Elementary particle physics: The origins

Val L. Fitch

Physics Department, Joseph Henry Laboratories, Princeton University, Princeton, New Jersey 08540

[S0034-6861(99)02502-7]

CONTENTS

With the standard model summarizing everything that has been learned about elementary particles in the past 50 to 60 years, it is perhaps difficult to remember that physics remains a subject that has its foundations in experiment. Not only is it because particle physics can be conveniently encapsulated in a theoretical model that we fail to remember, but it is also true that most physics textbooks devoted to the subject and popular accounts are written by theorists and are colored with their particular point of view. From a plethora of texts and memoirs I can point to relatively few written by experimental physicists. Immediately coming to mind is Perkins' *Introduction to High Energy Physics* and the fascinating memoir of Otto Frisch, "What Little I Remember." Bruno Rossi contributed both texts and a lively memoir. We can also point to Alvarez, Segrè, and Lederman. But still this genre by experimentalists is relatively rare. One can speculate why this is the case—that theorists are naturally more contemplative, that experimentalists are people of action (they have to be—the vacuum system always has a leak, there is always an excess of noise and cross-talk in the electronics, there is always something to be fixed).

In the late 1940s when it became clear that the muon was nothing more than a heavy brother of the electron with no obvious role in the scheme of things, Rabi made his oft-quoted remark, "Who ordered that?" In time, he also could have questioned who ordered strange particles, the tau-theta puzzle, *CP* violation, the avalanche of hadron and meson resonances and the tau lepton. Initially, these discoveries appeared on the scene unwanted, unloved, and with deep suspicion. Now they are all incorporated in the standard model.

It is probably with this in mind that the editor of this volume has asked me to write about the history of particle physics from the point of view of an experimentalist. In the limited space available I have decided to restrict myself to the early days when a large fraction of the new particles were discovered in cosmic rays, starting with Anderson's positron. Those who became interested in cosmic rays tended to be rugged individualists, to be iconoclastic, and to march to the drummer in their own heads rather than some distant one. After all, this was the period when nuclear physics was coming into its own, it was the fashionable subject, it was the subject that had the attention of the theorists, it was the subject for which great accelerators were being built. The cosmic-ray explorers eschewed all that and found their satisfactions in what might be called the backwater of the time.

I. THE MISTS OF SCOTLAND

Just as modern biology was launched with the invention of the microscope, in physics, too, areas for investigation have been opened with the development of new observational tools. The Wilson cloud chamber is one of these. What would inspire anyone to want to study the behavior of water vapor under unusual conditions? In his Nobel lecture Wilson (1927) answers the question. His curiosity about the condensation of water droplets in moist air was piqued through having watched and wondered about the "wonderful optical phenomena shown when the sun shone on the clouds" that engulfed his Scottish hilltops.

II. "COSMIC RAYS GO DOWNWARD, DR. ANDERSON"

The discovery of tracks in a cloud chamber associated with cosmic rays was made by Skobelzyn (1929) in the Soviet Union. Almost immediately Auger in France and Anderson and Milliken in the U.S. took up the technique (see Auger and Skobelzyn, 1929). Using electroscopes and ion chambers, Milliken and his students had already resolved a number of important questions about cosmic rays, e.g., that their origin was in the heavens and not terrestrial. Milliken was a forceful person, a skillful popularizer, and an excellent lecturer. He had a knack for memorable phrases. It was Milliken who had coined the name "cosmic rays." Referring to his pet theory on their origin, he called them the "birth cries" of the atoms. Carl Anderson had been a graduate student of Milliken's, and Milliken insisted that he remain at Caltech to build a cloud chamber for studying this new corpuscular radiation from space. As President of Caltech, Milliken was in an excellent position to supply Anderson with the resources required to design and construct a chamber to be operated in a high magnetic field, 17 000 gauss. The chamber was brought into operation in 1932, and in a short time Anderson had many photographs

 0034-6861/99/71(2)/25(8)/$16.60

showing positive and negative particles. Blind to the fact that the positives had, in general, an ionization density similar to the negative (electron) tracks, Milliken insisted that the positive particles must be protons. Anderson was troubled by the thought that the positives might be electrons moving upwards but Milliken was adamant. ‘‘Cosmic rays come down!’’ he said, ‘‘they are protons.’’ Anderson placed a 0.6-cm lead plate across the middle of the chamber. Almost at once he observed a particle moving upward and certainly losing energy as it passed through the plate; its momentum before entering the plate was 63 MeV/c and 23 MeV/c on exiting. It had to be a positive electron. And irony of ironies, with the history of Milliken's insistence that ‘‘cosmic rays go downwards,’’ this first example of a positron was moving upwards.[1]

III. ON MAKING A PARTICLE TAKE A PHOTOGRAPH OF ITSELF

Shortly afterward, in England, a stunning improvement in the use of cloud chambers led to a whole array of new discoveries. This was the development of the counter-controlled cloud chamber.

