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Recent developments in particle physics

Nobel Lecture, December 11, 1968

When I received my B. S. degree in 1932, only two of the fundamental particles of physics were known. Every bit of matter in the universe was thought to consist solely of protons and electrons. But in that same year, the number of particles was suddenly doubled. In two beautiful experiments, Chadwick[1] showed that the neutron existed, and Anderson[2] photographed the first unmistakable positron track. In the years since 1932, the list of known particles has increased rapidly, but not steadily. The growth has instead been concentrated into a series of spurts of activity.

Following the traditions of this occasion, my task this afternoon is to describe the latest of these periods of discovery, and to tell you of the development of the tools and techniques that made it possible. Most of us who become experimental physicists do so for two reasons; we love the tools of physics because to us they have intrinsic beauty, and we dream of finding new secrets of nature as important and as exciting as those uncovered by our scientific heroes. But we walk a narrow path with pitfalls on either side. If we spend all our time developing equipment, we risk the appellation of « plumber », and if we merely use the tools developed by others, we risk the censure of our peers for being parasitic. For these reasons, my colleagues and I are grateful to the Royal Swedish Academy of Science for citing both aspects of our work at the Lawrence Radiation Laboratory at the University of California - the observations of a new group of particles and the creation of the means for making those observations.

As a personal opinion, I would suggest that modern particle physics started in the last days of World War II, when a group of young Italians, Conversi, Pancini, and Piccioni, who were hiding from the German occupying forces, initiated a remarkable experiment. In 1946, they showed[3] that the « mesotron » which had been discovered in 1937 by Neddermeyer and Anderson[4] and by Street and Stevenson[5], was not the particle predicted by Yukawa[6] as the mediator of nuclear forces, but was instead almost completely unreactive in a nuclear sense. Most nuclear physicists had spent the war years in military-related activities, secure in the belief that the Yukawa meson was available for study as soon as hostilities ceased. But they were wrong.

The physics community had to endure less than a year of this nightmarish state; Powell and his collaborators[7] discovered in 1947 a singly charged particle (now known as the pion) that fulfilled the Yukawa prediction, and that decayed into the « mesotron », now known as the muon. Sanity was restored to particle physics, and the pion was found to be copiously produced in Ernest Lawrence's 184-inch cyclotron, by Gardner and Lattes[8] in 1948. The cosmic ray studies of Powell's group were made possible by the elegant nuclear-emulsion technique they developed in collaboration with the Ilford laboratories under the direction of C. Waller.

In 1950, the pion family was filled out with its neutral component by three independent experiments. In Berkeley, at the 184-inch cyclotron, Moyer, York *et al.*[9] measured a Doppler-shifted γ-ray spectrum that could only be explained as arising from the decay of a neutral pion, and Steinberger, Panofsky and Steller[10] made the case for this particle even more convincing by a beautiful experiment using McMillan's new 300-MeV synchrotron. And independently at Bristol, Ekspong, Hooper and King[11] observed the two-γ-ray decay of the π^0 in nuclear emulsion, and showed that its lifetime was less than $5 \cdot 10^{-14}$ sec.

In 1952 Anderson, Fermi and their collaborators[12] at Chicago started their classic experiments on the pion-nucleon interaction at what we would now call low energy. They used the external pion beams from the Chicago synchrocyclotron as a source of particles, and discovered what was for a long time called *the* pion-nucleon resonance. The isotopic spin formalism, which had been discussed for years by theorists since its enunciation in 1936 by Cassen and Condon[13], suddenly struck a responsive chord in the experimental physics community. They were impressed by the way Brueckner[14] showed that « *I*-spin » invariance could explain certain ratios of reaction cross sections, if the resonance, which had been predicted many years earlier by Pauli and Dancoff[15], were in the 3/2 isotopic spin state, and had an angular momentum of 3/2.

By any test we can now apply, the « 3,3-resonance » of Anderson, Fermi *et al.* was the first of the « new particles » to be discovered. But since the rules for determining what constitutes a discovery in physics have never been codified - as they have been in patent law - it is probably fair to say that it was not customary, in the days when the properties of the 3,3-resonance were of paramount importance to the high-energy physics community, to regard that resonance as a « particle ». Neutron spectroscopists study hundreds of resonances in neutron-nucleus systems which they do not regard as separate entities, even though their lives are billions of times as long. I don't believe that an early and general recognition that the 3,3-resonance should be listed in the « table of particles » would in any way have speeded up the development of high-energy physics.

Although the study of the production and the interaction of pions had passed in a decisive way from the cosmic-ray groups to the accelerator laboratories in the late 1940′s, the cosmic-ray-oriented physicsts soon found two new families of « strange particles » - the *K* mesons and the hyperons. The existence of the strange particles has had an enormous impact on the work done by our group at Berkeley. It is ironic that the parameters of the Bevatron were fixed and the decision to build that accelerator had been made before a single physicist in Berkeley really believed in the existence of strange particles. But as we look back on the evidence, it is obvious that the observations were well made, and the conclusions were properly drawn. Even if we had accepted the existence - and more pertinently the importance - of these particles, we would not have known what energy the Bevatron needed to produce strange particles; the associated production mechanism of Pais[16] and its experimental proof by Fowler, Shutt *et al.*[17] were still in the future. So the fact that, with a few notable exceptions, the Bevatron has made its greatest contributions to physics in the field of strange particles must be attributed to a very fortunate set of accidents.

The Bevatron's proton energy of 6.3 GeV was chosen so that it would be able to produce antiprotons, if such particles could be produced. Since, in the interest of keeping the « list of particles » tractable, we no longer count antiparticles nor individual members of *I*-spin multiplets, it is becoming fashionable to regard the discovery of the antiproton as an « obvious exercise for the student ». (If we were to apply the « new rules » to the classical work of Chadwick and Anderson, we would conclude that they hadn't done anything either - the neutron is simply another *I*-spin state of the proton, and Anderson's positron is simply the obvious antielectron!) In support of the non-obvious nature of the Segrè group's discovery of the antiproton[18] I need only recall that one of the most distinguished high-energy physicists I know, who didn't believe that antiprotons could be produced, was obliged to settle a 500-dollar bet with a colleague who held the now universally accepted belief that all particles can exist in an antistate.

I have just discussed in a very brief way the discovery of some particles that have been of importance in our bubble-chamber studies, and I will continue the discussion throughout my lecture. This account should not be taken to be authoritative - there is no authority in this area - but simply as a narrative to indicate the impact that certain experimental work had on my own thinking and on that of my colleagues.

I will now return to the story of the very important strange particles. In contrast to the discovery of the pion, which was accepted immediately by almost everyone - one apparent exception will be related later in this talk - the discovery and the eventual acceptance of the existence of the strange particles stretched out over a period of a few years. Heavy, unstable particles were first seen in 1947, by Rochester and Butler[19], who photographed and properly interpreted the first two « *V* particles » in a cosmic-ray-triggered cloud chamber. One of the *V*'s was charged, and was probably a *K* meson. The other was neutral, and was probably a K^0. For having made these observations, Rochester and Butler are generally credited with the discovery of strange particles. There was a disturbing period of two years in which Rochester and Butler operated their chamber and no more *V* particles were found. But in 1950 Anderson, Leighton *et al.*[20] took a cloud chamber to a mountain top and showed that it was possible to observe approximately one *V* particle per day under such conditions. They reported, « To interpret these photographs, one must come to the same remarkable conclusion as that drawn by Rochester and Butler on the basis of these two photographs, *viz.*, that these two types of events represent, respectively, the spontaneous decay of neutral and charged unstable particles of a new type. »

Butler and his collaborators then took their chamber to the Pic-du-Midi and confirmed the high event rate seen by the CalTech group on White Mountain. In 1952 they reported the first cascade decay[21]- now known as the Ξ^- hyperon.

While the cloud-chamber physicists were slowly making progress in understanding the strange particles, a parallel effort was under way in the nuclear emulsion-oriented laboratories. Although the first *K* meson was undoubtedly observed in Leprince-Ringuet's cloud chamber[22] in 1944, Bethe[23] cast sufficient doubt on its authenticity that it had no influence on the physics community and on the work that followed. The first overpowering evidence for a *K* meson appeared in nuclear emulsion, in an experiment by Brown and most of the Bristol group[24], in 1949. This so-called τ^+ meson decayed at rest into three coplanar pions. The measured ranges of the three pions gave a very accurate mass value for the τ meson of 493.6 MeV. Again there was a disturbing period of more than a year and a half before another τ meson showed up.

In 1951, the year after the τ meson and the I/particles were finally seen again, O'Ceallaigh[25] observed the first of his kappa mesons in nuclear emulsion. Each such event involved the decay at rest of a heavy meson into a muon with a different energy. We now know these particles as $K+$ mesons decaying into $^{+}\pi^{0}+\nu$, so the explanation of the broad muon energy spectrum is now obvious. But it took some time to understand this in the early 1950's, when these particles appeared one by one in different laboratories. In 1953, Menon and O'Ceallaigh[26] found the first $K_{\pi 2}$ or meson, with a decay into $\pi^{+}+\pi^{0}$. The identification of the and τ mesons as different decay modes of the same K meson is one of the great stories of particle physics, and it will be mentioned later in this lecture.

The identification of the neutral Λ emerged from the combined efforts of the cosmic-ray cloud-chambers groups, so I won't attempt to assign credit for its discovery. But it does seem clear that Thompson *et al.*[27] were the first to establish the decay scheme of what we now know as the $K_{\mathrm{I}}{}^{0}$ meson : $K_{\mathrm{I}}{}^{0}\rightarrow\pi^{+}+\pi^{-}$. The first example of a charged Σ hyperon was seen in emulsion by the Genoa and Milan groups[28], in 1953. And after that, the study of strange particles passed, to a large extent, from the cosmic-ray groups to the accelerator laboratories.

So by the time the Bevatron first operated, in 1954, a number of different strange particles had been identified: several charged particles and a neutral one all with masses in the neighborhood of 500 MeV, and three kinds of particles heavier than the proton. In order of increasing mass, these were the neutral Λ, the two charged Σ's (plus and minus), and the negative cascade (E-), which decayed into a Λ and a negative pion.

The strange particles all had lifetimes shorter than any known particles except the neutral pion. The hyperons all had lifetimes of approximately 10^{-10} sec, or less than 1% of the charged pion lifetime. When I say that they were called strange particles because their observed lifetimes presented such a puzzle for theoretical physicists to explain, I can imagine the lay members in this audience saying to themselves, « Yes, I can't see how anything could come apart so fast. » But the strangeness of the strange particles is not that they decay so rapidly, but that they last almost a million million times longer than they should-physicists couldn't explain why they didn't come apart in about 10^{-21} sec.

I won't go into the details of the dilemma, but we can note that a similar problem faced the physics community when the muon was found to be so inert, nuclearly. The suggestion by Marshak and Bethe[29] that it was the daughter of a strongly interacting particle was published almost simultaneously with the independent experimental demonstration by Powell *et al.*[7] mentioned earlier. Although invoking a similar mechanism to bring order into the strange-particle arena was tempting, Pais[16] made his suggestion that strange particles were produced « strongly » in pairs, but decayed « weakly » when separated from each other.

Gell-Mann[30] (and independently Nishijima[31]) then made the first of his several major contributions to particle physics by correctly guessing the rules that govern the production and decay of all the strange particles. I use the word « guessing » with the same sense of awe I feel when I say that Champollion guessed the meanings of the hieroglyphs on the Rosetta Stone. Gell-Mann had first to assume that the K meson was not an I-spin triplet, as it certainly appeared to bc, but an I-spin doublet plus its antiparticles, and he had further to assume the existence of the neutral Σ and of the neutral E. And finally, when he assigned appropriate values of his new quantum number, strangeness, to each family, his rules explained the one observed production reaction and predicted a score of others. And of course it explained all the known decays, and predicted another. My research group eventually confirmed all of Gell-Mann's and Nishijima's early predictions, many of them for the first time, and we continue to be impressed by their simple elegance.

This was the state of the art in particle physics in 1954, when William Brobeck turned his brainchild, the Bevatron, over to his Radiation Laboratory associates to use as a source of high-energy protons. I had been using the Berkeley proton linear accelerator in some studies of short-lived radioactive species, and I was pleased at the chance to switch to a field that appeared to be more interesting. My first Bevatron experiment was done in collaboration with Sula Goldhaber[32]; it gave the first real measurement of the τ meson lifetime. My next experiment was done with three talented young postdoctoral fellows, Frank S. Crawford Jr., Myron L. Good and M. Lynn Stevenson. An early puzzle in K-meson physics was that two of the particles (the and τ) had similar, but poorly determined, lifetimes and masses. That story has been told in his auditorium by Lee[33] and Yang[34], so I won't repeat it now. But I do like to think that our demonstration[35], simultaneously with and independently from one by Fitch and Motley[36], that the two lifetimes were not measurably different, plus similar small limits on possible mass differences set by von Friesen *et al.*[37] and by Birge *et a1.*[38], nudged Lee and Yang a bit toward their revolutionary conclusion.

