti, Maglich and Stevenson in an unsuccessful search for the ω^0 meson in 1960.*

In the reaction $1 + 2 \to 3 + X$, let the new particle X decay into many bodies which cannot be observed, either because they are neutral or, for some other reason, unmeasurable. However, by using the effective mass concept, the mass of particle X can still be measured if the initial state and particle 3 are observable. This follows from the conservation laws:

$$E_1 + E_2 = E_3 + E_X$$

and

$$\mathbf{p}_1 + \mathbf{p}_2 = \mathbf{p}_3 + \mathbf{p}_X.$$

Solving for E_X and $\mathbf{p}_X$, and plugging in the equation for the mass of X,

$$M_X = E_X{}^2 - \mathbf{p}_X{}^2,$$

we obtain M_X as a difference of the squares of the missing energy and the missing momentum:

$$M_X = (E_1 + E_2 - E_3)^2 - (\mathbf{p}_1 + \mathbf{p}_2 - \mathbf{p}_3)^2.$$

In the laboratory system (target at rest), E_2 equals M_2 and $\mathbf{p}_2 = 0$, which simplifies the equation considerably.

* *For a description of this search see Refs. 6 and 7.*

general mass region known today as the ω meson region. In addition to the 2-prong events, we had a smaller sample of 200 events in which 4 charged pions made visible tracks, and the missing mass of 2 pions was measured. In this sample, a peak was seen exactly at the ω mass, between 700 and 800 MeV (Fig. 2).

I believed that the peak in the larger sample was real, but whoever saw it just shrugged his shoulders. It was too far from the predicted mass of 420 MeV and too near the neutron mass of 940 MeV. One simple interpretation was plausible: the two prongs were not pions, π^+ and π^-, but a proton and π^-. In this case the missing particle would be a neutron.

The essence of our method was that in order to calculate the mass of the invisible particles one had to *assume* the masses of the visible particles. The mass misinterpretation could explain at least a part of the peak. Since it had been happening that almost every week somebody in the Alvarez group would claim to see 'something new' and invite his colleagues to see it, the rules against false alarms were strict. Thus there was a general alert to try to shoot down these 'new' things in terms of simple known phenomena. If you could not convincingly exclude known phenomena by quantitative arguments, you were not allowed to talk about your phenomenon as being 'new.' As the 1960 Rochester Conference was approaching, we asked Frank Solmitz to show our spectra without comment, which he did[7].

Today, there is no doubt in my mind that the two histograms were an indication of the ω meson. If we had pursued these measurements, the neutral vector meson would have been found somewhat earlier. Yet, it would have been difficult to make it believable because of the possible particle misidentification and, according to another of the group's rules: "It is not enough to make

3-Body Effective-Mass Method

Special Relativity renders it possible to calculate the rest mass of any number of bodies without transformation of the momenta and energies from the laboratory frame of reference to the center-of-mass frame of reference.

Rest mass is an invariant. *Three bodies whose momenta and energies have been measured have an invariant mass M, often called the effective mass, which is given by:*

$$M = (E_1 + E_2 + E_3)^2 - (p_1 + p_2 + p_3)^2,$$

where E is the total energy, $E_i = M_i + T_i$, *and* T_i = *kinetic energy. E's and p's can be given either in the laboratory system, or in the center-of-mass system; and as long as they are all evaluated in the same frame of reference, they will give a unique value of M, since M is invariant.*

This equation was applied to the 3-pion decay $\omega^0 \rightarrow \pi^+ + \pi^- + \pi^0$, *which may occur in the 5-pion annihilations:*

$$\bar{p} + p \rightarrow \pi^+ + \pi^+ + \pi^- + \pi^- + \pi^0$$

Ten 3-pion combinations can be formed for this case:

Neutral triplets	$\pi^+\pi^-\pi^0$	*4 comb.*
Singly charged	$\pi^+\pi^+\pi^\pm$	*4 comb.*
Doubly charged	$\pi^+\pi^0\pi^\pm$	*2 comb.*

This implies that for the neutral and singly charged combinations, each event is plotted on mass distributions and Dalitz plots four times; and for doubly charged combinations, each event is plotted twice.

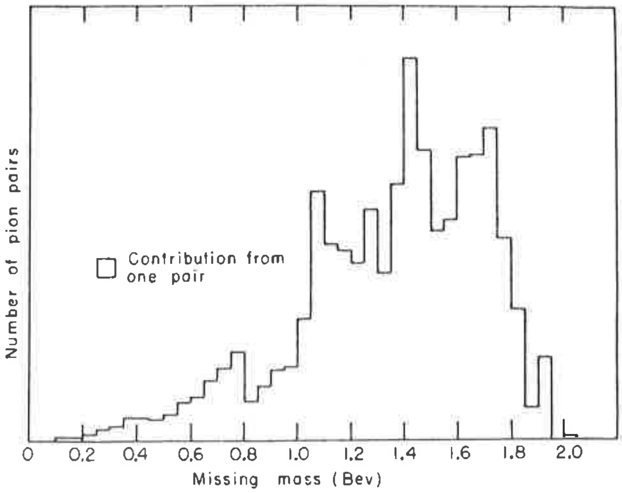

Fig. 2
Missing Mass Spectrum for $\pi^+\pi^-$ *Pairs of 4-Prong Annihilation Events:* $\bar{p}p \rightarrow \pi^+\pi^+\pi^-\pi^- + MM^0$ *(where* MM^0 *is the missing mass of the neutral particles produced). The total number of pairs is 172. [From Ref. 7.]*

a discovery, you must make it believable to your peers."

Another Miss—By Others

In mid-February 1960, several days after our proposal to search for the B° meson (ω° meson) was written, Ricardo Gomez of the California Institute of Technology visited Berkeley for several days to find out what new things were going on. I showed him the proposal. Before departing, obviously impressed by something in it, Gomez asked me if he could obtain a copy to take back to Caltech. I remembered having been told that pending proposals for new experiments were internal memoranda and should not be given to persons outside the group. But I was certain that Ricardo, having been a fellow graduate student with me at MIT, wouldn't do anything about the B° without informing me.

Six months later, a paper appeared in *Physical Review Letters*[8] entitled: "Evidence Against the Existence of the B° Meson" by Gomez *et al.* This was the second near miss in searches in the photon-proton interaction and, counting our miss in annihilations, the third near miss in the search for the neutral vector meson!

We know now that the reason Gomez did not observe the ω was that he focused his attention on a too-low mass of 420 MeV and, expecting the broad peak unanimously predicted by all theories, he failed to change the photon energy by small enough intervals to see a narrow peak.

