Pions to Quarks: Particle Physics in the 1950s

Laurie M. Brown, Max Dresden, and Lillian Hoddeson

Citation: Physics Today **41**, 11, 56 (1988); doi: 10.1063/1.881136
View online: https://doi.org/10.1063/1.881136
View Table of Contents: http://physicstoday.scitation.org/toc/pto/41/11
Published by the American Institute of Physics

PIONS TO QUARKS: PARTICLE PHYSICS IN THE 1950s

As high-energy nuclear physics became particle physics, revolutions occurred not only in models and technique but also in social organization.

Laurie M. Brown, Max Dresden and Lillian Hoddeson

The years 1947 to 1963, here freely referred to as "the fifties," form a well-marked period in the history of particle physics, not only in scientific discoveries, but also in technical developments, the scale and cost of projects, and social organization.

It was a time of intellectual turmoil, of shifting attitudes and changing theoretical fashions. The period began with the discovery of charged pions in cosmic rays and ended with the proposal that quarks might be the basic constituents of hadrons. It saw quantum field theory vindicated in renormalized QED, but rejected as a theory of the nuclear interactions. New symmetries were found, while older ones were questioned. New particles appeared that were so different from known ones that they were called "strange," while familiar interactions exhibited unexpected features.

During the same period, cosmic rays gave way to accelerators as the major source of high-energy particles. Accelerator energies increased by an order of magnitude to several GeV, with experiments increasingly using selected secondary beams of particles such as mesons. Detectors were revolutionized, moving from relatively small-scale counter arrays, cloud chambers and nuclear emulsions to large bubble chambers, scintillation counters and spark chambers, accompanied by the development of electronic data analysis.

For science in general, the period also was transitional, as energy was redirected from the problems of war to the concerns of peace. Profiting from their participation in the radar and atomic bomb projects, physicists in the United States planned and carried out cooperative scientific research on a scale hitherto unknown in peacetime, drawing on the prestige they had acquired through their wartime successes to obtain the necessary funding. Elementary-particle physics, then known as "high-energy nuclear physics," served as a prototype for other large-scale scientific projects such as the space programs in the United States and the Soviet Union.

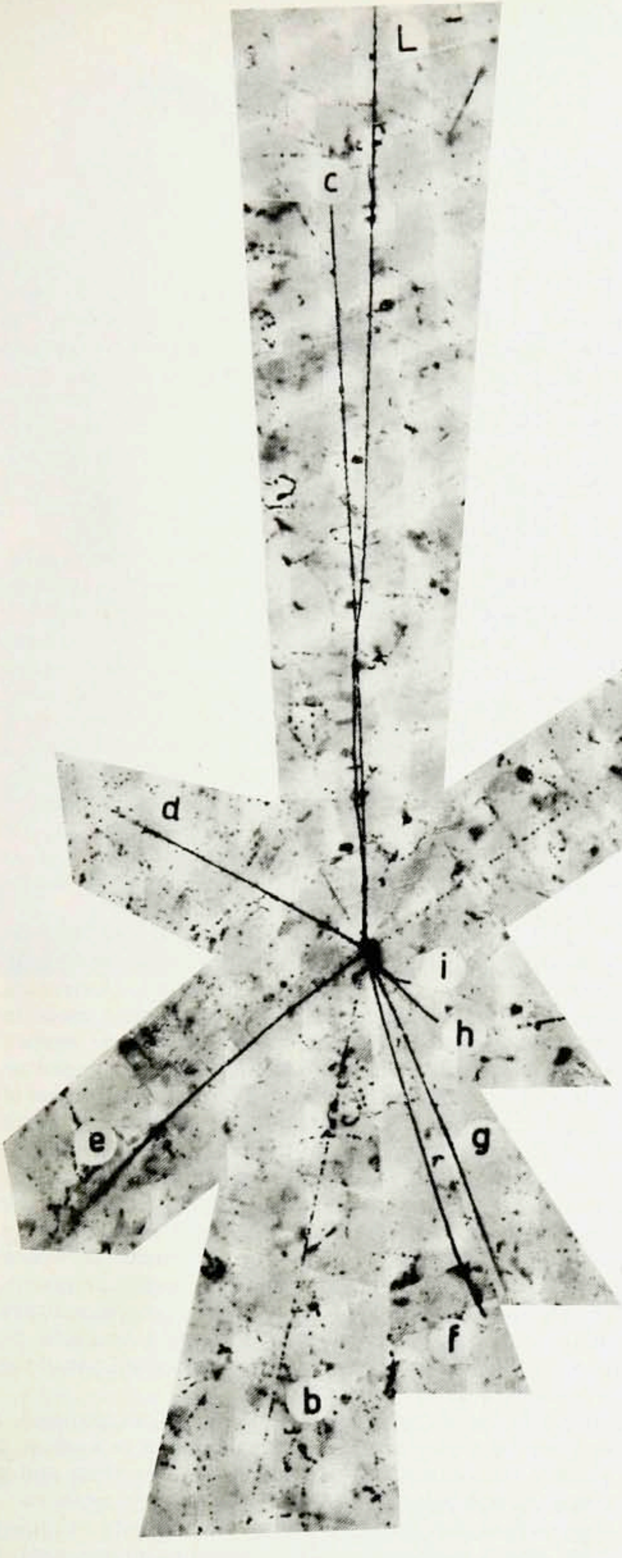
Small experimental groups in particle physics, based almost entirely in university physics departments, were replaced to a large extent by larger groups operating at national laboratories. Accompanying the shift to large national facilities was the formation of new user organizations representing researchers working on experiments away from their home institutions. Specialties formed within this new discipline included accelerator physics, bubble chamber physics and computerized data analysis. The remarkable increase in the scale of the enterprise was accompanied by an expanded bureaucracy and, of course, more politics.

An analysis of how particle physics developed in the fifties promises to give us valuable insight into today's particle physics—and into science in general. We first survey the most significant experimental, theoretical and technological changes that took place during this period of rapid development, and conclude by considering some of the difficult questions that such an analysis might help answer.

Laurie Brown is a professor of physics and astronomy at Northwestern University, in Evanston, Illinois. **Max Dresden** is a professor of physics at the State University of New York at Stony Brook, where he is executive director of the Institute for Theoretical Physics. **Lillian Hoddeson** is a research physicist at the University of Illinois at Urbana-Champaign and the historian of Fermilab, in Batavia, Illinois.