Our experiences with what was then a very complicated array of scintillation counters led me and my colleagues to despair of making meaningful measurements of what we perceived to be the basic reactions of strange particle physics:

$$\begin{array}{ccccccc} \pi^- & + & p & \rightarrow & \Lambda & + & K^0 \\ & & & & \downarrow & & \downarrow \\ & & & & p + \pi^- & & \pi^- + \pi^+ \end{array}$$

The production reaction is indicated by the horizontal arrows, the subsequent decays by the vertical arrows. Fig. 1 shows a typical example of this reaction, as we saw it later in the 10-inch bubble chamber. We concluded, correctly I believe, that none of the then known techniques was well suited to study this reaction. Counters appeared hopelessly inadequate to the task, and the spark chamber had not yet been invented. The Brookhaven diffusion-cloud-chamber group[17] had photographed only a few events like that shown in Fig. 1, in a period of two years. It seemed to us that a track-recording technique was called for, but each of the three known track devices had drawbacks that ruled it out as a serious contender for the role we envisaged. Nuclear

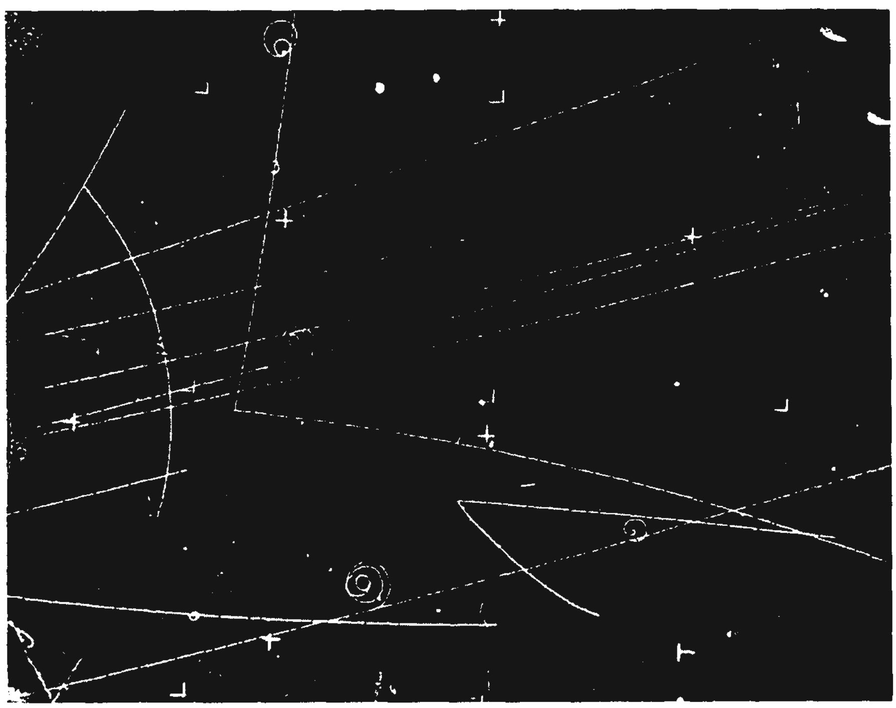

Fig. 1. $\pi^- + p \rightarrow K^0 + \Lambda$.

emulsion, which had been so spectacularly successful in the hands of Powell's group, depended on the contiguous nature of the successive tracks at a production or decay vertex. The presence of neutral and therefore nonionizing particles between related charged particles, plus lack of even a rudimentary time resolution, made nuclear-emulsion techniques virtually unusable in this new field. The two known types of cloud chambers appeared to have equally insurmountable difficulties. The older Wilson expansion chamber had two difficulties that rendered it unsuitable for the job: if used at atmospheric pressure, its cycling period was measured in minutes, and if one increased its pressure to compensate for the long mean free path of nuclear interactions, its cycling period increased at least as fast as the pressure was increased. Therefore the number of observed reactions per day started at an almost impossibly low value, and dropped as « corrective action » was taken. The diffusion cloud chamber was plagued by « background problems », and had an additional disadvantage - its sensitive volume was confined in the vertical direction to a height of only a few centimeters. What we concluded from all this was simply that particle physicists needed a track-recording device with solid or liquid density (to increase the rate of production of nuclear events by a factor of 100), with uniform sensitivity (to avoid the problems of the sensitive layer in the diffusion chamber), and with fast cycling time (to avoid the Wilson chamber problems). And of course, any cycling detector would permit the association of charged tracks joined by neutral tracks, which was denied to the user of nuclear emulsion.

In late April of 1953 I paid my annual visit to Washington, to attend the meeting of the American Physical Society. At lunch on the first day, I found myself seated at a large table in the garden of the Shoreham Hotel. All the seats but one were occupied by old friends from World War II days, and we reminisced about our experiences at the M. I. T. radar laboratory and at Los Alamos. A young chap who had not experienced those exciting days was seated at my left, and we were soon talking of our interests in physics. He expressed concern that no one would hear his 10-min contributed paper, because it was scheduled as the final paper of the Saturday afternoon session, and therefore the last talk to be presented at the meeting. In those days of slow airplanes, there were even fewer people in the audience for the last paper of the meeting than there are now - if that is possible. I admitted that I wouldn't be there, and asked him to tell me what he would be reporting. And that is how I heard first hand from Donald Glaser how he had invented the bubble chamber, and to what state he had brought its development. And of course he has since described those achievements from this platform[39]. He showed me photographs of bubble tracks in a small glass bulb, about 1 centimeter in diameter and 2 centimeters long, filled with diethyl ether. He stressed the need for absolute cleanliness of the glass bulb, and said that he could maintain the ether in a superheated state for an average of many seconds before spontaneous boiling took place. I was greatly impressed by his work, and it immediately occurred to me that this could be the « big idea » I felt was needed in particle physics.

That night in my hotel room I discussed what I had learned with my colleague from Berkeley, Frank Crawford. I told Frank that I hoped we could get started on the development of a liquid hydrogen chamber, much larger than anything Don Glaser was thinking about, as soon as I returned to Berkeley. He volunteered to stop off in Michigan on the way back to Berkeley, which he did, and learned everything he could about Glaser's technique.

I returned to Berkeley on Sunday, May 1, and on the next day Lynn Stevenson started to keep a new notebook on bubble chambers. The other day, when he saw me writing this talk, he showed me that old notebook with its first entry dated May 2, 1953, with Van der Waals' equation on the first page, and the isotherms of hydrogen traced by hand onto the second page. Frank Crawford came home a few days later, and he and Lynn moved into the « student shop » in the synchrotron building, to build their first bubble chamber. They were fortunate in enlisting the help of John Wood, who was an accelerator technician at the synchrotron. The three of them put their first efforts into a duplication of Glaser's work with hydrocarbons. When they had demonstrated radiation sensitivity in ether, they built a glass chamber in a Dewar flask to try first with liquid nitrogen and then with liquid hydrogen.

I remember that on several occasions I telephoned to the late Earl Long at the University of Chicago, for advice on cryogenic problems. Dr. Long gave active support to the liquid hydrogen bubble chamber that was being built at that time by Roger Hildebrand and Darragh Nagle at the Fermi Institute in Chicago. In August of 1953 Hildebrand and Nagle[40] showed that superheated hydrogen boiled faster in the presence of a γ-ray source than it did when the source was removed. This is a necessary (though not sufficient) condition for successful operation of a liquid hydrogen bubble chamber, and the Chicago work was therefore an important step in the development of such chambers. The important unanswered question concerned the bubble density - was it sufficient to see tracks of « minimum ionizing » particles, or did liquid hydrogen (as my colleagues had just shown that liquid nitrogen did) produce bubbles but no visible tracks?

John Wood[41] saw the first tracks in a 1.5-inch-diameter liquid hydrogen bubble chamber in February of 1954. The Chicago group could certainly have done so earlier, by rebuilding their apparatus, but they switched their efforts to hydrocarbon chambers, and were rewarded by being the first physicists to publish experimental results obtained by bubble chamber techniques. Fig. 2 is a photograph of Wood's first tracks.

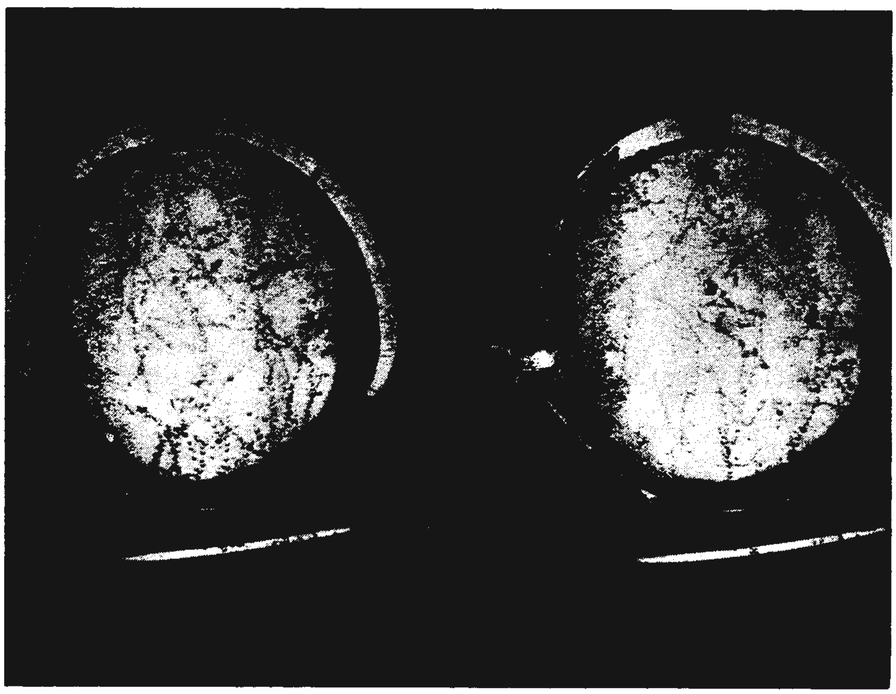

Fig. 2. First tracks in hydrogen.

At the Lawrence Radiation Laboratory, we have long had a tradition of close cooperation between physicists and technicians. The resulting atmosphere, which contributed so markedly to the rapid development of the liquid hydrogen bubble chamber, led to an unusual phenomenon: none of the scientific papers on the development of bubble-chamber techniques in my research group were signed by experimenters who were trained as physicists or who had had previous cryogenic experience. The papers all had authors who were listed on the Laboratory records as technicians, but of course the physicists concerned knew what was going on, and offered many suggestions. Nonetheless, our technical associates carried the main responsibility, and published their findings in the scientific literature. I believe this is a healthy change from practices that were common a generation ago; we all remember papers signed by a single physicist that ended with a paragraph saying, « I wish to thank Mr. . . . . . . , who built the apparatus and took much of the data ».

And speaking of acknowledgments, John Wood's first publication, in addition to thanking Crawford, Stevenson, and me for our advice and help, said, « I am indebted to A. J. Schwemin for help with the electronic circuits. » « Pete » Schwemin, the most versatile technician I have ever known, became so excited by his initial contact with John Wood's 1.5-inch-diameter all-glass chamber that he immediately started the construction of the first metal bubble chamber with glass windows. All earlier chambers had been made completely of smooth glass, without joints, to prevent accidental boiling at sharp points; such boiling of course destroyed the superheat and made the chamber insensitive to radiation. Both Glaser and Hildebrand stressed the long times their liquids could be held in the superheated condition; Hildebrand and Nagle averaged 22 sec, and observed one superheat period of 70 sec. John Wood[41] reported, « We were discouraged by our inability to attain the long times of superheat, until the track photographs showed that it was not important in the successful operation of a large bubble chamber. » I have always felt that second to Glaser's discovery of tracks this was the key observation in the whole development of bubble-chamber technique. As long as one « expanded the chamber » rapidly, bubbles forming on the wall didn't destroy the superheated condition of the main volume of the liquid, and it remained sensitive as a track-recording medium.

Pete Schwemin, with the help of Douglas Parmentier[42], built the 2.5-inch-diameter hydrogen chamber in record time, as the world's first « dirty chamber ». I've never liked that expression, but it was used for a while to distinguish chambers with windows gasketed to metal bodies from all-glass chambers. Because of its « dirtiness », the 2.5-inch chamber boiled at its walls, but still showed good tracks throughout its volume. Now that « clean » chambers are of historical interest only, we can be pleased that the modern chambers need no longer be stigmatized by the adjective « dirty ».

Lynn Stevenson's notebook shows a diagram of John Wood's chamber dated January 25, 1954, with Polaroid pictures of tracks in hydrogen. A month later he recorded details of Schwemin's 2.5-inch chamber, and drew a complete diagram dated March 5. (That was the day after the *Physical Review* received Wood's letter announcing the first observation of tracks.)

On April 29, Schwemin and Parmentier photographed their first tracks; these are shown in Fig. 3. (Things were happening so fast at this time that the 2,5-inch system was never photographed as a whole before it ended up on the scrap pile.)

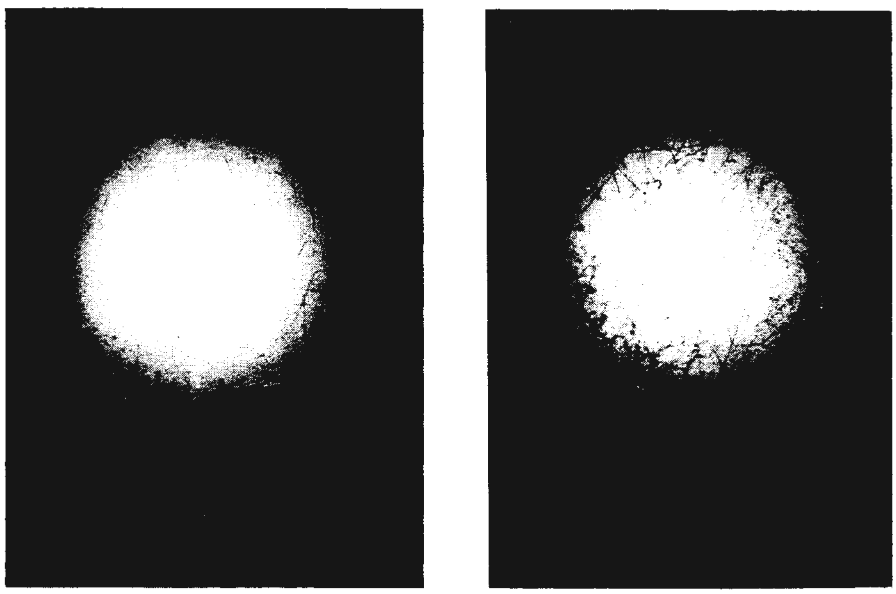

Fig. 3. Tracks in 2.5-inch chamber: neutrons (left); γ-rays (right).

In August, Schwemin and Parmentier separately built two different 4-inch-diameter chambers. Both were originally expanded by internal bellows, and Parmentier's 4-inch chamber gave tracks on October 6. Schwemin's chamber produced tracks three weeks later, and survived as *the* 4-inch chamber (see Fig. 4). The bellows systems in both chambers failed, but it turned out to be easier to convert Schwemin's chamber to the vapor expansion system that was used in all our subsequent chambers until 1962. (In that year, the 25-inch chamber introduced the « Ω -bellows » that is now standard for large chambers.)

Fig. 5 shows all our chambers displayed together a few weeks ago, at the request of Swedish Television. As you can see, we all look pretty pleased to see so many of our « old friends » side by side for the first time.

