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STRANGENESS

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Résumé - L'exposé ne prétend pas être une mise au point historique de la naissance du concept d'étrangeté. C'est simplement un récit de souvenirs personnels sur les idées et l'atmosphère de la période 1951 - 1953. Les raisons qui ont guidé l'auteur pour introduire "l'étrangeté" sont développées et il dit comment il a eu à convaincre et surmonter les oppositions.

Abstract - This paper is not a history of the discovery of the strangeness, but rather a contribution to such a history, consisting of personal reminiscences. The atmosphere and ideas of the period 1951 - 1953 are described. The author explains the reasons that led him to introduce the concept of the strangeness and how he had to convince people and to overcome oppositions.

I have not prepared a history of the discovery of strangeness, but rather a contribution to such a history, consisting entirely of personal reminiscences. I will not be able to discuss how Nishijima and his colleagues arrived at similar conclusions. Some of them are here, including Nishijima himself, and I hope that they can comment on it. Also I have not carefully studied the published material, not even my own published material, which is itself very sparse, and so I can't claim in any way to be giving a presentation that belongs in the realm of historical research. Rather it resembles a story told by an old farmer near a peat fire recollecting his youth, or something of that sort. Such accounts are often recorded these days.

Let me try first to recall briefly, especially to the younger people here, if there are any, what it was like at that time, 1951 to 1953.

Strange particles had been discovered experimentally, as you heard from many of those who took part in the work. Such particles were not considered respectable, especially among theorists. I am told (Dick Dalitz, who is here, can perhaps confirm it) that when he wrote his excellent paper on the decay of the tau particle into three pions Dalitz was warned that it might adversely affect his career, because he would be known as the sort of person who worked on that kind of thing. Second, speculation by theorists in the physics journals was not considered particularly respectable. In fact theoretical physics itself had not been respectable during the decade prior to 1948, when the muon didn't have the properties of the meson, and, even worse, theorists dealt with field theory, which, as soon as you tried to correct the lowest order, gave infinity. Apparent defects in theory had led to a situation in which theorists hung their heads in shame all the time and were not taken very seriously. Well, those defects had just been remedied at the time of which I am speaking, but theorists were still not encouraged to speculate. The journals did welcome innumerable articles on perturbation calculations in field theory, even when the coupling was strong and the theory, for example the pseudoscalar meson theory, was not very useful.

Not everyone thought at that time in terms of strong, weak, and electromagnetic interactions, but I was one of those who did. The weak interactions had been unified by Puppi, by Klein, by Lee, Rosenbluth, and Yang, by Tiomno and Wheeler, and so forth around 1949. Another concept that had just recently been clarified was that of baryon conservation, discussed by Wigner in 1949, and no one understood why it should be exact in the absence of a long range vectorial force to accompany the baryon charge. These days one says : "Well, probably it isn't exact". Very simple !

Now the problem of strange particles was in the air in 1951 and 1952, the puzzle of why they were produced copiously, at a reasonable fraction of the rate of production of pions, but decayed very slowly.

I arrived at Chicago as an instructor in January 1952 and I worked on a variety of subjects. One of them was, of course, the $(g \bar{N} \gamma_5 N \pi)$ field theory, which was so popular at that time, but at least Goldberger and I were studying a way to do non-perturbative calculations in that theory. I also collaborated with an experimental physicist called Telegdi on isotopic spin physics in the nuclear domain. These were my main occupations. As a sideline I began to look into the strange particles in the winter and spring of 1952.

Isotopic spin was again much in vogue. Although the charge independence of nuclear forces should have settled the usefulness of isotopic spin many years earlier, as indicated in the beautiful talk by Professor Kemmer, in fact it had been falling out of favor for reasons that were obscure to me ; but there was a great revival of interest in isotopic spin as a result of the work at Chicago, where Fermi and his collaborators found that pion-nucleon scattering was indeed charge independent, so that the pion had isotopic spin one, the nucleon had isotopic spin one half, and the vector sum was conserved. I had always been interested in isotopic spin conservation, and early in the winter of 1952 I began to wonder whether isotopic spin could explain the behavior of strange particles. I tried assigning $I = 5/2$ to a strange baryon, assuming that there would be many as yet undiscovered charged states. Isotopic spin conservation would then prevent the V^0 particle, as Λ^0 was called, from decaying into nucleon plus pion, and conservation of energy would prevent its going into nucleon plus two pions. The Q value was quite well known.

I soon realized though, in thinking about it and also in discussing it with Ed Adams and Murph Goldberger, that the electromagnetic interaction would ruin the scheme , by changing isotopic spin by one unit. I dropped the idea. Many years later I heard that Okun, when he had an idea that sounded good but to which there seemed to be fatal objections, was given the advice that he should publish the idea with the objections. It never entered my mind to do that, but it was done by another physicist, Dave Peaslee, whom I had never met but who had been my predecessor as graduate student and assistant to Viki Weisskopf at MIT, and who was at Columbia. Apparently he had the same idea, found the same objection, and published the idea with the objection. It appeared as a letter to the Physical Review on April 1, 1952. (No connection intended with the "poisson d'avril"). I didn't read the article at the time, I only glanced at it for a few seconds, but a couple of days ago I tried to read it and found it difficult to follow.