Swords into plowshares

World War II interrupted the careers, absorbed the creative energies and uprooted the lives of many physi-



Antiproton 'star' in emulsion exposed at the Berkeley Bevatron in 1955. This was the first identification of the antiproton in emulsion. L indicates the incoming antiproton track; a and b are pions; c is a proton. The remaining tracks could be protons or α particles. An inch in this magnified view represents about 0.1 mm. (From O. Chamberlain *et al.*, *Phys. Rev.* **101**, 909, 1956.)

cists. While normal scientific communication and research in pure physics almost ceased to exist, there were some notable exceptions, including Werner Heisenberg's S-matrix theory, the Japanese two-meson theory, the measurement of the lifetime of the cosmic-ray meson, experiments in Rome on the capture rates of slow mesons, and the strong-coupling meson theory.[1] Some physicists not engaged in weapons development managed to continue research, among them George D. Rochester and Cecil F. Powell in Britain. Rochester spent his nights at Manchester running a fire brigade, and his days teaching and doing cosmic-ray research with Lajos Jànossy. In 1947 Rochester and Clifford C. Butler discovered new particles that they called V particles because of the V-shaped tracks the particles left upon decaying in a cloud chamber. At Bristol, Powell used photographic emulsions to do nuclear research until in 1945 he was joined by Giuseppe P. S. Occhialini. The latter, an Italian expatriate physicist who had been working in Brazil, soon brought over his coworkers Cesare M. G. Lattes and Ugo Camerini. In 1947 Lattes, Occhialini and Powell discovered the π–μ–e decay chain in cosmic rays, establishing the existence of the charged pion.[2] The first artificially produced pions were found the following year at the Berkeley synchrocyclotron.

Butler and Rochester observed the first V particle in October 1946, in a cloud chamber crossed by a thick lead bar and operated between the poles of an 11-ton electromagnet. It was a neutral particle that decayed into two charged particles, each lighter than a proton, slightly downstream of the lead. The second V particle, which they observed in May 1947, was a charged particle whose track showed a sharp kink and a change of ionization above the lead. Both V particles appeared to have masses about half that of the proton. Curiously, no more V particles were observed for more than two years, a hiatus that Rochester later described as "tantalizing and embarrassing for the Manchester group."[3] A letter from Carl D. Anderson at Caltech to Patrick M. S. Blackett at the University of Manchester, dated 28 November 1949, ended the suspense[3]:

> Rochester and Butler may be glad to hear that we have about 30 cases of forked tracks similar to those they described in their article in *Nature* about two years ago, and so far as we can see now their

A chronology of fundamental particle characteristics

Characteristics	Decade established
Electric charge, mass, spin, magnetic moment	(1920s)
Anomalous magnetic moment	(1930s)
Particle–antiparticle distinction	(1930s)
Lepton–hadron distinction	(1930s)
Meson–baryon distinction	(1930s)
Isospin	(1930s)
Intrinsic parity	(1940s)
Electron–muon universality	(1940s)
Baryon constituents	(1940s)
Associated production and strangeness	(1950s)
Universal Fermi interaction	(1960s)
SU(3) and so on, quarks	(1960s)

interpretation of these events as caused by new unstable particles seems to be borne out by our experiments.

In December 1951 an entire conference in Bristol was devoted to "V-Particles and Heavy Mesons." By that time, good evidence existed for neutral as well as charged V particles, and particles heavier and lighter than the proton were known in both categories. Charged particles had been observed with masses about half that of the proton and with at least two modes of decay: κ mesons decayed into a pion, a muon and a neutrino, and τ mesons decayed into three pions. The Bristol conference introduced a standard nomenclature for the new particles and stimulated collaborative efforts. The unpublished conference proceedings contain some doggerel by Donald H. Perkins of Oxford that concludes:

So counter-control your cloud chamber
And up with emulsions sky high,
We'll find mesons in increasing numbers,
And understand all, by and by.

The last important contributions of cosmic ray physics to particle physics were discussed at two European conferences, which were held in 1953 in Bagnères de Bigorre, France, and in 1955 in Pisa, Italy. What became known as the "θ–τ puzzle" first emerged at these conferences. Thereafter accelerator data came to dominate the field.

Pion–nucleon resonance

Particle physicists tend to forget that their subject was once called high-energy nuclear physics, and that the postwar high-energy accelerators were built to study nuclear forces. Following their discovery in cosmic rays in 1947, charged pions were produced at the only two accelerators capable of achieving the necessary energies—Edwin M. McMillan's electron synchrotron and Ernest O. Lawrence's 184-inch proton synchrocyclotron, both at the Radiation Laboratory at Berkeley. Neutral pions were detected in 1950, first at the synchrocyclotron and later at the electron synchrotron and in high-altitude cosmic rays. Hideki Yukawa's meson was the accepted mediator of charge-independent nuclear forces, so neutral mesons were needed.

Herbert Anderson, who collaborated with Enrico Fermi on the Manhattan Project, has given a historical account of the experiments on pion scattering that were begun in 1951 at the University of Chicago synchrocyclotron.[4] Scattering of mesons in hydrogen qualitatively confirmed some of the main predictions of meson theory: Rising with energy, the scattering cross section rapidly attained its "geometrical" value, its p-wave threshold behavior consistent with the pion's being pseudoscalar. However, the charge ratio was unexpected: The π^+ cross section was larger than the π^-, even though the π^- has more interaction channels—namely, charge exchange and radiative capture, in addition to elastic scattering.

The scattering, as well as anomalous photoproduction cross sections, implied an excited nucleon state or, equivalently, a pion–nucleon resonance. The discovery of resonances ushered in a new era in particle physics.