Fig. 6 shows an early picture of multiple meson production in the 4-inch chamber. This chamber was soon equipped with a pulsed magnetic field, and in that configuration it was the first bubble chamber of any kind to show

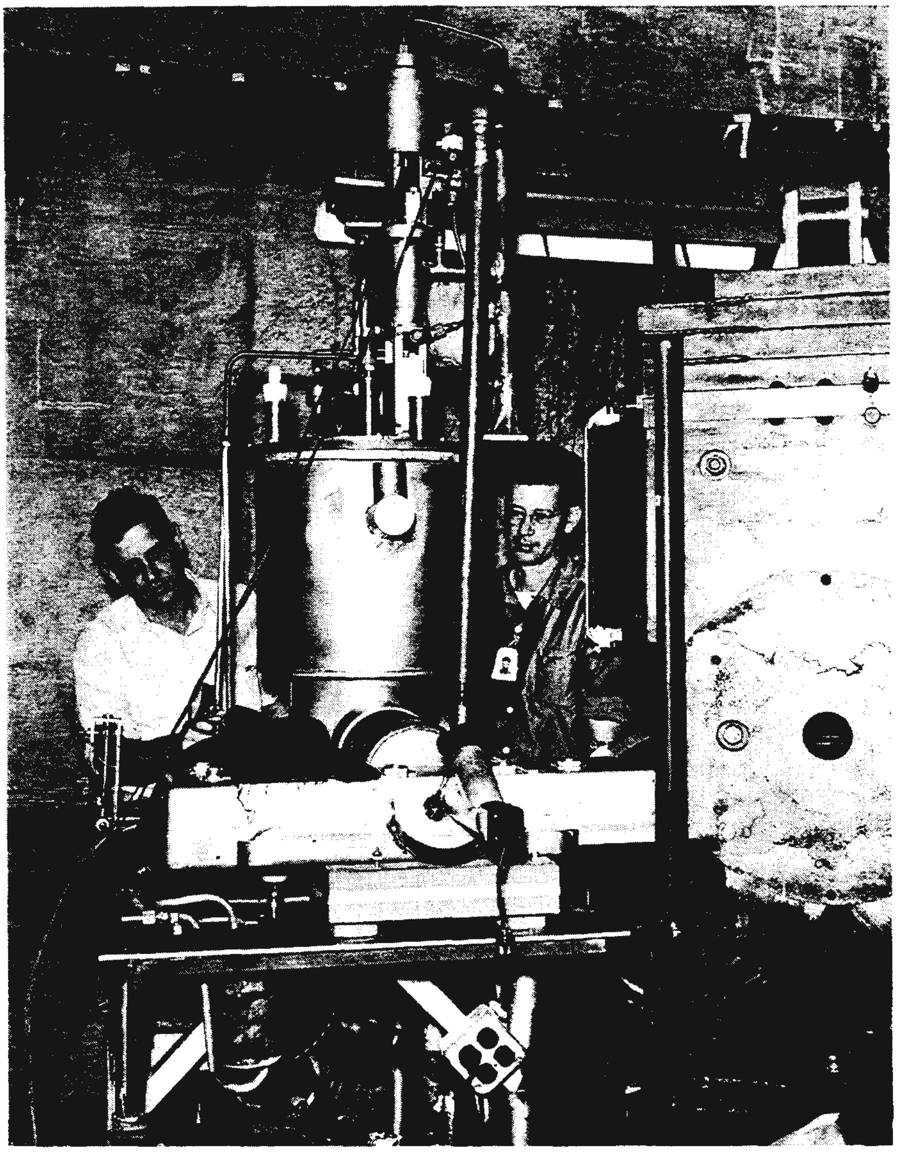

Fig. 4. The 4-inch chamber. D. Parmentier (left), A. J. Schwemin (right).

magnetically curved tracks. It was then set aside by our group as we pushed on to larger chambers. But it ended its career as a useful research tool at the Berkeley electron synchrotron, after almost two million photographs of 300-MeV Bremsstrahlung passing through it had been taken and analyzed by Bob Kenney *et al.*[43].

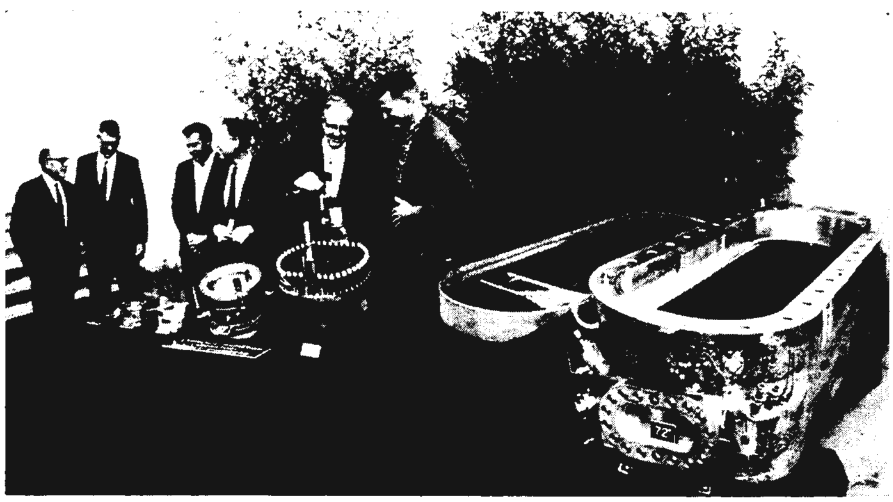

Fig. 5. Display of chambers, November 1968. From left to right, 1, 5, 4, 6, 10, 15 and 72 inch chambers; Hernandez, Schwemin, Rinta, Watt, Alvarez and Eckman.

In the year 1954, as I have just recounted, various members of my research group had been responsible for the successful operation of four separate liquid hydrogen bubble chambers, increasing in diameter from 1.5 inches to 4 inches. By the end of that eventful year, it was clear that it would take a more concerted engineering-type approach to the problem if we were to progress to the larger chambers we felt were essential to the solution of high-energy physics problems. I therefore enlisted the assistance of three close associates, J. Donald Gow, Robert Watt and Richard Blumberg. Don Gow and Bob Watt had taken over full responsibility for the development and operation of the 32-MeV linear accelerator that had occupied all my attention from its inception late in 1945 until it first operated in late 1947. Neither of them had any experience with cryogenic techniques, but they learned rapidly, and were soon leaders in the new technology of hydrogen-bubble chambers. Dick Blumberg had been trained as a mechanical engineer, and he had designed the equipment used by Crawford, Stevenson and me in our experiments, then in progress, on the Compton scattering of γ-rays by protons[44].

Wilson Powell had built two large magnets to accommodate his Wilson Cloud Chambers, pictures from which adorned the walls of every cyclotron laboratory in the world. He very generously placed one of these magnets at our disposal, and Dick Blumberg immediately started the mechanical design of the 10-inch chamber - the largest size we felt could be accommodated in the well of Powell's magnet. Blumberg's drafting table was in the middle of the single room that contained the desks of all the members of my research group. Not many engineers will tolerate such working conditions, but Blumberg was able to do so and he produced a design that was quickly built in the main machine shop. All earlier chambers had been built by the experimenters themselves. The design of the 10-inch chamber turned out to be a much larger job than we had foreseen. By the time it was completed, eleven members of the Laboratory's Mechanical Engineering Department had worked on it, including Rod Byrns and John Mark. The electrical engineering aspects of all our large chambers were formidable, and we are indebted to Jim Shand for his leadership in this work for many years.

Great difficulty was experienced with the first operation of the 10-inch

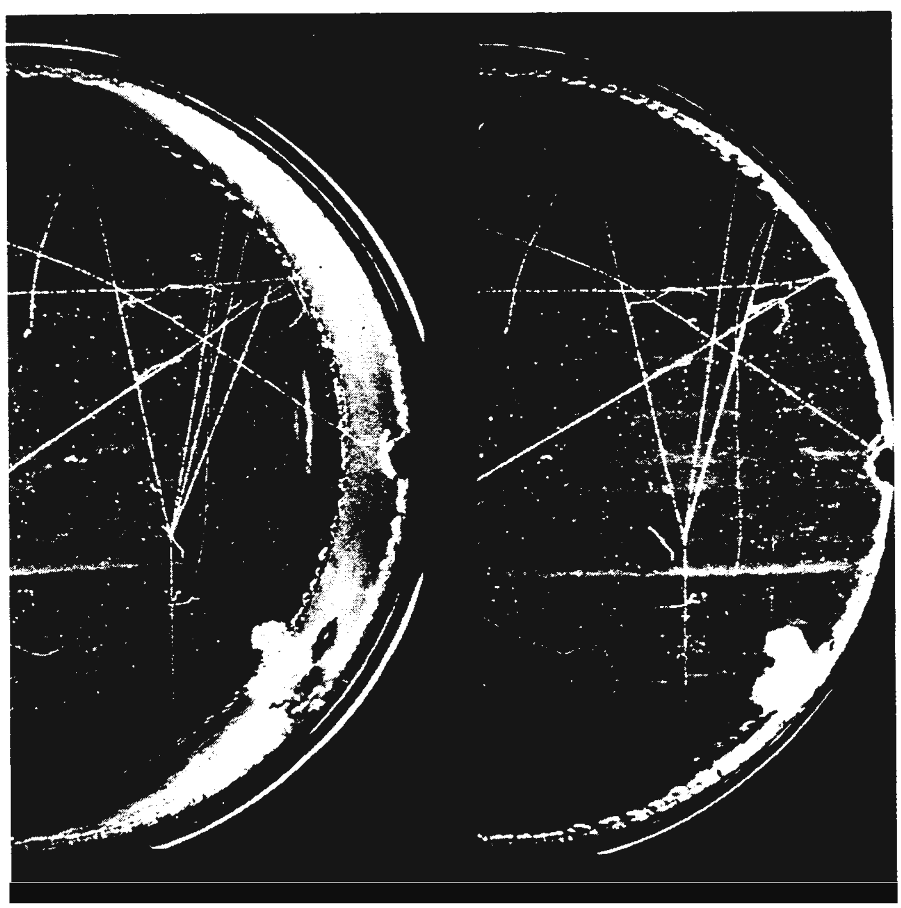

Fig. 6. Multiple meson production in 4-inch chamber.

chamber: too much hydrogen was vaporized at each « expansion ». Pete Schwemin quickly diagnosed the trouble and built a fast-acting valve that permitted the chamber to be pulsed every 6 sec, to match the Bevatron's cycling time.

It would be appropriate to interrupt this description of the bubble-chamber development program to describe the important observations made possible by the operation of the 10-inch chamber early in 1956, but instead, I will preserve the continuity by describing the further development of the hardware. In December of 1954, shortly after the 4-inch chamber had been operated in the cyclotron building for the first time, it became evident to me that the 10-inch chamber we had just started to design wouldn't be nearly large enough to tell us what we wanted to know about the strange particles. The tracks of these objects had been photographed at Brookhaven[17], and we knew they were produced copiously by the Bevatron.

The size of the « big chamber » was set by several different criteria, and fortunately all of them could be satisfied by one design. (Too often, a designer of new equipment finds that one essential criterion can be met only if the objects is very large, while an equally important criterion demands that it be very small.) All « dirty chambers » so far built throughout the world had been cylindrical in shape, and were characterized by their diameter measurement. By studying the relativistic kinematics of strange particles produced by Bevatron beams, and more particularly by studying the decay of these particles, I convinced myself that the big chamber should be rectangular, with a length of at least 30 inches. This length was next increased to 50 inches in order that there would be adequate amounts of hydrogen upstream from the required decay region, in which production reactions could take place. Later the length was changed to 72 inches, when it was realized that the depth of the chamber could properly be less than its width and that the change could be made without altering the volume. The production region corresponded to about 10% of a typical pion-proton mean free path, and the size of the decay region was set by the relativistic time-dilated decay lengths of the strange particles, plus the requirement that there be a sufficient track length available in which to measure magnetic curvature in a « practical magnetic field » of 15 000 gauss. In summary, then, the width and depth of the chamber came rather simply from an examination of the shape of the ellipses that characterize relativistic transformations at Bevatron energies, plus the fact that the magnetic field spreads the particles across the width but not along the depth of the chamber.

The result of this straightforward analysis was a rather frightening set of numbers: The chamber length was 72 inches; its width was 20 inches, and its depth was 15 inches. It had to be pervaded by a magnetic field of 15 000 gauss, so its magnet would weigh at least 100 tons and would require 2 or 3 megawatts to energize it. It would require a window 75 inches long by 23 inches wide and 5 inches thick to withstand the (deuterium) operating pressure of 8 atmospheres, exerting force of 100 tons on the glass. No one had any experience with such large volumes of liquid hydrogen; the hydrogen-oxygen rocket engines that now power the upper stages of the Saturn boosters were still gleams in the eyes of their designers - these were pre-Sputnik days. The safety aspects of the big chamber were particularly worrisome. Low temperature laboratories had a reputation for being dangerous places in which to work, and they didn't deal with such large quantities of liquid hydrogen, and what supplies they did use were kept at atmospheric pressure.

For some time, the glass-window problem seemed insurmountable - no one had ever cast and polished such a large piece of optical glass. Fortunately for the eventual success of the project, I was able to persuade myself that the chamber body could be constructed of a transparent plastic cylinder with metallic end plates. This notion was later demolished by my engineering colleagues, but it played an important role in keeping the project alive in my own mind until I was convinced that the glass window could be built. As an indication of the cryogenic ,« state of the art » at the time we worried about the big window, I can recall the following anecdote. One day, while looking through a list of titles of talks at a recent cryogenic conference, I spotted one that read, « Large glass window for viewing liquid hydrogen.» Eagerly I turned to the paper - but it described a metallicDewar vessel equipped with a glass window 1 inch in diameter!

Don Gow was now devoting all his time to hydrogen bubble chambers, and in January of 1955 we interested Paul Hernandez in taking a good hard engineering look at the problems involved in building and housing the 72-inch bubble chamber. We were also extremely fortunate in being able to interest the cryogenic engineers at the Boulder, Colorado, branch of the National Bureau of Standards in the project. Dudley Chelton, Bascomb Birmingham and Doug Mann spent a great deal of time with us, first educating us in large-scale liquid-hydrogen techniques, and later cooperating with us in the design and initial operation of the big chamber.

In April of 1955, after several months of discussion of the large chamber, I wrote a document entitled « The Bubble Chamber Program at UCRL ».

This paper showed in some detail why it was important to build the large chamber, and outlined a whole new way of doing high-energy physics with such a device. It stressed the need for semiautomatic measuring devices (which had not previously been proposed), and described how electronic computers would reconstruct tracks in space, compute momenta, and solve problems in relativistic mechanics. All these techniques are now part of the «standard bubble-chamber method», but in April of 1955 no one had yet applied them. Of all the papers I have written in my life, none gives me so much satisfaction on rereading as does this unpublished prospectus.

After Paul Hernandez and Don Gow had estimated that the big chamber, including its building and power supplies, would cost about 2.5 million dollars, it was clear that a special AEC appropriation was required; we could no longer build our chambers out of ordinary laboratory operating money. In fact, the document I've just described was written as a sort of proposal to the AEC for financial support - but without mentioning money! I asked Ernest Lawrence if he would help me in requesting extra funds from the AEC. He read the document, and agreed with the points I had made. He then asked me to remind him of the size of the world's largest hydrogen chamber. When I replied that it was 4 inches in diameter, he said he thought I was making too large an extrapolation in one step, to 72 inches. I told him that the 10-inch chamber was on the drawing board, and if we could make it work, the operation of the 72-inch chamber was assured. (And if we couldn't make it work, we could refund most of the 2.5 million.) This wasn't obvious until I explained the hydraulic aspects of the expansion system of the 72-inch chamber; it was arranged so that the 20-inch wide, 72-inch long chamber could be considered to be a large collection of essentially independently expanded 10-inch square chambers. He wasn't convinced of the wisdom of the program, but in a characteristic gesture, he said, « I don't believe in your big chamber, but I do believe in you, and I'll help you to obtain the money. » I therefore accompanied him on his next trip to Washington, and we talked in one day to three of the five Commissioners: Lewis Strauss, Willard Libby (who later spoke from this podium), and the late John Von Neumann, the greatest mathematical physicist then living. That evening, at a cocktail party at Johnny Von Neumann's home, I was told that the Commission had voted that afternoon to give the laboratory the 2.5 million dollars we had requested. All we had to do now was build the thing and make it work!