A few weeks after that, probably in May of 1952, I paid a visit to the Institute for Advanced Study, where I had spent the previous year. While I was there someone asked me whether I had read Peaslee's letter. I described the situation, and I was then asked to get up and talk for a few minutes in the seminar room on the idea and why it wouldn't work. I don't recall exactly who was there but I think Francis Low, T.D. Lee Abraham Pais, and various others. In my explanation, as I got to the $I = 5/2$ proposal I made a mistake, a slip of the tongue, and said $I = 1$. I paused and didn't go on with the talk for a minute or two because I was thinking to myself " $\Delta I = 1$ or 0 , $\Delta I_z = 0$ are the rules for electromagnetism ; if we need $\Delta I = 1/2$ and $\Delta I_z = \pm 1/2$ for decay electromagnetism will have trouble doing that, and the problem is solved. "I went on, but at the end I said : "by the way, a few minutes ago I got what I think is the right idea. If this V particle belongs to a triplet, plus, zero and minus, with $I = 1$, electromagnetism will have great difficulty causing decay ; we don't know of any kind of electromagnetic interaction that will change isotopic spin by a half unit, or the

z component by a half unit, and so the decay can be weak "I might have gotten very excited about it at that time but in fact the audience was not very enthusiastic.

Let me say a word now about getting ideas in that way. Years later in Aspen, Colorado, we had a discussion at the Aspen Center for Physics about how one gets ideas in physics, in poetry, in painting, and in other subjects. There were two painters, one poet, and a couple of other people. I spoke about this incident involving a slip of the tongue. The others spoke about some of their problems. It was agreed that in all these quite different domains one sometimes tries to achieve something that is not permitted by the traditional framework. It is necessary to go outside the usual framework in some way in order to accomplish the objective. In theoretical physics this frustration usually appears as a paradox. But a paradox is after all just one way of having your path blocked ; in art the blocking is manifested differently. Having filled your mind with the problem and the difficulty you may then find that in an odd moment while driving or shaving or while asleep and dreaming (as in the case of Kekulé and the benzene ring) or through a slip of the tongue as in this case one may suddenly find the path unblocked. Perhaps the solution comes, in the language of the psychoanalyst (a language that is not very popular in scientific circles today), from the preconscious mind, the portion of our mind that is just out of awareness.

To return to May, 1952, the audience, as I said, was not very enthusiastic. Abraham Pais came up and started to tell me that he had just written a long paper on associated production of strange particles with an even-odd rule. The strong interaction allowed even plus odd going to even plus odd or even plus even going into odd plus odd, but only a weak interaction would allow odd into even. My idea as I had described it (and I had mentioned that it would obey this kind of rule) was, he said, just a subcase of his idea and therefore not very important. What I should have done was to point out quickly that after all there were some differences. Isotopic spin was already familiar and not a new ad hoc symmetry. I would lead to an additive conservation law for I_z with experimental consequences. For example, neutron plus neutron going to what we would now call Λ plus Λ would be forbidden whereas that was allowed according to his scheme and was supposed to be one of the principal tests of the idea of associated production. Also the charge multiplets would be seen to conform to the new idea singlet and triplet for the baryons, doublet for the mesons, and this could be verified by observation of the states. But I didn't like to stress the importance of my own work and I didn't say much.

In my subsequent papers I have often started, in explaining the work on strangeness, from associated production and from the elegant paper of Abraham Pais. In fact, though I was unacquainted with his work and did not proceed from associated production. I learned about associated production just as I invented the scheme. But logically, for purposes of explanation, it was better to discuss associated production first and then the special idea of the connection with displaced isotopic multiplets, (for example, in the Scientific American article that I wrote later with Ted Rosenbaum). Historically, it was inaccurate.

Now associated production as it turned out had been treated earlier, particularly in Japan in 1951 in the Progress of Theoretical Physics by three sets of authors : by Nambu, Nishijima and Yamaguchi, by Oneda, and by Miyazawa. I have just tried to read, in the last few days, the papers by these authors and although I have never referred in my subsequent work to Miyazawa, I noticed that his paper was actually very good. He used the bound state approach but that didn't make much difference ; the effect of it was to predict more or less the correct situation of associated production. Oneda's work was somewhat less perspicuous but he certainly had the notion. Nambu, Nishijima, and Yamaguchi wrote up in an encyclopedic manner all possible explanations, but laid particular emphasis on associated production as being an interesting possibility although apparently contradicted by experiment. They even included the idea of high angular momentum as one possible explanation in their series of letters. Theirs is a very nice piece of work that is not usually mentioned. Feynman told me later that he had thought of the idea of associated production in 1951 and immediately began to talk with the Caltech experimentalists who were doing some very good work on strange particles. I don't believe they are represented here, but theirs was one of the important laboratories at that time. They told Feynman that associated production

did not seem to be correct. Cosmic rays were apparently not ideally suited for finding associated production and the experimentalists discouraged Feynman from continuing in that direction. He therefore took up the idea of high angular momentum as the way in which a particle could be restrained for a long time from decaying while being produced copiously. Fermi, on a visit to Caltech, discussed the same thing and the two of them collaborated a little bit at long distance on the idea of high angular momentum as an alternative explanation.