Laboratory revolution

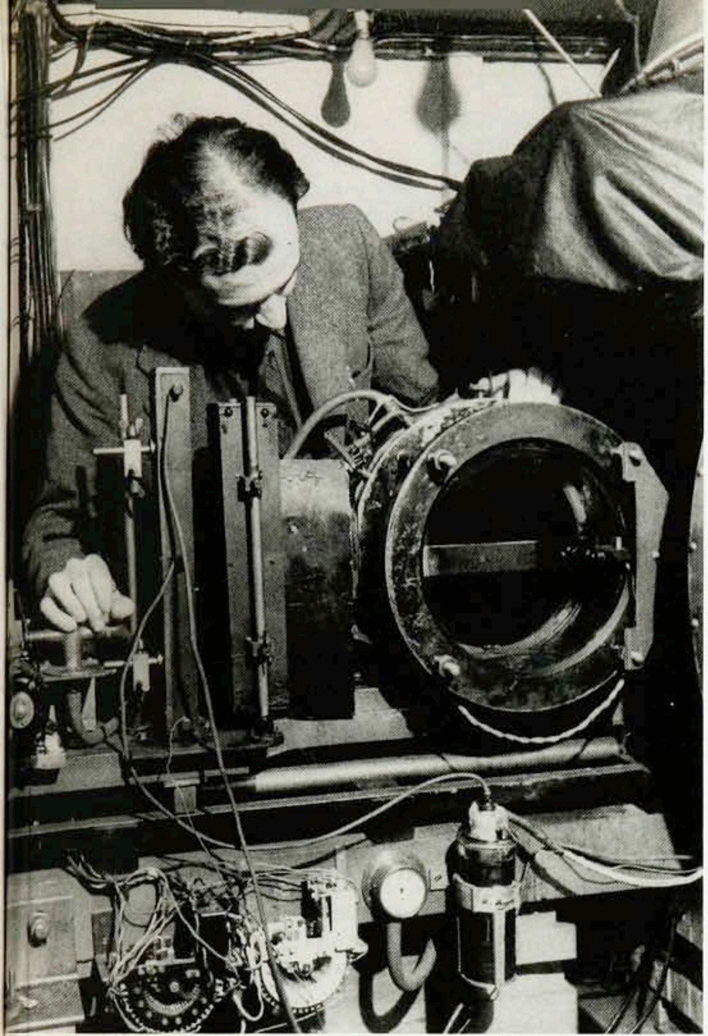
The fifties saw a remarkable change in the particle physicist's laboratory, which received new tools, greatly increased funding and new institutional settings. By 1960 accelerators had replaced cosmic rays as the principal source of high-energy particles. Bubble chambers, spark chambers and scintillation counters replaced cloud chambers and nuclear emulsions as the principal detectors. National funding agencies established in the wake of World War II supported the new technology generously. As a consequence of the increased size and cost of accelerators and detectors, the principal setting for experiments shifted from university laboratories to large facilities based at national or international laboratories and serving "users" from smaller institutions.

This transition relied upon the remarkable achievements of physicists and engineers during World War II. Technical strides in microwave techniques, electronics, vacuum technology, cryogenics and computing, made in programs such as the wartime radar and nuclear weapon projects, were widely exploited in the design and construction of accelerators and detectors. The wartime practice of having industry provide materials for weapons research and development was adapted to postwar research needs.

The US government's willingness to fund postwar science derived from an increased appreciation for the "practical," especially military, value of science, and a bargain was struck, with the government providing research funds to advance the purely scientific programs. The inseparability of nuclear physics from its military implications was underlined in a leading textbook by Milton Stanley Livingston and John P. Blewett, two of the main accelerator architects[5]:

> At the end of World War II, when physicists returned to their laboratories, the enhanced status of nuclear physics was immediately evident. The exciting and dangerous development of atomic energy, with its tremendous implications for national security, stimulated strong popular support for spending government funds on building still larger and higher-energy accelerators. With such impetus the new synchrocyclotrons were rapidly developed.

Technical developments in accelerators in the late 1940s were dominated by the discovery of the phase stability principle in 1944 by Vladimir Veksler in the Soviet Union and independently in 1945 by McMillan in the United States.[6] Application of this principle enabled particles in a circular accelerator to enter the relativistic regime. With the help of funding from the Atomic Energy Commission and the Office of Naval Research, it also made possible the postwar proton synchrocyclotrons, electron



Cloud chamber. Clifford Butler adjusts the cloud chamber used at the University of Manchester in the mid-1940s to study the particles in penetrating showers. The cloud chamber, crossed by a thick lead bar and operated between the poles of an 11-ton electromagnet, was later to play a significant role in determining that V events were decays and not nuclear interactions. (Photograph courtesy of Butler.)

synchrotrons and electron and proton linear accelerators. After much debate, the AEC decided in 1948 to fund both a 6-GeV synchrotron in Berkeley—the Bevatron—and a 3-GeV synchrotron at Brookhaven—the Cosmotron. These two accelerators started a trend in which larger grants for accelerators would be funneled into fewer locations. The Cosmotron achieved its first beam in May 1952; the Bevatron came on line in 1954.

As these synchrotrons were turning on, Ernest D. Courant, Livingston and Hartland Snyder, working at Brookhaven in the summer of 1952, made the major accelerator innovation of the fifties. They invented a new method for focusing particle beams, using an alternating sequence of converging and diverging magnetic lenses, called "alternating gradient" focusing or "strong" focusing.[7] This scheme enabled a reduction in the size of accelerator magnets, greatly cutting the cost of larger machines. (Unbeknownst to the Brookhaven team, this invention had also been made by Nicholas Christofilos in Greece, two years earlier.) Brookhaven then proposed an alternating gradient accelerator with energies in the 30-GeV range, the AGS, while CERN modified the design of its proton synchrotron, with similar energies, to make use of strong focusing. The CERN proton synchrotron came on line at 26 GeV in 1959 and the Brookhaven AGS at 33 GeV in 1960. Meanwhile, Robert R. Wilson built the first strong-focusing machine at Cornell, a 1.3-GeV electron synchrotron, which came on line in 1954.

The future of accelerators seemed firmly based upon strong focusing, thus the American particle physics community was startled to learn that the AEC had authorized the construction at Argonne National Laboratory of a conventional weak-focusing machine, the Zero Gradient Synchroton. This cold-war response to Veksler's announcement in 1955 that a 10-GeV proton synchroton was under construction at Dubna, near Moscow, set up severe tensions in the American accelerator physics community.[8]

Europeans vigorously pursued the building of accelerators in the late 1950s, putting six accelerators with energies between 1 and 30 GeV into operation during the years 1956–59 and having under construction 11 more GeV-range accelerators. The first in the series of International Conferences on High Energy Accelerators was held in 1956 in Geneva. Accelerator development was now an independent profession practiced by a new specialist, the "accelerator physicist."

Detectors. Throughout the boom in accelerator design and construction in the fifties, a revolution was also taking place in the means of detecting high-energy particles. Attempts to extend the usefulness of the cloud chamber led to the high-pressure cloud chamber, which operated at a pressure of 50 atmospheres, and the diffusion cloud chamber, which featured continuous sensitivity. During the second half of the decade the bubble chamber effectively replaced these modified cloud chambers. Invented in 1952 by Donald Glaser, the bubble chamber had a higher density, which enabled far better measurement of particle ranges and allowed operation in a magnetic field to measure particle momentum. By the latter part of the decade, it was developed into a large-scale experimental tool by Luis Alvarez, Jack Steinberger and others. Analyzing the large data sample from a bubble chamber required computers and turned data analysis into an enterprise employing many workers.[9]

Efforts to obtain good spatial resolution with a high triggering rate led to the scintillation counter array, or hodoscope, and later to the spark chamber. The scintillation detector grew out of a series of observations, including Hartmut Kallmann's counting of gamma rays in naphthalene (mothball material) in Germany in the late 1940s and Robert Hofstadter's use of sodium iodide in the United States in 1948. A descendant of the particle counters, the spark chamber was based on wartime advances in electronic timing circuits. The fifties also saw refinement of the nuclear emulsion technique, which gave precise information on mass, energy and the modes of interaction and decay of particles. Emulsion experiments involving cosmic-ray exposures on mountaintops and balloon flights were sometimes large international collaborations.