Bruno Rossi, working in Florence, had considerably refined the coincidence counter technique initiated by Bothe in Berlin, and he had launched an experimental program studying cosmic rays. In Italy, no one had yet operated a cloud chamber and Rossi was anxious to introduce the technique. Accordingly, he arranged for a young assistant, Giuseppe Occhialini, to go to England to work with Patrick Blackett. Blackett had already become widely known for his cloud-chamber work studying nuclear reactions (Lovell, 1975).

As they say, the collaboration of Blackett and Occhialini was a marriage made in heaven. Both men were consummate experimentalists. Both took enormous pleasure in working with their hands, as well as their heads. They both derived much satisfaction in creating experimental gear from scratch and making it work as planned. In Solley (Lord) Zuckerman's collection (1992) of biographical profiles, *Six Men Out of the Ordinary*,[2] Blackett is described as ‘‘having a remarkable facility of thinking most deeply when working with his hands.’’ Occhialini has been described as a man with a vivid imagination and a tempestuous enthusiasm: a renaissance man with a great interest in mountaineering, art, and literature as well as physics.

Occhialini arrived in England expecting to stay three months. He remained three years. It was he who knew about the Rossi coincidence circuits and the (then) black art needed to make successful Geiger counters. It was Blackett who must have known that the ion trails left behind by particles traversing a cloud chamber would remain in place the 10 to 100 milliseconds it took to expand the chamber after receipt of a pulse from the coincidence circuit.

In Blackett's own words (1948), ‘‘Occhialini and I set about, therefore, to devise a method of making cosmic rays take their own photographs, using the recently developed Geiger-Muller counter as detectors of the rays. Bothe and Rossi had shown that two Geiger counters placed near each other gave a considerable number of simultaneous discharges, called coincidences, which indicated, in general, the passage of a single cosmic ray through both counters. Rossi developed a neat valve circuit by which such coincidences could easily be recorded.’’

‘‘Occhialini and I decided to place Geiger counters above and below a vertical cloud chamber, so that any ray passing through the two counters would also pass through the chamber. By a relay mechanism the electric impulse from the coincident discharge of the counters was made to activate the expansion of the cloud chamber, which was made so rapid that the ions produced by the ray had no time to diffuse much before the expansion was complete.’’

After an appropriate delay to allow for droplet formation, the flash lamps were fired and the chamber was photographed. Today, this sounds relatively trivial until it is realized that not a single component was available as a commercial item. Each had to be fashioned from scratch. Previously, the chambers had been expanded at random with the obvious result, when trying to study cosmic rays, that only 1 in about 50 pictures (Anderson's experience) would show a track suitable for measurement. Occhialini (1975), known as Beppo to all his friends, described the excitement of their first success. Blackett emerged from the darkroom with four dripping photographic plates in his hands exclaiming for all the lab to hear, ‘‘one on each, Beppo, one on each!’’ He was, of course, exalting over having the track of at least one cosmic-ray particle in each picture instead of the one in fifty when the chamber was expanded at random. This work (Blackett and Occhialini, 1932) was first reported in *Nature* in a letter dated Aug. 21, 1932 with the title, ‘‘Photography of Penetrating Corpuscular Radiation.’’

Shortly after this initial success they started observing multiple particles: positive and negative electrons, which originated in the material immediately above the chamber. This was just a few months after Anderson (1932) had reported the existence of a positive particle with a mass much less than the proton. Here they were seeing pair production for the first time. Furthermore, they occasionally observed the production of particles showering from a metal (lead or copper) plate which spanned the middle of their chamber. These were clearly associated with particles contained in showers that had developed in the material above their chamber. The paper in which they first discuss these results is a classic and should be required reading by every budding experimental physicist (Blackett and Occhialini, 1933). In this

[1]Anderson's paper in *The Physical Review* is entitled ‘‘The Positive Electron.’’ In the abstract, written by the editors of the journal, it is said, ‘‘these particles will be called positrons.’’

[2]Of the ‘‘six men out of the ordinary,’’ two are physicists, I. I. Rabi and P. M. S. Blackett.

paper they describe in detail their innovative technique. They also analyze the new and surprising results from over 500 photographs. Their analysis is an amazing display of perspicacity. It must be remembered that this was nearly two years before the Bethe-Heitler formula (1934) and five years before Bhabha and Heitler (1937) and Carlson and Oppenheimer (1937) had extended the Bethe-Heitler formula to describe the cascade process in electromagnetic showers.

Blackett, Occhialini, and Chadwick (1933), as well as Anderson and Neddermeyer (1933), studied the energetics of the pairs emitted from metals when irradiated with the 2.62-MeV γ rays from thorium-C. They found, as expected, that no pair had an energy greater than 1.61 MeV. This measurement also permitted the mass of the positron to be determined to be the same as the electron, to about 15%. The ultimate demonstration that the positive particle was, indeed, the antiparticle of the electron came with the detection of 2 γ's by Klemperer (1934), the annihilation radiation from positrons coming to rest in material.