Unsuccessful Search For The ρ Meson

While our search for the ω was in progress, Sylvia Limentani brought to my attention a paper by Italian theorist F.Cerulus[9] which stirred discussions in the group and influenced my subsequent work. Cerulus made a strong case that the search for the vector $\pi\pi$ resonance should be made in $\bar{p}p$ annihilations, rather than in π-p collisions. A year earlier, W. Frazer and J. Fulco[10] had written their famous paper which pointed out that a vector π-π resonance with a mass of 420 MeV was essential in order to explain the Hofstadter experiments on the structure of nucleons. Previously, it had always been assumed that the data could be interpreted by a non-resonant $\pi\pi$ interaction *and* a neutral vector meson, the ω (see Explanatory Physics Note on p. 88). Cerulus' paper, written before Chew proposed that the neutral vector meson would manifest itself as a 3π resonance, dealt only with the 2π resonance; yet it convinced me that both 2π and 3π resonances must be produced in annihilations; and it resolved in my mind what had been considered a mystery in annihilation physics.

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Neutral Vector Meson

The term neutral vector meson *is used colloquially for the isospin 0 vector mesons, i.e., those that exist in the* neutral *state only. Strictly speaking, the correct designation should be* singlet vector meson, *as opposed to an isospin 1* triplet vector meson *(e.g., ρ), which exists in two charged states as well as in the neutral state.*

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7. Button, J. *et al., Proc. 1960 Ann. Int. Conf. on High-Energy Phys. at Rochester* (Interscience, 1960),p.166.
8. Gomez, R. *et al., Phys. Rev. Lett.* 5, 170(1960).
9. Cerulus, F. *Nuovo Cim.* **14**, 827(1959).
10. Frazer, W. and Fulco, J., *Phys. Rev. Lett.* 2, 365(1959).

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The observed average pion multiplicity (number of pions) produced in $\bar{p}p$ annihilations was well established as 5. But the expected multiplicity, based on Fermi's statistical model which was known to work quite well for other reactions, was 3. The expected multiplicity could not be stretched to 5 without making unreasonable assumptions. Cerulus showed how the expected multiplicity of 3 and the observed multiplicity of 5 could be made compatible: if the proton and antiproton first annihilate into a single pion and two 2π resonances, $\pi + (2\pi) + (2\pi)$, and the resonances subsequently decay via $(2\pi) \to \pi + \pi$, the resultant pion multiplicity would be 5, as observed. The agreement, I felt, would be even easier to obtain if both 2π and 3π resonances existed. I also began to believe that the pairs of K mesons, KK, produced in $\bar{p}p$ annihilations, could annihilate into 3 π's in the final state, thus further increasing the pion multiplicity. I gave my memo "The $K\bar{K}$ Annihilations"[11] to Phil Yager, a graduate student, to write a general computer program on 2-step annihilations, which he did.

Perhaps also influenced by Cerulus' paper, Jan Button initiated a search for the $\pi\pi$ resonance in antiproton annihilations into 3 pions. A simple measurement of the momentum of *one* pion should have shown if there was a resonance between the other two pions: the momentum distribution would have a peak. The simplicity of the method was striking. Chew encouraged the measurement. Lannutti, Stevenson and I closely followed Jan's search. A sample of 300 events was quickly measured and the momentum spectrum plotted for 135 events was identified as annihilations into $\pi^+ + \pi^- + \pi^0$. However, the momentum of the charged pions, which was to reveal the charged ρ, did not show any peak (Fig. 3). Frank Solmitz reported this negative result at Rochester[7], together with the curves calculated by Cerulus (Fig. 4).

"If There Is No ω, There Is No ρ . . ."

It's easy today to say that these were near misses, but at the time the prevalent feeling among experimentalists was that the neutral vector meson did not exist. "There seems to be no B° meson," I carefully said to theorists Bill Frazer and Jim Ball as we stood in line for hamburgers at the LBL Sunday picnic in August 1960. I wanted to find out if they had any new ideas to make existing theory compatible with the lack of evidence for vector mesons.

Frazer, visibly upset by my statement, responded by saying that if there was no B°, its charged counterpart, ρ, could not

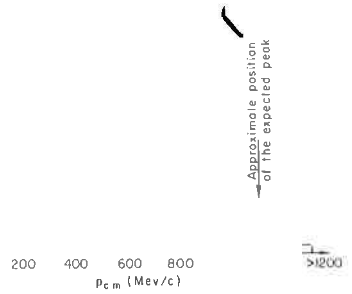

Fig. 3

Experimental Spectrum for 2-Prong Antiproton-Proton Annihilation Events. [From Ref. 7.]

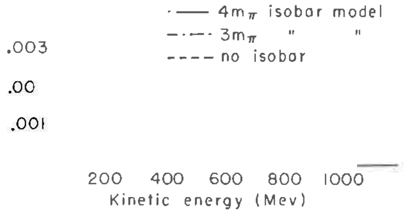

Fig. 4

Theoretical Spectrum for Particles Produced in Annihilations Yielding 2-Prong Events. The mass region shown is for a ρ mass between 3 and 4 pion masses. [From Ref. 9.]

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[11] Maglich, B. *UCID-1361*(1961).

exist either, which would mean that vector mesons could not exist. If vector mesons did not exist, we understood nothing about the structure of nucleons. Since we knew a lot, B° must exist and ρ must exist, too. The confidence of these young theorists in their understanding of Nature was the only encouraging argument for vector mesons I had heard in a long time.

Several hours after the picnic, I was back in the LBL library, which was open around the clock. I spread in front of me all the papers considered to show 'evidence' for the ρ meson (the 2π resonance), and looked at them. There were only a few experiments showing peaks in the 2π system, and their masses were different[12]. I. Derado at CERN reported a 2π peak at 660 MeV. There was the 'ABC effect' with a peak at 350 MeV. E. Pickup *et al* had a bump at 320 MeV. The best statistics were those of P. Burke, N. Schmitz and coworkers of Alvarez's group. Their data, still in preprint form, showed a deformation of the phase space toward 700 MeV, but no real peak. The Oxford group claimed that most of the $\pi\pi$ interaction was nonresonant, although its data showed what appeared to be two bumps.