National and international facilities. New institutions arose with the development of large accelerators and detectors in the fifties—namely, national and international high-energy research laboratories. The Argonne Laboratory, built in March 1943 for scientists working with Fermi at the Chicago Metallurgical Laboratory, entered the high-energy field in 1958, when plans were developed for the 12.5-GeV Zero Gradient Synchrotron. The Manhattan Engineer District established Brookhaven National Laboratory on Long Island in 1946 for peacetime research and as a means for the government to keep control of nuclear reactor technology.[10] Initially Brookhaven was to make available a nuclear reactor for research with neutrons, a 700-MeV synchrocyclotron and a particle accelerator of at least 1 GeV. The last project grew into the Cosmotron. The formation in July 1946 of Associated Universities Incorporated, a group of East Coast universities, to run the Brookhaven laboratory, marked the beginning of consortium management of large national laboratories in the United States. The national laboratory at Stanford was established in the early 1960s, on roots planted in the 1950s. (See Wolfgang K. H. Panofsky's article in PHYSICS TODAY, October 1983, page 34.)

The growth of interuniversity laboratories was slower outside the United States than within, because other countries had to make up for the devastating setbacks of World War II. The principal European accelerator laboratory, CERN, was planned in the early 1950s. The establishment of international laboratories reinforced a new trend toward collaboration as well as competition. By 1957 about 20 laboratories in Europe were engaged in high-energy physics.

Large laboratories brought a new life style to the university-based high-energy experimenter, who now traveled to a large facility and worked there with his students as a "user." At Argonne, and later at other national laboratories, the user community formed organizations to represent their interests, which lobbied to increase the funding for high-energy physics research, thus playing a political as well as a scientific role. As distant users found that the locals had easier access to machine time, a movement began for a "truly national laboratory," or "TNL"—a pun by Leon Lederman on BNL, or Brookhaven National Laboratory. This movement culminated in the creation in the late 1960s of the first National Accelerator Laboratory, which was renamed Fermilab in 1974.[11]

Classifying new particles

In the fifties, the principles used to classify particles assumed great importance. Except for strangeness, these principles had emerged in earlier decades, as the table on page 58 indicates. The idea of a universal weak Fermi interaction surfaced in the late 1940s, but "universality" was a mere speculation prior to the establishment of parity nonconservation in 1957, the V − A interaction in 1958 and the Cabibbo angle in 1963. Finally, the notion that "elementary" particles might be composites is traceable to the 1949 Fermi–Yang model of pions as nucleon–antinucleon bound states, a model that accounted for the pion's triplet isospin and its negative intrinsic parity.[12] The Fermi–Yang model directly inspired the Sakata model of the mid-1950s, and influenced the quark models of the 1960s.

The peculiar property of the V particles—the contrast between their long lifetimes and their relatively copious production—was well established by 1951, although the particles were not yet named "strange." The resolution of this puzzle was not long in appearing. At a symposium held in Tokyo on 7 July 1951 to consider possible explanations for the peculiar behavior of the V particles, several groups proposed theoretical models. As Kazuhiko Nishijima recalled at the Wingspread International Conference in 1984: "These models were all different, but there was one thing in common. They all assumed that the V particles were produced in pairs." Soon afterward, and independently, Abraham Pais also produced a model of that type, which incorporated what he called the "even–odd rule."[13]

The idea that V particles must be produced in pairs was known as "associated production" and was at first difficult to verify. During 1953–54 only nine examples were observed in Ralph Shutt's high-pressure hydrogen-diffusion cloud chamber at the Brookhaven Cosmotron. The liquid hydrogen bubble chamber soon replaced the high-pressure diffusion cloud chamber, and Steinberger's group made a two-day exposure to the Cosmotron π^- beam in 1956 that yielded 55 associated production events. By that time a more detailed description of the production and decay characteristics had been formulated in terms of the concept of "strangeness."

To explain a variety of puzzling experimental results such as the discovery of a long-lived Ξ particle—which decays as $\Xi \to \Lambda + \pi^-$—Murray Gell-Mann and Nishijima were led in 1955 to propose a new quantum number S, called strangeness. This quantum number is conserved additively in strong and electromagnetic interactions. A process that violates this law of conservation is either entirely forbidden or, at most, weak; it may be doubly weak, and so on. For example, the Λ is assigned strangeness -1, so that its decay to p and π^-, which have total strangeness 0, is weak. The Ξ is assigned strangeness -2, so that its decay occurs in *two* stages, each of which involves a change of strangeness of 1. Finally, the process $n + n \to \Lambda + \Lambda$ would also involve a change of strangeness of 2; hence, it would be doubly weak.

However, there is more to the concept of strangeness, for it predicts the electric charges of the members of a given particle multiplet. Both Nishijima and Gell-Mann introduced the quantity S, or more exactly $S/2$, as a displacement in the relationship among the charge Q, the baryon number B and the third component of the isospin vector I_3. The relation that holds for nucleons and pions, $Q = I_3 + B/2$, is modified to read $Q = I_3 + (B + S)/2$. The idea of strangeness is elegant, economical and powerful, and it proved to be a natural and simple way to describe the occurrence of a number of isospin multiplets and to categorize their interactions.

Beginning in 1955 many experiments performed in Berkeley at the 4.5-GeV Bevatron used stacks of nuclear emulsions, electronic counters and, later, hydrogen bubble chambers. These experiments did much to confirm and to fill out the strangeness scheme, finding new strange resonances analogous to the nonstrange pion–nucleon resonances, and also a K–π resonance, called K*. These experiments followed upon another major discovery at the Bevatron: the first observations of the antiproton and the antineutron, which proved the existence of antimatter.

The heavy mesons with mass about 500 MeV/c^2—namely, the K mesons, or kaons—have provided challenging experimental and theoretical problems from the time of their discovery to the present (see PHYSICS TODAY, October, page 17). In the 1950s the two outstanding

7

5) More light is needed to see smaller bubbles.

