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THE ROLE OF COSMIC RAYS IN THE DEVELOPMENT OF PARTICLE PHYSICS

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1. INTRODUCTION

As usual this paper has been written after the colloquium. I have not attempted to dissimulate that fact and some allusions or references to the colloquium are made in the past mood whereas the things I am referring to happened after I had delivered my speech.

For reasons which are known by the participants, my speech was only a very poor approximation of what I had prepared. The written version, by its length, probably overcompensates that fact.

Certain discoveries like the positive electron, the π meson came in one stroke with such clear and convincing evidence that they are easily told. Other like the strange particles, demanded, before final clarification was achieved, a long process of discussion contradictory experiments, controversies. To tell this story in a simplified didactic way will not make justice to all the efforts and will in fact distort the history. To give a full account of how things developed risks to overemphasize the importance of certain results, which in those times represented significant steps in the clarification process, but which, now, might look trivial. At the risk of being boring I have rather chosen this attitude for which I beg the indulgence of my reader, he will have to single by himself what he thinks were the really important facts.

The paper is divided in three parts of unequal importance. The Positive Electron, the Light Mesons, the Strange Particles. Since the subject is elementary particle physics and cosmic rays I have let completely aside some major discoveries like the large air showers and the heavy primaries. I apologize to the authors but my paper being already too long I could not do more than to mention their discoveries.

2. THE POSITIVE ELECTRON

In September 1932 a short article appeared in Science [1] written by Carl D. Anderson and entitled "The apparent existence of easily deflectable positives". The article did not contain the now classical picture of the first

positive electron reproduced in fig. 1. It just described it together with two other events (one is probably a pair) which could be considered as corroborative evidence. The picture represents a particle whose direction of flight is determined by the fact that crossing a lead plate it can only lose energy but not acquire more. This, together with the magnetic deflection, defines the sign of the particle and this sign is plus. Strangely enough for a cosmic ray picture, the particle is going upwards. Ionisation and curvature show that it cannot be as heavy as a proton (in fact it could not be heavier than $20 m_e$) and Anderson therefore concluded that he had observed "a positively charged particle comparable in mass and magnitude of charge with an electron".

The confirmation of Anderson's discovery came very rapidly as a consequence of an invention of great importance for physics: "The counter controlled cloud chamber". Anderson operated its apparatus at random times and observed a cosmic ray if one crossed the chamber during its sensitive time. Blackett and Occhialini [2] triggered the expansion of their cloud chamber by the coincidence of the pulses in two Geiger counters placed at the top and bottom of the chamber. They were therefore, in principle, observing at least one cosmic ray per picture. Most of them contained a single track, but many contained showers. An example of the latter is reproduced in fig. 2^(*). Clearly, the shower contains particles which are curved either to the right or to the left and which seem to come from a common region at the top of the chamber. If this is so, about half of the curved tracks are made of positive particles and in most of the cases comparison of curvature and ionisation indicate a mass much lighter than that of the proton. An interpretation of all the showers as due to negative electrons going up and rebounding at the top of the chamber was rather unrealistic. Furthermore several pictures had been taken with a plate in the chamber and the direction of motion was guaranteed by the loss of energy. In this way, the pictures of Blackett and Occhialini confirmed the existence of the positive electron. They also showed that showers were sometimes started by neutral particles in the middle plate of the chamber. In retrospect, it is to be noted that all that had been established with certainty by either Anderson or Blackett and Occhialini was the existence of positive particles much lighter than the proton. If in those times the people's minds had been used to the idea that there could exist many particles other than electron, proton or neutron (this last discovered also in 1932) the assurance of the authors might have appeared as a jump to conclusion. As a proof that the positive particles were just like electrons, showers who had a globally symmetric aspect between positive and negative particles represented an improvement on Anderson's evidence: i.e. if the

(*) They were not the first showers ever seen. Skobelzin had observed them in a random operated chamber. But, here, they could be studied systematically.

Fig. 1 The positive electron. The particle comes from the bottom, losing energy in the lead plate. This direction and the one of the magnetic field shows that the sign is positive. The ionization is much too weak for a proton.

Fig. 2 A shower coming from the top in the first counter triggered cloud chamber. The overall aspect is symmetric between negative particles (electrons) and positive (positrons) their ionization is too small for protons.

negative are electrons the positive are positrons. Furthermore, Blackett and Occhialini were guided by the hole theory derived from Dirac's equation (Dirac was also in Cambridge). Anderson does not seem to have known the theory in 1932, which makes his discovery even more admirable.

Anyhow the year 1932 with the discovery of the positive electron marks the advent of cosmic rays as a tool in the exploration of the particle world: the physics beyond the nuclear physics which, in the same year 1932, with the discovery of the neutron was going from youth into rich maturity.

Of all the discoveries made with cosmic rays, that of the positive electron is the only one which could have been made in the laboratory at the same time or even before. Indeed Neddermeyer and Anderson on one side, Chadwick, Blackett and Occhialini on the other, confirmed their discoveries with the observation of pairs produced by nuclear γ rays. From then on, the new particles discovered and studied in cosmic rays could not have been produced by any accelerator existing at the time of their discovery.

3. THE LIGHT MESONS

3.1 The mesotron

This was the name given in America to what is now known as the μ meson. Since its baptism with this new name coincides practically with the recognition of what we now call its leptonic nature and since this constitutes the discovery of a new and very important concept, the word mesotron will be used until I reach the time of the introduction of the word μ meson. I shall call "Yukawa meson" the theoretical particle predicted by Yukawa.

The mesotrons constitute 75% or more of the cosmic rays particles observed at sea level. In spite of this fact, it took 4 years after the discovery of the other component of cosmic rays (the showers) to establish the presence in nature of the mesotron. This was certainly due to the reluctance to imagine the existence of particles which did not seem to have any role to play in the explanation of the atom or the nucleus. Yukawa's article was published in February 1935 but it remained largely unnoticed. The idea of the mesotron (a particle of mass intermediate between electron and proton) emerged very slowly from 1934 to 1937. In 1934, Bethe and Heitler [3] published a famous article in which they calculated both the energy loss of an electron by bremsstrahlung and the pair production cross section of a γ and, immediately, cosmic rays refused to lose as much energy as they had predicted. On the contrary the astounding fact about cosmic rays was the ability of single particles to penetrate great thicknesses of matter (Bothe and

Kohlhörster, [4] Rossi [5]). There was nevertheless great difficulty to make the jump: "therefore cosmic rays are not electrons". Indeed, just because electrons lose a lot of energy in crossing matter, true electrons observed in cosmic rays (at sea level) were seen mostly with a moderate momentum, less than ~ 300 MeV/c. On the contrary, the bulk of particles who refused to obey Bethe and Heitler had greater energies and most physicists concluded that the Bethe and Heitler theory broke down at energies greater than 500 MeV or so. In fact, it is very difficult to know what people thought really. In 1935 Auger [6] made it very clear that there were two components in cosmic rays, the soft, i.e. electrons producing showers, and something else which penetrated matter more easily. He did not say, however, what was this something else, or half heartedly assumed they must be protons. Before him, Williams [7] in 1934 had thought that the penetrating particles were protons (positive and negative) but the absence of protons with energies between 300 and 1000 MeV made that conclusion unpalatable. During all the years 1934, 1935 and 1936 the circle narrows around the guilty mesotron. At the end of 1935 Auger and Ehrenfest [8] proved that cosmic rays members of a shower have a large probability to give another shower when they cross a lead plate but that this is not the case for isolated cosmic rays. Street and Stevenson [9] confirmed this in 1936. The same year Anderson and Neddermeyer [10] wrote "It is obvious that either the theory of absorption breaks down for energies greater than about 1000 MeV or else that these high energy particles are not electrons" and they further show that they are not protons either. In the meantime Williams and Weiszäcker had showed, by a shrewd change of frame of reference, that the Bethe and Heitler formulae must be valid even at high energies in the lab system. Nevertheless everybody recoiled from the conclusion. The mesotron was there perfectly visible (for us now) on the other side of the Rubicon but nobody dared to cross the Rubicon^(*). In February 1937 Carlson and Oppenheimer [11] wrote an article in which, using the elementary cross sections of Bethe and Heitler, they calculated the development of a cascade shower and proved that it cannot go very far (in radiation length). In the introduction (more than in conclusion) they stated clearly the usual dilemma. Either the theory (QED to give it its modern name) is not valid at high energies, or high energy cosmic rays are made of particles which are not electrons and they affirm that: if the second alternative is correct, then these particles "are not previously known to physics".

(*) I was a young student in Paris at the time. During a talk (on something I have long forgotten) the speaker (who was not a physicist) said he had recently met a physicist (Auger, Joliot, Leprince-Ringuet? I shall never know) who had told him "Les Américains sont en train de trouver une nouvelle particule". (The Americans are in the process of finding a new particle). This, to me, from my own memories at the time and from what I have read recently, is the best Résumé of the years 1934-1937.

Because of this sentence the article has been heralded by several authors (including Street) as the foundation of mesotron discovery. I, personally, disagree, there is still an alternative explanation which is not discarded explicitly. The authors have put their feet in the Rubicon and found the water too cold.

Two experiments made independently and two sentences written also independently and almost simultaneously were going to establish the crossing of the Rubicon and the existence of the new particle: "the mesotron". Using a cloud chamber with a platinum plate in the middle, Neddermeyer and Anderson [12] studied the energy loss in the plate of all the particles crossing the chamber, they divided them in two classes: (1) members of a shower, (2) isolated particles. The results are shown in fig. 3. Clearly, shower particles show an energy loss proportionnal to their primary one (as foreseen by the bremsstrahlung theory), isolated particles show a large dispersion of losses (there are some electrons among them) but, all in all, they have an energy loss which is independent of the initial energy. Here, the excuse of energy dependance of losses cannot be used since the two groups of particles have the same average energy, low enough to be certain of the validity of the Bethe and Heitler calculation. Furthermore, their ionisation and range are incompatible with a protonic mass. In conclusion, the shower particles are electrons + or - and then the authors write, "Interpretation of the penetrating ones encounter very great difficulties but at present appear to be limited to two hypothesis: (a) that one electron (+ or -) can possess some property other than its charge and mass which is capable of accounting for the absence of numerous large radiative losses on a heavy element or (b) that there exists particles of unit charge but with a mass (which may not have a unique value) larger than that of a normal free electron and much smaller than that of a proton; this assumption would also account for the absence of numerous large radiative losses as well as for the observed ionization. In as much as charge and mass are the only parameter which characterize the electron in the quantum theory, (b) seems to be the better working hypothesis. (The sentence has been underlined by me).

This article was received by Physical Review on 30th March 1937 and appeared in the 15th May issue. At the Washington meeting at the end of April, Street and Stevenson [13] described an experimental set up which was used then for the first time, I think, but which was going to have quite a success in future cosmic rays experiments. It is represented in fig. 4. Two cloud chambers on top of each other. The top one is in a magnetic field and measures the momentum and sign of particles, the bottom one contains lead plates. They are triggered by Geiger counters. The lead filter at the top selects penetrating particles. The particles are observed to have a range larger than a proton could have for the measured momentum and do not show heavy ionisation where protons should. They do not

Fig. 3 The energy losses of cosmic rays in a platinum slab. To the left this energy loss in function of energy, to the right the fractional energy loss, the shaded part represents the shower particles.