In contrast, there were two *high-statistics* experiments showing *no* peak in the 2π effective mass. One was a big counter experiment then being analyzed by Segrè's group, but both Tom Ypsilantis and Clyde Wiegand had told me that they did not see an effect in the $\pi\pi$ system. (We know now that they looked at a mass that was too low and at a momentum transfer that was too high.) The other was the propane bubble-chamber experiment of Gerson and Sula Goldhaber. They measured correlations between pairs of pions produced in $\bar{p}p$ annihilations and found no peak in the effective mass of the two pions[13].

I thus found nothing convincing to support the existence of a $\pi\pi$ resonance. But I felt sure a $\pi\pi$ interaction must exist, because strong interactions exist and pions are the quanta of strong interactions. Either the interactions between pions are nonresonant—I concluded—or, if they are resonant, the measurements had not been accurate enough to detect them. Remembering Cerulus' paper, I was positive that a high-statistics measurement of the pion-rich, clean annihilation events in the 72-inch hydrogen bubble chamber should be undertaken.

The Goldhaber Effect

The puzzling results of the Goldhabers' experiment triggered my detailed investigation of annihilations into four charged pions, an investigation which eventually resulted in the discovery of the ω. The Goldhabers observed a strange effect in the angular correlation: the opening angles between like pions, $\pi^+\pi^+$ and $\pi^-\pi^-$, were

12. *Experimental Data On $\pi\pi$ Interaction Which Was Available In The Fall Of 1960:* Derado, I., *Nuovo Cimento* 15, 853(1960); Abashian, A. *et al.*, ('ABC effect'), *Phys. Rev. Lett.* 5, 258 (1960); Pickup, E., *et al.*, *Phys. Rev. Lett.* 5, 161 (1960); Anderson, J.A. *et al.*, *Rev. Mod. Phys.* 33, 431(1961); Rushbroke, J. G. and Radojcic, D. *Phys. Rev. Lett.* 5, 567(1960).

13. Goldhaber, G., *et al., Phys. Rev. Lett.* 3,181(1959).

found to be considerably smaller than those between pions of unlike charge. Interpreted in a simple-minded way, this implied attraction between pions of like charge, and repulsion between pions of opposite charge, contrary to ordinary electrostatics. Since pions are bosons, 'forces' like this could be possible only as a consequence of Bose statistics. With the help of Abraham Pais, the Goldhabers interpreted their result as a "Bose condensation" effect, the first observed in particle physics[14].

Today I believe that the Goldhabers' effect was an indirect observation of the ρ, ω, and other heavy-mass 2π and 3π resonances. They would cause an increase in the average angle between π^+ and π^-, because of the large energy release in their decay. Since ρ and ω do not exist in a doubly charged state, the $\pi^+\pi^+$ angular correlations will remain unchanged. That no peak was seen in an effective mass distribution can be explained by the low statistics and low accuracy in the measurement of the pions' momenta (± 10%) in the propane bubble chamber. However, the question of whether or not there is Bose condensation between pions is still being debated.

Of course at that time I did not know all that. I knew only that something peculiar was happening but I did not believe in the Bose condensation effect.

On November 14 1960, five days after my return from an extended fall vacation in Europe, I submitted to Alvarez a five-page proposal: "Remarks on the Measurement of Goldhaber *et al.* on Pion-Pion Correlations in $\bar{p}p$ Annihilations."[15] I wrote about the Goldhabers' results:

> Therefore absence of an effect in Q^2-distribution (effective-mass distribution) suggests:
>
> (i) absence of any resonance, like the one in T=J-1 state, advanced by Chew et al;
>
> (ii) that the attraction between bosons is a 'temperature' independent effect.
>
> While consequence (i) is not too surprising, in view of the lack of evidence for π-π resonance in other experiments, consequence (ii) is somewhat surprising . . .
>
> The above discussion suggests a remeasurement.

Then I listed the improvements on their experiment which could be done by using the 72-inch bubble chamber and the *existing* pictures from a 1.65 GeV/c run. I proposed to measure all 4,000 events in which antiproton and proton annihilated into four charged pions, $\pi^+\pi^+\pi^-\pi^-$, and an unknown number of π°'s. It was known that 1/3 of these were 5-pion events; i.e., that they had only one missing π°. The proposal ended with: ". . . our measurement will simultaneously yield information about interaction

[14] Goldhaber, G. *et al., Phys. Rev.* 120, 300(1960).

[15] Maglich, B. *UCID–1315*(1960).

between the charged and the neutral pions, and thus on the I=J=1 state."

Several days later, Jan Button and I were invited by Alvarez to a meeting of his senior staff to participate in a typical Alvarezian session: "We want new ideas, bold experiments."

What new ideas did I have? I pointed to my recent proposal but someone said that to start something just to check the Goldhabers' measurements was not worthwhile. Then I said, "We can also look for the ρ meson and the B meson in the sample." "What's the B meson?" Alvarez asked. Jan explained that it was Chew's name for the Nambu particle.

My own proposal suddenly sounded like fantasy to me. The negative results of all the searches for the pionic resonances were too close to home. I was asked to leave the meeting and, because of the skepticism with which my proposal was met, I felt that the extensive measurement of 4-prong events, which I wanted, would not be approved. But it was. I was told it did meet with opposition but that Alvarez simply overruled it by saying: "Let's let him do it."

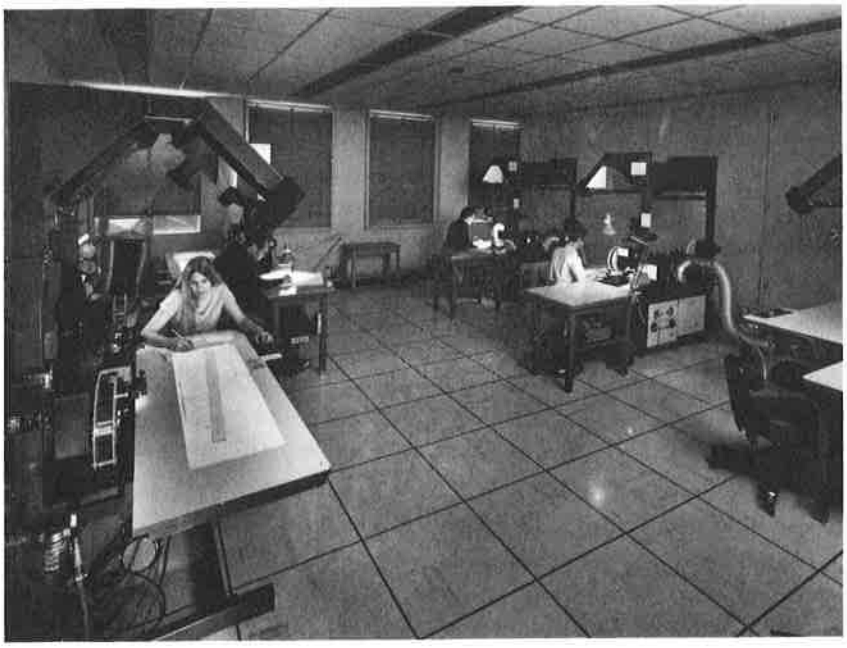
I believe that when you focus all your efforts onto one point, one task and one objective, things move ten times faster than if you divide your time and attention. Of course, nobody can run at full steam all the time; but some situations require all out effort.

