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The Discreet Charm of the Nuclear Emulsion Era

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■ **Abstract** By the 1950s, Europe was recovering from the human and material destruction of World War II. The fundamental rebirth of particle physics in this period was especially due to the development and diffusion of the nuclear emulsion technique, which was suitable for international collaboration. Research groups emerging from the catastrophe of the war had little more than their enthusiasm to contribute at the forefront of physics research, but by using the nuclear emulsion technique, they were able to disclose phenomena whose existence no one had suspected.

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Io stimo più il trovar un vero
benchè di cosa leggiera
che 'l disputar lungamente
delle massime questioni
*senza conseguir verità nissuna.**
Galileo Galilei, *Opere*

**Most highly I esteem a trifling truth*
About the merest mote, but do despise
Unending disputations seeking sooth
O'er great eternal quests that yield but lies.
(Transl. Chris Quigg)

I would like to recall events of many years ago; perhaps they are no longer essential to the understanding of today's science, but they took place at a time rich in expectations, tensions, struggles, and gratifications.

Above all I would like to rekindle the atmosphere of those days gone by, through the cosmic-ray and nuclear-emulsion research carried out in the 1950s. I propose to do it by way of three conferences in Italy, which were held after the famous conference at Bagnères-de-Bigorre (unpublished proceedings, Congrès sur le Rayonnement Cosmique, Bagnères-de-Bigorre, July 1953). The first of these was held in Padova in 1954 (1), the second in Pisa in 1955 (2), and the third was the Padova-Venice Conference on Mesons and Recently Discovered Particles in 1957 (unpublished proceedings). They practically span the period of the cosmic-ray strange-particle research.

But when I think of that period in my professional life, nostalgia overcomes me, along with an urge to speak about the enthusiasm and the vigor of those days when we had the feeling of advancing another step, however small, toward understanding the complex phenomena before us.

THE TIME OF WAR

I was born in Legnago, a rather pleasant, ancient town not far from Venice. I attended the local classical Lyceum, named after Giovanni Cotta, a famous local humanist of the fifteenth century. At my Lyceum, according to tradition, the teaching of science was weak and limited in favor of a humanistic orientation.

In general people believe that school is one of the most enjoyable periods in life, but for us it was a time of war and every day our life was deeply perturbed. All the time we were faced with a world in disorder and disaster. On my graduation from the Lyceum, the war entered, rather abruptly, its most brutal phase. Italy was occupied, the partisan struggle was intensifying, Jews and political prisoners were deported, and every day brought bombing and destruction. We could not go to the University, people were called up for army duty, there were departures without returns, the lacerations of the civil war were upon us, and as the poet Montale says, we felt the horrors of "a foreign foot on our heart." Suddenly our youth was gone.

My family had to leave town and take refuge in the nearby countryside, my father and my brother having refused any possible collaboration with the Nazi-fascists. During these long days I read many books of popular science. Arthur Eddington was one of my preferred authors. I also remember a little book on nuclei written by two ladies, Ginestra Amaldi and Laura Fermi (3). I am sure I understood only a fraction of what I was reading, but I had the intense experience of discovering what I wanted to do at the restarting of a normal life: to study physics.

I believe that my parents approved of my choice, which appeared suitable to them because I was a girl. In my youth, parents believed that the natural way a daughter should proceed in life was to get married, and, if she should have the need or the will to do some work outside the home, she could become a teacher in secondary school (which, by the way, was considered a rather good position for a woman). It would have been different if I had been a boy. I would have been urged to study medicine or engineering, to do a job that carried much higher social esteem and a higher salary.

After the end of the war, I enrolled at the University of Padova. My teachers in “Elementary Particle Physics” were Nicola Dallaporta, Michelangelo Merlin, and Gianpietro Puppi.

THE REBIRTH: COSMIC RAYS, NUCLEAR EMULSIONS, AND DISCOVERIES

Not until the 1950s did Italy slowly begin to recover from the destruction of war. The war was for Italy the ultimate tragedy. Nicola Dallaporta, who had arrived at the Padova Institute a few years earlier, described the postwar situation at the University of Padova (4):

> In 1945, at the end of the war, Padova’s Institute of Physics was left practically vacant, as most of the teaching and research staff were missing owing to the military situation. During the fall of 1945, Rostagni had already been able to collect a small number of staff members and newly graduated physicists returning from different war adventures . . . . There was practically nothing adequate for current research in the Institute; all of us were rather ignorant concerning the developments of physics during the six years of the war, since few journals had been available at that time. Thus, the only thing to do was to study as much as possible in order to recover time lost and become able to choose a field of research suited to the rather poor conditions we had to face. There was no heating in the building during this first winter. We huddled together, each one with his own books, in a single common room, the only one where a wood-burning stove provided some warmth . . . . We became rapidly aware that the most appropriate field of research to our present day conditions was Cosmic Rays which at that time did not require a very sophisticated and expensive apparatus for fruitful investigation.

The situation Dallaporta described in Padova was more or less the same at the other Italian universities, the only exception probably being Rome, where the war ended one year earlier.

In 1952 came a turning point for scientific life. In Italy the INFN (National Institute for Nuclear Physics) had been founded and, at the same time, CERN (European Center for Nuclear Research) was founded on a European scale.

It was at this time that I, having earned my degree with a thesis with Dallaporta on π^- meson nuclear absorption processes, was kindly offered a position as an unpaid researcher at the Institute of Physics in Padova. I was also mildly encouraged to join the emulsion study group. And I am still convinced that this happened because I was a woman. Emulsions indeed require patient exploration and do not need individual creativity. My family agreed to pay for my expenses, convinced as they were that it would only be a temporary situation.