Blackett and Occhialini[3] must have been disappointed to have been scooped in the discovery of the positron, but they graciously conclude that to explain their results it was ‘‘necessary to come to the same remarkable conclusion’’ as Anderson.

IV. THE SLOW DISCOVERY OF THE MESOTRON

In contrast to the sudden recognition of the existence of the positron from one remarkable photograph, the mesotron had a much longer gestation, almost five years. It was a period marked by an extreme reluctance to accept the idea that the roster of particles could extend beyond the electron-positron pair, the proton and neutron, and the neutrino and photon. It was a period of uncertainty concerning the validity of the newly minted quantum theory of radiation, the validity of the Bethe-Heitler formula. The second edition of Heitler's book, *The Quantum Theory of Radiation* (1944) serves, still, as a *vade mecum* on the subject. The first edition (1935), however, carries a statement revealing the discomfort many theorists felt at the time, to wit, the ‘‘theory of radiative energy loss breaks down at high energies.’’ The justification for this reservation came from measurements of Anderson and Neddermeyer and, independently, Blackett and Wilson, who showed that cosmic-ray particles had a much greater penetrating power than predicted by the theory which pertained to electrons, positrons, and their radiation. The threshold energy at which a deviation from theoretical expectations appeared was around 70 MeV, highly suggestive that things were breaking down at the mass of the electron divided by the fine-structure constant, 1/137. However, the theoretical predictions hardened in 1934 when C. F. von Weizsacker and, independently, E. J. Williams showed that in a selected coordinate system both bremsstrahlung and pair production involved energies of only a few mc^2, independent of the original energy. Finally, the ionization and range measurements, primarily by Anderson and Neddermeyer (1937) and Street and Stevenson (1937), forced the situation to the following conclusion: that the mass of the penetrating particles had to be greater than that of the electron and significantly less than that of the proton. In this regard, it is noted that Street and Stevenson were first to employ a double cloud-chamber arrangement that later was to become widely used, i.e., one chamber above the other with the top chamber in a magnetic field for momentum measurements and the lower chamber containing multiple metal plates for range measurements.[4]

About a month after the announcement of the new particle with a mass between that of the electron and the proton, Oppenheimer and Serber (1937) made the suggestion ‘‘that the particles discovered by Anderson and Neddermeyer and Street and Stevenson are those postulated by Hideki Yukawa (1935) to explain nuclear forces.’’ [5] Yukawa's paper had been published in 1935 in a Japanese journal, but there had been no reference to it in western physics journals until Oppenheimer and Serber called attention to it. Here at last was the possibility of some theoretical guidance. If the new particle discovered in cosmic rays was that postulated by Yukawa to explain nuclear forces, it would have a mass of the order of 200 electrons, it should be strongly interacting, it should have a spin of 0 or 1, and it should undergo β decay, most likely to an electron and a neutrino.[6]

Blackett, who with Wilson had made some of the earliest and best measurements on the penetrating particles, was curiously reluctant to embrace the new particle. He found it easier to believe that the theory was faulty than that a brand new particle existed.

The first evidence of mesotron decay came from the cloud-chamber pictures of Williams and Roberts (1940). These stimulated Franco Rasetti (1941) to make the first direct electronic measurements of the mean life. He obtained 1.5 ± 0.3 microseconds.

Earlier Rossi, now in America (another one of those marvelous gifts of the Fascist regimes in Europe to the United States), had measured the mean decay length of the mesotrons in the atmosphere by comparing the attenuation in carbon with an equivalent thickness of atmosphere. With measurements performed from sea

[3]There is an unusual symmetry associated with these men. The Englishman, Blackett, had an Italian wife; the Italian, Occhialini, had an English wife.

[4]Originally Anderson and Neddermeyer had suggested mesoton for the name of this new particle. Milliken, still a feisty laboratory director, objected and at his insistence the name became mesotron. With usage and time the name evolved into meson.

[5]Serber (1983) has commented, ‘‘Anderson and Neddermeyer were wiser: they suggested ‘higher mass states of ordinary electrons’.’’

[6]A highly illuminating and interesting account of post-mesotron theoretical developments has been provided by Robert Serber (1983).

level to the top of Mt. Evans in Colorado (14 000 ft) he determined the mean decay length to be 9.5 km. Blackett had measured the sea-level momentum spectrum. From that Rossi could obtain an average momentum and, assuming a mass, obtain a proper lifetime. Using his own best estimate of the mass of the mesotron, 80 MeV, he obtained a mean life of 2 microseconds. A bit later Rossi and Neresson (1942) considerably refined the direct method of Rasetti and obtained a lifetime value of 2.15 ± 0.07 microseconds, remarkably close to today's value. And talk about experimental ingenuity, how does one measure a time of the order of microseconds with a *mechanical* oscillograph? They first produced a pulse the amplitude of which was proportional to the time interval between the arrival of a stopping mesotron, as determined by one set of counters, and the appearance of the decay product from a separate set. Considerably stretched in time, these pulses could be displayed on the oscillograph. The distribution in pulse heights then gave the distribution in time, a beautiful exponential.