The moment I had the green light to measure the 4-prong events, I set aside everything else. First, I had to look at all these events to prepare them for measurements. I looked at three projections of about 4,100 events and selected 2,500 to be measured. In order to avoid the 'prime time' crowd of physicists and scanners in the scanning room (only six scanning tables were operational at that time), I would start my day shortly after midnight by driving to the lab, ten minutes from home, and then go over the frames containing 4-prong events. I would return home at 6 A.M. for breakfast, sleep until noon, and after lunch go to work in my office to process the events measured on Franckenstein and return the rejected events for remeasurements, checks, etc. At 5:30 P.M., I would return home to get some sleep before my midnight-to-dawn shift.

One morning I found a message on my desk, left from the previous night by Martin Block of Duke University: "Looked for you several times. Your working hours are too short." (In Alvarez's group everybody worked evenings, Saturdays and holidays.) I thought, "If you only knew my schedule," but I never explained. I decided it was better to let him think I was a lazy physicist, rather than a crazy physicist.

Two events in which $\overline{p}$ and p annihilate into 5 pions: $\overline{p} + p \rightarrow 2\pi^+ + 2\pi^- + \pi^\circ$. The presence of the invisible π°'s, as well as their energies and momenta, was established from the measured momenta of the 4 charged pions and the conservation laws (by the program KICK).

In the event in the lower part of the picture, the two-step decay $\pi^+ \rightarrow \mu^+ + \nu \rightarrow e^+ + \nu + \overline{\nu}$ of one of the pions from the annihilation is seen. In the event in the upper part of the picture, another type of two-step interaction is seen: one π^- undergoes a charge-exchange scattering with a proton in the hydrogen, $\pi^- + p \rightarrow n + \pi^\circ$; subsequently, the π° undergoes decay $\pi^\circ \rightarrow \gamma + \gamma$ in the vicinity of another proton. One of the photons is then converted directly into an electron-positron pair, $\gamma \rightarrow e^+ + e^-$, by the electric field of the proton; the e^+ and e^- are seen as spirals in the upper part of the picture. This type of event is called a 'Dalitz pair.' [For more information about this photograph, see p. 98.]



Scanning room at Lawrence Berkeley Laboratory.

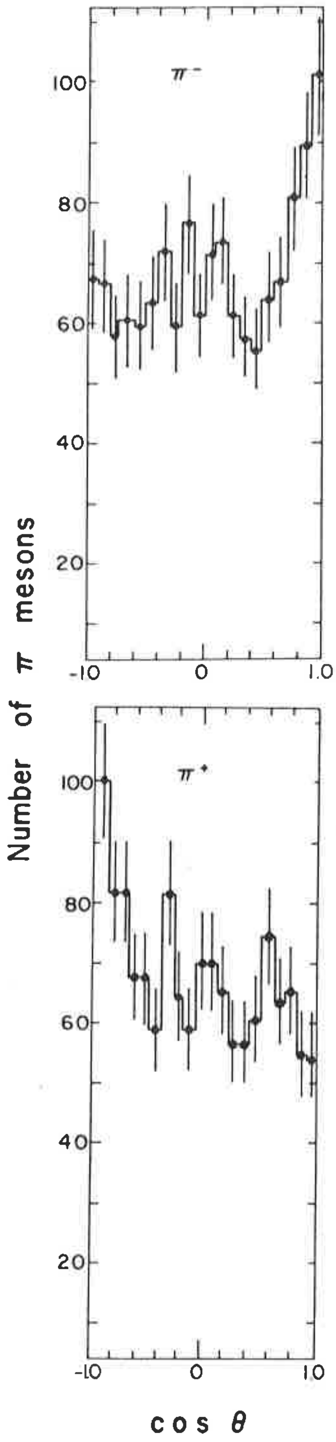


Fig. 5
Angular distributions, in the center-of-mass of the proton-antiproton system, of the negative (a) and positive pions (b), produced in annihilations $\bar{p} + p \to 2\pi^+ + 2\pi^- + \pi^0$. *It can be seen that* π^-*'s are emitted preferentially in the direction of* $\bar{p}$ *(0°); and* π^+*'s in the direction of p(180°). [From Ref. 17.]*

Discovery of the Charge Asymmetry Effect in Annihilations

By the end of February 1961, half of the 4-prong events had been measured and reconstructed by the kinematic program PANG, but not by the more sophisticated KICK. PANG was good enough for my first needs: angles and momenta, transformed into the center-of-mass frame. I could test my hypothesis that the Goldhaber effect was a result of a dynamic effect. We found that when the $\bar{p}p$ annihilates into a $\Lambda\bar{\Lambda}$ pair (see photo: p.83), antilambdas are emitted in the antiproton direction (0°) and lambdas are emitted in the direction of the proton (180°)[16]. I expected, on heuristic grounds, a similar effect in the annihilations into charged pions: that π^-'s would be emitted predominantly in the $\bar{p}$ direction, and π^+'s in the p direction.

I plotted the angular distributions of the π^+ and π^- separately and, incredibly, I observed an excess of π^- at 0° and an excess of π^+ at 180°. Lynn Stevenson fitted the whole distribution with Legendre polynomials and showed that the excess was 8 standard deviations! As soon as George Kalbfleisch saw the data, he processed his $\bar{p}p$ annihilations into K^+ and K^- mesons and observed a similar effect: an excess of K^- in the direction of $\bar{p}$, and an excess of K^+ in the direction of p.

Later when all events were measured, the effect became clear beyond any doubt, and we published our paper[17]. However, the excess of π^- (π^+) in the forward (backward) direction was too small to explain the Goldhaber effect.

Slow Going

The previous fall, the unexpected $K\pi$ resonance, K*(890) had been discovered by Alston, Good and coworkers of the Alvarez group[18]. This was the first meson resonance ever to be seen. If a resonance such as the K* existed, was it not reasonable to speculate that much-needed vector resonances, such as the 2π and 3π, also existed?