From Princeton, I proceeded to make my first visit to Europe in June 1952. Here in Paris, Bernard d'Espagnat, whom I knew from Chicago, kindly introduced me to the research group from the Ecole Polytechnique and Louis Leprince-Ringuet generously invited me, a complete stranger, to the meeting of his research group at his country home in Courcelles-Frémoy in Burgundy. Peyrou was away, unfortunately, and it was some years before I met him. But among those present were Bernard Gregory, whom I knew slightly from our graduate days at M.I.T., Louis Michel, Jacques Prentki, André Lagarrigue, Francis Muller, Agnès Lecourtois, and many many others, some of whom are here today.

It was a wonderful experience to meet them and in many cases we have been friends now for thirty years.

At Courcelles-Frémoy I gave the first talk on strangeness after the slip of the tongue in Princeton, but I went very easy on isotopic spin, because at that time it was considered extremely difficult to explain to experimentalists.

On returning to Chicago in the Fall of 1952 I related my idea in detail to the weekly seminar of the Institute for Nuclear Studies (now named after Fermi). It was a kind of Quaker meeting where one could get up and say anything one wanted. Fermi was unfortunately absent, but Dick Garwin was there and at the end of my little talk he was very negative, saying he couldn't see what use my idea could possibly be. Again, if I had been less averse to promoting my ideas, I would have explained that it had all sorts of experimental consequences such as the distribution of charge states, the prohibition of $n + n$ going to $\Lambda + \Lambda$, and so forth. He was at that moment engaged in the experiment on $n + n$ giving $\Lambda + \Lambda$, which gave a negative result. (I am told that Pontecorvo, by then called ИОПТЕКОПВО, did this experiment independently in the Soviet Union, but Garwin was doing it just then in Chicago). If only we had conversed in more detail the world would have become aware of strangeness much sooner, I should mention that Garwin later apologized handsomely for his skepticism.

I became discouraged again and put away strange particles for a while. I worked with Goldberger on the crossing theorem, dispersion relations, and other exact general results extracted from field theory. Early in the summer of 1953 I went to Urbana, Illinois, where Francis Low and I did our work on the renormalization group and on the spectral formulae for propagators, published more than a year later. It seems that I could not publish anything without leaving an interval of at least a year or a year and a half.

I mentioned the strangeness idea in its complete form to Francis Low and T. D. Lee, who had both been present at the slip of the tongue a year earlier. They were somewhat impressed, but I think not very much, probably because I did not explain things very forcefully. At that time I disliked giving a clear presentation in the didactic style, probably in reaction to my father who was a private teacher of languages and had a very didactic style.

I returned to Chicago late in July 1953, when it was terribly hot. I found a draft induction notice. The secretary of the Institute Director had failed to send in the yearly notification to the draft board of my being engaged in research at the University of Chicago, and the draft notice was the result. I imagined that I would immediately be drafted and sent to Korea. The fighting was over but guard duty in Korea would not be ideal for working on theoretical physics and I decided to write up strangeness immediately, after fifteen months' delay, on the grounds that it would be amusing to have this in print while I was over there in the Army.

I never did go into the Army but I did write up the paper. Valentine Telegdi kindly lent me a desk in his air-conditioned lab on which to do the writing. Only equipment was thought to require air-conditioning at that time ; there was no air-conditioning for brains. I often wanted to have a wax pencil that would melt at the same temperature at which I became incapable of thinking, so that I could say I needed the air-conditioning for my equipment, but it never worked. However, on this occasion air-conditioning was available and, at the desk of Valentine 's Manchurian student, I started writing up the idea of strangeness.

Meanwhile, Pais had come in the summer of 1953 to the idea that the even-odd rule he had proposed the year before would come from an orbital isotopic spin, which would be added to an intrinsic isotopic spin to make the total, and that it was the parity of this orbital isotopic spin that would give the even-odd rule. I heard in the summer of 53 that he was invited to the world conference in Kyoto and that he was making a big splash there with these ideas about orbital angular momentum in isotopic space. Jealousy was another reason why I decided I would put forward the strangeness scheme. I thought that it was probably correct and I resented the publicity being given to the scheme of Pais, which I was convinced was wrong !

In my letter to the Physical Review, I placed great emphasis on the conservation of I_z , which is equivalent to the conservation of strangeness, and I showed how $n^+ + n^- \rightarrow \Lambda + \Lambda$ is forbidden. (I still referred to Λ as V_1^0 but it had just been named Λ at Bagnères de Bigorre). I pointed out that $\pi^- + p^+ \rightarrow \Lambda^0 + K^0$ or $\Sigma^- + K^+$ is allowed but $\Sigma^+ + K^-$ is forbidden (here I use the names we invented later).