I believe that being a woman facilitated the start of my career, since nobody, at first, considered me a real competitor. But, more importantly, the fact that I was not required to have an immediate success allowed me to experience the pleasure and joy of the "maraviglia" that Galileo had so well described, which one feels when unexpected solutions open totally new, unimaginable worlds. I consider it a privilege to have taken part in this type of research, which in those days was fascinating and exciting.

The postwar reconstruction came rather quickly and I still believe that it was a miracle, in a secular sense, something marvellous, an extraordinary fact surpassing all limits of predictability, all expectations. There had been a number of positive coincidences, a novel atmosphere, a big cooperative effort, an atmosphere of renewed solidarity, and an overall conviction that finally the time had arrived when it was possible to raise one's hopes. Everyone was aware that a long night had come to an end, and that the new day would require a great common effort. All of Europe got together for the reconstruction. Although Europe had been a battlefield for the second time in the century, most people were convinced that this time it had been more than a war between nations, it had been a common struggle against Nazi-fascism.

Elements of paramount importance for the miracle were certainly the reconstruction of the high level of past cosmic-ray research along with the host of new particles soon discovered. Let us start with the three famous events that occurred in 1947 (5): the discovery by Marcello Conversi, Ettore Pancini, and Oreste Piccioni in Rome of the leptonic nature of the cosmic-ray meson (the mesotron in the language of the time), now known as the muon; the very important discovery by Cesare M.G. Lattes, Giuseppe Occhialini, and Cecil F. Powell of the $\pi \rightarrow \mu \rightarrow e$ decay chain in nuclear emulsions exposed to cosmic rays, establishing the existence of the charged pion; and the discovery of the strange particles, which were first observed by George D. Rochester and Clifford C. Butler in a cloud chamber in Manchester, and which were called V particles because they appeared in the shape of a fork.

The cosmic-ray discovery of the new unpredicted particles opened an entire new world, parallel but not exactly similar to the old one. Nuclear emulsions and cloud chambers, exposed to cosmic-ray radiation, soon revealed that V particles came in a variety of forms: mesons with masses heavier than pions, generically called K mesons, and hyperons, particles heavier than a nucleon, which originated in nuclear collisions and decayed into a great variety of end products with lifetimes only slightly shorter than the charged-pion lifetime.

After the Rochester & Butler discovery of V particles, the growth of the number of new particles was very rapid. As Murray Gell-Mann said, the story of their discovery and classification represents a nice example of order emerging from chaos.

Certainly the new particles, which had been the catalyst of our actual understanding, would eventually have been found independently of cosmic rays and of emulsions, but probably the tremendous surge of important scientific discoveries that followed would have been different.

THE EMULSION TECHNIQUE

The use of photographic plates to record ionizing radiation dates back to Henri Becquerel, who in 1896 discovered radioactivity from the blackening of plates by uranium salts. The development and diffusion of the nuclear emulsion technique (6), which came to the foreground around the mid-1940s, was mostly due to the work of Cecil F. Powell and Beppo Occhialini.

Senior physicists realized very soon that cosmic-ray research with the emulsion technique offered an ideal field for re-emerging university groups, not only because it was cheap and did not require large installations, but also because it was particularly suited to those coming back to, or just beginning, their research after the wartime vicissitudes—those who were eager to enter into scientific activity but lacked training for it. Moreover the emulsion technique did not require a long apprenticeship, and the nature of the research was such that it did not demand a profound theoretical background. This technique was well suited for international collaboration, and small groups emerging from the catastrophe of the war found in emulsions a way to contribute easily at the forefront of physics research. All they needed to do was to expose nuclear emulsion plates for a few days at mountain altitude, where the cosmic-ray intensity is about ten times larger than at sea level. Then, after being developed, the emulsions would reveal the tracks of the charged particles that traversed them. What was left to do was to pay careful attention to rare events when scanning. To give just one example: On average, to find a τ event, one had to scan with a microscope about 200 cm^2 of emulsion (the emulsions were 0.6 mm thick). Research in cosmic rays with the emulsion technique spread rapidly in Italy; groups were soon operating in Rome, Padova, Genova, Pavia, and Torino.

Physicists working with emulsions at that time constituted a large community, in which experimentalists and theorists, more or less side by side, shared a profound curiosity but little understanding.

In Padova we were a group of young, enthusiastic researchers. Misko Merlin from the experimental side and Nico Dallaporta from the theoretical side were our leaders, but probably at that time it was impossible to come up with a strict research plan; our imaginations and our choices were allowed to roam free. It might be that we were deliberately allowed to believe we were a rather important part of this research activity, as a psychological encouragement to sustain the painstaking routine work. If that was the idea, it succeeded.

In December 1951, Cecil F. Powell organized a general meeting in Bristol of all laboratories interested in emulsion research, with the idea that the exciting problem of the new particles discovered in nuclear emulsion plates and in Wilson cloud chambers could best be tackled by a collaboration of several laboratories. With the idea in mind that this would strongly increase the statistics concerning these rather rare events, he suggested the formation of an association whose aim would be to expose bunches of emulsion plates to cosmic rays at very high altitudes using polyethylene balloons; the plates would then be shared among all participants for

Figure 1 A balloon ready for the ascent in the Sardinia area in July 1953. For the first time, emulsion stacks were made up of 40 stripped emulsions bound together between thick glass plates, wrapped and sealed and subsequently marked with X-rays.

microscopic scanning. These launchings, with balloon fabrication concentrated at Bristol and Padova but the responsibility of preparation divided among many laboratories, were soon organized. Figure 1 shows a launch demonstration. Cagliari, in Sardinia, was chosen as the launch site. The balloons floated at constant height for some hours, after having reached an adequate altitude (between 20 and 30 km), and the emulsion loads were then liberated and dropped down with a parachute on the sea, where a ship was ready to recover them. About half were successfully recovered. The collaboration involved a large number of European universities, 22 laboratories from 12 countries.