At about this time research in cosmic rays was essentially stopped because of W.W.II. One summary of the state of knowledge about the subject at that time was provided by Heisenberg. In 1943 he edited a volume of papers devoted to cosmic rays. In this volume the best value for the mass of the mesotron came from the mean decay length in the atmosphere determined by Rossi as well as his direct lifetime measure. The mass was quoted as 100 MeV, which ''can be incorrect by 30%, at most.'' Furthermore, the authors in this volume still accepted, without question, the mesotron to be the Yukawa particle with spin 0 or 1 decaying to electron and neutrino.[7]

V. THE MESOTRON IS NOT THE YUKON

In naming the new particle, serious consideration was given to honoring Yukawa with the obvious appelation, the Yukon. However, this was considered too frivolous and mesotron was adopted. Now out of ravaged war-torn Italy came an astonishing new result: the mesotron was *not* the particle postulated by Yukawa. There had been disquieting indications of this. Despite numerous photographs of their passing through plates in chambers, never had mesotrons shown an indication that they had interacted. Furthermore, the best theoretical estimate of their lifetime was around 10^{-8} seconds, whereas the measured lifetime was 100 times longer. These discrepancies were largely ignored.

As far back as 1940 Araki and Tomonaga (later of QED fame) had published a paper in which they observed that a positively charged Yukawa particle, on coming to rest in matter, would be repelled by the Coulomb field of the nucleus and simply decay as though it were in free space. The negative particles, on the other hand, would interact with the nucleus long before they had a chance to decay. Fortunately, the paper was published in the *Physical Review* (Tomonaga and Araki, 1940), rather than in a Japanese journal, so the conclusions were disseminated widely and quickly.

Three Italians working in Rome, Conversi, Pancini, and Piccioni, set out to test the Araki-Tomonaga result. This was during the time the Germans, under the pressure of the allied armies, were withdrawing from central Italy. At one time or another, while setting up the experiment, Pancini was in northern Italy with the partisans; Piccioni, an officer in the Italian army, was arrested by the retreating Germans (but shortly released), while Conversi, immune to military service because of poor eyesight, was involved in the political underground. Despite the arduous circumstances and many interruptions, they managed to perform an elegant experiment. Data taking started in the spring of 1945 near the end of the war. Using a magnetic spectrometer of novel design, they selected first positive then negative stopping mesotrons and found that essentially no negative particles were observed to decay when stopped in iron, but, contrary to Araki and Tomonaga, those that stopped in carbon did decay and at the same rate as the positives (Conversi *et al.*, 1947). Fermi, Teller, and Weisskopf (1947) quickly showed that this implied the time for capture was of the order of 10^{12} longer than expected for a strongly interacting particle. It was the experiment that marked the end of the identification of the mesotron with the Yukawa particle.

VI. ''EVEN A THEORETICIAN MIGHT BE ABLE TO DO IT''

In Bristol in 1937 Walter Heitler showed Cecil Powell a paper by Blau and Wambacher (1937), which exhibited tracks produced by the interaction of cosmic-ray particles with emulsion nuclei. He made the remark that the method appeared so simple that ''even a theoretician might be able to do it.'' Powell and Heitler set about preparing a stack of photographic plates (ordinary lantern slide material) interspersed with sheets of lead. Heitler placed this assembly on the Jungfraujoch in the Alps for exposure in the summer of 1938. The plates were retrieved almost a year later and their scanned results led to a paper on ''Heavy cosmic-ray particles at Jungfraujoch and sea level.''

The photographic technique had had a long and spotty history which had led most people to the conclusion that it was not suitable for quantitative work. The emulsions swelled on development and shrank on drying. The latent images faded with time, so particles arriving earlier were more faint than those, with the same velocity, that arrived later. The technique was plagued by nonuniform development. Contrary to the unanimous advice of others, Powell became interested; he saw that what was needed was precise microscopy, highly

[7]The book was originally published to mark the 75th birthday of Heisenberg's teacher, Arnold Sommerfield. On the very day which the book was intended to commemorate, bombs fell on Berlin, destroying the plates and all the books that had not been distributed, nearly the entire stock. The English version, *Cosmic Radiation*, Dover Publications, New York (1946) is a translation by T. H. Johnson from a copy of the German edition loaned by Samuel Goudsmit.

controlled development of the emulsions, and emulsions, which up till then had been designed for other purposes, tailored to the special needs of nuclear research, richer in silver content and thicker. Powell attended to these things and convinced the film manufacturers to produce the special emulsions (Frank *et al.*, 1971). Initially, the new emulsions were not successful. Then W.W.II intervened. During the war, Powell was occupied with measuring neutron spectra by examining the proton recoils in the emulsions then available.

VII. THE RESERVOIR OF IDEAS RUNNETH OVER

Except for the highly unusual cases like that just described, most physicists had their usual research activities pushed aside by more pressing activities during W.W.II.[8] Some, disgusted with the political situation at home, found refuge in other countries. However, ideas were still being born to remain latent and await development at the end of the war.