As all the 4-prong events had been measured and all the rejects remeasured, I wanted to look for 2π and 3π resonances. But this could not be done without 'fitting' the momentum and the angle of the invisible fifth pion, π^0, to obtain the momenta of all *five* pions in the most abundant events among the antiproton-proton annihilations: $\bar{p}p \to \pi^+\pi^+\pi^-\pi^- + \pi^0$. The world's only program capable of doing that had just been developed: "KICK" not only determined the angle and momentum of the missing pion, but improved the accuracy of the measurement by varying the angles and momenta of all five particles until it obtained the best fit. My 4-prong events were one of the first samples to be analyzed by KICK.

Many hours were spent in meetings with both Rosenfeld and the programmer assigned to me to decide how the effective masses would be computed and displayed. Things were moving too slowly. Two months of programming, and no output. In early June of 1961, Stevenson had excitedly shown me a paper by Bill Walker's group of Wisconsin which claimed evidence for the I=1 $\pi\pi$ resonance at (what was considered then) a very heavy mass: 750 MeV. Their paper[19] was preceded by that of the Yale group[20] showing a bump at a different mass (400-500 MeV); furthermore, at that time, the Wisconsin data appeared to be inconsistent with previous data. Yet, I felt the bumps reported by these two groups were so much more outstanding than anything previously reported that similar bumps would have to show up in my sample. But my program had bugs and bugs. I wanted to fire the programmer, but Alvarez vetoed the idea.

Observation Of The ρ Meson And Non-Discovery Of ρ-ω Interference

Once KICK began to work, I started to plot, by hand, the effective mass of 2π's. I was bewildered by what I saw: a clear tower-like peak in the $\pi^+\pi^-$ effective mass distribution at nearly the same mass reported by Bill Walker of Wisconsin[21], whose data few people believed because his statistical significance was not convincing. The significance of my peak was in excess of 5 standard deviations; it was 100 MeV broad and centered at a mass of 765 MeV. The singly charged combinations $\pi^+\pi^0$ and $\pi^-\pi^0$ showed a peak of similar magnitude. The doubly charged states, $\pi^+\pi^+$ and $\pi^-\pi^-$, showed no peak; from this the isospin of the ρ was immediately confirmed to be 1. I showed the data to Alvarez and our closest associates; and general excitement set in within the group. This was in mid-June, 1961.

When more events had been measured, I started to plot the data in smaller bins of 10 MeV width, and an embarrassing feature began to appear: the neutral ρ^0 peak was *split* right down the middle! It looked like *two* resonances: one at 725 MeV, the other at 780. The dip in the middle was visible, yet statistically marginal (3 standard deviations as shown in Fig. 6, p. 96).

I decided to check whether something was wrong with either the program or the measurements, and to stop talking even to my closest associates until the whole matter was settled. I considered the possibility that the computer program, while good enough to compute effective masses to an accuracy of 30 MeV, might have

16. Lynch, G. *Rev. Mod. Phys.* 33, 395(1961). See also ref. 2 of this chapter.
17. Maglich, B. *et al.*, *Phys. Rev. Lett.* 7, 137(1961).
18. Alston, M. *et al.*, *Phys. Rev. Lett.* 5, 520(1960).
19. Erwin, A. R. *et al.*, *Phys. Rev. Lett.* 6, 628(1961).
20. Fickinger, W. *et al.*, *Phys. Rev. Lett.* 6, 625(1961).
21. Erwin, A., March, R., Walker, W. and West, E. *Phys. Rev. Lett.* 6, 628(1961).

introduced discontinuities when data was plotted into finer bins, perhaps due to some rounding errors used in computing.

No matter what I tried, I could not remove the 2-peak appearance. I began to believe the structure was physical but as the possibility that something was technically wrong with the data could not be excluded, I showed the ρ peak only in 20 MeV bins, to avoid arguments. Only after we submitted the paper announcing the discovery of the ω meson to *Physical Review Letters* on August 11, did I show the ρ peak in 10 MeV bins to my colleagues. The ensuing debates were so intense that these data could not be published until a year later[22].

A number of theoretical papers were published[23] at that time, all dealing with my controversial structure and proposing the ρ-ω interference effect and G-parity violation as its explanation. Although the detailed shape of my ρ peak triggered the *theoretical* discovery of the ρ-ω interference effect, it was not until 10 years later that this effect was established experimentally[24].

I should point out that while the two-peak structure was better fitted by a two-peak curve (40% probability), a *single*-peak fit to the data could not be rejected (12% probability). Hence, the effect was compatible with being a statistical fluctuation. But because its *appearance* was so suggestive, almost everyone was convinced that it was a real effect. Alvarez called it "fine structure" and showed it to the LBL program committee. Some months later, Alvarez suggested that Gerry Lynch write a special Monte Carlo program[25] to see if statistical fluctuations could fake such a peak. Indeed, 2 out of 100 randomly generated histograms looked to me like my structure; and I was unable to distinguish my experimental double structure from that generated by Lynch's program. In his Nobel Lecture, Alvarez described this and " . . . the experimenter who had felt confident that his bump was significant. . . " was me.

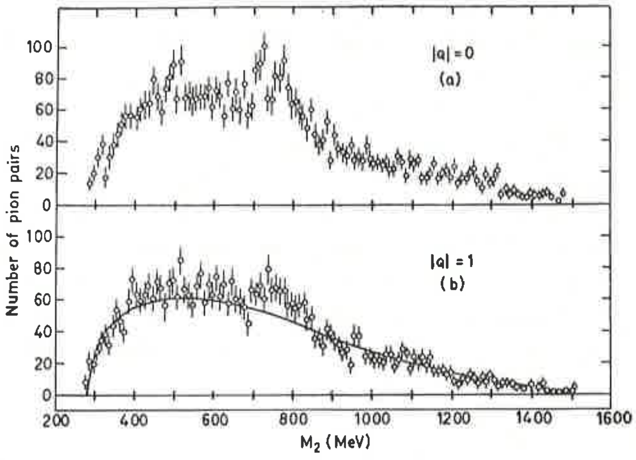


Fig. 6

Effective Mass Distribution of π Pairs. Data are plotted in 10 MeV bins. (a) Neutral pairs $\pi^+\pi^-$; (b) singly charged pairs $\pi^+\pi^0$ and $\pi^-\pi^0$.