Design work had of course been under way for some time, but it was now rapidly accelerated. Don Gow assumed a new role that is not common in physics laboratories, but is well known in military organizations; he became my « chief of staff ». In this position, he coordinated the efforts of the physicists and engineers; he had full responsibility for the careful spending of our precious 2.5 million dollars, and he undertook to become an expert second to none in all the technical phases of the operation, from low-temperature thermodynamics to safety engineering. His success in this difficult task can be recognized most easily in the success of the whole program, culminating in the fact that I am speaking here this afternoon. I am sorry that Don Gow can't be here today; he died several years ago, but I am reminded of him every day - my three-year-old son is named Donald in his memory.

The engineering team under Paul Hernandez's direction proceeded rapidly with the design, and in the process solved a number of difficult problems in ways that have become standard « in the industry ». A typical problem involved the very considerable differential expansion between the stainless steel chamber and the glass window. This could be lived with in the 10-inch chamber, but not in the 72-inch. Jack Franck's « inflatable gasket » allowed the glass to be seated against the chamber body only after both had been cooled to liquid hydrogen temperature.

Just before leaving for Stockholm, I attended a ceremony at which Paul Hernandez was presented with a trophy honoring him as a « Master Designer » for his achievements in the engineering of the 72-inch chamber. I had the pleasure of telling in more detail than I can today of his many contributions to the success of our program. One of his associates recalled a special service that he rendered not only to our group but to all those who followed us in building liquid hydrogen-bubble chambers. Hernandez and his associates wrote a series of « Engineering Notes », on matters of interest to designers of hydrogen-bubble chambers, that soon filled a series of notebooks that spanned 3 feet of shelf space. Copies of these were sent to all interested parties on both sides of the Atlantic, and I am sure that they resulted in a cumulative savings to all bubble-chamber builders of several million dollars; had not all this information been readily available, the test programs and calculations of our engineering group would have required duplication at many laboratories, at a large expense of money and time. Our program moved so rapidly that there was never time to put the Engineering Notes into finished form for publication in the regular literature. For this reason, one can now read review articles on bubble-chamber technology, and be quite unaware of the part that our Laboratory played in its development. There are no references to papers by members of our group, since those papers were never written - the data that would have been in them had been made available to everyone who needed them at a much earlier date.

And just to show that I was also deeply involved in the chamber design, I might recount how I purposely « designed myself into a corner » because I thought the results were important, and I thought I could invent a way out of a severe difficulty, if given the time. All previous chambers had had two windows, with « straight through » illumination. Such a configuration reduces the attainable magnetic field, because the existence of a rear pole piece would interfere with the light-projection system. I made the decision that the 72-inch chamber would have only a top window, thereby permitting the magnetic field to be increased by a lower pole piece and at the same time saving the cost of the extra glass window, and also providing added safety by eliminating the possibility that liquid hydrogen could spill through a broken lower window. The only difficulty was that for more than a year, as the design was firmed up and the parts were fabricated, none of us could invent a way both to illuminate and to photograph the bubbles through the same window. Duane Norgren, who has been responsible for the design of all our bubble-chamber cameras, discussed the matter with me at least once a week in that critical year, and we tried dozens of schemes that didn't quite do the job. But as a result of our many failures, we finally came to understand all the problems, and we eventually hit on the retrodirecting system known as coat hangers. This solution came none too soon; if it had been delayed by a month or more, the initial operation of the 72-inch chamber would have been correspondingly delayed. We took many other calculated risks in designing the system; if we had postponed the fabrication of the major hardware until we had solved all the problems on paper, the project might still not be completed. Engineers are conservative people by nature; it is the ultimate disgrace to have a boiler explode or a bridge collapse. We were therefore fortunate to have Paul Hernandez as our chief engineer; he would seriously consider anything his physics colleagues might suggest, no matter how outlandish it might seem at first sight. He would firmly reject it if it couldn't be made safe, but before rejecting any idea for lack of safety he would use all the ingenuity he possessed to make it safe.

We felt that we needed to built a test chamber to gain experience with a single-window system, and to learn to operate with a hydrogen refrigerator; our earlier chambers had all used liquid hydrogen as a coolant. We therefore built and operated the 15-inch chamber in the Powell magnet, in place of the 10-inch chamber that had served us so well.

The 72-inch chamber operated for the first time on March 24, 1959, very nearly four years from the time it was first seriously proposed. Fig. 7 shows it at about that time. The « start- up team » consisted of Don Gow, Paul Hernandez and Bob Watt, all of whom had played key roles in the initial operation

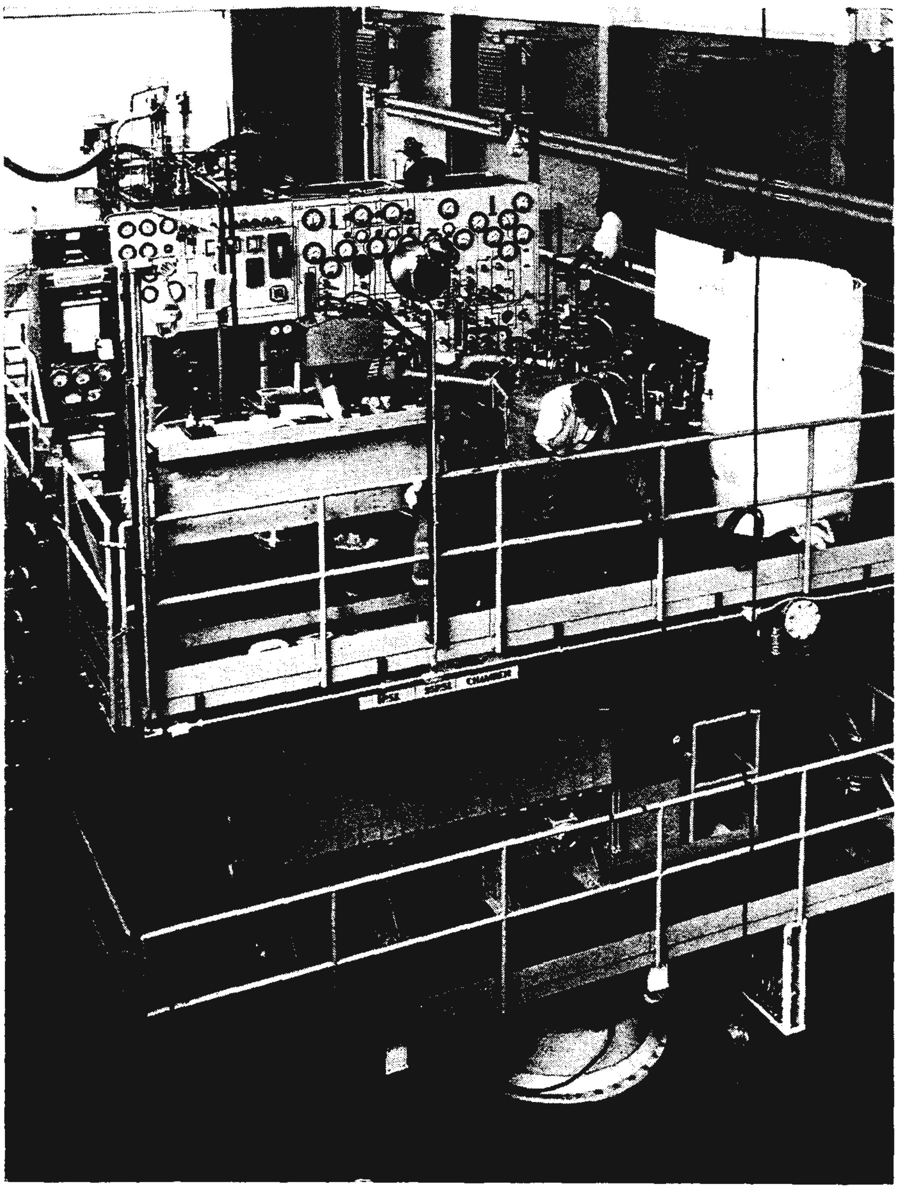

Fig. 7. The 72-inch bubble chamber in its building.

of the 15-inch chamber. Bob Watt and Glenn Eckman have been responsible for the operation of all our chambers from the earliest days of the 10-inch chamber, and the success of the whole program has most often rested in their hands. They have maintained an absolutely safe operating record in the face of very severe hazards, and they have supplied their colleagues in the physics community with approximately ten million high-quality stereo photographs. And most recently, they have shown that they can design chambers as well as they have operated them. The 72-inch chamber was recently enlarged to an 82-inch size, incorporating to a large extent the design concepts of Watt and Eckman.

Although I haven't done justice to the contributions of many close friends and associates who shared in our bubble-chamber development program, I must now turn to another important phase of our activities - the data-analysis program. Soon after my 1955 prospectus was finished, Hugh Bradner undertook to implement the semiautomatic measuring machine proposal. He first made an exhaustive study of commercially available measuring machines, encoding techniques, etc., and then, with Jack Franck, designed the first « Franckenstein ». This rather revolutionary device had been widely copied, to such an extent that objects of its kind are now called « conventional » measuring

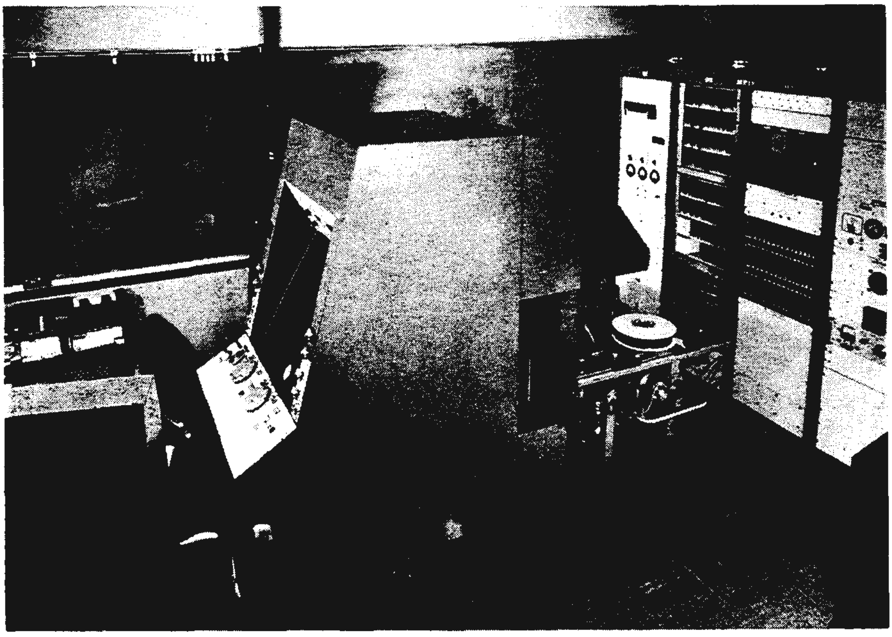

Fig. 8. « Franckenstein ».

machines (Fig. 8). Our first Franckenstein was operating reliably in 1957, and in the summer of 1958 a duplicate was installed in the U. S. exhibit at the « Atoms for Peace » exposition in Geneva. It excited a great deal of interest in the high-energy physics community, and a number of groups set out to make similar machines based on its design. Almost everyone thought at first that our provision for automatic track following was a needless waste of money, but over the years, that feature has also come to be « conventional ».

Jack Franck then went on to design the Mark II Franckenstein, to measure 72-inch bubble-chamber film. He had the first one ready to operate just in time to match the rapid turn-on of the big chamber, and he eventually built three more of the Mark II's. Other members of our group then designed and perfected the faster and less expensive SMP system, which added significantly to our « measuring power ». The moving forces in this development were Pete Schwemin, Bob Hulsizer, Peter Davey, Ron Ross and Bill Humphrey[45]. Our final and most rewarding effort to improve our measuring ability was fulfilled several years ago, when our first Spiral Reader became operational. This single machine has now measured more than one and a half million high-energy interactions, and has, together with its almost identical twin, measured one and a quarter million events in the last year. The SAAB Company here in Sweden is now building and selling Spiral Readers to European laboratories.

The Spiral Reader had a rather checkered career, and it was on several occasions believed by most workers in the field to have been abandoned by our group. The basic concept of the spiral scan was supplied by Bruce McCormick, in 1956. Our attempts to reduce his ideas to practice resulted in failure, and shortly after that, McCormick moved to Illinois, where he has since been engaged in computer development. As the cost of transistorized circuits dropped rapidly in the next years, we tried a second time to implement the Spiral Reader concept, using digital techniques to replace the analog devices of the earlier machine. The second device showed promise, but its « hard-wired logic » made it too inflexible, and the unreliability of its electronic components kept it undergoing repair most of the time. The mechanical and optical components of the second Spiral Reader were excellent, and we hated to drop the whole project simply because the circuitry didn't come up to the same standard. In 1963, Jack Lloyd suggested that we use one of the new breed of small high-speed, inexpensive computers to supply the logic and the control circuits for the Spiral Reader. He then demonstrated great qualities of leadership by delivering to our research group a machine that has performed even better than he had promised it would. In addition to his development of the hardware, he initiated POOH, the Spiral Reader filtering program, which was brought to a high degree of perfection by Jim Burkhard. The smooth and rapid transition of the Spiral Reader from a developmental stage into a useful operational tool was largely the result of several years of hard work on the part of Gerry Lynch and Frank Solmitz. Fig. 9, from a talk I gave two and a half years ago[46], shows how the measuring power of our group has increased over the years, with only a modest increase in personnel.