Fig. 4 The double chamber system of Street and Stevenson, at the top the magnetic chamber, at the bottom the multiplate chamber. Geiger counters for triggering.

produce showers in the bottom plates and have also too big a range for Bethe Heitler electrons even at energies where the theory has been shown to be valid. Conclusion (of the authors) "it is evident that the penetrating particles cannot be described as electrons obeying the Heitler theory nor can an appreciable fraction be protons". This sentence has been taken from the abstract of the contribution to the Washington meeting but some experimental details come from Street's article in the journal of The Franklin Institute (1939) [14] since a full account of the experiment never appeared in Physical Review. The Neddermeyer and Anderson paper which appeared after the Washington meeting contained a note added in proof which reads: "Excellent experimental evidence showing the existence of particles less massive than protons but more penetrating than electrons..... has just been reported by Street and Stevenson".

So March-April 1937 must be considered as the birthdate of the mesotron. Both evidences are convincing (and similar). Both the two groups are affirmative and do not offer any realistic alternative. It seems, however, that many people found the evidence too indirect and both teams started a hunt for a direct mass measurement of the new particle.

Street and Stevenson started an experiment in a cloud chamber triggered with anticoincidence counters under the chamber to favour slow particles. The expansion of the chamber was delayed to allow drop counting (i.e. ionization measurement). They published in December 1937 [15] the picture of fig. 5. The particle is negative (if it comes from the top) and ionizes much more than an electron and disappears from the illuminated region. It cannot be a proton coming from the bottom. A proton of such low momentum will travel only 1 cm. The best estimate of the mass is $130 m_e \pm 25\%$; the error is large because the ionization is difficult to measure.

Finally, in the middle of 1938 Neddermeyer and Anderson [16] published the text-book picture of fig. 6. In the middle of the chamber is a Geiger counter. The positive mesotron crosses the walls of the counter losing energy, comes out heavily ionizing and stops in the gaz. For people who think that the Street picture is not striking enough, here, there can be no doubt. Truly, the mass measurement by momentum range has not too great a precision because the amount of matter crossed is not accurately known but its value of $240 m_e$ cannot be transformed into $1 m_e$ or $1840 m_e$. Here is the mesotron and so it will be the first mesotron for many people and many years. After 45 years it is however difficult to say what was wrong with the picture of Street and Stevenson and furthermore there is no doubt that the mesotron was firmly established by the two articles of 1937. It is worth noting that neither Street nor Anderson's picture was the first. Fig. 7 reproduces a picture published in 1933 by Kunze [17] with

For reason of space economy fig. 7 is above fig. 6

Fig. 5 Street and Stevenson's first mesotron. The particule, if coming from the top, is negative. Curvature and ionization give a mass of $130 \pm 25\ m_e$.

Fig. 7 This picture was published by Kunze in 1933 with the remarks that the blacker track ionizes more than an electron and less than a proton.

Fig. 6 The classical picture of Neddermeyer and Anderson. The positive mesotron crosses a Geiger counter inside the chamber and stops underneath in the gaz. Curvature and range give a mass of $\sim 240\ m_e$, no errors quoted (N.B. The chamber was relatively insensitive, the decay electron, which should have been seen, is not).

the remark that the heavily ionizing particles ionizes more than an electron and less than a proton. This picture remained unnoticed.

3.2 Is the mesotron the Yukawa meson?

As everybody knows in the beginning of 1935 appeared an article written by Yukawa [18] suggesting that nuclear forces were transmitted by a field and that the quantum of this field was a particle with a mass inversely proportional to the range of the nuclear force i.e. $\sim 200 m_e$. Furthermore, to mediate β decay the particle had to be instable and decay into an electron plus a neutrino. As already mentioned the prediction was either not read or not taken seriously. Neither Carlson and Oppenheimer, nor the experimentalists teams mention Yukawa's idea. Yukawa [19] himself however, suggested the identification of the newly discovered mesotron with his particle as soon as the two articles of 1937 appeared. The idea found general agreement almost immediately and non negligible confirmation in the experimental facts. In their 1937 article, Neddermeyer and Anderson had remarked that, since mesotrons were not found in matter, there should exist a very efficient mechanism to remove them. A radioactive decay could indeed be such a mechanism. Proofs of the instability of the mesotron accumulated rapidly. The first one was a remark made by Kulenkampff [20]. All measurements had shown that the absorption power of different substances for mesotrons was proportional to the number of electrons per cm^3 , but the air of the atmosphere had an anomalous high absorption. Since air has a very low density compared to solid materials, a given amount of matter in (g/cm^2) represents a much longer path in air and the decay in flight of mesotrons will simulate an anomalous absorption. A decrease of cosmic rays intensity with rising temperature (at constant atmospheric pressure) had been observed, which Blackett [21] explained by the lifetime of the mesotron and the raising of the production layer with temperature.

Many experimenters started to measure the lifetime of the mesotron using the so called anomalous absorption in air and all came to values between 2 and 3 microseconds. In 1940 Williams and Roberts [22] using a high pressure cloud chamber, published the first picture of a stopped positive mesotron emitting a decay electron fig. 8 (page 13). In 1941 Rasetti [23] realized the first measure of the mesotron's lifetime with particles at rest. The stopping of the mesotron in a block of iron was recorded by an anticoincidence circuit. The decay electrons were recorded in Geiger counters placed on the side of the stopping block. Circuits of coincidence with different resolution widths showed that there was indeed a delay between the stopping and the signal of the Geiger counters. A not very precise measure of the lifetime could be achieved. Independently, Auger, Maze and Charminade [24] used a genuine delayed coincidence system to arrive at the same

conclusion a little later. In 1943 Nereson and Rossi [25] invented a clever electronic circuit which gave good statistics in a relatively short time. Instead of counting electrons delayed by a fixed (adjustable) time, in their system, each recorded electron gave the measure of its individual delay versus the arrival time of the stopped mesotron. They obtained a value of the lifetime with a precision of 0.1 microsecond (their value is very close to the modern agreed value). Incidentally the comparison between the lifetimes at rest and the lifetime in flight (anomalous absorption of air) confirmed the validity of the relativistic time dilatation.

There were however difficulties in the identification of the mesotron with the Yukawa meson: (a) the lifetime found experimentally was too long by two orders of magnitude for a particle which was supposed to mediate the β decay of nuclei; (b) the cross section with matter was indeed very small for a supposed quantum of a nuclear field. The mesotron had been identified because it could cross meters of lead and that was just contradictory to the identification with a Yukawa particle. In underground experiments the penetrating power was found to be even larger.

The contradiction did not appear, at least to experimentalists, as bad as it looks today. After all, who could tell anything about the interactions of very high energy particles? Nothing said that they should have a constant geometric cross section. And indeed, the idea that the cross section could go down with increasing energy was not so stupid. As we know it now, only hadrons, which are complex particles, have a geometric cross sections but positrons and electrons which are elementary, have an annihilation cross section which goes like $1/s$. So physicists adopted more or less an attitude of wait and see and one should not forget that the war did not leave to many people the time to think about that problem.

In November 1946, in one of the first issues of Progress of Theoretical Physics appeared an article by Sakata and Inoue [26] on the difficulties of the assumption: mesotron = Yukawa meson. (According to the authors the content of the article was already discussed in 1943 at a symposium on cosmic rays, but the printing was delayed by the war). Taking into account the difficulties outlined above they proposed the idea that the mesotron^(*) had something to do with the Yukawa meson but was not to be identified with it. Furthermore they suggested that the mesotron could have spin $1/2$ and be born from the Yukawa meson via the following

(*) As already mentioned at the beginning of this chapter, I am the one who uses systematically the world mesotron for the meson detected in cosmic rays.

scheme

$$y \rightarrow m + n$$

where y is the Yukawa meson, m the mesotron, n a neutral particle of spin $1/2$ which, they added, if it had a zero mass, could be identified with the neutrino. It is fair to say that, at the same time, another Japanese physicist, Tanikawa quoted by the authors, conceived also the filiation theory "Yukawa meson $\rightarrow$ mesotron" although giving to the mesotron the properties of a boson. Anyhow this text establish clearly that since 1943 the true nature of the mesotron was a subject of preoccupation, that the filiation hypothesis was written down 6 months before the first $\pi \rightarrow \mu$ was seen, a year before the fundamental articles on the subject, and that Sakata and Inoue proposed the right choice for the spin of the mesotron.

3.3 The mesotron is not the Yukawa meson

As far as I know, the paper of Sakata and Inoue remained more or less unknown in America and Europe, and the majority of physicists lived more or less happily with a Yukawa meson or several Yukawas mesons: One of these (the pseudoscalar) was the mesotron whose behaviour was not quite orthodox but not so bad either.

In February 1947 appeared in Physical Review a short letter signed by Conversi, Pancini and Piccioni [27]. The experiment it described was in its essence a measurement of the lifetime of the mesotron by delayed coincidence technique. The apparatus is shown in fig. 9. The important difference with previous experiments is the use of magnetized iron slabs above the mesotron stopper. This arrangement, invented by Rossi and Puccianti, allowed the authors to stop at will positive or negative mesotrons. Their results are reproduced in table I and make it clear that if negative mesotrons do not decay in iron, they do decay in carbon while positive mesotrons decay in both materials.

TABLE I

Results of measurements on β -decay rates for positive and negative mesons

Sign	Absorber	III	IV	Hours	M/100 Hours
(a) +	5 cm Fe	213	106	155.00'	67 ± 6.5
(b) -	5 cm Fe	172	158	206.00'	3
(c) -	none	71	69	107.45'	-1
(d) +	4 cm C	170	101	179.20'	36 ± 4.5
(e) -	4 cm C + 5 cm Fe	218	146	243.00'	27 ± 3.5
(f) -	6.2 cm Fe	128	120	240.00'	0

This table reproduces the table of the original publication of Conversi, Pancini, Piccioni. The delayed coincidence are given essentially by III-IV. The corrected rates are given in the last column. That negative mesons decay in carbon and not in iron is clear.

Fig. 8 The first mesotron decay photographed in a high pressure cloud chamber by Williams and Roberts.

Fig. 9 The apparatus of Conversi, Pancini and Piccioni. The magnetic slabs concentrate at will mesons of either sign in the stopping plate (carbon or iron).

This was a terrible blow to the identity: mesotron-Yukawa meson. Indeed Tomonaga and Araki [28] had calculated that a negative meson, coming at rest in matter, would be captured very soon into a Bohr orbit by a nucleus and from there, if it were a Yukawa meson, be absorbed by the nucleus: In this absorption the meson disappears and all its rest energy is used to make a nuclear reaction. The sum of the time needed for the two processes would be much shorter than the decay time and therefore no decay of negative mesotrons should be found. Indeed, until then, Rasetti, Nereson and Rossi had reported that only roughly half of the mesotrons stopped in iron were emitting a decay electron. On the contrary Maze and Chaminade [29] had found that in aluminium most of the mesotrons decayed. However, nobody had worried too much about this discrepancy (acceptance calculation were always rather uncertain).