BAGNÈRES-DE-BIGORRE 1953: ORDER EMERGES FROM CHAOS

While the 1953 flights were under way, an International Cosmic Ray Conference was held in Bagnères-de-Bigorre in the Southwest of France, at the foot of the French Pyrénées, entirely dedicated to the new unstable particles. At the conference many experimentalists, coming from about 20 groups from all over Europe and the United States, had an extraordinary opportunity to compare their results, and it soon became clear that all this widespread cosmic-ray work was leading to a substantial consensus.

Previously it had seemed as if a new decay mode, or perhaps a known decay mode for a new parent mass, was being reported almost every month. But now it turned out that the most frequent decay modes were quite limited in number, and they were associated with fairly definite mass values. Therefore, a coherent picture of the new particle physics began to emerge from many partial works; in an attempt to classify the new particles, we established together the existence and the properties of many particles.

In closing that meeting, P.M.S. Blackett called it "in many respects the best I ever attended," and Richard H. Dalitz and Charles Peyrou agreed, "All the participants, young or old, remember the Conference as the best of their lives."

Bruno Rossi had the difficult task of presenting the summary. After the presentation of the different hyperon decay modes known at the time of the Conference, namely $\Lambda^0 \rightarrow p + \pi^-$; $\Sigma^+ \rightarrow p + \pi^0$, $\Sigma^+ \rightarrow n + \pi^+$, $\Xi^- \rightarrow \Lambda^0 + \pi^-$, and the hyperfragments, he went on to discuss the *K*-meson sector, "the most difficult part of my task," where a plethora of decay modes were clustered, nourishing the widespread conviction that they were coming from several different particles.

Soon after the Bagnères-de-Bigorre Conference, Louis Leprince-Ringuet published in a paper on "Mesons and Heavy Unstable Particles in Cosmic Rays" (7) the following "Appendix on the Nomenclature" compiled at the Conference Conclusion.

GROUPS OF PARTICLES

1. *L*-mesons (symbol *L*): π-meson, μ-meson, any other possible lighter meson.
2. *K*-mesons (symbol *K*): particles with mass intermediate between those of the π-meson and the nucleon.
3. *H*-particles; hyperons (symbol *H*): particles with mass intermediate between those of the nucleon and the deuteron. This definition to be revised if "fundamental" particles heavier than the deuteron are found.

PHENOMENOLOGICAL DESCRIPTION

1. *V*-event: phenomenon which can be interpreted as the decay in flight of a *K*-meson or a hyperon. Subclasses V^0 and $V^{\pm}$.
2. *S*-event: phenomenon which can be interpreted as the decay or the nucleon capture of a *K*-meson or a hyperon at rest.

INDIVIDUAL NAMES

1. Use small Greek letters for mesons. Use capital Greek letters for hyperons.
2. Heavy mesons: The most probable are
   $\tau \rightarrow 3\pi$ (certain);
   $\kappa \rightarrow \mu + 2$ neutral particles (very probable; nature of neutral particles still uncertain);
   $\chi \rightarrow \pi +$ neutral (probable; nature of neutral particle undetermined);
   θ^0 (or V_2^0) $\rightarrow \pi + (\pi$ or $\mu) + Q$ (200 MeV; very probable).

Rossi cut through the bewildering variety of K-meson processes with the assertion, "I would like to take the point of view that two particles are equal until they are proven different." He concluded that the many different classes of events were probably different decay modes of a single particle. He underlined the fact that χ- and κ-decays and τ-decays occur in the same proportions in photographic emulsions and in cloud chambers, and he interpreted this to mean that the κ-, χ- particles group and the τ-particles have very approximately the same lifetime. Moreover he concluded that the very close similarity between the masses of the τ- and the θ^0-particles, the two best established particles, could hardly be considered an accident. Two more years would pass before the precision and reliability of the measurements would force the conclusion that these events were all different modes of the same particle.

The news, just arrived, of recent experiments at the Cosmotron did not create surprise or preoccupation. In this atmosphere of excited confusion, most people were convinced that Nature would have continued to reward patient work with discovery. And although C.F. Powell commented, "Gentlemen, we have been invaded . . . the accelerators are here," L. Leprince-Ringuet expressed, with an elegant metaphor, his attitude to this threat of rivalry: Rather than retire to the country and wait six months for Brookhaven to give the proper answers, the community would have to continue to work in the field of cosmic rays in the hope that the higher-energy components of the cosmic rays would still hold some surprises.

Having mentioned Bruno Rossi brings to mind that this man was a Professor of Physics at the University of Padova from 1932 to 1938, when, as he said himself (8), he received the unexpected notice that the University did not need him anymore. Because he was a Jew, he had been deprived not only of the right to teach and to have a salary, but also of the right to continue his research and even of his right to enter the Institute of Physics that he himself had founded. And we who arrived later in this Institute still feel his pain weighing on us, along with the responsibility of telling his story, hoping that such terrible pain will never again be inflicted, and that his impression of the faces around him suddenly turning into white masks, without eyes, expressionless, will never again be possible. This action left a deep sadness on Bruno Rossi's remaining life. I think he never forgave it.