According to a simple extrapolation of the exponential curve we had been

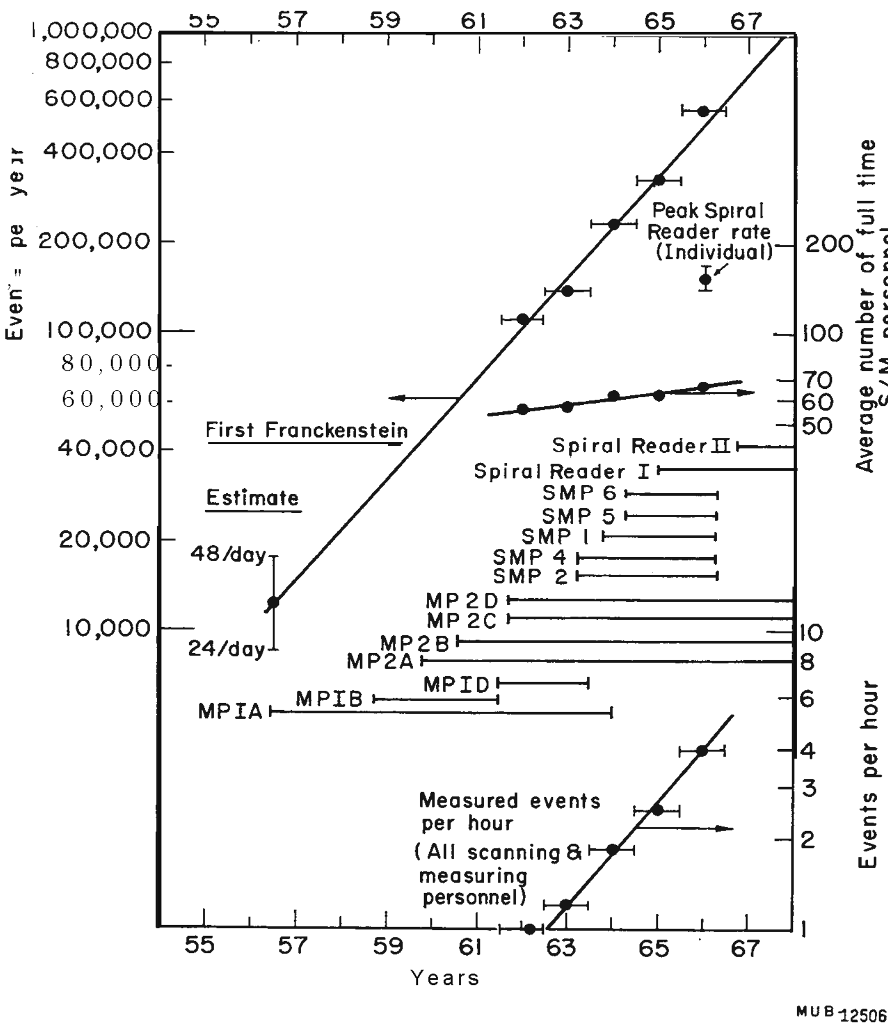

Fig. 9. Measuring rates.

on from 1957 through 1966, we would expect to be measuring 1.5 million events per year some time in 1969. But we have already reached that rate and we will soon be leveling off about there because we have stopped our development work in this area.

The third key ingredient of our development program has been the continually increasing sophistication in our utilization of computers, as they have increased in computational speed and memory capacity. While I can speak from a direct involvement in the development of bubble chambers and measuring machines, and in the physics done with those tools, my relationship to our computer programming efforts is largely that of an amazed spectator. We were most fortunate that in 1956 Frank Solmitz elected to join our group. Although the rest of the group thought of themselves as experimental physicists, Solmitz had been trained as a theorist, and had shown great aptitude in the development of statistical methods of evaluating experimental data. When he saw that our first Franckenstein was about to operate, and no computer programs were ready to handle the data it would generate, he immediately set out to remedy the situation. He wrote HYDRO, our first system program for use on the IBM 650 computer. In the succeeding twelve years he has continued to carry the heavy responsibility for all our programming efforts. A major breakthrough in the analysis of bubble-chamber events was made in the years 1957 through 1959. In this period, Solmitz and Art Rosenfeld, together with Horace Taft from Yale University and Jim Snyder from Illinois, wrote the first « fitting routine », GUTS, which was the core of our first « kinematics program », KICK. To explain what KICK did, it is easiest to describe what physicists had to do before it was written. HYDRO and its successor, PANG, listed for each vertex the momentum and space angles of the tracks entering or leaving that vertex, together with the calculated errors in these measurements. A physicist would plot the angular coordinates on a stereographic projection of a unit sphere known as a Wolff-plot. If he was dealing with a three-track vertex - and that was all we could handle in those days - he would move the points on the sphere, within their errors, if possible, to make them coplanar. And of course he would simultaneously change the momentum values, within their errors, to insure that the momentum vector triangle closed, and energy was conserved. Since momentum is a vector quantity, the various conditions could be simultaneously satisfied only after the angles and the absolute values of the momenta had been changed a number of times in an iterative procedure. The end result was a more reliable set of momenta and angles, constrained to fit the conservation laws of energy and momentum. In a typical case, an experienced physicist could solve only a few Wolff-plot problems in a day. (Lynn Stevenson had written a specific program, COPLAN, that solved a particular problem of interest to him that was later handled by the more versatile GUTS.)

GUTS was being written at a time when one highly respected visitor to the group saw the large pile of PANG printout that had gone unanalyzed because so many of our group members were writing GUTS - a program that was planned to do the job automatically. Our visitor was very upset at what he told me was a «foolish deployment of our forces ». He said, « If you would only get all those people away from their program writing, and put them to work on Wolff-plots, we'd have the answer to some really important physics in a month or two ». I said I was sure we'd end up with at lot more physics in the next years if my colleagues continued to write GUTS and KICK. I'm sure that those who wrote these pioneering « fitting and kinematics programs » were subjected to similar pressures. Everyone in the high-energy physics community has long been indebted to these farsighted men because they knew that what they were doing was right. KICK was soon developed so that it gave an overall fit to several interconnected vertices, with various hypothetical identities of the several tracks assumed in a series of attempts at a fit. The relationship between energy and momentum depends on mass, so a highly constrained fit can be obtained only if the particle responsible for each track is properly identified. If the degree of constraint is not so high, more than one « hypothesis » (set of track identifications) may give a fit, and the physicist must use his judgment in making the identification.

As another example in this all-too-brief sketch of the computational aspects of our work, I will mention an important program, initiated by Art Rosenfeld and Ron Ross, that has removed much of the remaining drudgery from the bubble-chamber physicists' life. SUMX is a program that can easily be instructed to search quickly through large volumes of « kinematics program output », printing out summaries and tabulations of interesting data. (Like all our pioneering programs, SUMX was replaced by an improved and more versatile program - in this case, KIOWA. But I will continue to talk as though SUMX were still used.) A typical SUMX printout will be a computer-printed document 3 inches thick, with hundreds of histograms, scatter plots, etc.

Hundreds of histograms are similarly printed showing numbers of events with effective masses for many different combinations of particles, with various « cuts » on momentum transfer, etc. What all this amounts to is simply that a physicist is no longer rewarded for his ability in deciding what histograms he should tediously plot and then examine. He simply tells the computer to plot all histograms of any possible significance, and then flips the pages to see which ones have interesting features.

One of my few real interactions with our programming effort came when I suggested to Gerry Lynch the need for a program he wrote that is known as GAME. In my work as a nuclear physicist before World War II, I had often been skeptical of the significance of the « bumps » in histograms, to which importance was attached by their authors. I developed my own criteria for judging statistical significance, by plotting simulated histograms, assuming curves to be smooth; I drew several samples of « Monte Carlo distributions », using a table of random numbers as the generator of the samples. I usually found that my skepticism was well founded because the « faked » histograms showed as much structure as the published ones. There are of course many statistical tests designed to help one evaluate the reality of bumps in histograms, but in my experience nothing is more convincing than an examination of a set of simulated histograms from an assumed smooth distribution.

GAME made it possible, with the aid of a few control cards, to generate a hundred histograms similar to those produced in any particular experiment. All would contain the same number of events as the real experiment, and would be based on a smooth curve through the experimental data. The standard procedure is to ask a group of physicists to leaf through the 100 histograms - with the experimental histogram somewhere in the pile - and vote on the apparent significance of the statistical fluctuations that appear. The first time this was tried, the experimenter - who had felt confident that his bump was significant - didn't know that his own histogram was in the pile, and didn't pick it out as convincing; he picked out two of the computer-generated histograms as looking significant, and pronounced all others - including his own - as of no significance! In view of this example, one can appreciate how many retractions of discovery claims have been avoided in our group by the liberal use of the GAME program.

As a final example from our program library, I'll mention FAKE, which, like SUMX, has been widely used by bubble-chamber groups all over the world. FAKE, written by Gerry Lynch, generates simulated measurements of bubble-chamber events to provide a method of testing the analysis programs to determine how frequently they arrive at an incorrect answer.

Now that I have brought you up to date on our parallel developments of hardware and software (computer programs), I can tell you what rewards we have reaped, as physicists, from their use. The work we did with the 4-inch chamber at the 184-inch cyclotron and at the Bevatron cannot be dignified by the designation « experiment », but it did show examples of π–μ–e decay and neutral strange-particle decay. The experiences we had in scanning the 4-inch film merely whetted our appetite for the exciting physics we felt sure would be manifest in the 10-inch chamber, when it came into operation in Wilson Powell's big magnet.

Robert Tripp joined the group in 1955, and as his first contribution to our program he designed a « separated beam » of negative K mesons that would stop in the 10-inch chamber. We had two different reasons for starting our bubble-chamber physics program with observations of the behavior of K^- mesons stopping in hydrogen. The first reason involved physics: The behavior of stopping π^- mesons in hydrogen had been shown by Panofsky[47] and his co-workers to be a most fruitful source of fundamental knowledge concerning particle physics. The second reason was of an engineering nature: Only one Bevatron « straight section » was available for use by physicists, and it was in constant use. In order not to interfere with other users, we decided to set the 10-inch chamber close to a curved section of the Bevatron, and use secondary particles, from an internal target, that penetrated the wall of the vacuum chamber and passed between neighboring iron blocks in the return yoke of the Bevatron magnet. This physical arrangement gave us negative particles (K^- and π^- mesons) of a well-defined low momentum. By introducing an absorber into the beam, we brought the K^- mesons almost to rest, but allowed the lighter π^- mesons to retain a major fraction of their original momentum. The Powell magnet provided a second bending that brought the K^- mesons into the chamber, but kept the π^- mesons out. That was the theory of this first separated beam for bubble-chamber use. But in practice, the chamber was filled with tracks of pions and muons, and we ended up with only one stopped K^- per roll of 400 stereo pairs. It is now common for experimenters to stop one million K^- mesons in hydrogen, in a single experimental run, but the 137 K^- mesons we stopped in 1956[48] gave us a remarkable preview of what has now been learned in the much longer exposures. We measured the relative branching of $K^- + p$ into

$$\Sigma^- + \pi^+ : \Sigma^+ + \pi^- : \Sigma^0 + \pi^0 : \Lambda + \pi^0$$

And in the process, we made a good measurement of the Σ^0 mass. We plotted the first decay curves for the Σ^+ and Σ^- hyperons, and we observed for the first time the interactions of Σ^- hyperons and protons at rest. We felt amply rewarded for our years of developmental work on bubble chambers by the very interesting observations we were now privileged to make.

We had a most exciting experience at this time, that was the result of two circumstances that no longer obtain in bubble-chamber physics. In the first place, we did all our own scanning of the photographic film. Such tasks are now carried out by professional scanners, who are carefully trained to recognize and record ((interesting events)). We had no professional scanners at the time, because we wouldn't have known how to train them before this first film became available. And even if they had been trained, we would not have let them look at the film-we found it so completely absorbing that there was always someone standing behind a person using one of our few film viewers, ready to take over when the first person's eyes tired. The second circumstance that made possible the accidental discovery I am about to describe was the very poor quality of our separated K^- beam-by modern standards. Most of the tracks we observed were made by negative pions or muons, but we also saw many positively charged particles-protons, pions and muons.

At first we kept no records of any events except those involving strange particles; we would look quickly at each frame in turn, and shift to the next one if no « interesting event » showed up. In doing this scanning, we saw many examples of π^+–μ^+–e^+ decays, usually from a pion at rest, and we soon learned about how long to expect the $^+$track to be-about 1 centimeter. I did my scanning on a stereo viewer, so I probably had a better feeling for the length of a $^+$track in space than did my colleagues, who looked at two projections of the stereo views, sequentially. Don Gow, Hugh Bradner, and I often scanned at the same time, and we showed each other whatever interesting events came into view. Each of us showed the others examples of what we thought was an unusual decay scheme: $\pi^- \rightarrow \mu^- \rightarrow$ e-. The decay of a at rest into an e-, in hydrogen, was expected from the early observations by Conversi et *al.*[3], but Panofsky[47] had shown that a π^- meson couldn't decay at rest in hydrogen. Our first explanation for our observations was simply that the pion had decayed just before stopping. But we gradually became convinced that this explanation really didn't fit the facts. There were too many muon tracks of about the same length, and none that were appreciably longer or shorter, as the decay-in-flight hypothesis would predict. We now began to keep records of these « anomalous decays », as we still called them, and we found occasional examples in which the muon was horizontal in the chamber, so its length could be measured. (We had as yet no way of reconstructing tracks in space from two stereo views.) By comparing the measured length of the negative muon track with that of its more normal positive counterpart, we estimated that the negative muons had an energy of 5.4MeV, rather than the well-known positive muon energy (from positive pion decay at rest) of 4.1 MeV. This confirmed our earlier suspicion that the long primary negative track couldn't be that of a pion, but it left us just as much in the dark as to the nature of the primary.

After these observations had been made, I gave a seminar describing what we had observed, and suggesting that the primary might be a previously unknown weakly interacting particle, heavier than the pion, that decayed into a muon and a neutral particle, either neutrino or photon. We had just made the surprising observation, shown in Fig.10, that there was often a gap, measured in millimeters, between the end of the primary and the beginning of the secondary. This finding suggested diffusion by a rather long-lived negative particle that orbited around and neutralized one of the protons in the liquid hydrogen. We had missed many tracks with these « gaps » because no one had seen such a thing before; we simply ignored such track configurations by subconsciously assuming that they were unassociated events in a badly cluttered bubble chamber.

One evening, one of the members of our research team, Harold Ticho from our Los Angeles campus, was dining with Jack Crawford, a Berkeley astrophysicist he had known when they were students together. They discussed our observations at some length, and Crawford suggested the possibility that a fusion reaction might somehow be responsible for the phenomenon. They calculated the energy released in several such reactions, and found that it agreed with experiment if a stopped muon were to be binding together a proton and a deuteron into an HD - molecular ion. In such a « molecule » the proton and deuteron would be brought into such close proximity for such a long time that they would fuse into ^{3}He, and could deliver their fusion energy to the muon by the process of internal conversion. However, Ticho and Crawford couldn't think of any mechanism that would make the reaction happen so often - the fraction of deuterons in liquid hydrogen is only 1 in 5000. They had, however, correctly identified the reaction, but a key ingredient in the theoretical explanation was still missing.