Immediately after the war the maker of photographic materials, Ilford, was successful in producing emulsions rich in silver halide, 50 microns thick, and sensitive to particles that ionized a minimum of six times. These were used by Perkins (1947), who flew them in aircraft at 30 000 ft. He observed "stars" when mesons came to the end of their range. It was assumed that these were negative mesotrons, which would interact instead of decay.

Occhialini[9] took these new plates to the Pic-du-Midi in the Pyrenees for a one-month exposure. On development and scanning back in Bristol, in addition to the "stars" that Perkins had observed, the Powell group discovered two events of a new type. A meson came to rest but then a second appeared with a range of the order of 600 microns.[10] This was the first evidence (Lattes *et al.*, 1947a) suggesting two types of mesons. The authors also conclude in this first paper that if there is a difference in mass between primary and secondary particles it is unlikely to be greater than 100 m_e.[11] More plates were exposed, this time by Lattes at 18 600 ft in the Andes in Bolivia and, on development back in Bristol, 30 events were found of the type seen earlier. Here it was possible to ascertain the mass ratio of the two particles, and they state that it is unlikely to be greater than 1.45. We now know it to be 1.32. The first, the π meson, was associated with the Yukawa particle and the second with the mesotron of cosmic rays, the μ meson.[12]

The work on emulsions continued, and by 1948 Kodak had produced an emulsion sensitive to minimum ionizing particles. The Powell group took them immediately to the Jungfraujoch for exposure under 10 cm of Pb for periods ranging from eight to sixteen days. They were immediately rewarded with images of the complete π-μ-e decay sequence. More exciting was the observation of the first tau-meson decay to three π mesons (Brown *et al.*, 1949) and like the Rochester and Butler particles, discussed below, its mass turned out to be around 1000 m_e. The emulsion technique continued to evolve. Emulsions 0.6-mm thick were produced. Dilworth, Occhialini, and Payne (1948) found a way to ensure uniform development of these thick pieces of gelatin richly embedded with silver halides, and problems associated with shrinkage were solved. Stripped from their glass plates, stacks of such material were exposed, fiducial marks inscribed, and the emulsions returned to the glass plates for development. Tracks could then be followed from one plate to another with relative ease. Emulsions became genuine three-dimensional detectors.

VIII. "THERE IS NO EXCELLENT BEAUTY THAT HATH NOT SOME STRANGENESS IN THE PROPORTION"[13]

Concurrent with the development of the emulsion technique by Occhialini and Powell, Rochester and Butler were taking pictures using the Blackett magnet chamber, refurbished, and with a new triggering arrangement to make it much more selective in favor of penetrating showers: Very soon, in October 1946 and May 1947, they had observed two unique events, forked tracks appearing in the chamber which could not have been due to interactions in the gas. It became clear that they were observing the decay of particles with a mass of the order of half the proton mass, about 1000 m_e, (Rochester and Butler, 1947). These were the first of a

[8]For example, in the U.K. Blackett was to become "the father of operations research" and was to be a bitter (and losing) foe of the policy of strategic bombing. In the U.S. Bruno Rossi was recruited by Oppenheimer to bring his expertise in electronics to Los Alamos.

[9]Occhialini had gone to the University of Sao Paulo in Brazil in 1938 but returned to England in 1945 to work with Powell at Bristol.

[10]One of the worries of the Powell group was that, on stopping, the first meson had somehow gained energy in a nuclear interaction and then continued on. This question was considered in depth by C. F. Frank (1947) who concluded that this would only happen if the mesotron fused a deuteron and a proton which would release 5.6 MeV. Frank concluded that it was "highly improbable that the small amount of deuterium in an emulsion could account for the observed phenomena." It was to be another ten years before "cold fusion" was discovered in a hydrogen bubble chamber by the Alvarez group in Berkeley. They were unaware of the previous work by Frank.

[11]The two-meson hypothesis was actively discussed by Bethe and Marshak at the famous Shelter Island conference, June 2–4, 1947 with no knowledge of the experimental evidence already obtained by Lattes, Muirhead, Occhialini, and Powell in *Nature* (1947a). This issue was on its way across the Atlantic, by ship in those days, at the time of the conference. The mesons are named m_1 and m_2 in the first paper and π and μ in the second and third papers (1947b)

[12]There is a story, perhaps apocryphal, that they were called the π and μ mesons because these were the only two Greek letters on Powell's typewriter. I am willing to believe it because I had such a typewriter myself (the author).

