25 Recollections on the establishment of the weak-interaction notion

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Introduction

I am very glad to have the opportunity to describe for this symposium some early, practically unknown, Dubna work on strange particles. It is quite natural that I would like people to be informed about some of my work, significant in my opinion, performed a long time ago, and the only way of fulfilling such a desire decently is to be invited to take part in a symposium. True, in his delightful talk on strange particles at the 1982 Paris colloquium on the history of particle physics, Murray Gell-Mann mentioned my work.[1] I shall cover mainly Dubna work on new particles performed in 1951–5[2,3] in the context of the notion of weak interaction, a notion that was certainly not taken for granted in the early 1950s, but that had become one of my pet ideas as early as 1947.[4]

Nuclear β decay, the first known weak process, was discovered by Ernest Rutherford about eighty-five years ago. However, not every physicist knows that the notion of weak interaction, a conception much wider than that of the single process of β decay, came to be well established only in the 1950s, that is, about fifty years after the discovery of β rays and about forty years after James Chadwick's discovery of the continuous β spectrum.[5] Here I shall present personal recollections about the way the notion of weak interaction was born and then became well established. Of course, my story is going to be neither objective nor complete. I shall talk about some episodes that I saw with my own eyes or in which I directly took part. Naturally, the reader must keep in mind that I am writing the present note in June 1985. Furthermore, I have been relying mostly on my memory, not on the literature.

I am very grateful to S. M. Bilenky for discussions.

There is no need to recall here the most decisive ideas and experiments, I would say the "final" contributions to the creation of the universal electroweak-interaction theory. I shall limit myself to the evidence in favor of my 1947 idea, that the β decay "is not alone." The processes, other than β decay, that pointed to some kind of universal behavior concerned first the muon and then strange particles. This story begins in 1947 and terminates in 1955.

Muon capture by nucleons and muon decay

Marcello Conversi, Ettore Pancini, and Oreste Piccioni[6] in 1947 demonstrated that the (cosmic) 2.2-μsec-lifetime mesotrons, that is, the muons, do not have the properties postulated for the Yukawa particles: The muon interacts much more weakly with nucleons than the Yukawa particle should.[7] I have already described in detail elsewhere how the experiment of Conversi and others personally influenced my way of thinking. Briefly, because the muon was not the Yukawa particle, there were no compelling reasons to believe that the muon had the properties that were being postulated for the Yukawa particle. Thus, in my opinion, the following questions were entirely open:

1. Why should the spin of the muon be integral?
2. Who said that the muon must decay into an electron and a neutrino, rather than into an electron and two neutrinos, or into an electron and a photon?
3 Is the charged particle emitted in the muon decay an electron?
4. Are particles other than electrons and neutrinos emitted in muon decay?
5. In what form is the nuclear muon capture energy mainly released?

Some of these questions were answered experimentally by E. P. Hincks and myself, and by other groups. I wish to discuss question 5 here in some detail.

The nuclear muon capture energy, I thought in 1947, must be released mainly in the form of neutrinos. The relevant reaction is then $\mu^- + Z \rightarrow (Z - 1) +$ neutrino, very similar to the process of nuclear K capture $e^- + Z \rightarrow (Z - 1) +$ neutrino. I interpreted the similarity of these two processes as a very significant and deep effect, because, as a matter of fact, the rate of nuclear electron capture and that of muon capture are quite close (when proper account is taken of phase-space effects and of the different electron and muon orbit volumes).[4] I excluded the possibility of a chance coincidence and reached the following conclusions:

(i) The muon capture must be a process in some way identical with the β process, proceeding according to the reaction $\mu^- + p \rightarrow$ neutrino + n.[5]

(ii) In muon capture, most of the released energy is "invisible," because it is carried away in the form of neutrinos, a conjecture that is supported by experiments and agrees with (i).

(iii) The muon spin must be $\frac{1}{2}$.

Thus, in 1947, I started to think in terms of weak-interaction processes[4] and understood first that both the muon capture by nuclei and the β decay are processes due to a definite weak interaction existing in nature. It was clear to me that the muon is a sort of heavy electron and that the muon–electron symmetry is taking place under a type of interaction that is properly called weak, thanks to the smallness of the corresponding constant G – the Fermi β decay constant. A similar point of view – namely, to include the muon decay among weak processes – was adopted later[8] by others: Oskar Klein; Giovanni Puppi; T. D. Lee, M. Rosenbluth, and C. N. Yang; Jayme Tiomno and John A. Wheeler.

The original 1947 idea that there exists a muon–electron symmetry in nature was the first hint of a universal weak interaction. (But how far this was still from the 1957 form of such interaction: the $V–A$ theory of Marshak–Sudarshan and Feynman–Gell-Mann, implemented later by the Cabibbo hadron mixing, the Glashow–Weinberg–Salam "final" electroweak interaction with the Higgs mechanism, and the discoveries of neutral currents and of W^+, W^-, Z^0!)