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22. Button, J. *et al., Phys. Rev.* **126**, 1858(1962).
23. *Theoretical Papers On ρ-ω Interference And Violation of G Parity, Triggered By The Data In Figure 4:* Fubini, S., *Phys. Rev. Lett.* **7**, 466(1961); Glashow, S., *Phys. Rev. Lett.* **7**, 469(1961); Pais A.,*Phys. Rev. Lett.* **8**,82(1961); Feinberg, G., *Phys. Rev. Lett.* **8**, 151(1962); Feld, B. T. *Phys. Rev. Lett.* **8**, 181(1962); Gell-mann, M. *et al., Phys. Rev. Lett.* **8**, 261(1962); Blankenbecker, R., *Phys. Rev.* **125**, 755(1962).
24. Goldhaber, G., *Experimental Meson Spectroscopy,* Baltay and Rosenfeld, Editors (Columbia Univ. Press 1970) p. 59. See also the Discovery Story p. 115 of this volume; also Button-Shafer, J. *et al.,* Proc. 1963 Int. Conf. Nuclear Structure (Stanford Univ. Press, 1964), p. 386; Murray, J. J. *et al., Phys. Rev. Lett.* **7**, 358(1963).
25. See Margin Note on p. 117.

Omega Meson Observed

At the end of one of those frustrating days when I could not remove the split ρ meson, my scanner, undergraduate student Floyd Richards, came to receive his instructions for the 5 P.M. to midnight shift. I told him to plot the effective mass of three pions—the four different (for each event) neutral triplets, $\pi^+\pi^-\pi^0$, of the entire sample—and to call me at home only if there was something unusual, such as a bump in the distribution.

At 8 P.M. the phone rang. Floyd told me: "There is a peak. Almost all the events in the peak are in one bin." The mass of the bin was 780 to 790 MeV.

This was the same mass at which the effective mass of the neutral pion *pairs,* $\pi^+\pi^-$, also had one of its two peaks. Moreover, the peak was too narrow. How could it be 10 MeV wide, when my calculated mass resolution was 30 MeV? I became even more worried that something was wrong either with the program or with the measuring process. Yet, I could not explain how this 'fault' could produce narrow peaks in both 2π and 3π combinations exactly at the same mass.

I asked Floyd to plot the singly charged and doubly charged combinations, $\pi^+\pi^+\pi^-$ and $\pi^+\pi^+\pi^0$. Three hours later, Floyd called to inform me that there were no comparable peaks in the singly and doubly charged samples.

I drove immediately to LBL to look at the data. What a magnificent narrow spike in the neutral 3π distribution! The three distributions—the neutral singly charged and doubly charged pion triplets, plotted on the same scale—showed the peak only in the neutral state; thus merely by glancing at the data it could be seen that the isotopic spin of the 3π state was zero! It was clear to me that only a major fault in the computer program or a real discovery could be responsible for such an effect (Fig. 7).

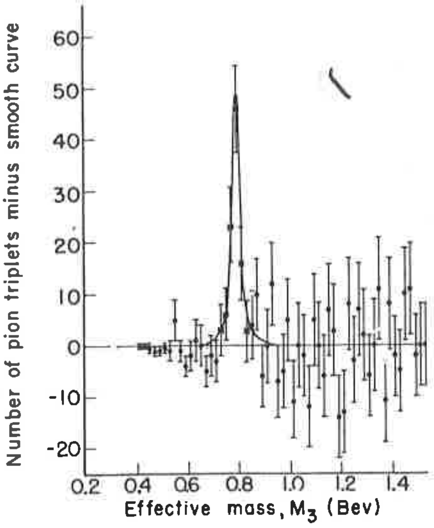


Fig. 7

Effective mass spectrum of the neutral pion triplets with the smooth background subtracted; a resonance curve is drawn through the peak at 787 Mev with $\Gamma/2$ = 15 MeV. The error flags are $N^{1/2}$, where N is the total number of triplets per 20-MeV interval before subtraction of the smooth background curve.[See the mass distributions for each charge combination in Fig. I on p. 107.]

The fact that both the upper peak in the ρ^- and the neutral 3π peak were at the same mass was tantalizing. I knew I would face an uphill battle with everyone, particularly with my peers, if I stated that I had seen one particle decay into both 2π and 3π. I remembered that the 'tau-theta puzzle' in which the decay of the K meson into 2π and 3π led to the discovery of the violation of parity in weak interactions. My 2π and 3π peaks at the same mass implied (I thought) parity violation in strong interactions, and thus was not believable. I knew that parity was conserved in strong interactions to at least 1 part in 10,000, so that the existence of two decay modes would be impossible.

It skipped my mind that what I was dealing with was electromagnetic decay. The neutral vector meson can be considered to be the heavy *photon*. The isotopic spin of the photon is not defined, so that one particle can decay into 2π or 3π *via* a photon.

If the 2π peak at the ω mass was real, what I saw was a violation of 'G-parity,' which is a much lesser violation than that of parity. G-parity must be conserved in strong, but *not* electromagnetic, interactions.

I felt that if I showed my results to anyone in the group my case would be shot down on the grounds that I had not eliminated an instrumental or computing error, so I prepared a list of random numbers and gave it to the programmer to feed into the program instead of the measured input. While awaiting the output, I received a phone call from George Sudarshan of Syracuse University who asked if I had a peak in the 3π effective mass! "Not below 500 MeV," I replied. He did not expect it there. His prediction, he said, was that the ω mass should be between the ρ and ρ+π mass. I was astonished, as my 3π peak was exactly in that mass region. I told him, "I see something there, but many things have to be checked." Sudarshan was the only theorist who did not insist that the mass of the neutral vector meson should be in the 400-500 MeV region. I decided to give him credit[26] for this, *if* it turned out that the 780 MeV spike was the neutral vector meson.

When a sample of events generated by an input of random numbers showed no spike at 780 MeV, I began to believe the reality of the ω peak. The only thing left to explain was how the peak could be so narrow. I soon realized that the observed width of the ω peak was compatible with the resolution and began making plots of the data that could be shown to Alvarez and my peers.

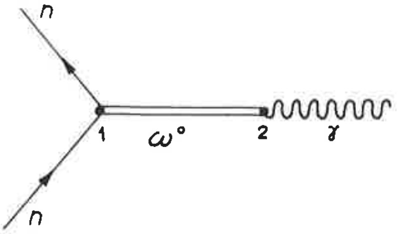
On July 28 1961, I went to show the plots to Stevenson, but he had left for vacation. Then to Alvarez, but he was away, too. Then to Rosenfeld; also on vacation! Of the entire group, only Kalbfleisch and Miller were there to see the data. Then I went to show the data to Frazer and Ball, and found out that a summer study group of theorists from all over the world had gathered at Berkeley to discuss high-energy topics--vector mesons being one of them. They were bewildered when they saw the data. To them, the mass of my 3π peak was too high, the width too narrow. However, the next day the width of the 3π peak had been explained by the centrifugal barrier, and the discovery became 'acceptable.'