He returned to Padova only in 1987 for the celebration of the fiftieth anniversary of his Institute.

PADOVA CONFERENCE 1954: GETTING ON

The groups who took part in the 1953 Sardinia launches met a few months afterward in Bern to share the emulsions, and there decided to meet soon again in Padova to compare their main results. Rostagni and Dallaporta, director and vice director of the Padova Institute of Physics, decided to enlarge the scope and participation of this meeting to make it an "International Congress on Heavy Unstable Particles and High Energy Events in Cosmic Rays." The conference was held in Padova in April 1954, one year after the Bagnères-de-Bigorre Conference. It was oriented particularly toward the cosmic-ray studies going on in Europe, especially to present and compare the main results obtained from the recent emulsion expeditions, but there were also sessions on new data on strange particles obtained with cloud chambers in Europe and in the United States. M. Annis from the Massachusetts Institute of Technology informed us about evidence obtained at the Cosmotron for the associated production of the new particles, the mechanism proposed (9) as a means of reconciling their copious production with their long lifetimes.

The new series of balloon launchings featured the important innovation of stripped emulsions—large stacks of emulsion layers, each of which had been stripped off its backing. Each layer of emulsion was in direct contact with the next, so that after exposure, the tracks of secondary particles could be followed from one layer to the adjacent one over much larger distances than before. The stacked emulsions enabled a large improvement in the precision of measurement that made it possible to identify the particles uniquely and to infer a particle's energy by measuring its range.

Exploiting stripped emulsion stacks involved developing new techniques for marking emulsion sheets to allow easy tracking from sheet to sheet, modifying microscopes with special stages to hold and manipulate those large emulsion sheets after they were mounted on glass, and constructing precise microscope stages for multiple scattering measurements.

To give an idea of the technical progress achieved, it may be sufficient to mention that for practically all of the unstable particles observed in stripped emulsions, both the characteristics of the event from which they emerged and the mode of their decay could be known, which was possible with single emulsions only in exceptionally lucky cases. Moreover, the much greater length of the tracks available with the stripped emulsions in many instances allowed one to follow the particles to the end of their range. They could then be identified through range-ionization or range-scattering measurements, yielding much more precise results (10). At the Padova Congress, a committee composed of E. Amaldi, E. Fabri, T.F. Hoang, W.O. Lock, L. Scarsi, B. Touschek, and B. Vitale was in charge of preparing an overall report on τ-meson data, with the conviction that it was now possible to obtain information on the τ decay process.

The τ-meson was indeed a type of particle belonging to the group of K particles, and from the beginning it had held a privileged position as one of the very few new particles that—since it decays into charged secondaries only—provided detailed

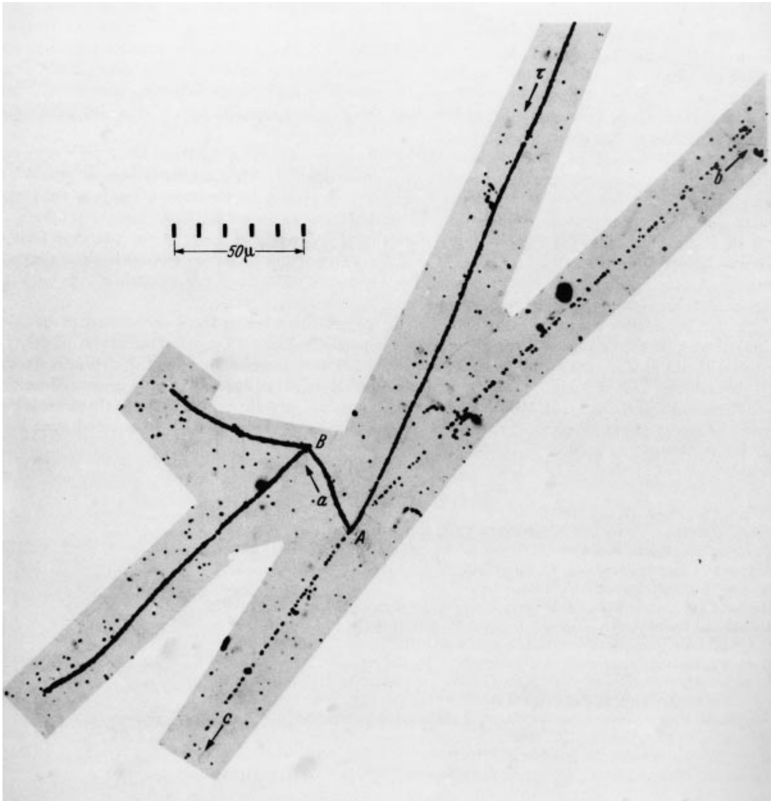

Figure 2 The first $\tau(K_{\pi 3})$ decay: the primary heavy meson (called τ in the picture) comes from left to right and stops. A slow π^- comes down and makes a two-pronged star. Two other lightly ionizing particles are emitted from the first stopping point.

information. The first event of this type (Figure 2) was discovered in Bristol in 1948 (11), about one year after the discovery of the π-meson. It was a spectacular event: A particle came to rest in the emulsion and decayed to give three charged pions, which, within the measurement errors, turned out to be coplanar with zero total momentum and a rather low Q value.