The next day, when we had all accepted the idea that stopped muons were catalyzing the fusion of protons and deuterons, our whole group paid a visit to Edward Teller, at his home. After a short period of introduction to the observations and to the proposed fusion reaction, he explained the high probability of the reaction as follows : the stopped muon radiated its way into

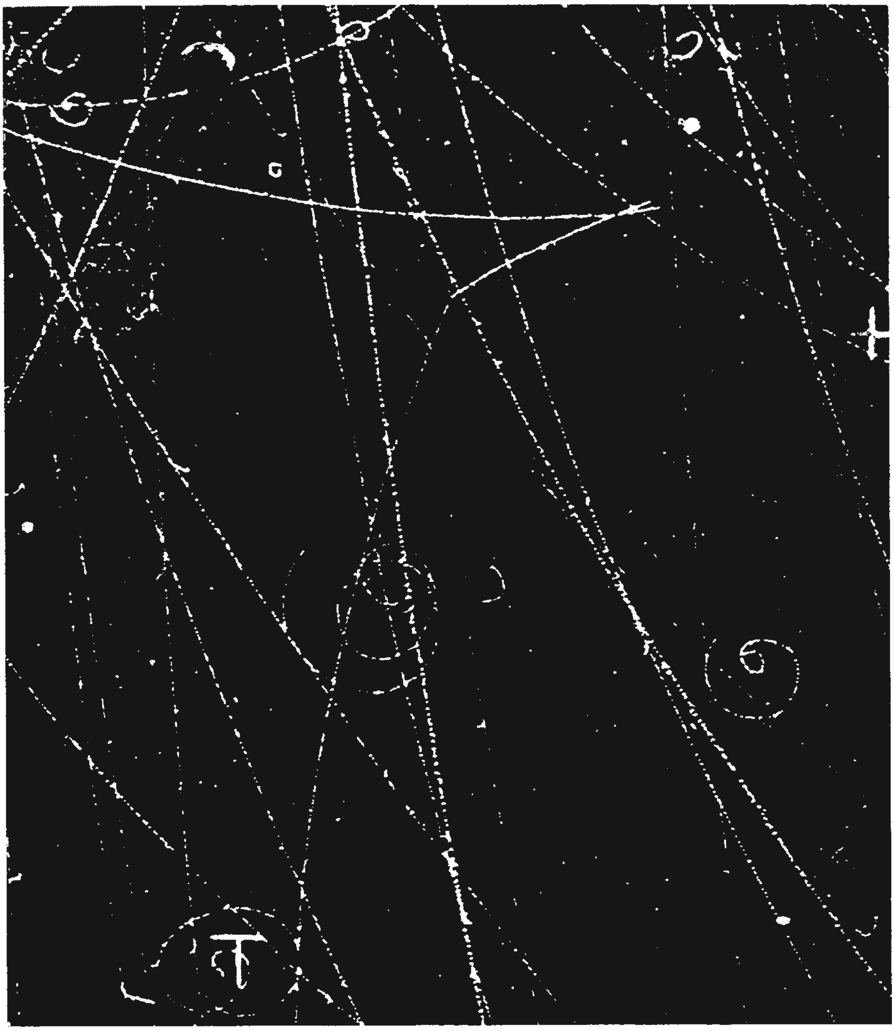

Fig. 10. Muon catalysis (with gap).

the lowest Bohr orbit around a proton. The resulting muonic hydrogen atom, $p\mu^-$, then had many of the properties of a neutron, and could diffuse freely through the liquid hydrogen. When it came close to the deuteron in an HD molecule, the muon would transfer to the deuteron, because the ground state of the μ^-d atom is lower than that of the μ^-p atom, in consequence of « reduced mass » effect. The new « heavy neutron » $d\mu^-$ might then recoil some distance as a result of the exchange reaction, thus explaining the « gap ». The final stage of capture of a proton into a $pd\mu^-$ molecular ion was also energetically favorable, so a proton and deuteron could now be confined close enough together by the heavy negative muon to fuse into a ^{3}He nucleus plus the energy given to the internally converted muon.

We had a short but exhilarating experience when we thought we had solved all of the fuel problems of mankind for the rest of time. A few hasty calculations indicated that in liquid HD a single negative muon would catalyze enough fusion reactions before it decayed to supply the energy to operate an accelerator to produce more muons, with energy left over after making the liquid HD from sea water. While everyone else had been trying to solve this problem by heating hydrogen plasmas to millions of degrees, we had apparently stumbled on the solution, involving very low temperatures instead. But soon, more realistic estimates showed that we were off the mark by several orders of magnitude-a « near miss » in this kind of physics!

Just before we published our results[49], we learned that the « -catalysis » reaction had been proposed in 1947 by Frank[50] as an alternative explanation of what Powell *et* al. had assumed (correctly) to be the decay of π^+ to $^+$. Frank suggested that it might be the reaction we had just seen in liquid hydrogen, starting with a , rather than with an+. Zel'dovitch[51] had extended the ideas of Frank concerning this reaction, but because their papers were not known to anyone in Berkeley, we had a great deal of personal pleasure that we otherwise would have missed.

I will conclude this episode by noting that we immediately increased the deuterium concentration in our liquid hydrogen and observed the expected increase in fusion reaction, and saw two examples of successive catalyses by a single muon (Fig. 11). We also observed the catalysis of $D + D \rightarrow {}^3H + {}^1H$ in pure liquid deuterium.

A few months after we had announced our -catalysis results, the world of particle physics was shaken by the discovery that parity was not conserved in -decay. Madame Wu and her collaborator+, acting on a suggestion by Lee and Yang[53], showed that the p-rays from the decay of oriented ^{60}Co nuclei were emitted preferentially in a direction opposite to that of the spin. Lee and Yang suggested that parity nonconservation might also manifest itself in the weak decay of the Λ hyperon into a proton plus a negative pion. Crawford *et* al. had moved the 10-inch chamber into a negative pion beam, and were analyzing a large sample of Λ's from associated production events. They looked for an « up- down asymmetry » in the emission of pions from Λ's, relative to the « normal to the production plane », as suggested by Lee and Yang. As a

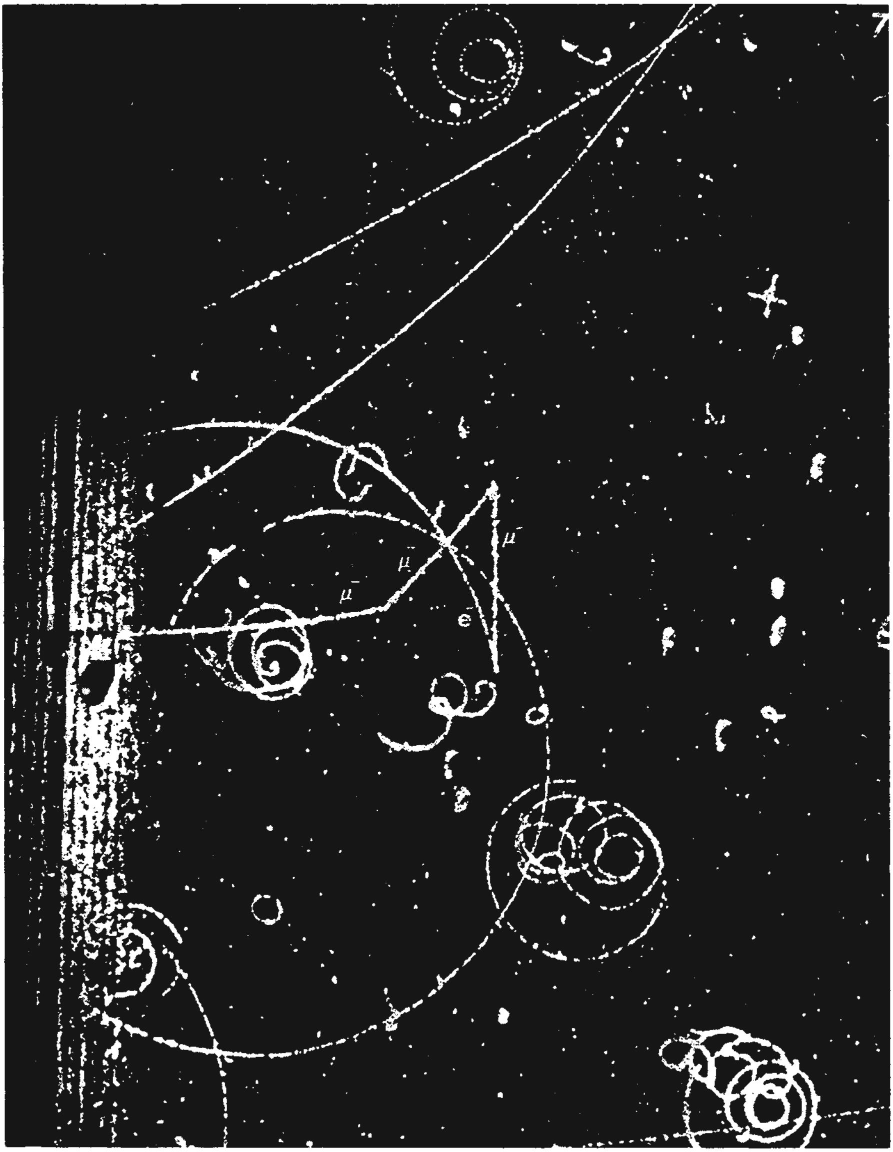

Fig. 11 . Double muon catalysis.

result, they had the pleasure of being the first to observe parity nonconservation in the decay of hyperons[54].

In the winter of 1958, the 15-inch chamber had completed its engineering test run as a prototype for the 72-inch chamber, and was operating for the first time as a physics instrument. Harold Ticho, Bud Good and Philippe a

b

c

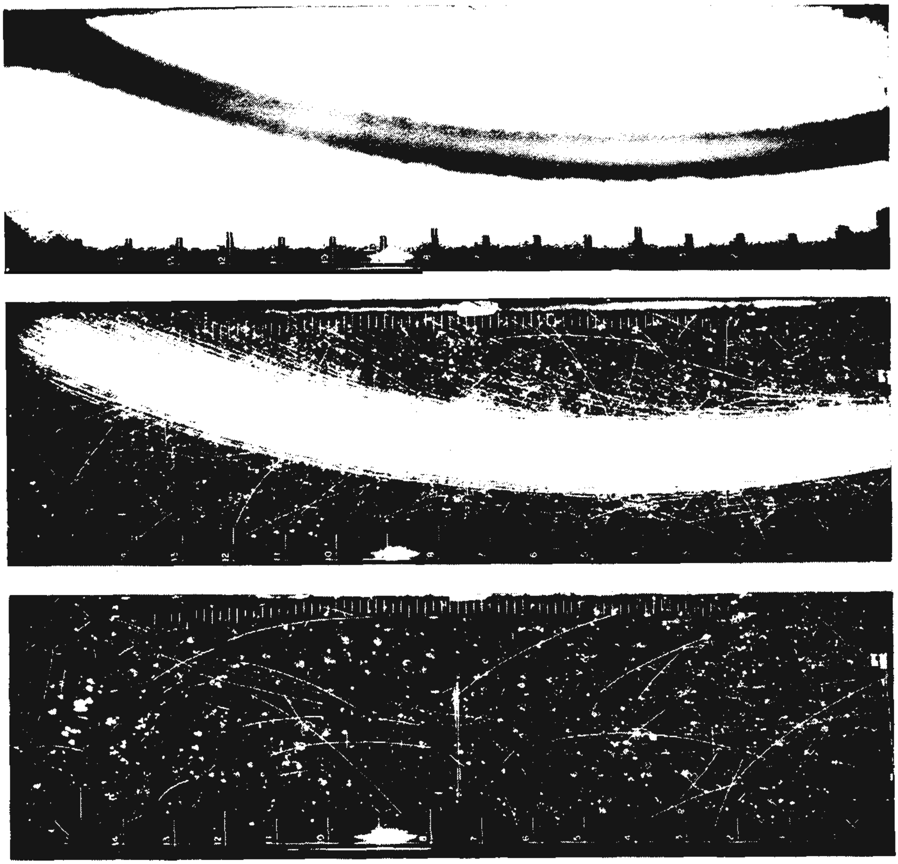

Fig.12. K^- beam in 72-inch bubble chamber. (a) No spectrometers on; (b) one spectrometer on; (c) two spectrometers on.

Eberhard[55] had designed and built the first separated beam of K^- mesons with a momentum of more than 1 GeV / c. Fig.12 shows the appearance of a bubble chamber when such a beam is passed through it, and when one or both of the electrostatic separators are turned off. The ingenuity which has been brought to bear on the problem of beam separation, largely by Ticho and Murray, is difficult to imagine, and its importance to the success of our program cannot be overestimated[55]. Joe Murray has recently joined the Stanford Linear Accelerator Center, where he has in a short period of time built a very successful radiofrequency-separated K beam and a backscattered laser beam.

The first problem we attacked with the 15-inch chamber was that of the Ξ^0. Gell-Mann had predicted that the Ξ^- was one member of an I-spin doublet, with strangeness minus 2. The predicted partner of the Ξ^- would be a neutral hyperon that decayed into a Λ and a π^0- both neutral particles that would, like the Ξ^0, leave no track in the bubble chamber. A few years earlier, as an after-dinner speaker at a physics conference, Victor Weisskopf had « brought down the house » by exhibiting an absolutely blank cloud-chamber photograph, and saying that it represented proof of the decay of a new neutral particle into two other neutral particles! And now we were seriously planning to do what had been considered patently ridiculous only a few years earlier .

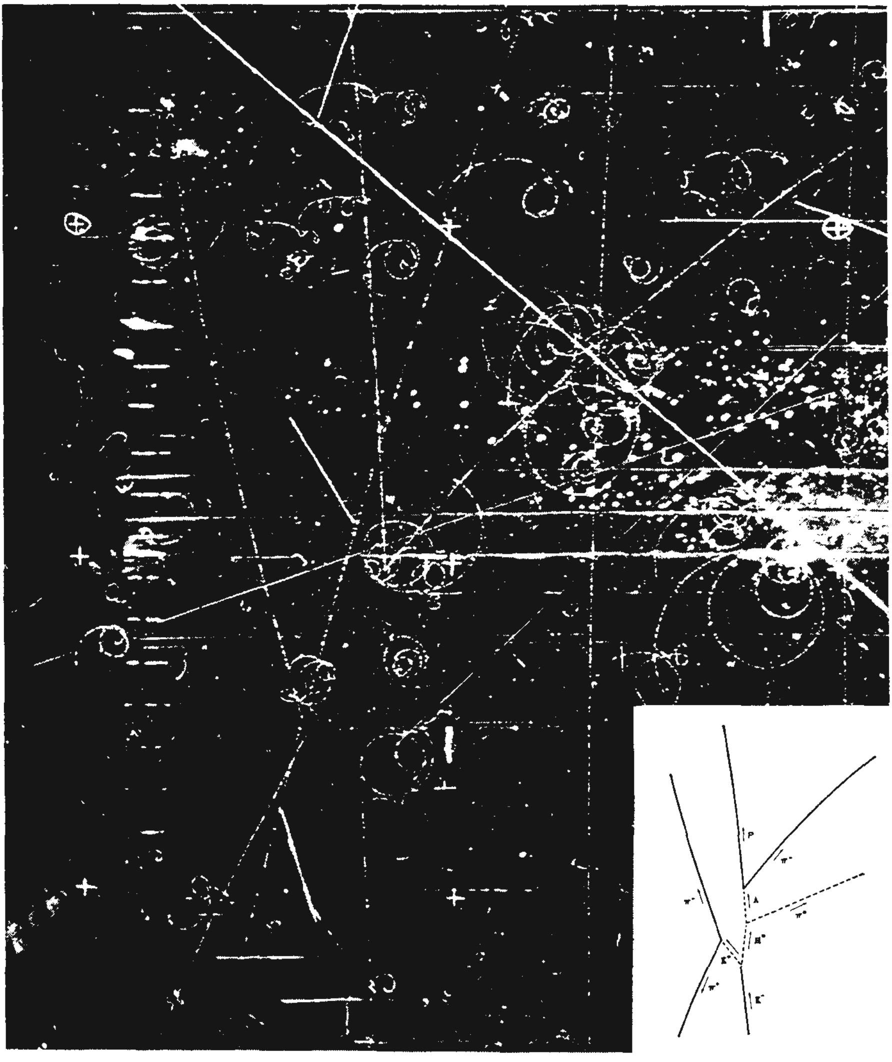

Fig. 13. Production and decay of a neutral cascade hyperon (Xi zero).