In the case of the Italian result the effect was clear; negative mesotrons were absorbed in heavy nuclei and not in light ones. Of course the direction of the effect was natural, mesotrons should be more absorbed in heavy nuclei than in light ones since the Bohr orbit is closer and there are more nucleons to be absorbed by. But the non absorption on light nuclei was untenable for a quantum of the nuclear field. Fermi, Teller and Weiskopf [30] calculated that the interaction was too weak by 12 orders of magnitude and furthermore no excuse of strange high energy behaviour could be invoked, since the particles were practically at rest. Even for the great experimentalists of this time, who had no great respect for theory and had seemed remarkably unconcerned by the difficulties of meson theory, twelve orders of magnitude were too much not to be taken seriously. What was happening?

As everybody knows the answer came remarkably fast. During the last years of the war Powell first and then Powell and Occhialini, collaborating with Ilford, had brought the nuclear emulsion technique to a high degree of sensitivity. In 1946 any interested physicist knew that, now, mesotrons could be detected in those emulsions. In May 1947 appeared in Nature under the signature of Lattes, Muirhead, Occhialini and Powell [31], the first two examples of what we call now a π , μ decay. They are reproduced in figs 10 and 11. Remarkably enough the secondary mesons had the same range but, on two examples, nothing could be concluded yet.

In October 1947 appeared two articles by Lattes, Occhialini and Powell [32] with the following main conclusions:

- There are two mesons: π and μ .
- The π decays into a μ and a unique neutral particle because the μ emitted in the decay has always the same range. See table II.
- The best interpretation of the Conversi, Pancini and Piccioni result is to assume that the π 's have a strong interaction and are produced in nuclear

Fig. 10 The first published π , μ decay.

Fig. 11 The second π , μ decay. The μ quits just before stopping but is clearly at the end of its range.

interactions (in particular the ones made by the primary cosmic rays at the top of the atmosphere).

- They decay very quickly into μ 's which have very weak interactions with nuclei and constitute the bulk of the cosmic ray components at sea level i.e. they are mesotrons^(*).
- Mesons stopping in emulsion and giving rise to a star (nuclear reaction) are negative π 's obeying the Tomonaga and Araki prescriptions (fig. 12) (page 19).
- Mesons stopping in emulsions and doing nothing are $\mu^\pm$ (decays electrons could not be seen in the emulsion of those days).

TABLE II

This table reproduces table I of the original publication of Lattes, Occhialini, Powell it shows that the secondary μ , have a unique range in the π , μ decay.

Event No.	Range in emulsion in microns of	
	Primary meson	Secondary meson
I	133	613
II	84	565
III	1040	621
IV	133	591
V	117	638
VI	49	595
VII	460	616
VIII	900	610
IX	239	666
X	256	637
XI	81	590

Mean range $614 \pm 8 \mu$. Straggling coefficient $\sqrt{\sum \Delta_i^2}/n = 4.3$ per cent, where $\Delta_i = R_i - R$, R_i being the range of a secondary meson, and R the mean value for n particles of this type.

In one stroke every difficulty seemed to be solved and the Yukawa meson was found. There were some obscurities though, the π meson was not the mediator of the β decay, since it emitted a μ meson and not an electron and also what was the μ meson anyhow? Nevertheless, if I remember correctly, the feeling of elation and triumph dominated among the physicists: the Yukawa meson had been discovered and therefore the nuclear forces would be soon understood

(*) It is fair to say that Bethe and Marshack [33] had formulated in June 1947, (at the Shelter Island Conference) a two meson explanation of the Conversi, Pancini and Piccioni effect, without knowledge of the two first π , μ examples which had appeared in the May 15 issue of Nature. As we have seen they had been preceded by Sakata and Inoue.

quantitatively; and one did not worry too much about the existence of a new and completely unexpected particle: the μ . One did not appreciate either the importance of its existence. The attribution of two Nobel prizes one to Yukawa and the other to Powell certainly confirms my memory of the events. This feeling has lasted so long that I am afraid I might upset some of my colleagues in saying that in my opinion and in view of what we think today, the order of importance for physics should be reversed. My arguments are the following: The problem of the nuclear forces has found formidable obstacles. The π meson is just the lightest of many mesons which are all considered as complex particles made of one quark and one antiquark, and only its light mass gives it a privileged position as "the Yukawa particle" (maybe).

The μ , on the contrary, was the first example of a particle which is exactly like an electron, but for its mass, or, in other words, a heavy electron which does not give us any clue to an explanation of why it is heavy.

Clearly in the late 40's the perfect resemblance muon, electron was not yet established and therefore not felt as much of a puzzle, but nevertheless one can say that it is the Conversi, Pancini and Piccioni experiment which is at the base of the concepts of leptons and Fermi Universality.

Let us go back to history. The first μ mesons were found by Gardner and Lattes [34] at the Berkeley synchro-cyclotron and from then on were studied with machines and disappear from this talk. μ mesons remained a little longer with us and the first steps towards the realisation of their true nature were made in cosmic rays experiments.

In June 1947 i.e. before the π , μ concept had been established, a letter to the editor appeared in Physical Review. In this letter the author, B. Pontecorvo [35], points out that the meson (in my language convention it should be the mesotron since the μ meson is not yet baptized) bears no resemblance with the Yukawa particle since its too weak interaction with nuclei does not even allow it to be a virtual intermediate particle in the β decay. Therefore, this one ought to be described by the original Fermi description: four Fermion interaction. Furthermore the rate of absorption of the mesotron by nuclei is of the same order of magnitude as that of K capture, provided one takes into account the size of the Bohr orbit and the phase space. The suggestion is therefore made that the mesotron has spin $1/2$ and is absorbed with the emission of a neutrino. This is, in my opinion, the first time where the idea of universality (i.e. generalization of the Fermi interaction to a particle other than the electron) is clearly stated. (In this letter Pontecorvo suggested also that the μ might decay into $e + \gamma$. Together with Hincks [36] he found that this is not the case).

In August 1947 Anderson, Adams, Lloyd and Rau [37] published an article in Physical Review reporting the observation of the decay of a mesotron giving an electron of 25 MeV only. This decay was interpreted as, either that of a new meson (the π already published, not yet named) decaying into an electron and a relatively massive neutral meson, or a proof of a three-body decay of the ordinary mesotron.

Soon enough many people [38] started experiments to decide if the μ meson decayed either in a two-body decay or several different two-body decays, or a three-body decay. All the counter experiments indicated a three-body decay but there were some cloud chamber measures which did not agree very well with that decay scheme and many people (cloud chamber and emulsions specialists of course) thought that absorption measurements of an electron could easily transform a line into a spectrum^(*). The question was finally settled by Leighton, Anderson and Seriff [39] who published in 1949 the spectrum, reproduced in fig. 13, obtained by a cloud chamber experiment. This practically closes the participation of cosmic rays to ordinary particle physics.

To sum up, in 1950, the following facts had been established by cosmic rays: There is a π meson which interacts strongly with nucleons; this and its mass makes it a good Yukawa meson to explain the nuclear forces but the π , μ decay do not allow it to be considered as an intermediate particle in the β decay.

There is a μ meson which decays into an electron and two other neutral particles of small mass, probably neutrinos; when it is captured there is practically no energy deposited in the nucleus and this is probably due to the fact that a light neutral particle (neutrino) is emitted in this capture (more on this subject on page 20). Both facts support the idea that the μ has a half integer spin, probably $1/2$. Also, together with the fact that π does not mediate the β decay, this supports the hypothesis of the Fermi interaction as a direct four fermions interaction involving nucleons, electrons, μ mesons and neutrinos with a unique coupling constant. The absence of the $\mu \rightarrow e + \gamma$ decay was also established in cosmic rays. All other fundamental facts the existence of the π^0 , its decay in two γ 's, the odd parity of the π^- etc. etc. have been established with machines.

To conclude this chapter I would like to add two remarks. At the beginning, the measured π/μ mass ratio was too large and this made the neutral recoil

(*) This being an historical paper I report the state of mind of the times. Absorption measurement can perfectly identify an electron line as shown in 1958 by the discovery of the π , e decay at CERN.

Fig. 12 A meson is emitted in a big star (nuclear event) at the bottom of the picture, travels up, stops and makes a new (more modest) star. It is a π^- absorbed by a nucleus.

Fig. 13 The first spectrum of the electron energy in μ, e decay obtained in a cloud chamber. (As we know now the curve dips much too much towards zero at the high energy end, but the measured points demonstrate clearly the existence of a spectrum).

particle have quite a sizable mass. The smallness of the mass was first established in Berkeley with artificial π 's, but before that, for a little more than a year, the companion of the μ in the π, μ decay could not be identified with a neutrino. This led to the concept that the neutral particle associated to the μ in the π, μ decay and associated to a neutrino and an electron in the μ, e decay was not a neutrino itself and it was said in this conference by several participants, that this difference never disappeared from their minds. I was probably a little too young or slightly too stupid but such is not my recollection. These were the good old times where the fashion was in the economy of particles and not, as now, in the invention of a new one at the first sign of a slight difficulty (Higgs, axions, etc.) and everybody or almost everybody was happy when the neutral meson lost its mass and could be thought of as a neutrino. For instance, Michel, who certainly was and is one of the best experts in this field, would not have discussed with an obvious delight the hypothesis Dirac neutrinos vs Majorana neutrinos if he had been persuaded that the two neutral particles involved in the μ, e decay were in fact essentially different.

My second remark is the following. It is clear from the list of fundamental facts established in cosmic rays physics that they were not all established by the epoch making experiments described above. Some are the results of more obscure work. For instance, the fact that μ^- 's at the end of their ranges do not make any star supports strongly the idea that the μ has spin 1/2 and that a neutrino is emitted in the capture. In his letter, Pontecorvo uses the example of a negative mesotron (μ) stopping in a cloud chamber with no visible star at the end as corroborative evidence to his hypothesis. But it was only a unique case. The emulsions collected many mesons which made nothing at the end of their range and, later on, when emulsions became electron sensitive it was clear that many of these mesons did not emit electrons. However, nobody took the credit for having demonstrated that μ^- meson capture goes like K capture by emission of a neutrino which takes most of the energy, it was already too obvious. Nevertheless it should be clear to everybody that it was an essential piece of evidence for establishing the nature of the μ .

Similarly the weak nature of the μ meson interaction appeared dramatically in the Conversi, Pancini, Piccioni experiment, but the Ticho [40] work which repeated the same experiment on many elements was essential for better quantitative fits. Also, improvements on the original Leighton spectrum of the μ, e decay with the first measure of the Michel's parameter ρ at a value considerably larger than 0 were done in cosmic rays experiments. Even the absence of the reaction $\mu^- + P \rightarrow e^- + P$, one of the clues to the idea of different leptonic number for the μ and the e and which has been proved later with extremely good accuracy by Conversi et al. [41]

was first established within 5% in a specific experiment by Lagarrigue and myself [42] using the μ of cosmic rays.

Anyhow, it is time now to close this chapter of the ordinary particles and to come to the other and possibly most important contribution of cosmic rays to particle physics: The strange particles.