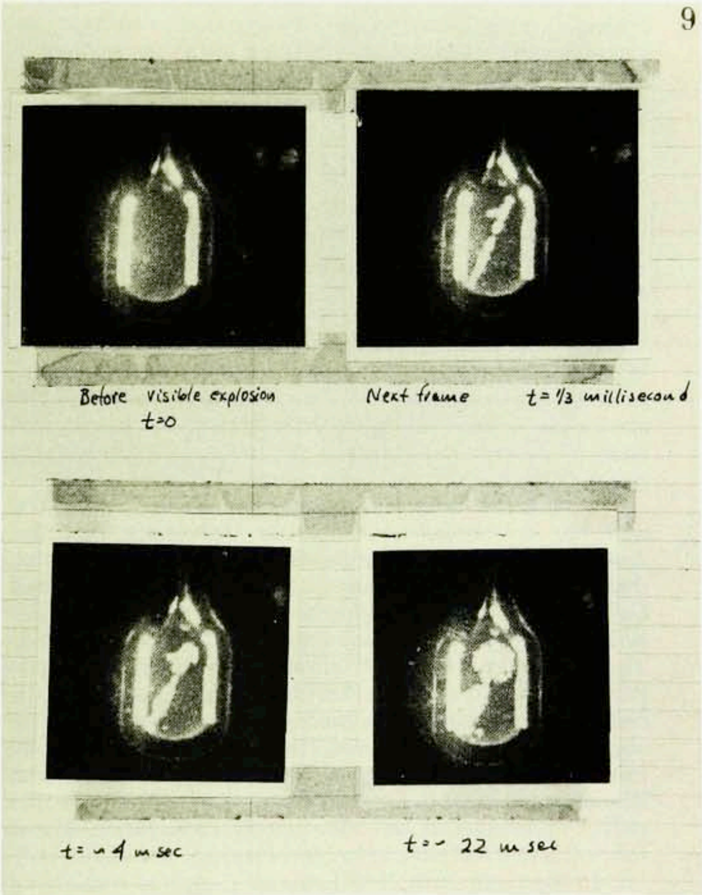
6) Consideration should be given to designing a system in which the low pressure can be maintained for a longer time than in the present system.

Movies Taken October 18, at 8:30 p.m. (Saturday
300 feet of Fastex 8mm super XX shot with two 1000 watt projector lamps for illumination with full-size images of their filaments projected on the bulb. T = 135°C

Results
Out of 8 events three had more than two bubbles and one was a magnificent straight track of around ten globes. It occurred in the 20th foot with the variac at 70v ac — corresponding to about 3000 frames/second. The prints here are about 1½ times the bulb size and show the event developing.

Conclusions
Tracks can be photographed.

Plans
Next One must attempt to observe counter-controlled expansions.



First bubble-chamber tracks. These two pages from Donald Glaser's laboratory notebook show notes and photographs he took on 18 October 1952 at the University of Michigan.

problems posed by K mesons were particle mixing and the θ–τ puzzle. By 1954 charged kaons were known to decay into two pions, three pions, or leptons with or without a pion. For the neutral K meson, only one decay mode was identified unambiguously: $\theta^0 \rightarrow \pi^+ + \pi^-$. Other possible neutral-kaon decay modes, similar to those of the charged K, were also indicated. The two- and three-pion decay modes of both charged and neutral kaons, labeled θ and τ, respectively, became the ingredients of the θ–τ puzzle. The notion of particle mixing concerns all neutral K mesons, independent of their decay modes, but we shall, for simplicity, refer here only to θ and τ mesons.

In an incisive analysis, Pais and Gell-Mann in 1955 investigated the charge conjugation properties of the K system. They suggested that there were two distinct types of neutral K mesons. This ideal led to the surprising result that the *produced* particles K^0 and $\overline{K}^0$ have definite strangeness, while the *decaying* particles K_1 and K_2 have definite eigenvalues of the charge conjugation operator C. Further beautiful consequences, derived by Pais and Oreste Piccioni, involve interference effects arising from the strong interaction with matter of these particle mixtures.[14] Lederman and his group at the Cosmotron set out to look for the long-lived neutral kaon using a 36-inch cloud chamber expanded in a strong magnetic field and exposed to a beam of 1.9-GeV negative pions. They found a particle decaying into three pions with a mean life near 5×10^{-8} sec (the current value), compared with the short-lived neutral kaon's mean life of just under 10^{-10} sec.

The question of the number of K mesons came into focus in 1956 at the sixth Rochester conference. So sharply was the question posed at that meeting that within months the puzzle was solved, with rich consequences for physics, as we will discuss below. J. Robert Oppenheimer opened the session on "Theoretical Interpretations of New Particles" at Rochester, commenting:

> There are the five objects $K_{\pi 3}$, $K_{\pi 2}$, $K_{\mu 2}$, $K_{\mu 3}$, K_{e3}. They have equal, or nearly equal, masses, and identical, or apparently identical, lifetimes. One tries to discover whether in fact one is dealing with five,

four, three, two, or one particle. Difficult problems arise no matter what assumption is made.

The τ mode, or $K_{\pi 3}$, was a special study of Richard H. Dalitz, who analyzed the data concerning it with a method that he invented and began to apply in 1953. By the beginning of 1954 he had found that the quantum numbers 0^- or 1^+ were both compatible with the data, and neither of these was compatible with the spin parity of θ. For two years Dalitz had in effect been telling whoever would listen that either θ and τ are not the same particle or some symmetry property (parity?) was being violated.[15]

The weak interaction

In April 1956, at the Rochester conference, the experimental data on K-meson decays confronted physicists with a puzzling picture: A single particle appeared to decay into two states having opposite parities. In the discussions, Richard Feynman asked a question Martin Block had posed to him: Is it possible that "parity is not conserved"? The conference summary continues: "[Chen Ning] Yang stated that he and [Tsung-Dao] Lee looked into the matter without arriving at any definite conclusions."[15]

Soon afterwards, Lee and Yang examined the experimental evidence for parity conservation. They concluded that there was substantial support for parity conservation in strong and electromagnetic interactions, but little in the case of weak interactions. In May 1956 they came to the startling recognition that none of the β decay experiments carried out up to that time had tested parity inversion invariance! Their analysis showed that no evidence either confirmed or refuted parity invariance in the weak interactions, and they suggested several experiments that could settle the issue.[16]

The suggestion that parity might not be conserved evoked strong negative reactions from physicists who believed strongly in invariance principles. They were not particularly upset by an invented symmetry like isospin not being exact, but they found it hard to accept the violation of what was thought to be an obvious space–time symmetry. Wolfgang Pauli was astounded when he learned that experiments in β decay and μ decay unequivocally showed the violation of parity invariance. What particularly bothered Pauli was that there appeared to be no physical reason why parity was conserved in the strong and electromagnetic interactions, but not in the weak.[17] He considered that to be the central issue, and it has not been satisfactorily resolved to this day.