At the Bagnéres-de-Bigorre Conference, when the world supply of τ-decays was limited to 13 events, Richard Dalitz suggested a method to measure the τ-meson spin-parity. His idea, which soon become popular as the Dalitz plot, has proved extremely valuable in high-energy physics (12). Dalitz's investigation was a precursor to the discovery of parity nonconservation. The key question was: Could the θ^0 particle, namely the $K^0 \rightarrow \pi^+\pi^-$, and the τ^+ particle ($K^+ \rightarrow \pi^+\pi^+\pi^-$), be closely related? Both τ and θ decayed to pions only and their masses were quite comparable. One would have expected them—following the Rossi argument—to be different decay modes of the same particle. The $\pi^+\pi^-$ state resulting from θ^0

decay, since the π-meson is a pseudoscalar particle, has parity $(-1)^J$, where J is the relative orbital angular momentum, so $0^+, 1^-, 2^+, \ldots$. The question was, could the $\pi^+\pi^+\pi^-$ state resulting from τ^+ decay have the same spin and parity as the $\pi^+\pi^-$ state resulting from the θ^0?

The stripped emulsion stacks, which could fully contain the τ events, allowed easy collection of the needed information on the angular distribution and energy spectra of the τ secondaries. The summary presented by the Padova committee was based on a total of 39 τ events, 25 of which had been observed in stripped emulsions, and 16 of which were fully identified. Their analysis led to the conclusion that although the statistics were still not very significant and the analysis could be influenced by some experimental bias, the data indicate that the spin-parity for the τ was 0^-, a value not allowed to the θ^0 particle!

Another relevant point raised at the Padova Conference was the postulate of a new K-decay scheme, the $K_{\mu 2}$, proposed by the École Polytechnique group. The Paris group, from a large number of events observed in their large double cloud chamber, found one in which the primary mass indicated a K-decay and the secondary had a residual range larger than 75 g/cm^2 of copper, which then could not have come from a $K_{\pi 2}$. They also noted the presence of a large number of high-momentum secondaries, which strongly suggested a two-body decay of the type $K_{\mu 2}$.

It was at first sight surprising that several emulsion teams, all working on this subject, could have failed to identify this decay mode! Therefore, at this Padova Conference, in consideration of the K-meson decay uncertainties, the new École Polytechnique decay mode, and the problem of the different τ and θ spin-parities, the decision was made to undertake an experiment projected ad hoc that became known as the G-stack experiment (G for "giant"). The Padova, Milano, and Bristol groups joined together to launch a single large stack with 15 liters of emulsion, with the dimensions $37 \times 27 \times 15$ cm^3, so as to offer full containment of a reasonable number of tracks of the decay products of the K-mesons. In particular, if a heavy meson emitted a secondary particle that could cross 75 g/cm^2 of copper, the same particle in an emulsion stack would have come to rest after traversing 20 cm of emulsion. The idea was that if the emulsion stack were large enough, in many cases the secondaries would stop in the emulsion, making it possible to determine their energy properly, as well as to verify their nature (μ or π).

Although the idea of having to follow a minimum ionizing particle that traversed many emulsion layers up to 20 cm range, with a microscope, was a horrible nightmare for most people, for an experienced scanner it was a kind of sport. For compensation, the event would be named after the scanner who found it. The G-stack provided definitive information on the K-meson decay modes and demonstrated how much it pays to make the effort to have the right instrument.

PISA CONFERENCE 1955: A BURST OF CHEERING

The Pisa International Conference on Elementary Particles was the last major conference on particle physics at which the data presented were dominated by cosmic-ray contributions.

The G-stack was launched a few months after the Padova Conference, and the new results from the emulsion analysis were presented at the Pisa Conference. The G-stack members made a tremendous effort to be on time for the Pisa Conference. C.O'Ceallaigh, an Irish physicist from the G-stack collaboration, full of humor, wrote (13), "The organization of the results of the measurements and their presentations was again largely the work of the Italians and took place at Padova. I never will forget the fever and excitement associated with the effort . . . Occhialini was in ultimate charge and strode up and down the scene like an avenging Jehovah or thundering Jove . . . ."

Giuseppe Occhialini, who was universally known as Beppo, had an enormous impact on the rebirth of Italian physics. We admired him unconditionally for his rich, original nature. He was deeply intelligent and at the same time rich in humanity and curiosity toward everyone and everything. In discussions he would frequently express himself through parables, or surprise you with unexpected questions, which often appeared totally unconnected to the question at hand. He could quote poetry and literature for hours . . . . It was impossible not to feel a great fondness for Beppo. Figure 3 shows him preaching the G-stack exposure.

The G-stack was launched over Northern Italy, near Genova, six months after the Padova Conference. Because of the heavy weight of the package and the huge cost of the emulsion block, it was decided to make a single launch, and to hope for good

Figure 3 Beppo Occhialini talking in favor of the G-stack exposure at the Pisa Conference.

fortune, which in fact became a reality. The launch went perfectly and the balloon floated for 6 h at a substained height of 27 km, but after cut-off the parachute did not open. The payload was recovered, with some dramatic phases, in a wild and desolate region of the Alps. Only about 10% of the emulsion stack was damaged.

At the Pisa Conference the G-stack collaboration, 36 authors from 10 institutions, established beyond any doubt that the phenomenologically different decay modes observed up to then (τ, τ', $K_{\pi 2}$, $K_{\pi 3}$, $K_{\mu 2}$, $K_{\mu 3}$, K_{e3}) were due to a single type of particle, which since then has been termed the K^+ meson; the θ^0 was its neutral counterpart. The existence of $K_{\mu 2}$-decay was confirmed definitely, and the different K^+ decay branching ratios were also measured. The $K_{\mu 2}$ mode turned out to be the most frequent one, about 10 times more frequent than the τ mode.