At the same time I wrote a companion piece, which is exhibited in the next room. In August 1953, they were distributed together as preprints to laboratories all over the world. The companion piece was called : "On the Classification of Particles" and it went much further than the other preprint. It went into great detail on the multiplet structure and on the existence of doubly strange cascade particles, of which two had been seen, one by Armenteros et al. and one by the group at Caltech. Herb Anderson, who was most enthusiastic about strangeness right from the first day he heard about it, had brought me a copy of the preprint from Caltech and had challenged me to explain the cascade particle and to include the explanation in my written work. I predicted what we now call the Ξ^0 particle to accompany the Ξ^- and I also suggested that to explain the decay in two steps via Λ we should postulate that weak non-leptonic strange decays obey $|\Delta I_z| = 1/2$ (or strangeness changing by one unit).

Although I didn't use the word strangeness yet, I did have the quantity, which I called y , and in effect gave the formula

$$Q = I_z + \frac{N}{2} + \frac{y}{2}$$

In fact, I described each particle as equivalent to N nucleons and y π mesons, and I explained that "equivalent to" meant having the same difference between I_z and charge. Evidently N is the baryon number.

I wrote a third paper around the same time (about September 1953), which I didn't even distribute widely as a preprint, in which I suggested that not only was $|\Delta I_z|$ equal to a half in weak nonleptonic decays but also that ΔI was approximately equal to a half, thereby explaining Dalitz's work on the isotopic spin of the pions in τ decay. You start with isotopic spin one half for the τ particle, you add a half unit, and you get either one or zero. But in the charged state of pions you can't have $I=0$, so you have only $I = 1$, and therefore the three pions in the final state have a pure isotopic spin of one to the extent that $\Delta I = 1/2$ is correct. Why I didn't publish the second and third preprints right then I don't know. It seems that I just had to let things ripen for a year or two.

Much of my work was included the next summer in the article in the Proceedings of the Glasgow Conference, July 1954, which I wrote together with Abraham Pais. We included three models in that paper, after discussing the high angular momentum hypothesis, which we said we didn't believe. First we gave his orbital isotopic angular momentum scheme, then my strangeness scheme, and then a third one, which generalized strangeness to restore the symmetry around charge one-half in the baryon system. We said that

the last model was very speculative and also that it didn't appear to be right, because the experiments didn't seem to find the extra states. Looking back on it in the last few days I realized that what the third scheme amounts to, if we supply a couple of missing states, is assuming a charmed quark with a mass similar to that of the strange quark.

Anyway I then didn't bother to send the 1953 preprint "On the Classification of Particles" for publication since its content was mostly included in a section of the joint paper for the Glasgow meeting in 1954.

Now let me return to the paper that I did send off in August 1953. It is also on display in the next room: Isotopic Spin and New Unstable Particles. That was not my title, which was: Isotopic Spin and Curious Particles. Physical Review rejected "Curious Particles". I tried "Strange Particles", and they rejected that too. They insisted on: "New Unstable Particles". That was the only phrase sufficiently pompous for the editors of the Physical Review. I should say now that I have always hated the Physical Review Letters and almost twenty years ago I decided never again to publish in that journal, but in 1953 I was scarcely in a position to shop around.

They also objected to the neutral boson being different from the neutral anti-boson; that was a very sore point. Their referees couldn't understand how K^0 could be different from \bar{K}^0 . I didn't know what to do to convince them. I tried saying merely "It's all right, they can be like that," but failed to change their minds. Then a thought occurred to me. I decided, in order to learn about neutral mesons, to look up the paper by Nick Kemmer in which he had proposed the isotopic triplet. I had met him that previous summer of '52 in Cambridge, where he was very kind to me and I was very impressed with him. I discovered that a large portion of his paper was devoted to showing that a neutral boson does not have to be different from its anti-particle! What he did was to take the Pauli-Weisskopf theory of the charged scalar particle and take away the charge, which left him with a neutral particle different from its anti-particle. Then he argued at great length that it was not absolutely necessary to have it that way. He wrote the complex field as a real field plus i times another real field and pointed out that it was possible to use just one of the real fields and omit the other. In that way he was able to get what we now call the π^0 and adjoin it to π^+ and π^- to make the isotopic spin triplet. When I recounted this story to the Physical Review they finally agreed that it was O.K. to have a neutral boson different from its anti-particle.

In the meantime, though, I was reminded that you could take the two real parts and consider them as real fields if you wanted to. That was to be useful later.

Another thing I had to do for the Physical Review was to explain that the generalized Pauli principle was applicable to fermions with integral isotopic spin and to bosons with half integral isotopic spin. It was widely believed that there was a mathematical demonstration that fermions had to be isofermions and bosons had to be isobosons because that was the only way the Pauli principle could be generalized to include isotopic spin. It simply wasn't true, and I succeeded in pointing that out.