4. THE DISCOVERY AND STUDY OF STRANGE PARTICLES

Before starting I will try to clarify the language. In the time of discoveries it was extremely confused and changing but I would like to keep it as it was, not only to recall the flavour of those times but also not to anticipate conclusions which were not agreed upon in the beginning. For instance nowadays the symbol $K\pi_3$ signifies a decay mode or an event but not a particle since there is only one K particle (with different charges of course). But this was not clear in 1950 and therefore I may speak for the sake of clarity of a $K\pi_3$ particle, meaning "the heavy meson which undergoes a 3π decay" and which in those years was also called τ (a notation I shall use too). I shall also speak of V^0 particles as well as of V^0 events etc. To clarify the understanding one may look at table III which is a table of equivalence between ancient and modern times.

TABLE III

<u>Old</u>			<u>Modern</u>	
	V_1^0	$\rightarrow$	Λ_0	
V_2^0	$\rightarrow \theta_0$	$\rightarrow$	K_1^0	$\rightarrow K_S^0$
anomalous V_2^0		$\rightarrow$	K_2^0	$\rightarrow K_L^0$
SUPERPROTON		$\rightarrow$	Σ^+ Σ^-	
CASCADE PARTICLE		$\rightarrow$	Ξ^-	
τ		$\rightarrow$	$K\pi_3$	
κ		$\rightarrow$	$K\mu_3$	
χ		$\rightarrow$	$K\pi_2$	
$K\mu$		$\rightarrow$	$K\mu_2$	
$K\beta$		$\rightarrow$	Ke_3	

There is another point I would like to clarify from the beginning. I shall refer many times to the Bagnères de Bigorre conference and it will also be mentioned all along this colloquium. I think it is good to explain from the start what it was and why it has made such a durable impression on its participants.

Bagnères de Bigorre is a resort in the South West of France, at the foot of the French Pyrénées. It is very close to the Pic du Midi de Bigorre at which top (2850 alt) there was, and still is, an astronomical observatory. It was a place very suitable for the installation of heavy equipment since it could be reached by road in the summer time and, thanks to the initiative of the director, Prof. Rösch, it was equipped with unique facilities for cosmic rays study; above all, a power line which could feed electromagnets of respectable size and appetite in current. The Manchester group installed its chamber there in 1950 and so did the Ecole Polytechnique in 1952. That is why Bagnères appeared a very suitable place for a meeting of cosmic ray physicists.

The Bagnères meeting was the regular cosmic ray conference held every odd year. The organizing committee decided that the main weight of the conference should be on new particles (i.e. strange in modern language). In fact, on 6 full days of conference only one morning was devoted to other topics and the cosmic aspect of cosmic rays was almost completely neglected. The last one and a half days were entirely devoted to a résumé of the situation as coming out of the preceding days. All participants, young or old, remember the conference as the best of their lives. Indeed, it is there that a coherent picture of the new particle physics began to emerge from many partial works. The existence of many particles with well defined properties was firmly and definitely established here, the Λ^0 , the $\theta^0(K_1^0)$, the Ξ^- . Unsolved problems were at least clearly stated: the nature of K^+ decays, the absence of K^- captures etc. but I should not anticipate too much. In short it is very much due to this conference (and to the first Rochester conferences) that the physics of new (strange) particles began to be considered seriously by "serious" physicists (Serious having, as usual, the sense of either theorists or people busy with classical antiquities and especially physicists combining the two qualities c.f. Gell Mann's talk at this colloquium).

Two years later the cosmos took its revenge. The cosmic ray conference in Mexico city was entirely devoted to topics other than new particles and it is at another conference, at Pisa, that the cosmic rays celebrated a final triumph in that field, only to abandon it definitely to accelerators.

The chapter on strange particles will be divided in three parts. First a description of what I call the forerunners, that is to say the few events which announced that there were still new things in cosmic rays but which remained for quite a long time isolated pieces of evidence. Second I shall describe how the properties of neutral strange particles were established, and then I shall do the same for charged ones.

4.1 The forerunners

The first piece of evidence was the picture published by Leprince Ringuet and Lheritier in 1943 [43]. It showed a positive particle knocking on an electron of the gaz of a cloud chamber. The calculation of the collision (supposed elastic) gave the mass of the incident particle at $990 m_e \pm 12\%$, very close indeed to the actual K meson mass. Till to day the event remains a bit of a puzzle. Knock on electrons, fit to give a good mass measurement of the primary, are very rare, and in the conditions of triggering which were used, the number of K particles crossing the chamber should have been very small. On the other hand, as shown by Bethe, one needs a considerable stretching of errors to change the primary mass into that of a proton. So there are only two possible interpretations: Either the first K meson was recorded in conditions where it should not have been there, or an ordinary proton managed to give a knock-on electron who managed to fake angle and momentum to the extent of giving a false mass measurement coinciding almost exactly with the mass of a truly existing particle. A nice piece of luck in both cases.

The next three events are completely different. They are the first examples of phenomenons, which, later on, were going to be studied extensively.

In 1947 [44], Rochester and Butler published the two pictures reproduced in figs 14 and 15. The cloud chamber was triggered by what was called at the time: "penetrating showers" and would be called now: "high energy nuclear interactions", produced in a lead block above the chamber. This triggering type requests essentially the coincidence between several counters placed in the same horizontal plane (shower signature), but protected by enough lead to insure that the particles of the event are not the electrons and photons of a cascade shower.

The two V shaped events did not show any blob (of a recoil nucleus) at the vertex. They were, therefore, interpreted as the decay of particles, a neutral one in the first case, a charged one in the second. In both cases, however small one assumed the rest mass of the secondaries to be, the mass of the primary remained always larger than $800 m_e$, and close to $1000 m_e$ if the secondaries were identified to mesons. The calculation was straightforward, nothing drastic could be done with measurements errors, so, indeed, new particles had been discovered and all that remained for the fiercest partisans of the non proliferation of particles was the hope that the events were peculiar nuclear interactions. In fact the two events were the first examples of K^0 and K^+ decay.

The last of the advanced evidences for K particle was published in January 1949 by Brown, Camerini, Fowler, Muirhead, Powell and Ritson: "The Bristol group" [45]. Since the discovery of the π , the technique of emulsions had made considerable progress. Electron sensitive emulsions had been produced which meant that any

Fig. 14 The first V^0 is visible to the right just under the middle lead plate.

Fig. 15 The first V^+ is visible to the right of the top part of the chamber, the secondary crosses the lead plate.

charged particle of charge 1 even at relativistic velocity could be recorded. Temperature development^(*) had been introduced by Dillworth, Occhialini and Payne and allowed the uniform development of thick emulsions. The event reproduced in fig. 16 was obtained in a photographic plate, processed by this technique. Unless use is made of a very peculiar twist of the mind (a necessary exercise before a new type of event is accepted at its face value) the interpretation is quite obvious. A heavy particle, but not as heavy as a proton, comes to the end of its range in the emulsion. Mass is measured by ionization-range and by scattering-range and is found close to $1000 m_e$. At the stopping point three particles are emitted, one of them is very slow, stops and makes a two-prong event. It is an obvious π^- making a nuclear disintegration with emission of two protons. The two other particles have a light but not too light ionization (about twice the minimum) they are probably mesons rather than electrons. Indeed, one track is long enough to have its scattering measured, comparison of momentum and ionization completes the identification as a meson. Furthermore, the three tracks are coplanar suggesting a 3-body decay. The almost obvious interpretation is that a heavy particle comes to rest and disintegrate into three mesons one being surely a π^- and the other two μ 's or π 's. The momentum of the slow π is given by range and the momenta of the two others are calculated by momentum conservation. The final result is a mass of $985 m_e$ (error not given) if the three decay particles are all pions.

It was the first example of what is called now a $K\pi$ decay. For a long time the particles decaying into 3π 's were called τ and the $K\pi$ decay the τ decay. It is amusing to note that in the original publication the primary particle was not called τ but κ .

4.2 The neutral V particles. The distinction between Λ^0 and K^0

During more than 2 years the Manchester group worked in the hope of confirmations of the two events published by Rochester and Butler. Nothing happened. Finally in 1950 (Serif, Leighton, Hsiao, Cowan and Anderson) [46] published the observation of 34 "forked tracks" similar to the two first ones of Manchester. Asked by Anderson to give a name to the events but also urged not to use Greek letters, Blackett baptized them V events, V^0 for the one who represented the decay of a neutral primary, $V^\pm$ for the charged ones. The Caltech group established further that among the secondaries of V^0 's events there were "at least one meson: π or μ , and at least a strongly interacting particle: π meson or proton".

(*) See conference remarks (Rossi's paper).

Fig. 16 The first τ ($K\pi_3$) the primary heavy meson (called κ on the picture) comes from left to right and stops. A slow π^- comes down and makes a two-prong star. Two others lightly ionizing particle are emitted from the first stopping point.

In 1950, also, the Manchester group had moved their apparatus to the top of the Pic du Midi de Bigorre in the hope that altitude would break the spell which was stopping them from seeing other V particles. In March 1951 [47] after six months of work Armenteros, Barker, Butler, Cachon and Chapman reported the observation of 36 V^0 's and 7 $V^{\pm}$. Their publication achieved a big step forward. Indeed in 4 of the V^0 decays, the positive particle appeared to be much heavier than a meson (by momentum, ionization comparison) and very likely to be a proton (figs 17 and 18). The corresponding negative particles appeared to be lighter and hence of mesonic nature. In certain other cases of V^0 however, the positive particle was definitely lighter than a proton.

In August 1951 the same authors published in Phil. Mag. a fundamental article which tried to clarify in a systematic way the nature of the V^0 particles [48]. For this purpose, they utilized a parameter α , invented by Podolanski and Armenteros which characterized the symmetry or asymmetry of the decay. α was defined as:

$$\alpha = \frac{p^+ \cos \theta^+ - p^- \cos \theta^-}{p^+ \cos \theta^+ + p^- \cos \theta^-} \quad p^+, p^- \text{ momenta of the decay particles}$$

θ^+ and θ^- angles made by the momenta with the resulting momentum: $\vec{p}^+ + \vec{p}^- = \vec{P}$.

This parameter α can be expressed as a function of quantities defining the decay of the particle in its own frame of reference and its dynamical variable in the lab. frame

$$\alpha = \frac{m^{+2} - m^{-2}}{M^2} + \frac{1}{\beta} \times \frac{2p^* \cos \theta^*}{M}$$

M mass of the primary, the other symbols are self explanatory, * designating quantities in the centre-of-mass system. The meaning of α is evident: The mean value for 2-body decays into particles of the same mass is equal to 0 and different from 0 if the particles have different masses. The width of the distribution is smaller for particles of high energy ($\beta = 1$). α is a measure of the asymmetry of a decay ($\theta^* \neq 90^\circ$) around an average value ($\theta^* = 90^\circ$) which is itself a measure of the asymmetry in masses of the decay particles. The Manchester group summed up its findings in the plot α vs $1/P$ of fig. 19. It is clear that the events do not group around a unique average value of α and that although there is a certain grouping around $\alpha = +0,68$ characteristic of a $P + \pi^-$ decay, the others do not group around $\alpha = -0,68$ but rather around 0 characteristic of a symmetric decay into, say, π^+, π^- . They therefore concluded in favour of the existence of two particles: a sort of superneutron decaying in $P + \pi^-$ and a meson decaying in $\pi^+ + \pi^-$. The first particle was called V_1^0 , the

Fig. 17 The first V_1^0 , the heavy ionization of the positive is clearly seen.