[13]Francis Bacon, 1597, "Of Beauty."

new class of particles, the so-called strange particles. They created a sensation in Blackett's laboratory. However, no more such events were seen in more than a year of running. It was then decided to move the chamber to the high mountains for a higher event rate. But where? Two sites were possible, the Aiguille-du-Midi near Chamonix or the Pic-du-Midi in the Pyrenees. The Blackett magnet was much too massive to be transported to the Aiguille; this could be solved by building a new magnet that could be broken down into small pieces for transport on the téléférique up the mountain. The Pic-du-Midi was at a much lower altitude. It was accessible in winter only on skis, and supplies had to be carried in. However, the heavy Blackett magnet could be installed and adequate power for it was promised. They chose the site in the Pyrenees and were in operation in the summer of 1950. Almost immediately they began observing forked tracks similar to those observed in Manchester.[14]

Somewhat before, the Anderson group at Caltech had also observed events like those originally seen by Rochester and Butler. It was at this time that Anderson and Blackett got together and decided that these new types of particles should be called V particles.

IX. AND SO WAS BORN THE TAU-THETA PUZZLE

It was Thompson at Indiana University (he had earlier been a student of Rossi's at MIT) who singlehandedly brought the cloud-chamber technique to its ultimate precision. His contribution to the field has been tellingly described by Steinberger (1989).

> "Because many new particles were being observed, the early experimental situation was most confused. I would like to recall here an incident at the 1952 Rochester conference, in which the puzzle of the neutral V's was instantly clarified. It was the session on neutral V particles. Anderson was in the chair, but J. Robert Oppenheimer was dominant. He called on his old friends, Leighton from Caltech and W. B. Fretter from Berkeley, to present their results, but no one was much the wiser after that. Some in the audience, clearly better informed than I was, asked to hear from Robert W. Thompson from Indiana, but Oppenheimer did not know Thompson, and the call went unheeded. Finally there was an undeniable insistence by the audience, and reluctantly the lanky young midwesterner was called on. He started slowly and deliberately to describe his cloud chamber, which in fact was especially designed to have less convection than previous chambers, an improvement crucial to the quality of the measurements and the importance of the results. Oppenheimer was impatient with these details, and sallied forth from his corner to tell this unknown that we were not interested in details, that he should get on to the results. But Thompson was magnificently imperturbable: 'Do you want to hear what I have to say, or not?' The audience wanted to hear, and he continued as if the great master had never been there. A few minutes later, Oppenheimer could again no longer restrain himself, and tried again, with the same effect. The young man went on, exhibited a dozen well-measured V^0's, and, with a beautiful and original analysis, showed that there were two different particles, the $\Lambda^0 \rightarrow p + \pi^-$ and $\theta^0 \rightarrow \pi^+ + \pi^-$. The θ^0 (θ for Thompson) is the present K^0."

When the events of the Rochester conference of 1952 were unfolding, additional examples of tau-meson decay had been observed in photographic emulsions. In the next three years several hundred fully reconstructed decays were observed worldwide, largely in emulsions. Almost immediately, a fundamental problem presented itself. A τ^+ decays to $\pi^+ + \pi^+ + \pi^-$. A few instances were seen where the π^- had very little energy, i.e., was carrying away no angular momentum. In that the $\pi^+ + \pi^+$ system must be in an even state of angular momentum (Bose statistics) and that the π has an odd intrinsic parity, there was no way the τ and the θ could have the same parity. These rather primitive observations were borne out by detailed analyses prescribed by Dalitz (1954). So was born the tau-theta puzzle.

What appeared to be a clear difference in the tau and theta mesons made it imperative to know just how many different mesons existed with a mass of about 1000 m_e. To answer this question an enormous stack of emulsion was prepared, large enough to stop any of the charged secondaries from the decay. The experiment was the culmination of the development of the photographic technique. The so-called "G stack" collaboration, Davies *et al.* (1955), involved the Universities of Bristol, Milan, and Padua. In this 1954 experiment 250 sheets of emulsion, each 37×27 cm and 0.6 mm thick were packed together separated only by thin paper. The package was 15 cm thick and weighed 63 kg. It was flown over northern Italy supported by a balloon at 27 km for six hours. Because of a parachute failure on descent about 10% of the emulsion stack was damaged but the remainder was little affected. This endeavor marked the start of large collaborative efforts. In all, there were 36 authors from 10 institutions.

Cloud-chamber groups in Europe and the United States were discovering new particles. There were, in addition to Thompson working at sea level at Indiana, the Manchester group at the Pic-du-Midi and the French group under Louis Leprince-Ringuet from the Ecole Polytechnique working at the Aiguille-du-Midi and the Pic-du-Midi. Rossi's group from MIT and a Princeton group under Reynolds were on Mt. Evans in Colorado; the group of Brode was at Berkeley, and Anderson's at Caltech was on Mt. Wilson. The camaraderie of this in-

[14]Not without a price. One young researcher suddenly died when skiing up the mountain to the laboratory.

ternational group was remarkable, perhaps unique. Sharing data and ideas, this collection of researchers strove mightily to untangle the web being woven by the appearance of many new strange particles, literally and figuratively.