Around the same time, August or September 1953, the first accelerator results were being obtained on strange particles. One or two associated production events were observed. I called Brookhaven to find out in the case of the charged associated production whether they had seen what we would now call $\Sigma^+ + K^-$, which would make me unhappy, or $\Sigma^- + K^+$, which would be good. (At that time Σ^\pm was called V_1^\pm). I phoned Brookhaven and got hold of Courtenay Wright, an experimentalist from Chicago who was visiting Brookhaven. He asked: "What possible difference does it make? who cares?" I said merely "I care; please find out". He asked me to hold the phone, he was gone quite a while, and when he came back he said: "I checked and they are sure that it is V_1^- ". I let out a cheer over the telephone, which mystified Courtenay Wright, but which meant that the one event that had been seen was compatible with strangeness.

On a visit back to Urbana I saw Geoff Chew, who had been away during the summer. Chew was much taken with the cosmic ray result that K^+ production predominated over K^- production and he said mine was the first theory he had ever heard of that would

explain it. He gave a colloquium a couple of days later in which he presented the strangeness theory from that point of view, as an explanation of the predominance of K^+ over K^- . T.D. Lee wrote me to say that he had just invented a new scheme, but that it occurred to him that maybe it was the same one that I had told him about (It was in fact the same). That was a very nice thing for him to do.

Fermi then returned to Chicago and I went to see him. That was an important moment. He sounded very skeptical when I told him about explaining the strange particles by means of displaced isotopic spin multiplets. He said he was convinced more than ever that high angular momentum was the right explanation. I was a great admirer of Fermi; I also liked him very much and enjoyed his company. I was unhappy when he rejected my scheme. A day or two later, though, I did something that no gentleman is supposed to do, I read someone else's mail. I was in the office late in the evening, and out of boredom I started to look at what our secretary (I think her name was Vivian) was typing. It was a reply from Enrico to Giuseppe Cocconi, who had written him that he was looking at the consequences of Fermi's and Feynman's proposal of high angular momentum for the new particles and that he had gotten some nice mathematical results that he wanted to communicate. Enrico wrote him as follows, more or less, (I paraphrase because I don't remember the exact words or indeed the language) : "Dear Cocconi, I was pleased to receive your results. However, I should tell you that here at Chicago Gell Mann is speculating about a new scheme involving displaced isotopic spin multiplets and perhaps that is the explanation of the curious particles rather than high angular momentum". I stopped instantly being depressed, but for a while I was somewhat annoyed at Enrico.

On a visit to New York and Princeton in September 1953, I gave the name strangeness to this quantity γ , and after talking with Serber and Lee at Columbia I decided that it was necessary to postulate a triplet and a singlet, that the mass difference was just too great between V_1^0 and the charged V_1^\pm 's for them to form a triplet and that there must be a Σ^0 which decayed by γ emission to Λ . I was predicting three new neutral particles : Σ^0 , Ξ^0 , and, with K^0 being different from \bar{K}^0 , a second neutral K particle, the properties of which I was then thinking about. All of those neutral objects were the objects of experimental searches during the next year or so.

In June of 1953, at Bagnères de Bigorre, it had been recommended that baryons be assigned a capital Greek letter and mesons a small Greek letter. I decided to use Σ for the triplet, Ξ for the new doublet. For the bosons it was very complicated. We had θ for the decay into two π 's and τ for the decay into three π 's, and people were very confused about the relation between the two. Some wanted to use K for θ and τ together although it is not a small Greek letter, and was intended as a generic term for strange bosons. I wanted to use K, but κ was assigned to a leptonic decay mode discovered by O'Ceallaigh et al., and so we were stuck with the Latin capital letter K.

Back in Chicago I gave a colloquium and then or later Fermi attacked at least one of my ideas, namely my statement that the electromagnetic interaction would have $\Delta I = 1$, $\Delta I_z = 0$. Fermi said it was not necessarily true, that in fact it could also have $|\Delta I_z| = 1/2$. $|\Delta S| = 1$ and he wrote down an electromagnetic interaction that would have that character, namely

$$F_{\mu\nu} (\bar{n} \gamma_{\mu\nu} \Lambda + \text{herm. conj.})$$

a gauge-invariant coupling through which n and Λ would be interconverted directly by the emission of a photon. I replied to Fermi that he was violating what I considered to be a fundamental principle of electromagnetism, namely that electromagnetism doesn't do dirty little jobs for people, but has a coupling that flows directly from the properties of matter. In fact, as I explained it in the new few weeks, thinking about Fermi's objection, we have the physicist's equivalent of the biblical "Fiat lux" "Let there be light" which looks like this : Take the Lagrangian without light and then let p_{μ} go into $p_{\mu} - eA_{\mu}$. This is what I called pompously the principle of minimal electromagnetic interaction. As Valentine Telegdi kindly pointed out to me, it was merely a generalization of Ampère's law.