Fig. 18 Another of the first examples of V_1^0 .

Fig. 19 The $1/p$, α plot of the Manchester events. The grouping in two decay modes is clearly seen.

Fig. 20 A typical V_1^0 event of low energy in the multiplate cloud chamber of M.I.T. together with its origin 0 contained in the decay plane of the V_1^0 .

other V_2^0 . This paper is certainly the first critical paper which attempts to give a clear and quantitative description of the new particles, and, as we know now, it is essentially correct apart of an error in the mass of the V_2^0 particle which was measured almost 100 MeV under the true value.

Another objection could be made to the conclusions of the article. Indeed, although it established clearly that there were two sorts of decay the proof that they were due to two different particles undergoing a two-body decay was not quite as convincing. One could consider also that there was only one neutral particle heavier than the neutron decaying by two possible channels: $p + \pi^- + \text{neutral}$, $n + \pi^+ + \pi^-$ and the Caltech group, for instance, took that hypothesis very seriously.

To come out of the dilemma one needed to prove, or disprove, that there were two and only two bodies involved in the decay of at least one type. There were three possible ways to prove two-body decay: (a) Verify that the plane made by the two charged secondaries contained the origin of the V^0 i.e. the apex of the nuclear interaction in which it was produced. That was the coplanarity test; (b) One could check that there was a balance of the transverse momentum components of the secondaries (transverse to the line-of-flight of the V^0 defined as the line joining the origin to the apex of the decay); (c) One could also check the uniqueness, within small experimental errors, of the Q value of a given decay mode: $Q = (M_{V^0} - m^+ - m^-)c^2$ a variable used at that time rather than the mass of the primary.

All these tests had been performed, of course, but their results were not too convincing. For instance Brueckner and Thompson [49] pointed out that in a three-body decay the uncoplanarity angle (i.e. the measure of the non coplanarity) had a most probable value of 0 and that the width of its distribution, calculated for a "reasonable" three-body decay, was not much broader than the one found experimentally. The reason for that was that, most of the time, in cloud chambers operated with a magnetic field, the origin of the V was in some thick piece of lead outside the chamber. There the point of origin was badly reconstructed from the tracks resulting from the nuclear interaction. This resulted in a relatively broad experimental distribution of the uncoplanarity angle. On the other hand, in order to be observed, the V had to live long enough i.e. have a large momentum and this narrowed the theoretically expected distribution in a three-body decay.

The M.I.T. group (Bridge, Peyrou, Rossi and Safford) [50] utilized the pictures of a multiplate cloud chamber. In this case the origin of the V particles were in the plates of the chamber and were accurately measured. Furthermore V's events close to the origin could be observed and had most of the time low momenta: An

example is given in fig. 20 (page 29). They announced at the 3rd Rochester conference in December 1952 and published later in Physical Review a proof of two-body decay of the V_1^0 which is shown in fig. 21. One sees there that the experimental uncoplanarity angle distribution is convincingly narrower than the one expected for a three-body decay. Incidentally the same authors were able to measure the Q value of the V_1^0 at 37 ± 2 MeV very close to the modern value. They used, for this, range measurements and transverse momentum balance. This (although a rather minor point) illustrates the fact that the multiplate chamber which was a long time a "parent pauvre" of the magnetic cloud chamber was capable of producing important quantitative results.

At the same Rochester conference, Thompson [51] announced his results acquired with a magnetic cloud chamber in which he obtained a high accuracy in momentum measurements. His events are displayed on the plot of fig. 22. Every decay is represented in function of α and p_T (p_T = transverse momentum of the secondaries). As one can see immediately from the expression of α given above, for a two-body decay, each point will lie on an ellipse, whose α axis depends of β . In particular all the representative points of fast ($\beta \sim 1$) V particles will be on the same ellipse. If β is smaller than 1, the points will lie outside this ellipse. The parameters of the ellipse depend of m^+ , m^- and the Q value. As one can see in fig. 22 all the points, but few, lie on two ellipses the one centered on $\alpha = 0,69$ and which correspond to the V_1^0 with a Q value of 37 MeV, the other corresponds to a symmetric two-body decay π^+ , π^- and a Q value of 214 MeV. The importance of the work was not only that it proved directly the two-body character of the π^+ , π^- decay, but even more in the precise and correct measurement of the Q values. This gave to the V_2^0 particle a mass very close to that of the τ which was the particle with the $K\pi_3$ decay, a fact which, eventually, established the V_2^0 as the neutral version of the τ . At the Bagnères de Bigorre conference, in conformity with the proposal of Rossi to give Greek names only to those particles which had well established, well defined properties, the V_1^0 received the name of Λ^0 that it still has and the V_2^0 the name of θ^0 which it has lost since to take the one of K_S^0 . I cannot resist the pleasure of confusing the matter a little more by noting that the particle which once was V_2^0 became K_1^0 . Fortunately it became K_S^0 in due time. So after the Bagnères de Bigorre conference the question of the neutral particles was practically settled. The Σ^0 and the Ξ^0 were going to be discovered much later, in bubble chambers, after they had been predicted by Gell-Mann.

For reason of space economy fig. 23 is above fig. 22

Fig. 21 The distribution of uncoplanarity angle δ for V_1^0 . Due to relativity the figure of merit is $\gamma\beta\delta$. The difference with theoretical curves for three-body decay is evident.

Fig. 23 A geometric reconstruction of the first E^- (cascade particle) photograph taken at the Pic du Midi by the Manchester group.

Fig. 22 The α , p_T plot of Thompson the grouping on the ellipses of two-body decays for the V_1^0 and V_2^0 is evident. One point in the middle is a first example of anomalous V_2^0 (K_L^0).

4.3 Charged strange particles

This story contains two parts with completely different characters. On one hand the hyperons Σ^+ and Ξ^- which were established from the observation of very few events because these were very characteristic and yielded an almost complete quantitative information, on the other hand the charged Ks which for many reasons, to be explained later were going to give a lot of work and cause many quarrels before a coherent picture could be established.

The hyperon received this generic name also at the Bagnères de Bigorre conference, it was defined as "any particle heavier than a neutron and lighter than a deuteron (a definition to be reversed if fundamental particles heavier than the deuteron are found)" and the symbol was H. A not too rigorous definition, but what it meant was clear. A generic name had been invented because, besides the Λ^0 , there was good evidence for at least two other particles of this type, heavier than the proton. They were called the superprotons i.e. our Σ^+ and the cascade particle, our Ξ^- . This last one, in particular, was firmly established during the conference on the basis of 4 events only. This was due to the fact that the Ξ^- has a very characteristic signature which gave it its first name "the cascade particle". The first example was published in 1952 [52] by the Manchester group and is reproduced in fig. 23 (page 32). This picture is only a drawing since the original one is difficult to reproduce, it represents two orthogonal projections of the event. A V^- decay followed by a V^0 . The point of decay of the V^- lies in the plane defined by the secondaries of the V^0 . The charged secondary of the V^- is identified as a light particle: electron or meson (π or μ). All this is very suggestive of the decay

$$V^- \rightarrow V^0 + \text{meson.}$$

Unfortunately the point O, vertex of a nuclear interaction is almost in the plane of the V^0 and the hypothesis of a chance coincidence between the V^- and the V^0 could not be completely eliminated.

In 1953, however, at the Bagnères de Bigorre Conference, Leighton [53] reported three other similar examples of $V^- \rightarrow V^0 + \text{meson}$. In one case the light charged secondary had an ionization which identified it as a meson and not an electron. In another case the positive particle of the V^0 was a proton so the V was a V_1^0 . The most striking and almost shocking feature was the negative charge of the primary. This, of course, helped to establish that the four examples represented the decay of a unique sort of particle but, if the neutral particle was indeed a Λ^0 (and this was likely in all cases) then the new particle was a hyperon of negative charge!!! which via a cascade returned to an ordinary proton. Nobody seemed to have wondered very much however, an attitude very characteristic of this time. If some people, influenced by theory, were very surprised not to

find a + charged partner of the Λ^0 , others, the true experimentalists, were ready to accept anything, even negative baryons, from this new and "strange" world revealed by cosmic rays (provided the proof was good, of course).

Anyhow, thanks to its characteristic signature, the Ξ^- was firmly established at Bagnères. The Σ^+ was going to take a little more time. At the Conference, several examples of so called superprotons had been reported. The main features were the following. A particle whose mass was estimated (by ionization and scattering) to be at least as large as that of the proton, decayed (sometimes in flight) emitting a light particle π or μ (or electron). When possible, the scattering measurements allowed a calculation of the Q value (for the decay $\pi +$ Neutron) of about 120 MeV, a value contrasting with the 37 MeV of the Λ^0 . However, the sign of the superprotons remained unknown and, due to the uncertainties of the scattering measurement, even the hyperonic nature of the primary could not be considered as firmly established (the errors on the mass of the primary were of the order of $500 m_e$).

In November 1953 the groups of Milano and Genova published an event recorded in a photographic emulsion which established definitely the existence of the Σ^+ . It is reproduced in fig. 24 [54]. A particle came to rest in an emulsion and its mass was measured at $2500 \pm 600 m_e$. The important fact, however, was that after stopping it emitted a particle with a range of 1255 microns which was as heavy as a proton (as well as it could be established on such a short range) and which did not emit a visible secondary after its own stop. It was therefore assumed that this secondary was a proton and the event represented the first example of $\Sigma^+ \rightarrow p + ?$. If ? was considered as a π^0 the Q value turned out to be 115 ± 3 MeV. This event together with the other superprotons considered as examples of the alternate decay $\Sigma \rightarrow \pi +$ Neutron, established firmly the existence of the Σ^+ with a mass much too large to allow its identification with an hypothetical Λ^+ (isospin partner of the Λ^0). Unfortunately for the glory of cosmic rays, the existence of the Ξ^- was established with the cosmotron of Brookhaven in the same experiment which demonstrated the existence of the associated production mechanism [55].

Before leaving the subject of hyperons I have still to report on a most important discovery made also in cosmic rays even before the Bagnères conference. At the beginning of 1953 appeared in Phil. Mag. a short article by Danysz and Pniewski [56]. It is about a single event reproduced in fig. 25, which can be described briefly in the following terms. A high energy nuclear event is produced at A in a heavy nucleus of the emulsion. The lightly ionizing (high energy) tracks, probably π mesons, have no special interest, but one heavily ionizing track f has a special character. The grain density thins down along the track and

Fig. 24 The first $\Sigma^+ \rightarrow P + \pi^0$ recorded in emulsion by the Milano, Genova Group.