Here at the Pisa Conference, we cosmic-ray physicists (emulsion and cloud-chamber experts) celebrated our final triumph just a few years after the real beginning. The basic properties (decay modes, mass, mean life, etc.) of the new particles, K mesons and hyperons, were known. We were proud to have found all possible decays of the heavy mesons and made sure that there was only one K meson.

The most important event in my life occurred simultaneously with the G-stack conclusion: My daughter Maria was born.

At the same Pisa Conference, the Berkeley physicists brought better proof of this, presenting their first results. At the Berkeley Bevatron, the possibility to expose stacks of emulsions in momentum-analyzed beams offered many advantages. There, the three positively charged types of particles, π^+, K^+, and p, all with the same momentum, had different, well-defined ranges in the emulsion, so that one could proceed directly to the region where the K^+ meson came to rest without scanning the entire emulsion volume. Moreover, the yield of the K^+ decay events was incomparably higher than in cosmic-ray events. The question of K mesons was quickly and definitely settled. The principal result was that all primaries of different decay modes ($K_{\mu 2}$, $K_{\pi 2}$, $K_{\mu 3}$, K_{e3}, etc.) had the same range for the same momentum, meaning that everything found in cosmic rays was a different decay mode of a unique particle.

Therefore the 1955 Pisa conference, which was so successful for cosmic rays, also marked the time when this type of research had to come to an end. The work on the K properties from the Bevatron groups showed clearly that, in the future, conclusions depending on large statistics and accurate measurements were more likely to come from accelerator experiments.

It was, however, the cosmic rays that had initiated the physics of strange particles, which would reveal parity violation and weak-interaction universality. Moreover, cosmic rays continue to produce intense excitement in the year 2002. Recent results on solar and atmospheric neutrino oscillations may very possibly indicate real signs of new physics beyond the standard model. And emulsions are still taken up whenever extremely high spatial resolution is needed.

A central topic of the Pisa Conference was Edoardo Amaldi's report on τ-decay data analysis, performed using the Dalitz plot. The analysis was now based on 106 τ emulsion events, almost three times larger than the sample analyzed at the

Padova Conference. Of the new events, about half came from the scanning of the G-stack's 15-liter emulsions. The rest were produced at Berkeley in an emulsion stack that was exposed to the Bevatron, in which a much smaller volume was scanned. Amaldi concluded that the more τ particles in the plot, the stronger the evidence became that their distribution was uniform, and that the spin-parity of the three-pion state from the τ decay was 0^-.

It was then obvious that if the spin of both θ and τ were zero, the two-pion decay mode had to be scalar, in contrast with the pseudoscalar τ. Two decay modes of the same particle had different parity. That raised a serious issue, the tau-theta puzzle.

The straightforward solution to this puzzle could be simple; it implied parity nonconservation, namely a failure of invariance with respect to space reflection. However, at the time it was almost a credo that mirror invariance—parity conservation—was an a priori law of nature, rather than a hypothesis to be tested by experiment. Therefore, a substantially more complicated situation was suggested: Parity was conserved but the K meson consisted of a degenerate charge doublet, labeled τ and θ, close in mass but with spin-parities of 0^- and 0^+ respectively. Therefore, most of the emulsion groups interested in studying the systematics of K mesons turned to work with emulsions exposed to K beams available at the Bevatron, to further study the $K^+ \to \pi^+\pi^+\pi^-$ decay (14). A lot of empirical effort was devoted in 1955–1957 to measuring the masses, lifetimes, branching ratios, production cross-section, scattering properties, etc., in order to demonstrate some tau-theta differences, but the results were always compatible with zero differences (15).

It was only in 1957 that the tau-theta puzzle was solved. Richard Feynman (16) wrote that it was Martin Block who, at the 1956 Rochester Conference, during a night discussion on the tau-theta puzzle, asked him: "How do we actually know positively that weak interactions conserve parity?" Feynman, after considerable thought, concluded that actually there was no compelling evidence, and the next morning he raised the question at a session. Indeed, Martin had the gift of asking the right people the right questions at the right time. In this story, he reminds me of the little child in "The Emperor's New Clothes."

Tsung Dao Lee and Chen Ning Yang, in their celebrated paper published soon after (17), suggested the startling hypothesis of parity violation, namely that nature discriminates between right and left in weak interactions. They examined the experimental evidence for parity invariance and found substantial experimental support for parity conservation in strong and electromagnetic interactions, but no experimental evidence either to confirm or to refute parity invariance in the weak interactions. Therefore they discussed a series of experimental conditions under which such checks could be made, and suggested several experiments to settle the issue.

Less than a year after the Lee & Yang paper, a series of the crucial experiments they had proposed were done. The results showed that not only P (spatial reflection invariance) but also C (interchange of matter and antimatter invariance) was violated in a variety of weak interaction processes, e.g., β-decay, μ-decay, and π-decay (18), and discrete symmetries—earlier believed to hold true for all interactions—turned out not to be obeyed by weak interactions. This was the most astonishing and revolutionary conclusion of the cosmic-ray emulsion era.

Valentine Telegdi (19) wrote about this period that "the few years following the discovery of parity violation were most exciting. Theory and experiment marched hand in hand. Crucial experiments could be performed by small teams, using modest apparatus, but often entirely novel techniques. It was a time when—to quote a statement made by P.A.M. Dirac in another context—average people could make outstanding contributions."