The role of cosmic rays in particle physics reached its apex at the time of the conference in the summer of 1953 at Bagnères-de-Bigorre in the French Pyrenees, not far from the Pic-du-Midi. It was a conference characterized by great food and wines and greater physics, a truly memorable occasion. All of the distinguished pioneers were there: Anderson, Blackett, LePrince-Ringuet, Occhialini, Powell, and Rossi. It was a conference at which much order was achieved out of a rather chaotic situation through nomenclature alone. For example, it was decided that all particles with a mass around 1000 m_e were to be called K mesons. There was a strong admonition from Rossi (1953) that they were to be the same particle until proven otherwise. All particles with a mass greater than the neutron and less than the deuteron were to be called hyperons. And finally, at the end, Powell announced, "Gentlemen, we have been invaded… the accelerators are here."

X. THE CREPUSCULAR YEARS FOR CLOUD CHAMBERS

The study of cosmic rays with cloud chambers and emulsions remained the only source of information about strange particles through most of 1953. That information was enough for Gell-Mann (1953) and Nakano *et al.* (1953) to see a pattern based on isotopic spin that was to be the forerunner of SU(3) and the quark model. Then data from the new accelerators started to take over, beginning with the observation of associated production by Shutt and collaborators at Brookhaven (Fowler *et al.*, 1953). It was an experiment that still used the cloud chamber as the detector, in this case a diffusion chamber. The continuously sensitive diffusion chamber had been developed by Alex Langsdorf (1936) before W.W.II but had never found use studying cosmic rays because the sensitive volume was a relatively thin horizontal layer of vapor whereas, as Milliken said, "cosmic rays come down." However, with the high-energy horizontal π^- beams at the Brookhaven cosmotron, the diffusion chamber had a natural application.

In these last years of the cloud chamber one more magnificent experiment was performed. In one of the transcendent theoretical papers of the decade, M. Gell-Mann and A. Pais (1955) proposed a resolution of a puzzle posed by Fermi two years before, i.e., if one observes a $\pi^+ + \pi^-$ pair in a detector, how can one tell if the source is a θ^0 or its antiparticle, the $\overline{\theta^0}$? The conclusion of the Gell-Mann and Pais analysis was that the particles which decay are two linear combinations of θ^0 and $\overline{\theta^0}$ states, one short lived and decaying to the familiar $\pi^+ + \pi^-$ and the other, long lived. It was a proposal so daring in its presumption that many leading theorists were reluctant to give it credence. However, Lederman and his group accepted the challenge of searching for the long-lived neutral counterpart. And they were successful in discovering the θ_2 which lives 600 times longer than the θ_1, the object that decays to $\pi^+ + \pi^-$.

This was the last great experiment performed using the Wilson cloud chamber, which had had its origins in the curiosity of a man ruminating about the mists over his beloved Scottish hillsides. Glaser's bubble chamber, the inspiration for which came from a glass of beer in a pub, was ideally suited for use with accelerators and soon took over as the visual detector of choice. By 1955 K mesons were being detected by purely counter techniques at the Brookhaven Cosmotron and the Berkeley Bevatron, and the antiproton was discovered at the Bevatron. Data from large emulsion stacks exposed in the beams from the accelerators quickly surpassed the cosmic-ray results in quality and quantity.

The big questions, which were tantalizingly posed by the cosmic-ray results, defined the directions for research using accelerators. The tau-theta puzzle was sharpened to a major conundrum. Following the edict of Hippocrates that serious diseases justify extreme treatments, Lee and Yang were to propose two different remedies: the first, that particles exist as parity doublets; and the second, much more revolutionary than the first, that a cherished conservation principle, that of parity, was violated in the weak interactions. They suggested a number of explicit experimental tests which, when carried out, revealed a new symmetry, that of *CP*. This, too, was later shown to be only approximate.[15] Indeed, within the framework of our current understanding, the preponderance of matter over antimatter in our universe is due to a lack of *CP* symmetry. Furthermore, as we have already noted, a large fraction of the discoveries that were key to the theoretical developments in the 1950s and early 1960s, discoveries which led to the quark model, also were made in cosmic-ray studies. Most were unpredicted, unsolicited, and in many cases, unwanted at their birth. Nonetheless, these formed the foundations of the standard model.

Today, discoveries in cosmic rays continue to amaze and confound. The recent evidence (Fukuda *et al.*, 1998) that neutrinos have mass has been the result of studying the nature of the neutrinos originating from the π-μ-e decay sequence in the atmosphere. This is a story that remains to be completed.

GENERAL READING

Brown, L. M., and L. Hoddeson, 1983, Eds., *The Birth of Particle Physics* (Cambridge University, Cambridge, England.

Colston Research Society, 1949, *Cosmic Radiation*, Symposium Proceedings (Butterworths, London). American edition (Interscience, New York).

Marshak, R., 1952, *Meson Theory* (McGraw-Hill, New York).

[15]See Henley and Schiffer in this issue.

Occhialini, G. P. S., and C. F. Powell, 1947, *Nuclear Physics in Photographs* (Oxford University, New York).

Pais, A., 1986, *Inward Bound* (Oxford University, New York).

Peyrou, C., 1982, International Colloquium on the History of Particle Physics, J. Phys. (Paris) Colloq. **43**, C-8, Suppl. to No. 12, 7.