Fig. 25 The first hyperfragment. The track f is a heavy fragment which thins down at the end of its range and then explodes. The explosion is due to the decay of a bound Λ^0 .

at the end of its range there is another "nuclear star" of moderate energy. The thing out is a proof that the particle emitted was a nuclear fragment with a charge greater than 1 which in slowing down collects more and more orbital electrons until it stops. At the end, this fragment has not enough kinetic energy to make the star B, this one has to be the effect of some internal energy. The hypothesis was made that the nuclear fragment contained a Λ^0 bound in a nucleus just like an ordinary nucleon. This, as everybody knows, has been found time and time again and became a topic of physics "per se". It is important, however, to realize that it is an essential fact in the chain of reasoning which makes of hyperons just a new species of baryons, and hence leads to SU_3 , quarks etc.

5. THE CHARGED K's

During almost two years the $K\pi_3$ (τ) of the Bristol group remained the only one of its kind. An uncomfortable situation even for such convincing an evidence. Finally, Harding [57] found two others and since then nobody could doubt the existence of heavy mesons with a mass slightly under $1000 m_e$. In 1951 O'Ceallaigh [58] published in Phil. Mag. two new types of events which are reproduced in figs 26 and 27. In both cases a heavy particle (heavy means heavier than the π meson) stops and emits a secondary. For both events the mass of the primary was measured, at $1385 \pm 170 m_e$ in the first case and $1125 \pm 140 m_e$ in the second. The most important feature of these two events, however, was in the nature and the energies of the secondaries. In case 2 the secondary is a slow particle which stops after crossing 1090 microns of emulsion and then emits an electron without an intermediate π , μ decay, the secondary was therefore identified as a μ . The secondary of the first event is a fast particle, scattering measurement gave $250 \text{ MeV}/c < p < 350 \text{ MeV}/c$: it could be either a π or a μ . If one thought that the two events were examples of the same sort of decay, then it had to be a three-body decay since the energies of the charged particles emitted were so different. The second event by itself made the hypothesis of a three-body decay rather likely. If it were not the case, the neutral recoil particle to the slow μ had to be a neutral heavy meson with a spin different of the one of the primary since a μ meson has spin $1/2$. This multiplication of particles was not very attractive. As we know now, the second event was certainly the first example of $K\mu_3$ decay. The first event was possibly a $K\mu_3$ with a high energy μ , as suggested at this time, but more likely a $K\mu_2$. The new particle was called κ and the proposed decay was $\kappa \rightarrow \mu + ? + ?$

During the next two years data accumulated on the new decay "heavy meson $\rightarrow$ one charged particle". In 1952, Menon and O'Ceallaigh [59], after many measurements of such decays, proposed to split the so called κ decays in two different categories the true κ with the three-body decay $\mu + ? + ?$ and a χ

Fig. 26 The first κ of O'Ceallaigh. The picture, as published, is very dark the secondary goes to the right with high energy. The primary is certainly heavier than a π .

Fig. 27 A heavy meson stops in R and sends, backwards, a secondary which travels 1100 microns before stopping and emitting directly an electron.

Fig. 28 The evidence of the χ ($K\pi_2$) as opposed to κ ($K\mu_3$) as presented at the Bagnères conference.

particle which emitted a π meson with always the same momentum ~ 210 MeV/c, that is in a two-body decay. μ 's and π 's were differentiated by measurement of ionization (grain density) and its comparison to the measured momentum. Fig. 28 shows the results as presented at the Bagnères de Bigorre conference. Nobody was very convinced. At 210 MeV/c, π s can be separated from μ s but very marginally so and all the critically minded physicist at the conference (99,9% of them) could not refrain from remarking that the so called π secondaries corresponded to a hole in the spectrum of the μ secondaries. Nevertheless, as we now know, Menon and O'Ceallaigh were right. The two-body decay $K\pi_2$ exists and the π secondary has almost exactly the energy they found. But the confirmation was going to wait a year. Indeed, at the time of the Bagnères conference the emulsion technique seemed rather in a deadlock for what concerned charged heavy mesons. The mass measurements of the primaries were relatively inaccurate and it was therefore difficult to answer the question: is there a unique heavy meson? The measurements of the secondaries energies were also imprecise (up to the point, as we now know, to change the $K\mu_2$ into $K\mu_3$) and therefore the separation of the $K\pi_2(\chi)$ from the $K\mu_3(\kappa)$ was considered by many people as not well established^(*). Fortunately, also at Bagnères, new results from the cloud chamber gave hope that they could help out of the deadlock. This was rather surprising, since at the beginning the technique had been rather disappointing for that particular subject, but that was due only to the fact that charged heavy mesons were studied via their decay in flight as $V^\pm$.

The first charged V decay had been found, as already seen, by Rochester and Butler and many of them (but much less than V^0 's) had been observed in the successive work of Caltech, Manchester, Bloomington etc. but very little quantitative progress was done. For technical reasons, too long to be explained here, charged Vs are more difficult to exploit than V_s^0 , and as we know now, what was observed was a cocktail of essentially K^+ and Σ^- ; a mixture not easy to disentangle. Therefore all the progress in the study of $K \rightarrow$ one charged secondary, seemed in the hands of emulsions. In 1952 however, the M.I.T. group, [60] using a multiplate chamber, reported the finding of what they called S particles. They were heavy particles stopping in one plate and emitting, after coming to rest, a particle at minimum ionization crossing several plates i.e. an energetic fast light secondary (π or μ). An example is shown in fig. 29 (page 42). A systematic study was published in Physical Review 1953 and more complete results were announced at the Bagnères conference. A priori, one would have thought that the multiplate cloud chambers had too many drawbacks to be very useful in the study of heavy mesons. The mass of the primary could not be estimated to any suitable degree of accuracy

(*) Indeed only the gigantic effort of the G stack, described later, was going to take the emulsion out of this situation.

in spite of the fact that ionization, scattering and range made it clear to anybody that it was heavy. The momentum of the secondary also was very badly measured by multiple scattering in very few plates. But two new facts were important in the publication of M.I.T.: (a) Sometimes, but probably not in the majority of cases, a γ ray was emitted in the back of the charged secondary, (b) the absence of nuclear interactions or stops of the secondary showed that most of them were μ s and not π s. It is amusing, in retrospect, to remember that each of the first four γ s appeared not only backwards of the charged secondary but really opposite to it in the strict sense of colinearity. This gave rise to many speculations on a possible $\pi + \gamma$ decay whereas it was $\pi^+ + \pi^0$. Nevertheless, these were most important results and clearly with characteristics that made them inaccessible to either emulsions technics or magnetic cloud chambers which did not contain plates.

At the end of the Bagnères conference the situation could be summed up in the following way. The τ particle ($K\pi_3$) was well established and its mass was well known: $966 \pm 5 m_e$. There existed certainly a κ particle emitting μ s. Indeed the evidence of O'Ceallaigh where a μ stops and emits an electron had remained unique but it was there. Furthermore, the absence of nuclear interactions of the secondaries of S particles was good evidence for a majority of μ 's in the secondaries. The χ or $K\pi_2$ was there or was not there. Sometimes γ rays were emitted in the decay of particles. The question: were γ rays always there? was not quite well answered. They were not always there in the back of the charged secondary but if they were emitted in any direction like in a three-body decay they could have been missed. The identity of κ and τ was wished for but not proved. (Incidentally nobody seemed to make the following reasoning: Decay of χ resembles decay of $\theta^0 \rightarrow \pi^+ + \pi^-$ also in energy release, therefore $\chi = \theta^+$ and since θ^0 has the same mass as τ , $\chi = \theta^+ = \tau$). The great, conservative idea seemed to be the following, there is no χ and the κ decays in $\mu + \nu + \text{neutral}$, the neutral being probably a γ .

There remained a puzzle that I did not yet mention. Some heavy mesons had been seen which instead of decaying made a star at the stopping point, but there were much less numerous than the decaying ones. Three hypothesis could explain this fact. (a) There is a unique heavy meson but then it interacts rather weakly with a nucleus. (b) There are two heavy mesons, one interacts strongly but is not often seen and the other does not interact at all. (c) Heavy mesons are produced much more abundantly with the positive rather than with the negative sign. Needless to say, this last hypothesis was not taken seriously. We had already heard too much about charge symmetry, charge independance, charge conjugation and nobody (but possibly Gell-Mann who was not at the conference) thought of connecting the absence of negative Ks with the fact that all Ξ 's are negative.

Now it is time to speak about the Ecole Polytechnique experiment. Here I will take the liberty to tell it as a personal story. My motivation is pure vanity, of course, my excuse is that personal souvenirs should be welcome in an historical conference and that indeed they have been used extensively. The Pic du Midi experiment was the result of the first meeting between Gregory and myself. Since 1947 I had been working during summer time with Lagarrigue on an experiment, invented by Lheritier, to find heavy mesons. We were using the same chamber where he had found the famous knock-on electron event reported above, but this time the search was systematic. The momentum was given by the chamber, the range by an electronic anticoincidence method. Already in 1948 we knew that the heavy mesons were not where we looked for them. In 1949 the experiment, suitably modified, gave a reasonably good measurement of the mass of the μ meson and the ratio of protons to μ in the normal cosmic ray component.

However, in the meantime the $\tau(K\pi_3)$ of Bristol had been published and it was practically sure that heavy mesons existed. If such was the case and if they were not to be found in the normal component of cosmic rays, it was, very probably, due to a short lifetime before decay, like for the π meson. Therefore, if a cloud chamber experiment wanted to detect them it was necessary to have a trigger for nuclear events (penetrating showers) to observe heavy mesons just after their birth. But in a measurement of their mass by momentum range (by far the best method) the latter could not be measured electronically as we had been doing (other particles of the shower will fool the anticoincidences counters). Therefore, the evident set up was the one used by Fretter^(*) [61] to measure the mass of the μ meson; two chambers, a top one with magnetic field to measure the momentum and a bottom one to measure the range; the whole being triggered by penetrating showers coming from a lead block placed above the top chamber fig. 30. This set up had a supplementary and decisive advantage: the possibility of studying the decay particle in the multiplate chamber. From the start it was evident that decent statistical work could only be done if the two chambers were large i.e. as large as one would dare to build them. This project was conceived at the end of 1949 but Leprince-Ringuet who concentrated his own work on nuclear emulsions was not persuaded to give the green light (which meant for him to find the money); so in 1950 I had mounted in L'Argentière an experiment based on the same principle, with some simplifications. (One chamber only, a magnetic space at the top, some plates at the bottom of the chamber). The apparatus was too narrow and could not possibly give more than 1 event a year but one always believes in his own luck.

(*) The careful reader will remember that the first to use this set up were Street and Stevenson in the mesotron existence experiment, page (7), but we did not know it at this time.

Fig. 29 One of the first examples of what M.I.T. called an S particle or S event. A particle K^+ stops in the third plate from the top and emits a secondary.

Fig. 30 The two cloud chambers arrangement of the Ecole Polytechnique group at the Pic du Midi. The top chamber measures momentum, the bottom one the range. They are triggered together by a counter system which selects penetrating showers.