The main physical content of my 1947 idea is still not understood today; it concerns the existence of families of leptons (and families of quarks). Why do such families exist in nature? I must say that the existence of several weak processes, in addition to the β decay process, seemed clear to me in 1947 (much clearer than today). Anyway, my credo in 1947 led to my expectation that there must exist a number of weak-interaction processes in addition to the β decay. Herein I shall be concerned only with processes of the "charged-current" type, although neutral-current processes later turned out to be quite relevant.

Because the weak-interaction conception was first formulated for the capture of muons and electrons, I believed for some time that every weak process must imply the participation of neutrinos. That wrong idea may have slowed down the development of the notion of weak interaction, but the discovery of new unstable particles unmistakably widened the weak-interaction conception to include hadrons.

Strange particles and the weak interaction

I shall not give details about the very important investigations and discoveries of new particles.[9] I am limiting myself to a few particle discoveries, sufficient to illustrate the question about the weak interaction being responsible for the particle decay. In a short period, starting in 1947, a number of unstable new particles were discovered, some electrically neutral and some electrically charged. Among the neutral particles, one could definitely recognize in a cloud chamber those having baryonic charge, later called Λ^0, and decaying slowly according to the scheme $\Lambda^0 \to p + \pi^-$.[9] Besides, it was shown in a very clean way[8] that some charged mesons, now called K mesons, decay into pions: $K^+ \to \pi^+\pi^+\pi^-$. Here, too, the decay is slow, as indicated by the very fact that the meson has enough time to stop in a thick photographic plate before its decay. The properties of Λ^0 and K^+ were, in my opinion, an indication that the decays $\Lambda^0 \to p + \pi^-$ and $K^+ \to \pi^+\pi^+\pi^-$ are due to a weak interaction, probably the same weak interaction that is responsible for the β decay and muon processes. A similar point of view was expressed independently by N. Dallaporta.[10]

At the time, physicists usually reasoned in terms of the Yukawa process, and at high energy only strong processes were considered. However, such a picture would fail to explain the generation and the decay of such (strange) particles as Λ^0 and K. These particles are copiously produced in cosmic rays, but have quite long lifetimes; they demonstrate strange behavior, if one assumes that the process of particle generation is fundamentally the same as the decay process. However, if we assume that the strange particles are generated in strong processes, but decay in weak-interaction processes, then there are no more difficulties. If we assume that Λ^0 and K are generated together, the difficulties connected with the long mean lives of baryons such as Λ^0 and of mesons such as K are resolved together.[2]

In the early 1950s, several theoreticians came once a week from Moscow to Dubna to conduct seminars on a high level, often among them Isaak Pomeranchuk. I presented my arguments briefly to Pomeranchuk, who liked them very much and right away organized a seminar to illustrate the curious properties of hyperons and kaons along the lines I had suggested; that is, the (weak) decays of hyperons and kaons are not due to the (strong) interaction that generates them together. Since 1947 I had been expecting new weak processes; so I was very happy about all this. I felt that the notion of weak interaction became wider once again, but in new processes. Thus, at the time, the weak interaction appeared to me to be a universal interaction acting between any group of four fermions. That was not very far from today's point of view: W decays into elementary particles, leptons and quarks (and only that way), there being some choice of flavor (masses) for the decay products.

On the basis of these simple arguments, I introduced,[2] independently of Abraham Pais,[11] the idea of pair production of the new particles, more exactly, pair production of hyperons and kaons.

The reactions $N + N \to N + \Lambda^0$ and $n + n \to \Lambda^0 + \Lambda^0$

The question of strange-particle generation can be investigated effectively in experiments performed near the production threshold. We investigated experimentally the question of a possible generation of Λ^0 particles in nucleon–nucleon collisions.[3] The method we used was due to a brilliant suggestion of Richard Garwin,[12] who also was investigating the production of

Λ^0 particles. Garwin's idea was that in some experiments it is convenient to register Λ^0 particles by detecting photons from π^0's emitted in the channel $\Lambda^0 \rightarrow n + \pi^0$. Our experiment at the time was interesting, because Marcel Schein and associates[13] claimed to have detected the production of Λ^0 particles, and the question whether or not Λ^0 is produced singly is one of principle.[1,2,3]

In our experiment, in which 670-MeV protons from the Dubna synchrocyclotron impinged on the accelerator's internal carbon target, we reached the conclusion that Λ^0's are not produced, either in the reaction $N + N \rightarrow N + \Lambda^0$ or in the reaction $n + n \rightarrow \Lambda^0 + \Lambda^0$. The absence of the reaction $N + N \rightarrow N + \Lambda^0$ agreed well with the idea[2,10] of the generation of two new particles together.

As for the vanishing value of the cross section for the reaction $n + n \rightarrow \Lambda^0 + \Lambda^0$, this was just the expectation of Gell-Mann (and of K. Nishijima), for reasons that today are obvious to everybody. Two words about our interpretation of this vanishing value, which we were able to give correctly, even without possessing the notion of strangeness.