Gell-Mann Names Omega

Both Stevenson and Rosenfeld cut short their vacations and returned to Berkeley to work on the analysis of the data. Alvarez, who was on a business trip, came back a week later.

ω Meson— The Heavy Photon

The ω meson, which is strongly *coupled to the nucleon, can be directly coupled to or virtually 'transformed' into a photon because it has the same quantum numbers as the photon. The schematic diagram of an ω coupling to a nucleon on the one hand, and to a photon on the other hand, is shown below.*



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26. See ref. 7 on p. 110.

27. Gell-Mann, M. *CTSL–20*(1961). See also: Ne'eman, Y. *Nucl. Phys.* 26, 222(1961); Gell-Mann, M. and Ne'eman, Y. editors, *The Eightfold Way* (Benjamin 1964).

It is usually up to the discoverer to name a new effect. But the neutral vector meson was named by Murray Gell-Mann and I accepted the name before we had even met. In fact, Gell-Mann had predicted a meson with the same properties as our meson and had referred to it as ω in his famous Eightfold Way[27]. (Until this writing I was unaware of the latter, always assuming that Gell-Mann had named ω after the discovery.)

That summer Gell-Mann was staying at Berkeley, so that as soon as he saw the data, he began writing all the consequences of the symmetry in an isospin zero system of 3 pions. He then wrote down the simplest matrix elements (see p. 108) and recommended to Stevenson and Rosenfeld that they fold the Dalitz plot into six pieces. This made the statistics good enough to eliminate some matrix elements. Thus the spin J and parity P of the ω were determined within days after its discovery: $J^P = 1^-$ (see p. 100). It was indeed a *vector* meson, as predicted. Its lifetime we determined from its width to be about 6×10^{-23} sec. I was told by Alvarez that this was the first case that a new particle had been discovered and all its quantum numbers determined in one experiment.

At that time, Robert Hofstadter, whose experiments had led to the prediction of vector mesons, arrived from Stanford to view the data. He looked at all the distributions and checked the statistical errors, and told me that as soon as he heard the high mass of both ω and ρ he was convinced that these were the right mesons: the form-factor fits in his experimental data required higher masses than 420 MeV and nearly the same masses for both the ω and ρ.

Discovery of the ω Announced–By Others

It is ironical that the announcement of the confirmation of the ω preceded the announcement of its discovery!

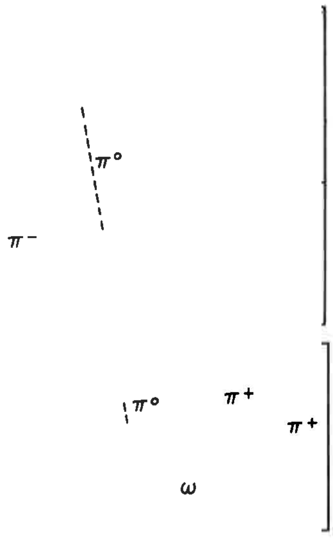
In early August when rumors of the ω peak began to spread, the Pevsner group of Johns Hopkins University was in the midst of measuring the events from π + d collisions. They had miles of film taken with the Alvarez 72-inch bubble chamber, filled with deuterium. When news of the ω's discovery reached them, Pevsner's group quickly shifted the emphasis of their experiment to search for the ω to check our result. On August 28, four days before our paper appeared in the September 1 issue of *Physical Review Letters,* at the European Conference on Elementary Particle Physics in Aix-en-Provence, Aihud Pevsner presented his ω peak observed in the reaction $\pi^+ + d \rightarrow \omega^\circ + p + p$. Since no one from the Alvarez group attended this conference, theirs was the

Discovery Story Cont. p. 103

Topography of ω° Production

The topography of a 5π annihilation event, in which a pion triplet $\pi^+\pi^-\pi^\circ$ is produced whose mass is about 783 MeV, i.e., consistent with being the decay of ω°, is shown below. The short lifetime of the ω, $\tau \sim 7 \times 10^{-23}$ sec, implies that it decays within a distance $\lambda \sim c\tau \sim 10^{-12}$ cm from the production point. Thus, the production and decay vertices cannot be separated even by the best microscopes. If the vertex area could be magnified 10^{12} times, one would observe a "V" with a short gap between the ω production and its decay, as illustrated in the lower diagram.

The effective mass method makes possible the observation of such invisible decays statistically, but not on the individual event basis (unlike, for example, the $\Lambda\bar{\Lambda}$ event on p. 83).