According to the Gell-Mann and Nishijima strangeness rules, the Ξ^0 should be seen in the reaction

$$\begin{array}{lllll} K^- + p \rightarrow & \Xi^0 & & + & K^0 \\ & \downarrow & & & \downarrow \\ & \Lambda & + \pi^0 & & \pi^- + \pi^+ \\ & \downarrow & & & \\ & \pi^- + \mathrm{p} & & & \end{array}$$

In the one example of this reaction that we observed, Fig.13, the charged pions from the decay of the neutral K^0 yielded a measurement of the energy and direction of the unobserved K^0. Through the conservation laws of energy and momentum (plus a measurement of the momentum of the interacting K^- track) we could calculate the mass of the coproduced Ξ^0 hyperon plus its velocity and direction of motion. Similarly, measurements of the π^- and proton gave the energy and direction of motion of the unobserved Λ, and proved that it did not come directly from the point at which the K^- meson interacted with the proton. The calculated flight path of the Λ intersected the calculated flight path of the Ξ^0, and the angle of intersection of the two unobserved but calculated tracks gave a confirming measurement of the mass of the Ξ^0 hyperon, and proved that it decayed into a Λ plus a π^0. This single hard-won event was a sort of tour de force that demonstrated clearly the power of the liquid hydrogen bubble chamber plus its associated data-analysis techniques.

Although only one Ξ^0 was observed in the short time the 15 -inch chamber was in the separated K^- beam, large numbers of events showing strange-particle production were available for study. The Franckensteins were kept busy around the clock measuring these events, and those of us who had helped to build and maintain the beam now concentrated our attention on the analysis of these reactions. The most copious of the simple «topologies» was $K^-p \rightarrow$ two charged prongs plus a neutral V-particle. According to the strangeness rules, this topology could represent either

$$\begin{array}{l} K^- + \mathrm{p} \rightarrow \Lambda + \pi^+ + \pi^- \\ \qquad\qquad\quad \downarrow \\ \qquad\qquad \pi^- + p \end{array}$$

or

$$\begin{array}{l} K^- + p \rightarrow \overline{K}^0 + p + \pi^- \\ \qquad\qquad\quad \downarrow \\ \qquad\qquad \pi^- + \pi^+ \end{array}$$

The kinematics program KICK was now available to distinguish between these two reactions, and to eliminate those examples of the same topology in which an unobserved π^0 was produced at the first vertex. SUMX had not yet been written, so the labor of plotting histograms was assumed by the two very able graduate students who had been associated with the K^- beam and its exposure to the 15-inch chamber since its planning stages: Stanley Wojcicki and Bill Graziano. They first concentrated their attention on the energies of the charged pions from the production vertex in the first of the two reactions listed above. Since there were three particles produced at the vertex - a charged pion of each sign plus a Λ -one expected to find the energies of each of the three particles distributed in a smooth and calculable way from a minimum value to a maximum value. The calculated curve is known in particle physics as the « phase-space distribution ». The decay of a τ meson into three charged pions was a well known « three-particle reaction» in which the dictates of phase space were rather precisely followed.

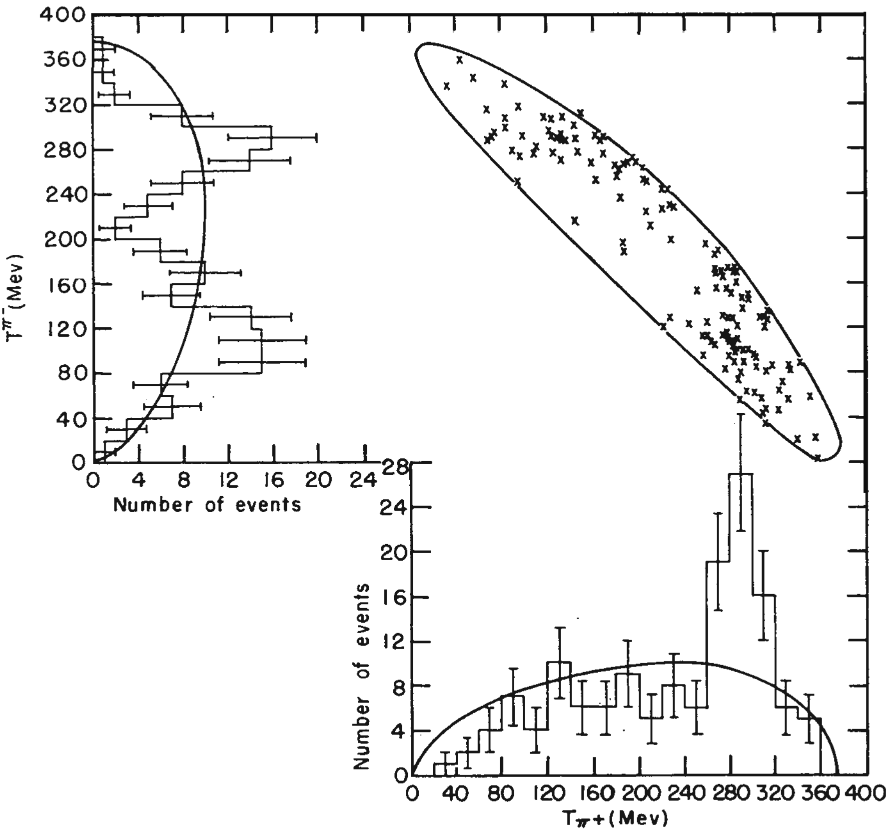

Fig. 14. Discovery of the Y_1^* (1385).

But when Wojcicki and Graziano finished transcribing their data from KICK printout into histograms, they found that phase-space distributions were poor approximations to what they observed. Fig. 14 shows the distribution of energy of both positive and negative mesons, together with the corresponding « Dalitz plot », which Richard Dalitz[56] had originated to elucidate the « τ–θ puzzle », which had in turn led to Lee and Yang's parity-nonconservation hypothesis.

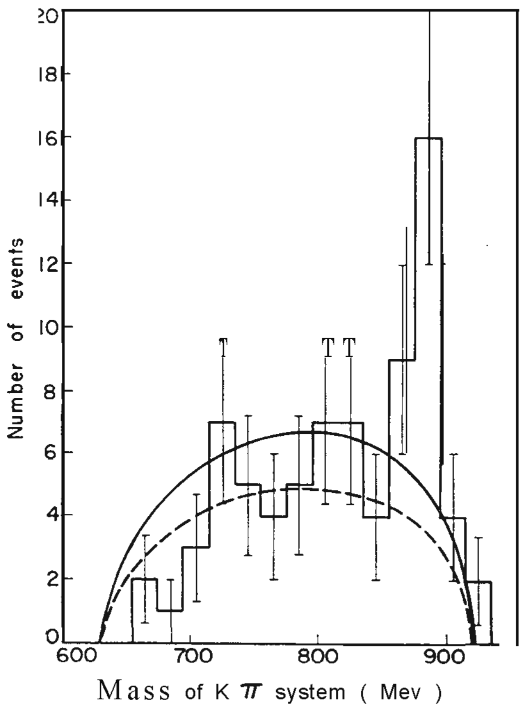

Fig. 15. Discovery of the K^* (890).

The peaked departure from a phase-space distribution had been observed only once before in particle physics, where it had distinguished the reaction $p+p\rightarrow\pi^{+}+d$ from the « three-body reaction » $p+p$--$\pi+$ $+p+n$. (Although no new particles were discovered in these reactions, they did contribute to our knowledge of the spin of the pion[57].) But such a peaking had been observed in the earliest days of experimentation in the artificial disintegration of nuclei, and its explanation was known from that time. Oliphant and Rutherford[58] observed the reaction p + ^{11}B + 3 ^{4}He. This is a three-body reaction, and the energies of the α particles had a phase-space-like distribution except for the fact that there was a sharp spike in the energy distribution at the highest α - particle energy. This was quickly and properly attributed[58] to the reaction

$$p + {}^{11}\mathrm{B} \rightarrow {}^{8}\mathrm{Be} + {}^{4}\mathrm{He}$$
$$\downarrow$$
$${}^{4}\mathrm{He} + {}^{4}\mathrm{He}$$

In other words, some of the reactions proceeded via a two-body reaction, in which one α particle recoiled with unique energy against a quasistable ^{8}Be nucleus. But the ^{8}Be nucleus was itself unstable, coming apart in 10^{-16} sec into two α particles of low relative energy. The proof of the fleeting existence of ^{8}Be was the peak in the high energy α-particle distribution, showing that initially only two particles, ^{8}Be and ^{4}He, participated in the reaction.

The peaks seen in Fig. 14 were thus a proof that the $\pi^{\pm}$ recoiled against a combination of $\Lambda + \pi^{\mp}$ that had a unique mass, broadened by the effects of the uncertainty principle. The mass of the $\Lambda\pi$ combination was easily calculable as 1385 MeV, and the *I*-spin of the system was obviously 1, since the *I*-spin of the Λ is 0, and the *I*-spin of the π is 1. This was then the discovery of the first « strange resonance », the $Y_1^*(1385)$: Although the famous Fermi 3,3-resonance had been known for years, and although other resonances in the π^- nucleon system had since shown up in total cross-section experiments at Brookhaven and Berkeley, CalTech and Cornell[59], the impact of the Y_1^* resonance on the thinking of particle physicists was quite different - *the* Y_1^* *really acted like a new particle,* and not simply as a resonance in a cross section.

We announced the Y_1^* at the 1960 Rochester High Energy Physics Conference[60], and the hunt for more short-lived particles began in earnest. The same team from our bubble- chamber group that had found the $Y_1^*(1385)$ now found two other strange resonances before the end of 1960 - the $K^*(890)$[61], and the $Y_0^*(1405)$[62].

Although the authors of these three papers have for years been referred to as « Alston *et al.* », I think that on this occasion it is proper that the full list be named explicitly. In addition to Margaret Alston (now Margaret Garnjost) and Luis W. Alvarez, and still in alphabetical order, the authors are: Philippe Eberhard, Myron L. Good, William Graziano, Harold K. Ticho, and Stanley G. Wojcicki.

Figs. 15 and 16 show the histograms from the papers announcing these two new particles; the K^* was the first example of a « boson resonance » found by any technique. Instead of plotting these histograms against the energy of one particle, we introduced the now universally accepted technique of plotting

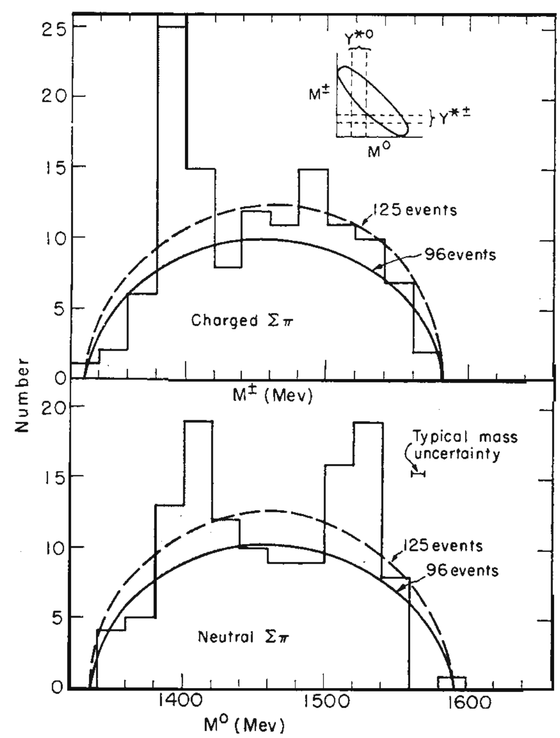

Fig. 16. Discovery of the Y_0^* (1405).

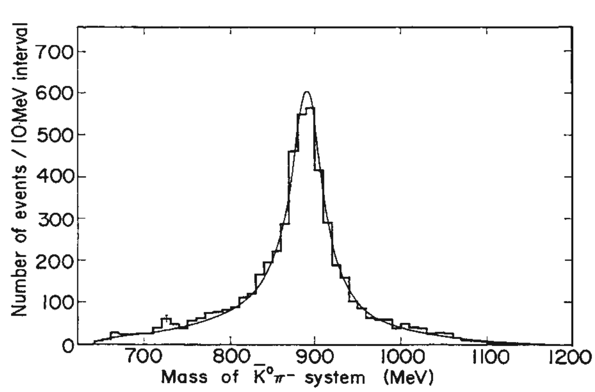

Fig. 17. Present-day K^* *(890).*

them against the effective mass of the composite system: $\Sigma+\pi$ for the Y_0^* (1405) and $K+\pi$ for the K^* (890). Fig. 17 shows the present state of the art relative to the K*(890); there is essentially no phase-space background in this histogram, and the width of the resonance is clearly measurable to give the lifetime of the resonant state via the uncertainty principle.

These three earliest examples of strange-particle resonances all had lifetimes of the order of 10^{-23} sec, so the particles all decayed before they could traverse more than a few nuclear radii. No one had foreseen that the bubble chamber could be used to investigate particles with such short lives; our chambers had been designed to investigate the strange particle swith lifetimes of 10^{-10} sec-10^{13} times as long.

In the summer of 1959, the 72-inch chamber was used in its first planned physics experiment. Lynn Stevenson and Philippe Eberhard designed and constructed a separated beam of about 1.6- GeV/c antiprotons, and a quick scan of the pictures showed the now famous first example of antilambda production, *via* the reaction

$$\begin{array}{ccccc} \bar{p} + p \rightarrow & \Lambda & + & \Lambda & \\ & \downarrow & & \downarrow & \\ & \pi^+ + \bar{p} & & \pi^- + p & \end{array}$$

Fig.18 shows this photograph, with the antiproton from the antilambda decay annihilating in a four-pion event. I believe that everyone who attended the 1959 High Energy Physics Conference in Kiev will remember the showing of this photograph - the first interesting event from the newly operating 72-inch chamber.

Hofstadter's classic experiments on the scattering of high-energy electrons by protons and neutrons[63] showed for the first time how the electric charge was distributed throughout the nucleons. The theoretical interpretation of the experimental results[64] required the existence of two new particles, the vector mesons now known as the ω and the ϱ. The adjective « vector » simply means that these two mesons have one unit of spin, rather than zero, as the ordinary π and K mesons have. The ω was postulated to have I-spin = 0, and the ϱ to have I- spin = 1; the ω would therefore exist only in the neutral state, while the ϱ would occur in the + , - , and 0 charged states.