To test the ideas of Lee and Yang, Chien-Shiung Wu and her colleagues at Columbia University and the National Bureau of Standards measured the angular distribution of electrons emitted in the decay of polarized Co^{60} nuclei, and showed that the electrons are preferentially emitted in a direction opposite to the nuclear spin. The projection of the electron velocity on the nuclear spin is a pseudoscalar quantity; hence the experiment dramatically demonstrated the nonconservation of parity. The observed effect was maximal, that is, as large as possible. Further theoretical analysis, prompted by a letter from Rheinhard Oehme to Lee and Yang, showed that not only parity invariance P but also charge conjugation invariance C had to be violated in the decay.[18]

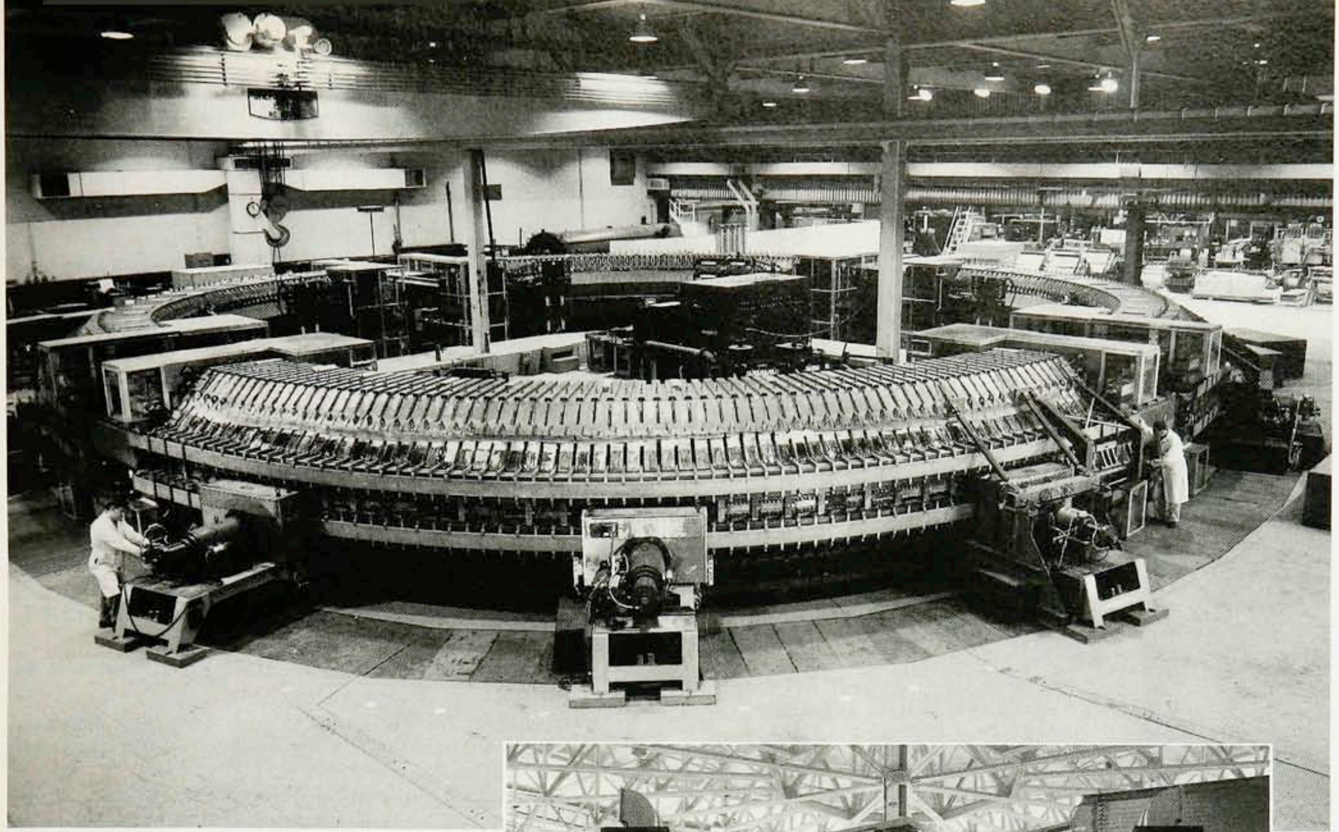
In May and June 1956 Lee and Yang had considered other consequences of the violation of P and C invariance, such as the successive weak interactions in the π–μ–e decay chain. They showed that the μ produced in the decay of a π would be longitudinally polarized, and that the muon's subsequent decay would give its decay electron an asymmetric angular distribution. Richard Garwin, Lederman and Marcel Weinrich set out to test this idea using a stopping beam of positive muons obtained from the decay of pions produced by the Columbia University cyclotron at Nevis, New York. They found a large electron angular asymmetry, showing that the muons were strongly polarized, and establishing the nonconservation of parity beyond a doubt.

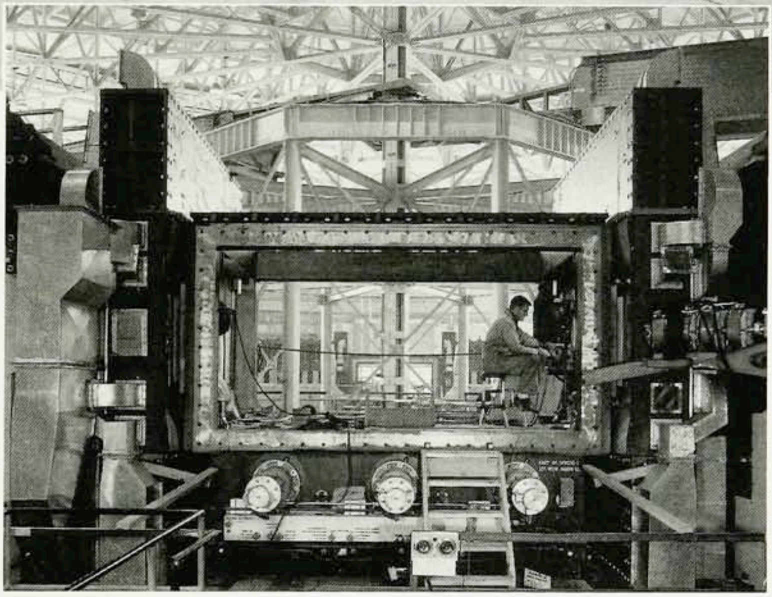
In the summer of 1956 Valentine Telegdi at the University of Chicago, stimulated by the work of Lee and Yang, decided to study in a nuclear emulsion the same π–μ–e decay chain. As in the Garwin experiment, the main objective was to detect the polarization of the muon. However, in an emulsion a slow positive muon can easily pick up an electron and form muonium, an exotic atom analogous to hydrogen. Because of the large magnetic moment of the electron, this atom can precess in a magnetic field such as the fringing field of the cyclotron in which the exposure was made, so careful magnetic shielding was done in Telegdi's experiment. The time required to do this, and the relatively slow pace of data analysis using nuclear emulsions, delayed publication of the Chicago results. Wu's and Garwin's experiments were published in the same issue of *Physical Review Letters*; Telegdi's was published a little later.[19] While all three experiments showed P and C to be violated, the results were consistent with CP conservation.