At the Pisa Conference, Murray Gell-Mann summarized the strange-particle status from the theoretical point of view in an extraordinary talk entitled "The Interpretation of the New Particles as Displaced Charge Multiplets." He set out his scheme for the new particles in a coherent and rather settled form, through the relation

$$Q = T_3 + \tfrac{1}{2}B + \tfrac{1}{2}S,$$

where Q is the particle charge, T_3 is the third component of the isotopic spin, and S is a new quantum number he dubbed the strangeness. So for example S is zero for the proton and neutron, $+1$ for K^+ and K^0, and -1 for K^- and $\bar{K}^0$, which are all isotopic multiplets with $T = 1/2$ (20). Strangeness is conserved in strong and electromagnetic interactions; the weak interactions obey the rules $|\Delta S| = 1$, and $|\Delta T_3| = 1/2$.

It is interesting to recall that at the time Gell-Mann introduced the strangeness scheme, only one K^0 decay mode was known, the $K^0 \to \pi^+\pi^-$. The strangeness scheme demanded the existence of two neutral K particles, K^0 and its antiparticle $\bar{K}^0$, with distinct properties. One of the most fascinating problems in particle physics turned out to be a consequence of the assignment of $S = +1$ to K^0 and $S = -1$ to $\bar{K}^0$. Because the weak interactions do not conserve S, both the K^0 and $\bar{K}^0$ particles can decay into the same final state, such as $\pi^+\pi^-$. Accordingly, a two-step weak interaction such as $K^0 \to \pi^+\pi^- \to \bar{K}^0$ induces mixing between K^0 and $\bar{K}^0$, and therefore neither K^0 nor $\bar{K}^0$ will have a definite lifetime.

Murray Gell-Mann and Abraham Pais (21) then suggested that the true particles of definite mass are linear combinations of K^0 and $\bar{K}^0$, characterized by different charge conjugation eigenvalues.[1] The two new states, K_1^0 and K_2^0, with the assumption of charge conjugation invariance, cannot transform into each other. Their masses, m_1 and m_2, are different; so are their lifetimes, τ_1 and τ_2, with $\tau_2 \gg \tau_1$. The known $K^0 \to \pi^+\pi^-$ could be considered as the short-lived K_1^0 component, and according to the predictions of Gell-Mann & Pais, there should also be a neutral K_2^0 meson with a long lifetime. Both of them should be capable of interacting in both the strangeness modes, $S = +1$ and $S = -1$. The K_2^0 particle was

[1]Gell-Mann and Pais's 1955 proposal came before the discovery of P and C violation in the weak interactions. After 1957, the $K_1^0 - K_2^0$ analysis holds with the substitution of CP for charge conjugation—to the extent that CP is a symmetry of the weak interactions.

detected, according to predictions, in 1956, in two simultaneous experiments (22). The experiment by Lande et al. detected the free decay of K_2^0, and an emulsion experiment by Fry et al. detected its interactions.

It was on the problems of $K^0 - \bar{K}^0$ mixing that I first began to work with W.F. (Jack) Fry. A close collaboration and friendship developed between us from the very beginning (23), one that has flourished ever since.

We have also met frequently in more recent years, the last time during the summer of 2001, when he came as usual to one of the meetings I organize in Venice on neutrino telescopes. He gave a splendid lecture, with practical demonstrations, on music and the violin. The violin, how it works and why, is one of his passions. We have over the years collaborated in many experiments or met just to discuss physics, as well as art, history, Italian literature . . . .

Jack and I concentrated on the fact that the K_2^0 should be a $K^0 - \bar{K}^0$ mixture, and gave the first proof that the long-lived K_2^0 component, built in a K^0 beam, interacts with practically the same probability in both the K^0 and $\bar{K}^0$ modes (24). A $K^0 = (K_1^0 + K_2^0)/\sqrt{2}$ beam, indeed, after a time t such that $\tau_2 \gg t \gg \tau_1$, changes into a mixture of K^0 and $\bar{K}^0$, because meanwhile K_1^0 particles have decayed and the surviving K_2^0 component, which is a mixture of K^0 and $\bar{K}^0$ states, interacts in both strangeness modes.

We then decided to look more closely into the process of $K \rightarrow \bar{K}^0$ conversion in the context of its time dependence. A pioneering experiment was then performed for actually measuring Δm, the tiny $|m_2 - m_1|$ mass difference generated by weak-interaction effects such as $K^0 \rightarrow \pi\pi \rightarrow \bar{K}^0$ (25). Our aim was to follow the development in time of a K^0 beam by detecting the interactions of its $\bar{K}^0$ component.

A beam of K^0s was produced through charge-exchange interactions by bombarding a heavy target with a 750 MeV/c K^+ beam from the Berkeley Bevatron. Emulsion stacks were placed near the target, which allowed observation of hyperfragments produced at distances ranging from 75 mm to 215 mm (i.e., at times from about $2\tau_1$ to $6\tau_1$) from the K^0 source. Forty-nine hyperfragments were detected and their spatial (time) distribution indicated that the most probable value of the mass difference $|m_2 - m_1|$ was $\Delta m = (1.5 \pm 1)\tau^{-1}$ eV. This was the first evidence that the $K^0 - \bar{K}^0$ transition was a two-step weak process (26). However, the result showed that conclusions depending on time measurements were more likely to come from detectors with good time resolution. Therefore we turned to the bubble chambers.

Although seeing K^0 decay events fully contained and clearly illustrated in a single photograph was to me fantastic and exciting, the scanners, experienced as they were on the emulsions, did not appreciate the change; they thought their new work no longer required the skill they had developed in the past. However, as a general trend, analyzing the statistically more significant data sample produced by the bubble chambers required the use of computers, which turned the data analysis into an enterprise involving many workers. And people hunched over microscopes were soon replaced by people hunched over keypunches.