Fermi made another objection at a course that I gave during the Fall of 1953 (or perhaps the winter of 1954) in Chicago. Whenever Enrico came to a seminar, a lecture, a colloquium, or a course, if he didn't like anything he interrupted. The interruption was not a minor matter ; it continued until Enrico felt happy about what the speaker was saying, which often took essentially forever, that is to say the seminar ended, Enrico was still not happy, and the speaker never finished what he was going to say. If it was a course, as in this case, the course could be blocked for a week or two, while at each class he came in and started objecting where he had left off at the end of the previous class. At my course his principal objection was to the idea that one could have a neutral boson different from its anti-particle. I thought, "Here we go again, just like the Physical Review. I only hope he isn't the referee with whom I had all the trouble". Finally he came up with a clinching argument. He said, "I can write $K^0 = A + iB$, where A and B are both real fields with definite charge conjugation, and you have in each case a neutral particle that is its own charge conjugate". Well, I had already been through this and I was able to answer : "Yes , that's true, but in the production of strange particles, because of strangeness conservation it is the K^0 and \bar{K}^0 that matter ; in the decay, if it is into pions or photons or both, then it will be your A and B that matter and that have different lifetimes." I don't remember whether my reply was delivered in class or privately afterwards. I think it must have been the latter because I hadn't explained the strangeness theory in detail to the class. Anyway, Fermi's objections gradually subsided. This was the origin of the work on K_1^0 and K_2^0 because A is K_1^0 and B is K_2^0 .

I didn't write it up though for another year and when I did it was with Abraham Pais, who gave me much encouragement in publishing the idea. Again it required that peculiar neurotic gestation period of a year or a year and a half before I could manage to publish. It is very strange !

Of course charge conjugation, which was so important in this argument, later had to be amended to CP, but then the argument went through exactly the same with CP as it had previously with C. Finally CP was found to be violated too and even K_1^0 and K_2^0 got slightly mixed, but that is of course a much later story, dating from 1964.

I think it was early in 1955 at Rochester that I discussed the weak leptonic decays of strange particles, with the rules $\Delta I = 1/2$ and $\Delta S/\Delta Q = +1$, but I am not sure whether the discussion appeared in the proceedings.

In July, 1955, in Pisa, I finally gave a straightforward full and didactic presentation of all these ideas in public, and published it. Nishijima also waited until about then to give a full presentation of the whole scheme, with the classification of particles, the selection rules, and everything. He also must have thought of all these things earlier and perhaps he can explain the delay in his case ! Meanwhile the experimental labs had been sent copies of the preprints, even the ones that weren't published. They all knew of the predictions of Σ^0 , Ξ^0 and the second kind of neutral K particle, K_2^0 (K_1^0 was the θ^0 , which was very well known) and various experimentalists very kindly sent me beautiful signed photographs of the events in which these predicted particles were unambiguously found. Jack Steinberger was particularly nice about sending me such photos and in his published comments about the usefulness of my predictions. Later on, in another connection, he sent Feynman and me a photograph inscribed "You may stuff this and hang it".

During those years I was concerned with renormalization group, with dispersion relations, crossing relations, and combining these with unitarity to make a theory of the S-matrix, and so forth. Strange particles were only a part of my work. But they helped me to get those souvenirs from my experimental friends that I treasure to this day.

Thank you.

DISCUSSION

N. KEMMER.- May I add a brief observation on Prof Gell Mann's account of finding in my 1938 paper support for his proposal to introduce a neutral particle with a distinct antiparticle ? In fact, when I wrote that paper, that kind of particle seemed the more natural thing to have and my lengthy discussion on how to introduce a neutral "pion" that had a real state vector was supposed to be a justification of this strange step. When Pauli and Weisskopf first showed how to quantize a charged boson field it was easy to see the link between their field, based on what we then always called the relativistic Schrödinger equation which later somehow get to be called the Klein-Gordon equation) and the non-relativistic Schrödinger equation. Schrödinger's expression for probability density stood in a very simple relationship to the Pauli-Weisskopf charge density : confined to wave packets with only $E > 0$ or only $E < 0$ components they were essentially the same thing. Whether the particles described were charged or not, this seemed the natural way of interpreting the relativistic Schrödinger equation. The relation between the charged and uncharged bosons on this view was the same as between electron and Dirac neutrinos. The equivalent step to passing from Dirac to Majorana neutrinos for the boson case was just to make the Pauli-Weisskopf ψ real. This seemed to present a problem : there was no longer an easy way of linking anything in the Pauli-Weisskopf formalism to the non relativistic probability density $|\psi|^2$. I think I was quite clear in my mind that this point could be settled and in 1946 I asked my research student at Cambridge K. J. Le Couteur to do this (Proc. Camb. Phil. Soc. 44(1947) 229). I think that the "great detail" of my discussion of this point in my paper is explained by my awareness of this problem.

Professor Nishijima was then invited to give the contribution he had been asked to prepare on the subject of strangeness :

K. NISHIJIMA.- The history of the discovery of strangeness has been told in detail by Professor Gell-Mann, and I would like to add to it personal reflections from another corner of the world.