This last property, CP invariance, or "combined inversion" invariance, holds to good accuracy, but James Cronin and Val Fitch in 1964 found it to be violated in certain rare processes.[20] Before that, however, Lee and Yang, Abdus Salam and Lev Landau had independently observed that a two-component version of neutrino theory would allow a natural formulation of a CP-conserving but P- and C-violating weak interaction.[21] Because such a neutrino is massless, its spin inevitably has a "handedness." It was found that neutrinos spin in a left-handed sense relative to their direction of flight, while antineutrinos are right-handed. To restore the symmetry in passing from particle to antiparticle—charge conjugation C—one must also change the handedness, or chirality, by a parity transformation P. Maurice Goldhaber performed an experiment that demonstrated the left-handedness of the β decay neutrino.

The correct form of the four-fermion weak interaction was eventually established by experiment to be a mixture of the "vector" (V) and "axial vector" (A) forms of interaction. Before that, however, several theoretical groups had speculated,[22] on grounds of symmetry, that the interaction should be the difference of the two, V − A. The V − A theory yields a two-component theory of left-handed neutrinos and is CP invariant. The version of Feynman and Gell-Mann went considerably further, postulating that the weak interaction has a current–current form (analogous to the electromagnetic interaction of two fundamental charges), with the V current being conserved (as is the electromagnetic current vector) and the A current being nearly conserved, a suggestion first put forward[23] in 1955 by S. S. Gerstein and Yakov B. Zel'dovich. However, the decay of strange particles required substantial modification of the form of the universal weak interaction theory.

The 1950s saw the actual detection of the neutrino as a particle. After years of effort, Frederick Reines and Clyde L. Cowan Jr cabled Pauli in 1956 that the particle he had suggested in 1930 had at last been detected. An equally trailblazing experiment at Brookhaven by a Columbia University group found that the neutrino associated with the muon, as in π–μ decay, is different from that of β decay. The two-neutrino experiment involved the first large-scale use of spark chambers and demonstrated the feasibility of experiments with neutrino beams.[24]





Cosmotron and Bevatron. After much debate, the Atomic Energy Commission decided in 1948 to fund both a 3-GeV synchroton at Brookhaven—the Cosmotron—and a 6-GeV synchrotron in Berkeley—the Bevatron (vacuum tank shown in inset). (Photographs courtesy of Brookhaven National Laboratory and Lawrence Berkeley Laboratory.)

The discoveries in the weak interactions in the 1950s confirmed the importance of these interactions and demonstrated again their ability to surprise. In themselves, these findings constitute a revolution in the science of elementary particles. Revolutions in science used to be paced by the centuries. Now they seem to occur every decade, often in the most unexpected places.

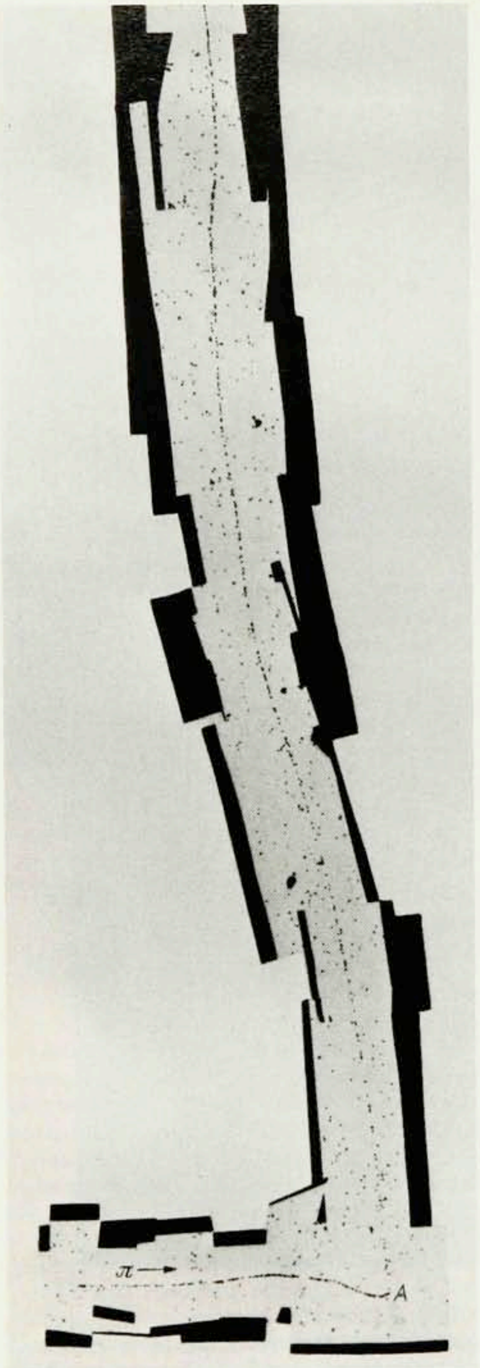
General issues

The relation between theory and experiment has changed radically since the fifties. In this period experiment led theory and produced major surprises to which theory had to respond. Nowadays, by contrast, with the advent of the electroweak theory and the subsequent discovery of the predicted W and Z particles, theory tends to dominate, even though it is unable to give a detailed explanation of a vast accumulation of "low energy" data (such as the classical pion experiments). This may be one reason why theory is becoming more speculative as theorists search for a grander synthesis.

The experience of the fifties also raises an important historical question: When experimentation dominates, does the available technique—especially equipment, and hence funding—play a larger-than-usual role in the scientific process? Does this influence extend so far that it affects the content of scientific theories?

Most physicists reject the notion that scientific objectivity could be threatened by that kind of external influence. But the charge was made at a recent symposium on the history of particle physics held at Fermilab, that such influence was effective in the fifties. It was asked, for example, whether the reason theorists in the fifties turned to methods such as phase-shift analysis and S-matrix methods, and in the 1960s to Regge pole phenomenology, neglecting fundamental field theory, was because expensive experiments were turning out huge quantities of data that demanded immediate analysis and scientific justification.