Many experimentalists, using a number of techniques, set out to find these important particles, whose masses were only roughly predicted. The first success came to Bogdan Maglić, a visitor to our group, who analyzed film from the 72-inch chamber's antiproton exposure. He made the important decision to concentrate his attention on proton-antiproton annihilations into five pions - two negative, two positive, and one neutral. KICK gave him a selected sample of such events; the tracks of the π^0 couldn't be seen, of course, but the constraints of the conservation laws permitted its energy and direction

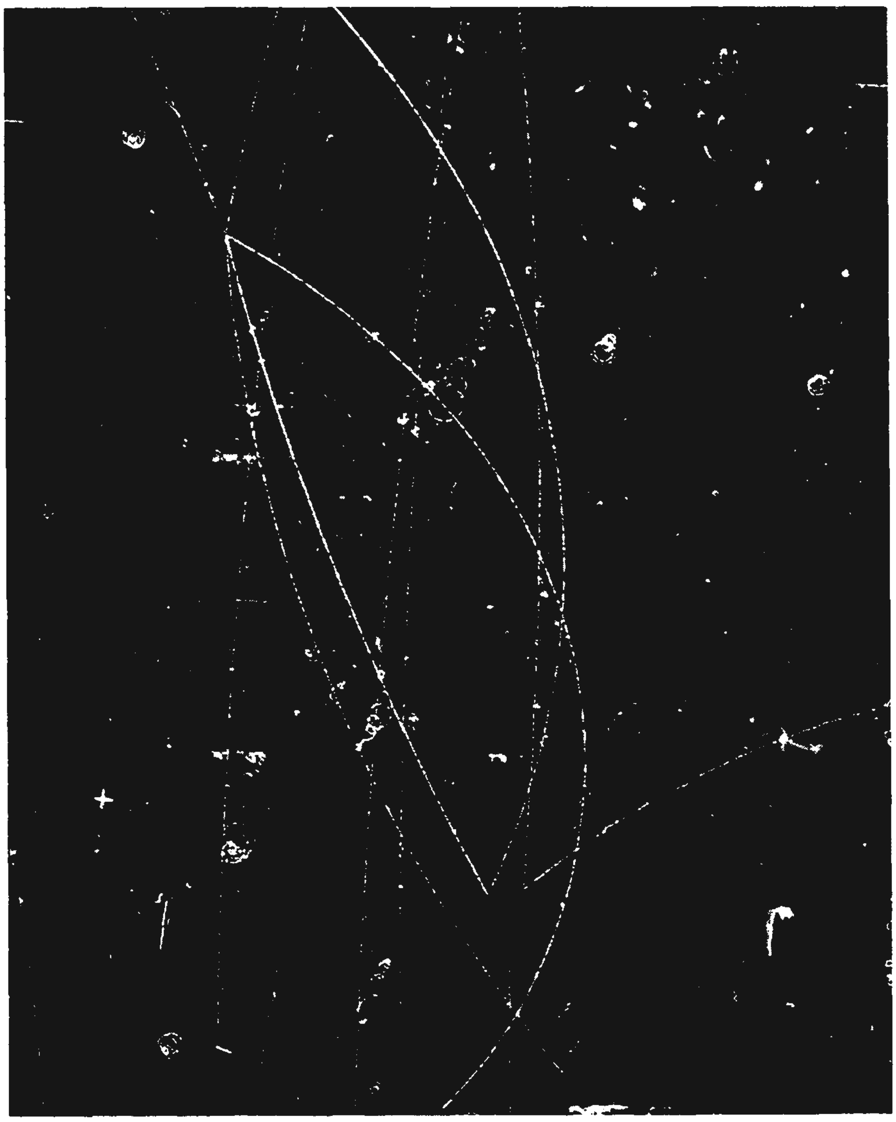

Fig. 18. First production of anti-lambda.

to be computed. Maglid then plotted a histogram of the effective mass of all neutral three-pion combinations. There were four such neutral combinations for each event; the neutral pion was taken each time together with all four possible pairs of oppositely charged pions. SUMX was just beginning to work, and still had bugs in it, so the preparation of the histogram was a very tedious and time-consuming chore, but as it slowly emerged, Maglic had the thrill of seeing a bump appear in the side of his phase-space distribution. Fig.19 shows a small portion of the whole distributions, with the peak that signaled the discovery of the very important ω meson.

Although Bogdan Maglić originated the plan for this search, and pushed through the measurements by himself, he graciously insisted that the paper announcing his discovery[65] should be co-authored by three of us who had developed the chamber, the beam, and the analysis program that made it possible.

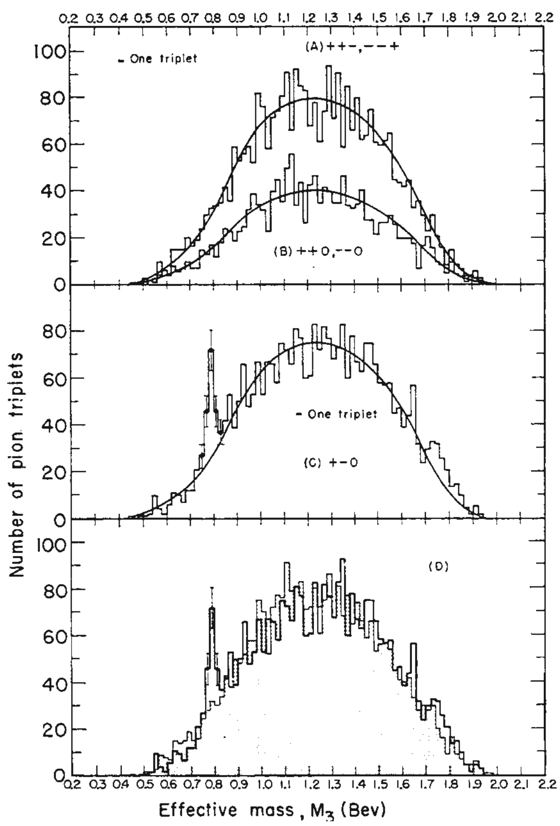

Fig. 19. Discovery of the ω meson.

The ϱ meson is the only one from this exciting period in the development of particle physics whose discovery cannot be assigned uniquely. In our group, the two Franckensteins were being used full time on problems that the senior members felt had higher priority. But a team of junior physicists and graduate students, Anderson *et al.*[66], found that they could make accurate enough measurements directly on the scanning tables to accomplish a « Chew-Low extrapolation ». Chew and Low had described a rather complicated procedure to look for the predicted dipion resonance now known as the ϱ meson. Fig. 20 shows the results of this work, which convinced me that the ϱ existed and had its predicted spin of 1. The mass of the ϱ was given as about 650 MeV, rather than its now accepted value of 765 MeV. (This low value is now explained in terms of the extreme width of the ϱ resonance.) The evidence for the ϱ seemed to me even more convincing than the early evidence Fermi and his co-workers produced in favor of the famous 3,3 pion-nucleon resonance.

But one of the unwritten laws of physics is that one really hasn't made a discovery until he has convinced his peers that he has done so. We had just persuaded high-energy physicists that the way to find new particles was to look for bumps on effective-mass histograms, and some of them were therefore unimpressed by the Chew-Low demonstration of the ϱ. Fortunately, Walker and his collaborators[67] at Wisconsin soon produced an effective-mass ideogram with a convincing bump at 765 MeV, and they are therefore most often listed as the discoverers of the ϱ.

Ernest Lawrence very early established the tradition that his laboratory would share its resources with others outside its walls. He supplied short-lived

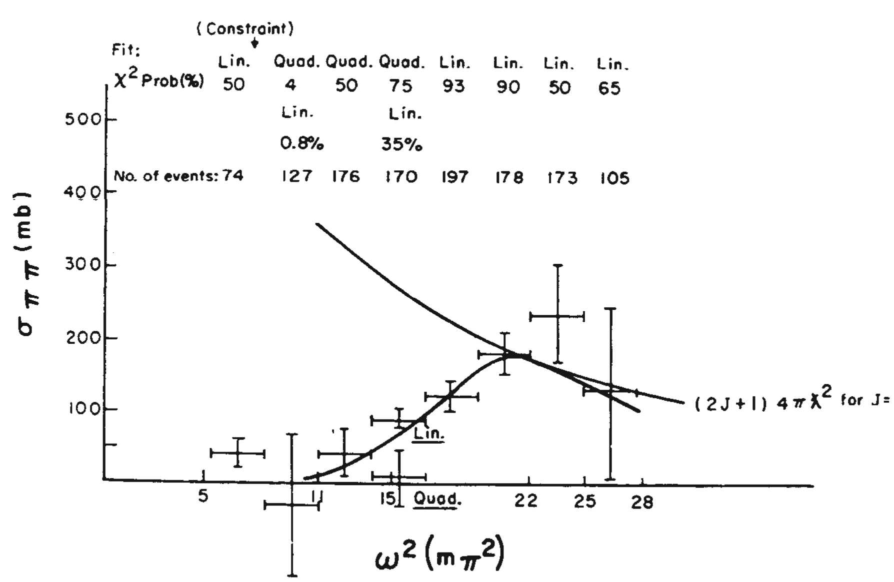

Fig. 20. First evidence for the ϱ meson.

radioactive materials to scientists in all departments at Berkeley, and he sent longer- lived samples to laboratories throughout the world. The first artificially created element, technetium, was found by Perrier and Segrè[68], who did their work in Palermo, Sicily. They analyzed the radioactivity in a molybdenum deflector strip from the Berkeley 28-inch cyclotron that had been bombarded for many months by 6-MeV deuterons.

We followed Ernest Lawrence's example, and thus participated vicariously in a number of important discoveries of new particles. The first was the η found at Johns Hopkins, by a group headed by Aihud Pevsner[69]. They analyzed film from the 72-inch chamber, and found the η with a mass of 550 MeV, decaying into $\pi^+\pi^-\pi^0$. Within a few weeks of the discovery of the η,

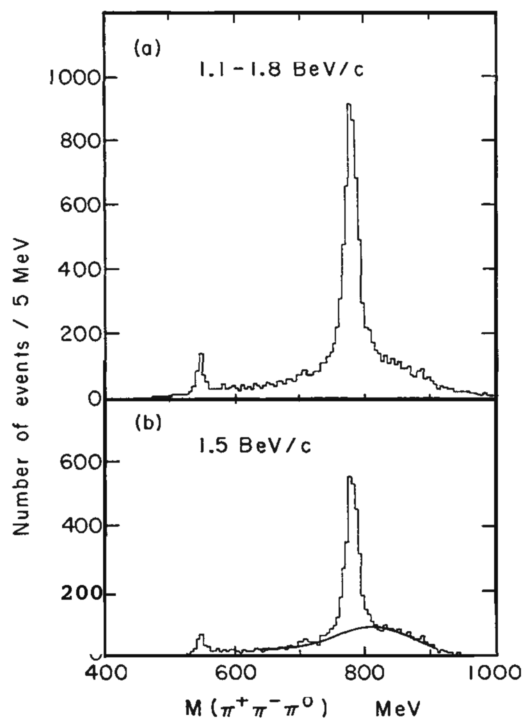

Fig.21. Present-day histogram showing ω and η mesons.

Rosenfeld and his co-workers[70] at Berkeley, who had independently observed the η, showed quite unexpectedly that *I* spin was not conserved in its decay. Fig. 21 shows the present state of the art with respect to the ω and η mesons; the strengths of their signatures in this single histogram is in marked contrast to their first appearances in 72-inch bubble-chamber experiments.

In the short interval of time between the first and second publications on the η, the discovery of the $Y_0^\star$ (1520) was announced by Ferro-Luzzi, Tripp, and Watson[71], using a new and elegant method. Bob Tripp has continued to be a leader in the application of powerful methods of analysis to the study of the new particles.

The discovery of the $\Xi^\star$(1530) hyperon was accomplished in Los Angeles by Ticho and his associates[72], using 72-inch bubble-chamber film. Harold Ticho had spent most of his time in Berkeley for several years, working tirelessly on every phase of our work, and many of his colleagues had helped prepare the high-energy separated K- beam for what came to be known as the K72 experiment. The UCLA group analyzed the two highest-momentum K- exposures in the 72-inch chamber, and found the $\Xi^\star$(1530) just in time to report it at the 1962 High Energy Physics Conference in Geneva. (Confirming evidence for this resonance soon came from Brookhaven[73].)

Murray Gell-Mann had recently enunciated his important ideas concerning the « Eightfold Way »[74], but his paper had not generated the interest it deserved. It was soon learned that Ne'eman had published the same suggestions, independently[75].

The announcement of the $\Xi^\star$ (1530) fitted exactly with their predictions of the mass and other properties of that particle. One of their suggestions was that four I-spin multiplets, all with the same spin and parity, would exist in a « decuplet » with a mass spectrum of « lines » showing an equal spacing. They put the Fermi 3,3-resonance as the lowest mass member, at 1238 MeV. The second member was the $Y_1^\star$ (1385), so the third member should have a mass of (1385) +(1385-1238) = 1532. The strangeness and the multiplicity of each member of the spectrum was predicted to drop 1 unit per member, so the $\Xi^\star$ (1530) fitted their predictions completely. It was then a matter of simple arithmetic to set the mass, the strangeness, and the charge of the final member -the . The realization that there was now a workable theory in particle physics was probably the high point of the 1962 International Conference on High Energy Physics.

Since the second and third members of the series-the ones that permitted the prediction of the properties of the to be made-had come out of our bubble chambers, it was a matter of great disappointment to us that the Bevatron energy was insufficient to permit us to look for the . Its widely acclaimed discovery[76] had to wait almost two years, until the 80-inch chamber at Brookhaven came into operation.

Since the name of the had been picked to indicate that it was the last of the particles, the mention of its discovery is a logical point at which to conclude this lecture. I will do so, but not because the discovery of the signaled the end of what is sometimes called the population explosion in particle physics -the latest list[77] contains between 70 and 100 particle multiplets, depending upon the degree of certainty one demands before « certification ». My reason for stopping at this point is simply that I have discussed most of the particles found by 1962 -the ones that were used by Gell-Mann and Ne'eman to formulate their S U (3) theories-and things became much too involved after that time. So many groups were then in the ((bump-hunting business)) that most discoveries of new resonances were made simultaneously in two or more laboratories.

I am sorry that I have neither the time nor the ability to tell you of the great beauty and the power that has been brought to particle physics by our theoretical friends. But I hope that before long, you will hear it directly from them.

In conclusion, I would like to apologize to those of my colleagues and my friends in other laboratories: whose important work could not be mentioned because of time limitations. By making my published lecture longer than the oral presentation, I have reduced the number of apologies that are necessary, but unfortunately I could not completely eliminate such debts.

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