At this moment Gregory came back from America with the same apparatus in mind but for quite different reasons. He had been working at M.I.T., in the Rossi group, on nuclear interactions produced by cosmic rays in a multiplate cloud chamber. He was thinking that, for this physics, a system of two cloud chambers would be ideal; π mesons produced in a primary interaction would have their momenta measured in the top chamber, and produce secondary interactions in the plates of the bottom chamber. Clearly there is no reason of dispute between two physicists if they want the same apparatus, even if it is for two different reasons. Furthermore our association was good to convince Leprince-Ringuet. In September 1950 there was a meeting of the Ecole Polytechnique physicists in L'Argentière and the decision to go ahead was taken. Lagarrigue, who had always been with me, and Müller, who had just joined the lab, constituted the team together with Gregory and me. The dual origin of the apparatus appeared clearly in the fact that the first plate assembly was made of alternate lead and carbon plates, an interesting feature for the study of nuclear interactions.

However, in 1952 the S events found in M.I.T. made it clear to all of us that the hot thing was K particles stopping in the bottom chamber. Therefore, the most important figure of merit for that chamber was to be its stopping power, both for primaries (better yield of useful events) and secondaries (better analysis). The group decided unanimously to replace the old plate assembly by 15 plates of copper, 1 cm thick each and that was going to be the essential element for what follows. At the Bagnères conference, all that the Pic du Midi could contribute were 5 measurements of K^+ masses which were all more precise than all the measurements in emulsions. They were all compatible with the mass of the τ ($966 m_e$) but rather on the low side.

Six months later we were able to publish a hypothesis which would question some of the results which seemed well established at the time of the conference. One reason for that was a reexamination of the results announced there. When we looked at the S particles results of M.I.T., we were struck by the fact that, apart of two exceptions, no secondaries had stopped in one of the plates of the chamber. This was rather peculiar if most of the S particles were K emitting μ mesons on a reasonably smooth spectrum of energy. The two secondaries which stopped had the same range and it was the one which was expected for a π secondary of Menon and O'Ceallaigh's χ . Furthermore, as time passed, our own results accumulated and showed the same behaviour. So, in a sense, we had predicted to ourselves around October 1953 the results we were able to publish in January 1954, that is to say that with some exceptions no secondary of S particles will stop unless its direction of emission will give him more than 75 gr/cm² of copper to cross before leaving the chamber and then, it will stop at this "magic" range. Here is, with more details, the reasoning more or less as it was published [62].

"The mass of S particles as measured by momentum-range is either smaller than the one of the τ or can be equal to it, but it is highly unlikely that the heavy mesons we observe may be contaminated by particles of a mass larger than $1000 m_e$. It is also very probable that S particles whose mass has not been measured are of the same type as the other ones. If among all the S particles there are τ 's ($K\pi_3$) we will observe either no secondary at all i.e. all the π 's will remain in the copper plate, or if we observe a secondary it will stop before crossing 15 g/cm^2 of copper, and indeed we observe three events of this type. If a particle χ stops, its secondary will either have a nuclear interaction or stop with a range of 45 g/cm^2 of copper and indeed we observe one event of this type. If secondaries are going considerably further than 50 g/cm^2 of copper, they are μ mesons since π 's of such range cannot be emitted by primaries of mass less than $1000 m_e$. If there is among the S particles a great proportion of κ we should observe stopping secondaries at ranges different from the preceeding ones unless the spectrum of emission has a peculiar shape (peaked at high energies). If the shape of the spectrum is peculiar enough to be a line, the secondary μ would have a range going from 73 g/cm^2 to 76 g/cm^2 of copper depending on the assumed primary mass between $920 m_e$ and $960 m_e$ (if the neutral secondary had a light mass). And indeed we observe all that, ranges greater than 50 g/cm^2 of copper hence μ 's. No stops but the ones mentioned above and two others at the expected range of about 75 g/cm^2 . The comparison between that range and the assumed mass smaller or not larger than $966 m_e$ proves that the recoil neutral particle has a small mass, possibly equal to zero. Therefore, we think that there is a heavy meson which has a two-body decay into a μ meson and a light neutral, probably a neutrino since no γ is detected in the direction opposite to the one of the charged secondary". (End of the reasoning).

In order to avoid any premature identification and also, of course, in the hope that we had a brand new particle we called it K_μ (pronounced in French camu or Camus). It was only a new decay mode and Rossi utilized its name to propose the modern classification $K\mu_2$, $K\mu_3$, $K\pi_2$ etc.

Our rather elaborate argumentation was, for us, absolutely convincing and we were very surprised, but not dismayed, in finding that our publication was considered with a lot of scepticism and even excited some laughter. How could one conclude to the existence of a two-body decay from the coincidence of two ranges only? "Ridiculous". Nobody realized that the non existence of what we called intermediate ranges either in our own work or in the MIT results demonstrated the absence of a three-body decay (in any sizable proportion) and that the two stops at 75 g/cm^2 only confirmed the two-body decay. Our worries, if any, did not last long, new results were coming and no secondary had an intermediate stop and some confirmed the magic range of about 75 g/cm^2 of copper. At the end of 1954 we

were able to write another article confirming our hypothesis [63]. The results are summed up in figs 31 and 32. The first shows the results of the mass measurement. The mass found for the $S(K^+)$ particles is still low. As we found out later, this was the result of a small systematic error of $15 m_e$ associated to a statistical fluctuation of a little more than one standard deviation which had the same sign. It is thrilling to think in retrospect that if the errors had had both the opposite sign our reasoning would have been more difficult. Luck is often part of physics. More important is fig. 32, there the ranges of the secondaries are displayed in a relatively evident manner. The solid line with arrows represent the amount of matter crossed before leaving the chamber, the bolder bars indicate the limits of range for particles which stopped, the dashed arrows indicate the amount of matter the secondaries would have had to cross before leaving the chamber had they not stopped before. The τ decays are clearly seen at the top, one $\chi(K\pi_2)$ decay only has been seen (event 21162) the absence of "intermediate ranges" is blatant. Eight particles have stopped with a range around 75 g/cm^2 of copper they are $K\mu_2$, γ rays are found only with particles which are either τ 's or can be $K\pi_2$. It is amusing to note that six out of the eight of the good $K\mu_2$ secondaries come from primaries which did not cross the top chamber and the two which crossed it did it just when the chamber was in such bad conditions that no mass measurement could be made. Bad luck is also part of physics. From the average range of the 8 secondaries we deduced for the primary a mass of 942 ± 13 pretty close to the truth but still a little low. Figs 33 and 34 show examples of $K\mu_2$ whose secondaries stop (pages 47 and 48).

In the same issue of Nuovo Cimento appeared an article of the M.I.T. group Bridge, De Stabler, Rossi and Sreekantan [64] which confirmed our hypothesis. Their results are represented in fig. 35 (page 49) and they look very much like ours with a better confirmation of the $K\pi_2(\chi)$, less examples of $K\mu_2$ but again the overwhelming evidence of no intermediate ranges, but one associated with a γ .

They found the mass of the $K\mu_2$ at $950 \pm 15 m_e$ in very good agreement with ours but also very close to the one of the τ and their mass of the $K\pi_2$ was 952 ± 11 . Clearly in spite of our hopes of having found a new particle (which was and still is a glamorous thing to do) we were approaching the moment where all the particles (τ , κ , χ , $K\mu$) would be only the decay modes of a unique K ($K\pi_3$, $K\mu_3$, $K\pi_2$ and $K\mu_2$). Another important result of the Ecole Polytechnique team can be seen also in fig. 32. The ratio of stopped positive to stopped negative was 15 to 1 a result easy to interpret now by associated production, but which was at that time of very great importance to explain the small number of heavy mesons nuclear capture in emulsions, which had been one of the unresolved problems of the Bagnères conference.

Fig. 31 The mass measurements of the Ecole Polytechnique in the final publication. As mentioned in the text the average is slightly to low for the K's but the grouping around a unique value is the most striking and important feature.

Fig. 32 The ranges of the secondaries of the K's observed by the Ecole Polytechnique team. The solid arrows indicate the amount of matter crossed before the secondaries left the bottom chamber. The bolder lines show the limits of range for secondaries which stopped. For these particles the dashed arrows indicate the amount of matter the secondaries would have crossed, had they not stopped. The second larger range indicated for event 40 896 is due to the fact that the decay electron was emitted in about the same direction as the μ was going (see fig. 34). A γ indicates the presence of a γ ray correlated with the K decay. There are 8 $K\mu_2$, one $K\pi_2$ and 5 probable $K\pi_3$.

Fig. 33 The first observed K_{u_2} with stopped secondary. The primary comes from the top left, stops in the 10th copper plate. The secondary is emitted upwards crosses 6 plates and stops in the seventh (the third from the top).

Fig. 34 Another example of K_{u_2} , the K is produced in a nuclear interaction A, together with a Λ^0 , it stops in S the secondary is emitted upwards crosses 7 plates stops in C and emits a decay electron which multiplies in plate 4 to give electrons D and E. If C-D is interpreted as the continuation of the μ the range would be too long for a K_{u_2} (see fig. 32, it is the event 40 896). A slight increase in ionization makes the stop at C more probable.

Fig. 35 The ranges of the secondaries observed by the M.I.T. group. The convention is about the same as in fig. 32. The black rectangles indicate the limits of range of secondaries which stopped. They are 4 $K\pi_2$ ranges and 5 $K\mu_2$.

Fig. 36 The miraculous $K\pi_2$ or $K\pi_3$ of the Princeton group. The primary enters at the top left of the chambers and emits, behind a little cloud, five particles: 2 Dalitz pairs and a π going almost horizontally to the left.

That the $K\pi_2(\chi)$ decay was in $\pi^+ + \pi^0$ rather than $\pi^+ + \gamma$ was proved by the fact that the γ 's connected with the decay were not collinear with the π secondary (as they had been in the first 4 examples). In 1954 Hodson, Ballam, Arnold, Harris, Rau, Reynolds and Treiman [65] published an event which in spite of its uniqueness established even better that decay. It is represented in fig. 36 (page 49). A particle decays in flight into "5" secondaries. The interpretation is straightforward, 4 of the particles group in two obvious electron pairs, they are interpreted as Dalitz pairs emitted in the decay of a π^0 instead of the usuals γ 's. There is no way of deciding, if the 5th is a μ or a π meson but if the hypothesis of π^+ is taken the mass of the primary is found to be very close to that of the θ^0 (K_1^0) which is equal to the one of the τ . This unique event established therefore the π^0 nature of the neutral in the $K\pi_2$ decay. It represents, as such, a fantastic piece of luck. Only one event among about 20 000 $K\pi_2$ decays will have two Dalitz pairs and that means one in 80 000 K^+ decays. I wonder if any other example has ever been found later in bubble chambers (if so, it was probably skipped because it had not been foreseen in the scanning rules).

The list of the possible decays of the K particle was still to be completed. In July 1954, Friedlander, Keefe, Menon and van Rossum [66] published a paper which established the K_β decay i.e. the decay known now as Ke_3 . In contrast to the $K\mu_2$ which had required the collaboration of three large cloud chambers (the two of the Pic du Midi and the one of M.I.T.) and a sizable statistic, the K_β was proved with one event. A heavy meson with a mass of $\sim 1000 m_e$, found by scattering, ionization, and range, stopped and emitted a light particle with a momentum of about 90 MeV/c which after 2,3 mm of range made a large angle scattering and became unmistakably a low energy electron. No good explanation of the event could be found but the assumption that the secondary was an electron already at start and that it demonstrated its nature by Bremsstrahlung and large angle scattering. It was the last case of the sort of physic where a unique event, provided it was characteristic enough, could establish a novel fact. But it was in another way that the emulsion technique was going to celebrate its final triumph.