Powell, C. F., P. H. Fowler, and D. H. Perkins, 1959, *The Study of Elementary Particles by the Photographic Method* (Pergamon, London).

Rochester, G. D., and J. G. Wilson, 1952, *Cloud Chamber Photographs of the Cosmic Radiation* (Pergamon, London).

REFERENCES

Anderson, C. D., 1932, Science **76**, 238.

Anderson, C. D., 1933, Phys. Rev. **43**, 491.

Anderson, C. D., and S. H. Neddermeyer, 1933, Phys. Rev. **43**, 1034.

Anderson, C. D., and S. H. Neddermeyer, 1937, Phys. Rev. **51**, 884.

Auger, P., and D. Skobelzyn, 1929, C. R. Acad. Sci. **189**, 55.

Bhabha, H., and W. Heitler, 1937, Proc. R. Soc. London, Ser. A **159**, 432.

Blackett, P. M. S., 1948, Nobel Address.

Blackett, P. M. S., and G. P. S. Occhialini, 1933, Proc. R. Soc. London, Ser. A **139**, 699.

Blackett, P. M. S., G. P. S. Occhialini, and J. Chadwick, 1933, Nature (London) **131**, 473.

Blau, M., and Wambacher, 1937, Nature (London) **140**, 585.

Brown, R., U. Camerini, P. H. Fowler, H. Muirhead, C. F. Powell, and D. M. Ritson, 1949, Nature (London) **163**, 82.

Carlson, J. F., and J. R. Oppenheimer, 1937, Phys. Rev. **51**, 220.

Conversi, M., E. Pancini, and D. Piccioni, 1947, Phys. Rev. **71**, 209.

Dalitz, R. H., 1954, Phys. Rev. **94**, 1046.

Davis, *et al.*, 1955, Nuovo Cimento, Series X, Vol. **2**, 1063.

Dillworth, C. C., G. P. S. Occhialini, and R. M. Payne, 1948, Nature **162**, 102.

Fermi, E., E. Teller, and V. Weisskopf, 1947, Phys. Rev. **71**, 314.

Fowler, W. B., *et al.*, 1953, Phys. Rev. **98**, 12 1.

Frank, F. C., 1947, Nature (London) **160**, 525.

Frank, F. C., D. H. Perkins, and A. M. Tyndall, 1971, Biogr. Mem. Fellows R. Soc. **17**, 541.

Fukuda, Y., *et al.*, 1998, Phys. Rev. Lett. **81**, 1562.

Gell-Mann, M., 1953, Phys. Rev. **92**, 833.

Gell-Mann, M., and A. Pais, 1955, Phys. Rev. **97**, 1387.

Heisenberg, W., 1946, Ed., *Cosmic Radiation* (Dover, New York), translated by T. H. Johnson from the 1943 German edition.

Heitler, W., 1935, *The Quantum Theory of Radiation* (Oxford, University Press, London). Second Ed. 1944.

Klemperer, O., 1934, Proc. Cambridge Philos. Soc. **30**, 347.

Langsdorf, A., 1936, Phys. Rev. **49**, 422.

Lattes, C. M. G., H. Muirhead, G. P. S. Occhialini, and C. F. Powell, 1947a, Nature (London) **159**, 694.

Lattes, C. M. G., H. Muirhead, G. P. S. Occhialini, and C. F. Powell, 1947b, Nature (London) **160**, 453, 486.

Lovell, B., 1975, Biogr. Mem. Fellows R. Soc. **21**, 1.

Nakano, T. and K. Nishijima, 1953, Prog. Theor. Phys. **10**, 581.

Occhialini, G. P. S., 1975, Notes Rec. R. Soc. **29**, 144.

Oppenheimer, J. R., and R. Serber, 1937, Phys. Rev. **51**, 113.

Perkins, D. H., 1947, Nature (London) **159**, 126.

Rasetti, F., 1941, Phys. Rev. **60**, 198.

Rochester, G. D., and C. C. Butler, 1947, Nature (London) **160**, 855.

Rossi, B., and N. Neresson, 1942, Phys. Rev. **62**, 417.

Serber, Robert, 1983, *The Birth of Particle Physics*, edited by Laurie M. Brown and Lillian Hoddeson (Cambridge University, Cambridge, England), p. 206.

Skobelzyn, D., 1929, Z. Phys. **54**, 686.

Steinberger, J., 1989, in *Pions to Quarks*, edited by L. M. Brown, M. Dresden, and L. Hoddeson (Cambridge University, Cambridge, England), p. 317.

Street, J. C., and E. C. Stevenson, 1937, Phys. Rev. **51**, 1005.

Tomonaga, S., and G. Araki, 1940, Phys. Rev. **58**, 90.

Williams, E. J., and G. E. Roberts, 1940, Nature (London) **145**, 102.

Yukawa, H., 1935, Proc. Phys. Math. Soc. Jpn. **17**, 48.

Zuckerman, J., 1992, *Six Men Out of the Ordinary* (Peter Owen, London).