The experimental observations of V particles by the Pasadena and Manchester groups ¹⁾ gave us a strong stimulus to start working on this problem. The natural question to be asked was that of how to reconcile their abundance with their longevity. Years before we had a similar problem and its solution was given by the recognition of the existence of two kinds of mesons, π and μ . Such an idea could work only once, however, and could not be extended to cover V particles.

Many groups in Japan started to work on this challenge, and each group reached its own solution. In order to compare and exchange ideas among them, a symposium was held on July 7, 1951 in Tokyo. There were reports by Nambu, Yamaguchi and myself ²⁾ from Osaka, by Miyazawa ³⁾ from Tokyo, and by Oneda ⁴⁾ from Tohoku.

Although various models of V particles had been presented by different groups, everybody had recognized that one thing was almost in common. That was the pair production of V particles. It struck all of us that this could be the only way to prevent V particles from decaying rapidly through strong interactions. At that time, however, cosmic ray experiments did not seem to support this idea. We had to wait for the Cosmotron experiment ⁵⁾ in 1953 to confirm the pair production of V particles.

Meanwhile various theoretical ideas had been reorganized and reformulated. For instance, the formulation of the pair production of V particles in terms of the so-called even-odd rule by Pais ⁶⁾, and the reformulation of various selection rules originally discovered by Fukuda and Miyamoto ⁷⁾.

In the even-odd rule one assigns an integral quantum number to each hadron. What is relevant is whether that quantum number is even or odd, and one assigns a sort of parity to each hadron. Let us call it V parity to distinguish it from space parity. One assigns even V parity to nucleons and pions and odd V parity to V particles. Then this multiplicative quantum number is postulated to be conserved in strong interactions or in production processes, but it is then postulated to be violated in weak interactions or in decay processes.

I called this multiplicative quantum number the V parity tentatively, but it was really a forerunner of the space parity in the sense that both are conserved in strong interactions but are violated in weak interactions. It is interesting to recall that the violation of space parity in weak interactions shocked the world whereas the corresponding aspect of V parity slipped in without calling any resistance.

Introduction of the multiplicative quantum number was not sufficient, however, in interpreting the experimental data that had been accumulated by then. First of all, the cascade particle Ξ^- decaying into $\Lambda^0 + \pi^-$ had already been known. In order to forbid Ξ^- from decaying into this channel through strong interactions one has to assign even V parity to Ξ^- , but then one cannot forbid the decay $\Xi^- \rightarrow n + \pi^-$ through strong interactions.

Another difficult problem was the interpretation of the positive excess of the heavy mesons then known experimentally⁸⁾. The identified charges of the most of the observed heavy mesons were positive, and it was one of the key problems to explain this property since the multiplicative quantum number was of limited capability.

From a theoretical point of view we did not have a basic principle which enabled us to assign V parity to each hadron.

A great leap forward was made when the cosmotron at Brookhaven started to operate in 1953. The experiment by Fowler, Shutt, Thorndike and Whittemore⁵⁾ clearly revealed the pair production of V particles that could not be confirmed by cosmic ray experiments. The abundance of V particles also assured us of the fact that they are produced by strong interactions.

Since strong pion-nucleon interactions are charge independent and observed deviations from it are rather small⁹⁾, strong interactions of V particles must also respect charge independence in order not to disturb the charge independence in pion-nucleon interactions.

Once charge independence is assumed for V particles the next step is the isospin assignment to V particles, through which the concept of strangeness emerged.

Therefore, I think that the key issue in the introduction of strangeness consists in the charge independence hypothesis. Once this postulate is made, everything follows automatically. Charge independence is respected by strong interactions but is violated by electromagnetic interactions and small mass differences among members of an isospin multiplet. I_3 , the third component of the isospin is respected by both of them. It is violated only by weak interactions such as the beta-decay. These properties remind us of V parity, and it seemed to be convenient to describe the properties of V particles in terms of I_3 .

At that time the only established hadrons were nucleons and pions :

$$p, n \text{ and } \pi^+, \pi^0, \pi^-$$

The relationship between the charge and I_3 may be most simply given by

$$\Delta Q = e\Delta I_3$$

The increase of I_3 by one results in the increase of the charge also by one unit.

Strangeness or hyper charge is introduced as a constant of integration of this difference equation.

$$Q = e\left(I_3 + \frac{Y}{2}\right), \quad Y = B + S \quad (2)$$

From the cosmotron experiment it was natural to assign $I = 0$ to Λ^0 and $I = 1$ to Σ^+ , (Σ^0), Σ^- , where Σ^0 was not directly seen and was assumed to decay into $\Lambda^0 + \gamma$ in a short time. Then we had to assign $I = 1/2$ to the heavy mesons or the K mesons.