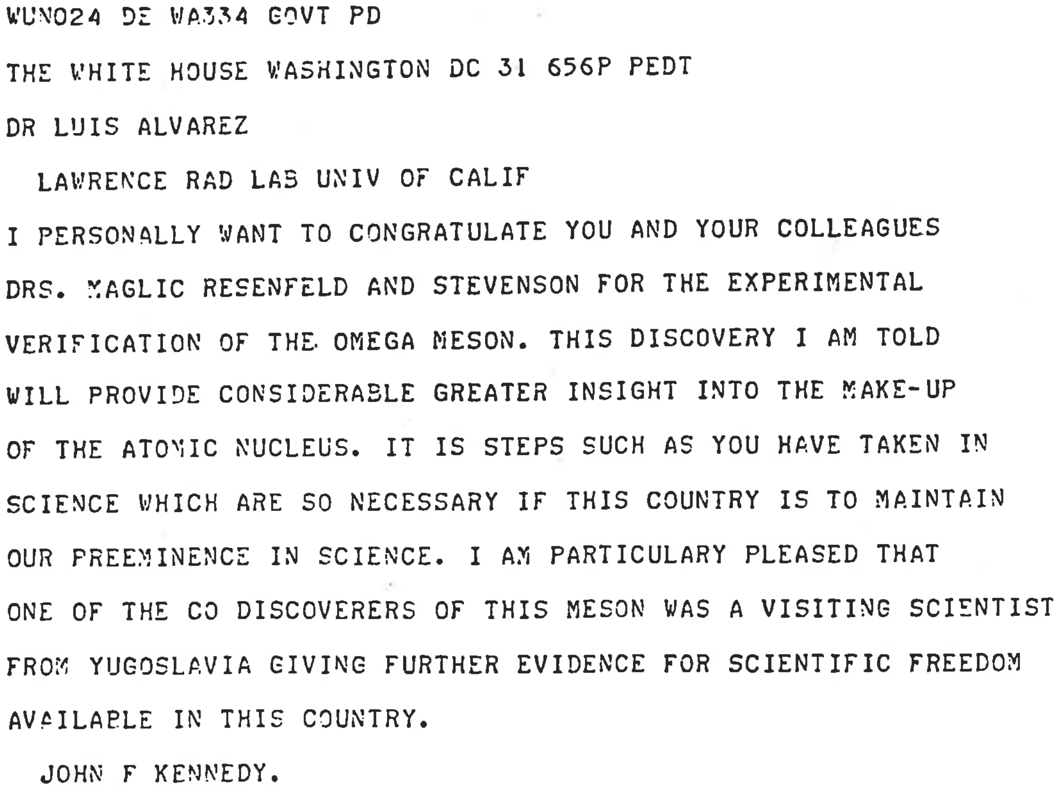


Vertex Magnified ∿ 10^{12} Times.

Discovery Story Cont. from p. 99

first public announcement of the discovery of the ω°! However, Pevsner presented the result as "the first confirmation of the Berkeley result," and a number of physicists in the audience already knew about our paper that had been received by the editor of *Physical Review Letters* on August 14. But if we had delayed submitting our paper by two weeks—which might have happened if I had disclosed the ρ structure at the same time I had showed the ω peak within LBL, and if it had been someone other than Pevsner, then the question of who was the actual discoverer of the ω might still be a matter of conjecture.

On August 31, the day before the paper was to appear, Edwin McMillan, the LBL director, chaired a televised press conference at the Lab, after which I flew to British Columbia to join my family vacationing there. Just as I arrived in my hotel room, the telephone rang. Lynn was calling from Berkeley: "We thought you would like to hear the text of the telegram Luis received from President Kennedy this morning." He read:



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THE WHITE HOUSE WASHINGTON DC 31 656P PEDT

DR LUIS ALVAREZ

LAWRENCE RAD LAB UNIV OF CALIF

I PERSONALLY WANT TO CONGRATULATE YOU AND YOUR COLLEAGUES
DRS. MAGLIC RESENFELD AND STEVENSON FOR THE EXPERIMENTAL
VERIFICATION OF THE OMEGA MESON. THIS DISCOVERY I AM TOLD
WILL PROVIDE CONSIDERABLE GREATER INSIGHT INTO THE MAKE-UP
OF THE ATOMIC NUCLEUS. IT IS STEPS SUCH AS YOU HAVE TAKEN IN
SCIENCE WHICH ARE SO NECESSARY IF THIS COUNTRY IS TO MAINTAIN
OUR PREEMINENCE IN SCIENCE. I AM PARTICULARY PLEASED THAT
ONE OF THE CO DISCOVERERS OF THIS MESON WAS A VISITING SCIENTIST
FROM YUGOSLAVIA GIVING FURTHER EVIDENCE FOR SCIENTIFIC FREEDOM
AVAILAPLE IN THIS COUNTRY.

JOHN F KENNEDY.

Press Conference at Lawrence Berkeley Laboratory at which the discovery of the ω was announced, August 1961. Left to right: M. Lynn Stevenson, Bogdan Maglich, LBL Director Edwin McMillan, Luis Alvarez and Arthur Rosenfeld.

Upon my return to Berkeley, I received a moving telegram from my native Yugoslavia's Serbian Academy of Sciences and Arts.

Heisenberg Questions Quantum Numbers of ω

Some time after the discovery was announced, Alvarez received a letter from Werner Heisenberg. Heisenberg's Non-Linear Spinor Theory of elementary particles predicted an I=0 pseudoscalar meson, and he firmly believed the ω was it. He doubted that the spin of the ω was equal to 1. He believed our experimental data was compatible with an assignment of spin zero and odd parity, i.e., ω would be a pseudoscalar like the π, except that the isospin would be zero. The Dalitz plot for 0^- can be similar to 1^- *if* one allows violation of G-parity. Since G-parity was not conserved in electromagnetic decay, how could we exclude spin zero?

While I was in Europe that fall, I visited Heisenberg at his institute in Munich. Before my lecture, Heisenberg held a closed session in his office with Hans-Peter Dürr and some associates to discuss the spin of the ω. I could not yield to their contention that ω was a 0^- particle. The arguments are stated in Ref. 28.

Norbert Schmitz, who had just returned to Munich from Berkeley, told me the following story: Queen Frederika of Greece (who is known to be an amateur physicist) had visited Heisenberg on September 1. She had seen a news dispatch in a German newspaper that a physicist of Yugoslav origin had discovered a new meson at Berkeley and asked Heisenberg what this meson was. Heisenberg, who at the time knew nothing about it, was excited by the news, hoping that it might be the zero spin particle he had predicted.

Soon after my visit, Heisenberg and Dürr wrote a paper on

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28. See the section "Notes Added in Proof" in Stevenson, M.L. *et al.*, 125, 687(1962).

the determination of spin and parity for particles decaying into 3 pions with isospin violation *via* electromagnetic decay[29]. This allowed more possibilities for the matrix elements than the simpler ones which Gell-Mann had proposed to determine the spin of the ω (see p. 108). According to their paper, our Dalitz plots could also be interpreted as evidence for a meson of zero spin and odd parity.

Suddenly, the discovery of the η meson was announced by the Pevsner group of Johns Hopkins University. By July 1962, its spin and parity had been confirmed to be 0^-, possessing exactly the properties of the meson Heisenberg had been awaiting. (Such a spin zero particle was also predicted by a number of theories listed under Ref. 4.)

In September 1962, I gave an invited paper in Stuttgart at the Annual Meeting of the Physical Society of West Germany. I informed the large audience of this new result. After I descended from the platform, I encountered Heisenberg and during our conversation I told him: "Now that you have your *own* meson, will you please admit that the omega has spin 1?" I coined the expression *Eigenmeson* . . . a pun on the quantum mechanics term *Eigenvalue*, and at this Heisenberg burst into laughter. A press photographer, probably surprised at seeing the famous German scientist laugh so heartily, captured the scene.☐

Werner Heisenberg (right) and author of the Discovery Story at the 1962 Annual Meeting of the German Physical Society, in Stuttgart (1962).

[29] Dürr, H.-P. and Heisenberg, W., *Nuovo Cim.* 23, 807(1962).