A little before the Bagnères conference the technique of stripped emulsions had been successfully tried. Emulsion pellicles were no longer glued to glass. Sheets of emulsion, 600 microns thick each, were assembled in batches before exposition at high altitudes. These were later glued individually on plates and processed. A coordinate system, photographically printed on them, defined a precise correspondance from sheet to sheet and allowed, in principle, the scanner to follow a trajectory from one strip to the other.

As already mentioned, many or most of the emulsions physicists had remained sceptical about the K_{μ} announced by the Ecole Polytechnique group. They could not believe that their scattering measurements had transformed a monoenergetic μ into a nice continuous spectrum (the true K_{μ} , appeared indeed only in 3% of the cases). They were also aware, as I have noted in my conclusions of the Bagnères conference, that unless a technical jump was made, no significant progress could be made with the techniques used before this time. They, therefore, resorted to an heroic experiment to come out of this deadlock. This is known in the saga of the physicists of this time as the G stack experiment (G for Giant).

The reasoning was simple enough, if indeed there was a heavy meson emitting a μ which stopped only after crossing 75 g/cm² of copper, the same sort of meson when stopped in a stack emits a μ meson which will stop after 20 cm of emulsion. Therefore, if the stack were large enough, in many cases, the secondary would have to cross more than that amount of matter before getting out. In following such a secondary from sheet to sheet one would find the stopping point and verify if the particle was indeed a μ (and not a π) and if the range had always the same value. To the layman the idea looks simple enough. For anyone who ever looked through a microscope into a photographic emulsion the idea of following a minimum ionizing particle through 20 cm, with the added nicety of refinding it at each change of sheet, it looks like the quintessence of a nightmare. But as I said, the people were heroic and also very competent in this kind of sport. So in October 1954 they flew for six hours, at 27 000 meters of altitude, a stack of 250 sheets of emulsion which had a dimension of 37 x 27 cm². The batch had 15 cm thickness and thus the total volume was 15 litres. After processing, the emulsions sheet were distributed between the participating laboratories, Bristol, Milano and Padova. Their results were announced at the Pisa conference in the summer of 1955 and published in Nuovo Cimento [67] in November of the same year. Apart from being the last great experiment of cosmic rays in particle physics the G stack experiment was "une grande première" in modern physics since the list of authors comprised 36 names instead of the two to seven of those times!!

Fig. 37 shows the results. It represents the energies of secondary particles of heavy mesons together with their nature (π , μ , e). The A group represents the secondaries which stopped and therefore gave the signature of either a μ , e decay or a π , μ , e decay, the group B the one which almost stopped (i.e. had travelled 2/3 of their potential range), C the other ones with still good measuring conditions, here π 's are differentiated from μ 's by ionization vs scattering. It is enough to glance at the figure to see the evidence for the K_{μ} , the $K\pi_2$, the Ke_3 . It looked almost as a mockery that the result of such an effort could be summed up in so simple a figure, but on the other hand it had achieved much more than many years of painstaking measurement on not so large

Fig. 37 The results of the G stack. Energies of the secondaries of K stopped in emulsion. The secondaries of group A stopped in emulsion, the one, of group B had more than $2/3$ of their range in emulsion, the ones of group C were long enough still to give a good measurement. The $K_{\mu 2}$, the $K_{\pi 2}$, the K_{e_2} are very clearly visible O indicates μ mesons, $\bullet$ π mesons, $\square$ electrons.

stacks. In fact the G stack was an excellent demonstration of how much it pays to make the effort to have the right instrument for a given subject rather than repeating indefinitely the same, badly adapted, experiment. Fortunately for us, the cloud chamber men, it came only as a confirmation of our work.

So at the Pisa conference the cosmic ray physicists (emulsion, or cloud chamber specialists) could celebrate a final triumph. Less than eight years after the original publication of Rochester and Butler, five years after the real start, all the animals of the strange particle jungle had been found and classified, the Λ^0 , the Σ^+ , the Ξ^- , the K^0 , five different decay modes of the K^+ . And starting from that, Gell-Mann had been able to predict the Σ^0 and the Ξ^0 both to be found later. Alone among the discoveries of this time the Ξ^- had been identified first in a machine experiment.

Nevertheless at the same Pisa conference arrived the first results of the Berkeley Bevatron. A stack of emulsion was exposed to a monochromatic beam of particles. The momentum was such that protons stopped in a preabsorber, π mesons crossed the stack and only K mesons stopped. It was therefore easy to see that the primaries of all possible decay modes had one and unique range i.e. the same mass. Furthermore, if the stack was put at different distances along the beam, the relative ratio of different decay modes did not change, hence the primaries had the same life-time. This experiment decided definitively that there was one and only one heavy meson with many decay modes. It also showed to everybody that the cosmic rays had finished their task in elementary particle physics "our kind of days were over"^(*). We had survived the arrival of the Cosmotron because of the impetus we had but now the story was finished.

6. CONCLUSION

Before summing up the role of cosmic rays in the birth of the physics of particles, it is, maybe, necessary to understand what is behind this notion. After all, the electron, the proton, the neutron and the photon are particles and one could therefore think that particle physics started at the discovery of the electron.

Nevertheless, when those particles were discovered the purpose of physics was to understand the world as it is around us and indeed electrons, protons and neutrons form all the matter we know and photons the light we see. But for the 8

(*) Shane, a movie of George Stevens.

radioactivity and the cosmic rays, Hamlet remark to Horatio^(*) would have been wrong. Everything on Earth and in Heaven was almost perfectly explained, and still is, by the 4 ordinary particles, electron, proton, neutron and photon.

The modern particle physics is something very different. When the mesotron was found, Rabbi is supposed to have said, "who ordered that?". If the answer had been, "Yukawa" the question might not have been too profound but, as we know, they were μ mesons, and the question was justified. Modern particle physics considers on an equal footing what I call necessary and unnecessary particles: This notion meaning: necessary or (apparently) unnecessary to the description of the world around us. Electrons, necessary constituents of every atom, are put together with μ and τ leptons^(**). u and d quarks, constituents of all nucleons present in all known matter from a piece of iron to a neutron star are considered together with s , c and b quarks which I call unnecessary realities. Of course I am aware that in calling some particles unnecessary I just confess my, and, possibly, our ignorance but I just want to underline this absolutely novel and somehow artificial character of the fundamental physics of today which creates its own objects of study.

The purpose of this introduction should now be clear. It is really the cosmic rays which have launched that new aspect of physics. In all fairness, as I have implicitly said, the invention of the neutrino by Pauli was a forerunner of modern particle physics since its only role is to make β radioactivity possible without change of the character of the spin of the nucleus (half integer, or integer). But, apart of that, it is the cosmic rays which opened the new ways and discovered the new horizons of the physics I described above. The fact that the cosmic radiation produces "naturally" the particles to be studied does not change anything to the apparently "artificial" character of these particles "An object has first to be created to be studied".

Two of the discoveries described in this article were predicted by theory: the positron and the π meson but in no case was the hand of the experimenter guided by the theorist. In the case of the positron, Anderson did not know about the Dirac's holes. In the case of the π it was a new experimental method and not a theory-inspired experiment which brought the discovery.

(*) There are more things in Heaven and Earth, Horatio, than are dreamt of in our philosophy (Hamlet Act I scene V).

(**) Here τ is now more the $K\pi$, but the third charged lepton.

But most important for the new aspects of particle physics were the completely unpredicted discovery of the most unnecessary, unwanted particles of this time the μ meson and the strange particles (hyperons and K meson). Here, I must insist, nobody was ever helped by the beginning of a shadow of a theory: I mean, of course, a predictive one. The theory of Bremsstrahlung and ionization were helpful all right, so was classical electromagnetism. I have already insisted on the importance of the μ meson. I shall briefly repeat. It is the first new lepton. Its decay is the first example of a Fermi interaction between 4 leptons without interference of strong interactions, it is therefore one of the corner stones of the universality of weak interactions. Its long life time and its weakness in nuclear interaction make it a favourite instrument of modern physics: As secondary in Drell-Yan pairs, ψ decays etc: and as a probe in deep inelastic experiments.

Last but not least are the strange particles. As I was visiting SLAC in 1976 there was the ψ and later there was the charm and everybody called that "new physics". They were young and they were enthusiastic and they should be forgiven but to me there is only one physics. There can be new aspects of physics but nothing like "new physics". Nevertheless, if something should ever have been called by that name (and I still would not like it) it should be the start of strange particle studies. Stated in modern language, here appeared for the first time a new flavour. Baryons were found which were not nucleons, not even excited ones since they decayed much too slowly. Heavy mesons were found which had all thinkable weak decays with all possible lighter particles (π , μ , e , ν). Here for the first time an entire new world, parallel but not exactly similar to the old one, was discovered without any guidance by theory. (The fact that the final explanation came from the genius of a theorist (Gell-Mann) has made his glory but does not change anything to our solitude, in this time, and our ingenuity).

Abandoning any lyrism, I would like to list some reasons why strange particles were so important.

- (a) They existed: There were hyperons, baryons which did not exist in stable nature but which decayed into normal baryons via weak interactions.

There was a heavy meson which coupled to all possible lighter mesons (π) or leptons, and whose production seemed intimately connected to that one of hyperons. Therefore a new completely "unnecessary world" was born.

- (b) The existence of a $K\pi_2$ and $K\pi_1$ decay of the same particle "The famous (θ , τ) riddle" led to the notion of parity violation in weak interactions.

- (c) The slow decay of strange particles generalized the notion of weak interactions with the nice addition of the Cabibbo angle.
- (d) The existence of strange particles was an indispensable piece of evidence to lead us to the quark model and, this to the best of my knowledge, has never been stated before. Proof: The existence of strangeness led to the old SU_3 (flavour) in the form of the eightfold way. This finally gave the quark model (independently) by Gell Mann and Zweig in which the nucleon contains three quarks, and mesons contains a pair of quark antiquark.

In fact, the existence of three quarks inside the nucleon was just a coincidence with the existence of three flavours. Indeed there are now five flavours and still only three quarks inside the nucleon, and the magic number 3 comes, now, from SU_3 colour. And also the quark model stems from two origins. The resonances, both mesonic and baryonic and the deep inelastic experiments. But it is important to realize that without the third flavour, the presence of three quarks might have remained hidden to us for a very long time. The isotopic spin doublet does not reveal three quarks, only the fundamental octet and decuplet do. (In other words if you want to count 1, 2, 3 it is good to have three signs 1, 2 and 3 at your disposal).

This ironically completes and nullifies my argument on unnecessary particles. The world is made of nucleons, the proton made of three quarks (u, u, d) and the neutron of (u, d, d), the s quarks is nowhere to be seen (but in high energy interactions). However, had it not been there, we might still not know the nature of the nucleons. And in this way the kind of "artificial" character of the particle physics disappears and is justified. So the most important role of strange particles has been to be the catalyzer, the 18th camel, of our actual understanding of the nature of hadrons (baryons, or mesons) and their discovery and study shall remain the everlasting contribution of cosmic rays to our understanding of nature.

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