Some strong-willed scientists insist that science is not in essence social, and that there is no philosophically valid way to speak of the development of scientific ideas *per se*, but only of the development of the ideas of individual



Pi–mu decay. This track in emulsion, made in 1947 at Pic-du-Midi, France, was the first complete recording of such an event. (From reference 2.)

scientists. We do not agree with this position. The precise relationship between scientific ideas and social demands is delicate, but even the most individualistic of scientists reacts to other individuals and to scientific surroundings, which is clearly a social process.

* * *

This article is based on the introductory essay to the proceedings of a May 1985 international symposium on the history of particle physics in the 1950s (to be published by Cambridge U. P., New York). The three-day symposium was held at Fermilab with the support of the Sloan Foundation, the Argonne Universities Trust Fund and the National Science Foundation. The introductory essay draws on papers that physicists and historians of science contributed to the symposium. We have used ideas and analyses presented by the historians, and would like to acknowledge the following individuals: Allan Franklin, Peter Galison, John L. Heilbron, Armin Hermann, Abraham Pais, Andrew Pickering, Helmut Rechenberg, Sylvan S. Schweber, Robert Seidel and D. Hywel White.

References

1. L. M. Brown, L. Hoddeson, eds., *The Birth of Particle Physics*, Cambridge U. P., New York (1983).
2. C. M. G. Lattes, H. Muirhead, G. P. S. Occhialini, C. F. Powell, Nature **159**, 694 (1947).
3. G. D. Rochester, in *Early History of Cosmic Ray Studies*, Y. Sekido, H. Elliot, eds., Reidel, Dordrecht, The Netherlands (1985), p. 299. See also C. Peyrou, J. Phys. (Paris) **43**, suppl. to no. 12, 7 (1982).
4. H. L. Anderson, J. de Phys. (Paris) **43**, suppl. to no. 12, 101 (1982). H. L. Anderson, Rev. Mod. Phys. **27**, 269 (1955).
5. M. S. Livingston, J. P. Blewett, *Particle Accelerators*, McGraw–Hill, New York (1962), p. 3.
6. E. M. McMillan, Phys. Rev. **68**, 143 (1945). V. Veksler, Dokl. Akad. Nauk SSSR **43**, 329 (1944; english translation in C. R. Acad. Sci. USSR **44**, 393, 1944).
7. E. D. Courant, M. S. Livingston, H. Snyder, Phys. Rev. **88**, 1190 (1952).
8. D. Greenberg, *The Politics of Pure Science*, New American Library, New York (1967). L. Greenbaum, *A Special Interest*, U. of Michigan P., Ann Arbor (1971).
9. L. W. Alvarez, Science **165**, 1071 (1969). J. Heilbron, R. W. Seidel, B. W. Wheaton, *Lawrence and His Laboratory: Nuclear Science at Berkeley, 1931–1961*, Office for History of Science and Technology, U. of Calif., Berkeley (1982). J. Krige, D. Pestre, Hist. Studies Phys. Sci. **16**, 255 (1986).
10. A. Needell, Hist. Studies Phys. Sci. **14**, 93 (1983).
11. L. Hoddeson, Social Studies Sci. **13**, 1 (1983).
12. E. Fermi, C. N. Yang, Phys. Rev. **76**, 1739 (1949). S. Sakata, Prog. Theor. Phys. **16**, 686 (1956).
13. Y. Nambu, K. Nishijima, Y. Yamaguchi, Prog. Theor. Phys. **6**, 615, 619 (1951). K. Aizu, T. Kinoshita, Prog. Theor. Phys. **6**, 630 (1951). H. Miyazawa, Prog. Theor. Phys. **6**, 631 (1951). S. Oneda, Prog. Theor. Phys. **6**, 633 (1951). A. Pais, Phys. Rev. **86**, 663 (1952).
14. M. Gell-Mann, A. Pais, Phys. Rev. **97**, 1387 (1955). A. Pais, O. Piccioni, Phys. Rev. **100**, 1487 (1955). R. H. Good, R. P. Matsen, F. Muller, O. Piccioni, W. M. Powell, H. S. White, W. B. Fowler, R. W. Birge, Phys. Rev. **124**, 1223 (1961).
15. J. Ballam, V.L. Fitch, T. Fulton, K. Huang, R. R. Rau, S. B. Treiman, eds., *High Energy Nuclear Physics: Proceedings of the Sixth Annual Rochester Conference*, Interscience, New York (1956), section VIII.
16. T.-D. Lee, C. N. Yang, Phys. Rev. **104**, 254 (1956).
17. W. Pauli, *Collected Scientific Papers*, vol. 1, R. Kronig, V. F. Weisskopf, eds., Wiley Interscience, New York (1964), p. 13.
18. T.-D. Lee, R. Oehme, C. N. Yang, Phys. Rev. **106**, 340 (1957).
19. C.-S. Wu, E. Ambler, R. W. Hayward, D. D. Hoppes, R. P. Hudson, Phys. Rev. **105**, 1413 (1957). R. Garwin, L. M. Lederman, M. Weinrich, Phys. Rev. **105**, 1415 (1957). J. I. Friedman, V. L. Telegdi, Phys. Rev. **105**, 1681 (1957).
20. J. H. Christenson, J. W. Cronin, V. L. Fitch, R. Turlay, Phys. Rev. Lett. **13**, 138 (1964).
21. T.-D. Lee, C. N. Yang, Phys. Rev. **105**, 1671 (1957). A. Salam, Nuovo Cimento **5**, 299 (1957). L. D. Landau, Nucl. Phys. **3**, 127 (1957). R. Gatto, Phys. Rev. **106**, 168 (1957).
22. R. Marshak, E. C. G. Sudarshan, Phys. Rev. **109**, 1860 (1958). R. P. Feynman, M. Gell-Mann, Phys. Rev. **109**, 193 (1958). J. Sakurai, Nuovo Cimento **7**, 649 (1958).
23. S. S. Gerstein, Ya. B. Zel'dovich, Sov. Phys. JETP **2**, 576 (1956).
24. F. Reines, C. L. Cowan, Phys. Rev. **92**, 830 (1953). C. L. Cowan, F. Reines, F. B. Harrison, H. W. Kruso, A. D. McGuire, Science **124**, 103 (1956). G. Danby, J.-M. Gaillard, K. Goulianos, L. M. Lederman, N. Mistry, M. Schwartz, J. Steinberger, Phys. Rev. Lett. **9**, 36 (1962). ■