I had figured out a scheme based on the assumption that there is a strong interaction responsible for the generation of new particles (two at the same time), and conserving the isotopic spin, and the weak interaction responsible for the decays of particles, and not conserving the isotopic spin. The isotopic spin has a meaning only in strong interactions and cannot be determined by weak decays. There arises the possibility of the existence of fermions with integral isotopic spin (e.g., Λ^0) and of bosons with half-integral isotopic spin (e.g., kaons). The scheme allowed one to interpret the failure to observe the reaction $n + n \rightarrow \Lambda^0 + \Lambda^0$ through the assumption that the isotopic spin of the kaon is $\frac{1}{2}$ (that is, $K^0 \neq \bar{K}^0$) and to make a number of predictions. Nevertheless, the notion of strangeness was a very powerful tool without which physics could not have made the great advances it did. As we know now, the physical content of strangeness is that charge multiplets of hadrons are classified by the number $(0, 1, 2, \ldots)$ of some material particles – the number of s quarks – they contain.

In conclusion, I would like to say that at the Pisa conference of 1955, mainly as a result of Gell-Mann's wonderful talk, the notion of weak interaction, which was introduced in 1947,[4] finally became established.

Notes

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2 B. M. Pontecorvo, "On the Processes of Production of Heavy Mesons and V_1^0 Particles," *Zh. Eksp. Teor. Fiz. [Sov. Phys.-JETP] 29* (1955), 140–6.

3 M. P. Balandin, B. D. Balashov, V. A. Zhukov, B. M. Pontecorvo, and G. I. Selivanov, "On the Possibility of Production of Λ^0-Particles by Protons of Energy Up to 700 MeV," *Zh. Eksp. Teor. Fiz. [Sov. Phys.-JETP] 29* (1955), 265–73.

4 B. Pontecorvo, "Nuclear Capture of Mesons and the Meson Decay," *Phys. Rev. 72* (1947), 246–7.

5 It took 15 years before the two particle reactions $\mu^- + p \rightarrow n + \nu_\mu$, $\mu^- + {}^3\text{He} \rightarrow {}^3\text{H} + \nu_\mu$ were directly observed in the experiments of R. Hildebrand and in our own experiments (together with R. Sulyaev et al.).

6 M. Conversi, E. Pancini, and O. Piccioni, "On the Disintegration of Negative Mesons," *Phys. Rev. 71* (1947), 209–10.

7 E. Fermi, E. Teller, and V. Weisskopf, "The Decay of Negative Mesotrons in Matter," *Phys. Rev. 71* (1947), 314–15.

8 O. Klein, "Mesons and Nucleons," *Nature (London) 161* (1948), 897–9; G. Puppi, "On Cosmic Ray Mesons," *Nuovo Cimento 5* (1948), 587–8; T. D. Lee, M. Rosenbluth, and C. N. Yang, "Interaction of Mesons with Nucleons and Light Particles," *Phys. Rev. 75* (1949), 905; J. Tiomno and J. A. Wheeler, "Energy Spectrum of Electrons from Meson Decay," *Rev. Mod. Phys. 21* (1949), 144–52.

9 See, for example, C. C. Butler, "Early Cloud Chamber Experiments at the Pic-du-Midi," in Colloque International sur l'Histoire de la Physique des Particules, *J. Phys. (Paris) (Suppl.) 43:12* (1982), 177–84; R. H. Dalitz, "Strange Particle Theory in the Cosmic Ray Period," ibid., 195–205; W. S. Fretter, "Cosmic Rays and Particle Physics at Berkeley," ibid., 191–4; L. Leprince-Ringuet, "Les Rayons Cosmiques et la Physique des Particules á l'Ecole Polytechnique," ibid., 165–8; C. O'Ceallaigh, "A Contribution to the History of C. F. Powell's Group in the University of Bristol 1949–65," ibid., 185–9; C. Peyrou, "The Role of Cosmic Rays in the Development of Particle Physics," ibid., 7–66; G. D. Rochester, "Observation on the Discovery of the Strange Particles," ibid., 169–75; J. Rösch, "La Venue au Pic-du-Midi du Groupe Blackett et du Groupe Leprince-Ringuet," ibid., 215–18; J. Six and X. Artru, "An Essay of Chronology of Particle Physics until 1965," ibid., 465–93.

10 N. Dallaporta, "On the Mean Lives of Heavy Unstable Particles," *Nuovo Cimento 1* (1955), 962–5.

11 A. Pais, "Some Remarks on the *V*-Particles," *Phys. Rev. 86* (1952), 663–72.

12 R. L. Garwin, "A Search for V^0 Particles Produced by 450-MeV Protons," *Phys. Rev. 90* (1953), 274–8.

13 M. Schein, D. Haskin, R. Glasser, F. Fainberg, and K. Brown (eds.), *Congrès International sur le Rayonnement Cosmique*, Bagnéres de Bigorre (University of Toulouse, 1953).