PADOVA-VENICE CONFERENCE 1957: THE FADING OF EMULSION'S APPEAL

The September 1957 Padova-Venice International Conference on "Mesons and Recently Discovered Particles" was characterized by a completely new climate. It was the year of great theoretical success, when the breakdown of parity conservation was confirmed by experiment, the antiproton and the neutrino had been discovered, the K_2^0 detected, neutrino oscillations predicted . . . .

Moreover, by 1957, accelerators had replaced cosmic rays as the principal source of high-energy particles. The stream of results from the Cosmotron and the Bevatron monopolized strange-particle physics, while bubble chambers, counters, and spark chambers were slowly replacing cloud chambers and nuclear emulsions as the principal detectors.

Most of the G-stack collaboration groups soon passed on to a similar effort with stacks of emulsions exposed to the Berkeley accelerator; some of the traditional emulsion groups started to transfer their experience of collaboration, as well as their scanners, to the examination of films from bubble chambers. At the Padova-Venice Conference, many results were presented from emulsions and bubble chambers that were fully consistent with the conservation of isospin and strangeness, thereby providing evidence for the Gell-Mann–Nishijima relation $Q = T_3 + \frac{1}{2}B + \frac{1}{2}S$.

Emilio Segrè himself reported on the discovery of the antiproton, whose production was just being achieved at the Berkeley Bevatron, an important milestone in the further confirmation of the Dirac theory. Studies on the annihilation processes, still largely performed with the use of emulsions, were also presented.

Shortly after the discovery of the proton and neutron antiparticles, I was stimulated by the idea of extending it to the realm of hyperons, which were expected to exist and fit naturally into the present scheme of particles.

The major problem was their threshold energy, too high for their production at the Berkeley Bevatron. However, to me the use of nuclear emulsion seemed convenient as a target and as a detector, and it had the advantage that the threshold energy on a bound nucleon could be considerably less if the struck nucleon were moving in a favorable direction. At the Padova-Venice Conference, I discussed this idea with D.J. Prowse, and we designed an emulsion-stack exposure to a Bevatron high-energy π^- beam. We struck it rich (27), finding the anti-Lambda hyperon shown in Figure 4.

In the same year that the antiproton was observed, 1956, the neutrino was experimentally detected (28) at the Savannah River nuclear plant. The idea to detect neutrinos at reactors was first proposed by Bruno Pontecorvo in 1946 (*Chalk River Report* PD-205, November 1946, http://pontecorvo.jinr.ru/work.html), an idea that Fermi judged ingenious but not immediately achievable. Bruno was a fantastic friend to most of us all his life. He always preserved his youthful enthusiasm. It is a great pity that he did not live longer, to see the triumph of his conjecture that neutrinos oscillate (29).

Figure 4 A drawing of the $\overline{\Lambda}^0$ event. (From Reference (27) with permission.)

At the Venice Conference, on the weak-interaction side, a great deal of attention was given to parity nonconservation searches, and Jack Steinberger presented evidence for parity nonconservation in Λ^0 decay, which had been detected for the first time in purely hadronic interactions.

A large theoretical participation characterized the Padova-Venice Conference (Figure 5). To give just a few examples, T.D. Lee gave a talk on weak interactions with particular emphasis on the more recent work dealing with the two-component formulation of neutrino theory and with the concept of lepton conservation; neutrino zero mass was in the air. Bruno Touschek, whose contribution to the e^+e^-

Figure 5 The Padova-Venice Conference in 1957. A rest in the area of the San Giorgio isle in Venice. From left to right: B. Touschek, T.D. Lee, W. Pauli, and R. Marshak.

storage rings is well-recognized, proposed that a suitable gauge transformation of the neutrino field, imposed to keep $m_\nu = 0$, leads to two-component neutrinos; moreover, he elaborated on the equivalence of two-component and Majorana neutrinos. A.H. Rosenfeld, G.F. Chew, M. Lévy, S. Fubini, and many others discussed the nucleon-nucleon and nucleon-antinucleon forces and pion physics. Here the nucleon itself was becoming an important object of investigation after Hofstadter's recent results. Moreover, some fresh approaches were presented by W. Heisenberg, N. Dallaporta, L. Okun, and others toward models where hadrons are composite particles, foreseeing new symmetries and quarks. "And small fleas have smaller ones, and so ad infinitum"?

One of the most successful theoretical models presented at the conference was the one presented by Robert Marshak and George Sudarshan, leading to the universal *V-A* theory—another triumph for the Fermi theory. Marshak & Sudarshan stated, contrary to the then-current experimental evidence, that all weak interactions are of type *V-A* with $G_V \simeq G_A$, where lepton conservation is incorporated, neutrinos are two-component spinors with ν ($\overline{\nu}$) left (right)-handed, and all particles participate in the weak interactions in the same two-component manner. Here also came the suggestion that the weak interactions arose from the exchange of charged vector bosons, the $W^{\pm}$ (30).

Those were the years in which the particle physics literature began its tremendous growth. It was a time for catching up with each other and for interaction between experimental discoveries, for anticipatory theories and revelations of unexpected facts that open perspectives on new worlds. And of course it was not only in physics, it was happening also in everyday life. It was near the end of the 1950s that my superiors called me, happy to announce that finally a new position for an assistant professor was available. "Of course you are the first on our list," they said—then continued, "Nevertheless, after considerable thought, we decided, since you are married, have a daughter, and therefore already have reasons to be satisfied, to assign this position to the second on our list," a young male.

Soon afterward, a national competition for a full professorship in physics opened up. I competed and I won. And it was my own department that offered me the professorship at the University of Padova, where I became the first woman to obtain a chair since the University's foundation in 1222.

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