If one considers a system consisting of pions and nucleons, one observes that there is a connection between isospin and ordinary spin. Namely, both of them must be integers or half-integers. What is new here is that the isospin assignment of V particles does not respect this rule. The assignments $I = 0$ to Λ and $I = 1$ to Σ are readily acceptable, but the assignment $I = 1/2$ to K^+ , K^0 implies a new feature in that their antiparticles K^- , \bar{K}^0 form a separate isospin doublet. Two points should be emphasized here. First K^+ and K^- do not belong to the same isospin multiplet. This gives a clue to the understanding of the positive excess mentioned already. Second, we encountered for the first time a neutral boson which is different from its antiparticle. So far we had known only γ and π^0 , which are identical with their antiparticles. In the beginning I doubted whether such an assignment was right, but after discussing this subject with Nakano 10) I was convinced that this should be the only possibility. The K mesons kept playing the most important rôle in particle physics for many years to come, providing such subjects as θ - τ puzzle, CP violation and so on. They entered the history of particle physics as the most important object next only to the hydrogen atom.

Now we come back to the question of the cascade particle. One assigns $I = 1/2$ to E^+ , E^- although E^+ had not been observed at that time. The multiplicative selection rule based on V parity failed to account for the metastability of E^- , because of the presence of two decay channels of opposite V parities. Now V parity can be identified with $(-1)^S$. The additive quantum number S can be utilized to formulate a more detailed selection rule than the multiplicative one. Since $S = -2$ for the cascade particles, their instability can be explained by postulating a selection rule

$$\Delta S = 0, \pm 1 \quad (3)$$

for weak interactions. The V parity selection rule cannot forbid processes obeying $\Delta S \equiv 0 \pmod{2}$.

After completing these isospin assignments to V particles we have learned from Professor Nambu that Professor Gell-Mann was also developing a similar theory 11). These recollections exhaust what I wanted to say in addition to what Professor Gell-Mann has told us about strangeness.

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C. PEYROU.- One has often accused the cosmic rays physicists of making difficulties to the Gell-Mann scheme in not finding associated production. In fact before September 1953 the cosmic rays physicists were asked to verify the Pais theory which predicted the production of $\Lambda_0 \bar{\Lambda}_0$ pairs. They said they had no evidence for it and they were right. True associated production was almost impossible to prove in the complicated situation of cosmic rays events. Emulsions could not see Λ_0 's; K_0 's have only $\frac{1}{3}$ probability to decay in π^+ , π^- ; K^+ were very difficult to detect in a systematic way. There was in MIT chamber the beginning of an indication that when you see a K^0 you had good chance of seeing a Λ_0 but on a very poor statistics.

O. PICCIONI.- It is interesting to note that the suggestion that a large angular momentum could explain the long life of strange particles, rejected by Fermi as mentioned by Gell-Mann, was the same as the suggestion of Niels Bohr to explain the non capture of muon in carbon (what Bohr called "Pinocchio effect"). There also, the large l should have explained a discrepancy of $\sim 10^{10}$. In the case of the muons Fermi with Teller and Weisskopf showed that the hypothesis of a large angular momentum was untenable.

R. DALITZ.- Yes, as Gell-Mann said, pion physics was indeed the central topic for theoretical physics in the mid 1950s, and that was what the young theoretician was expected to work on. The strange particles were considered generally to be an obscure and uncertain area of phenomena, as some kind of dirt effect which could not have much role to play in the nuclear forces, whose comprehension was considered to be the purpose of our research. Gell-Mann remarked that he spent the major part of his effort on pion physics in that period, and I did the same, although with much less success, of course.

Fashions have always been strong in theoretical physics, and that holds true today as much as ever. The young physicist who is not working on those problems considered central and promising at the time, is at a disadvantage when he seeks a post. This tendency stems from human nature, of course, but it is unfortunate, I think, that the system operates in such a way as to discourage the young physicist from following an independent line of thought.

There is one aspect of Gell-Mann's scheme which I have not heard mentioned here, namely the $\Delta I = 1/2$ rule for strange particle decays, which he proposed at a very early stage [1954, I believe]. This rule gave a simple explanation for one fact which puzzled the early workers, namely that the $\theta^0 \rightarrow \pi\pi$ lifetime was about 100 times shorter than the K^+ lifetime, despite the fact that the K^+ meson had so many additional modes of decay. Strictly applied, this rule forbids the θ^+ mode, $K^+ \rightarrow \pi^+\pi^0$, since the K^+ meson has $I = 1/2$ and the $\pi^+\pi^0$ system for $J = 0$ has $I = 2$ only, while allowing the θ^0 mode $K^0 \rightarrow \pi^+\pi^-$. The observed rate for the θ^+ mode may be due to electromagnetic effects or, more likely, to deviations from a strict $\Delta I = 1/2$ rule in the weak interaction itself. This rule also gave correct predictions for the ratios of the various $K \rightarrow 3\pi$ decay modes and for the Λ and Σ hyperon decay amplitudes, as well as for all semi-leptonic decay modes. In fact, the dominance of the $\Delta I = 1/2$ component in the weak interaction has been successful everywhere it has been tested, whereas its theoretical origin has remained quite obscure, as far as the non-leptonic decay modes are concerned. The reason for the validity of this rule is not yet understood.

R.A. SALMERON. - The first examples of associated production of (K^0, \bar{K}^0) pairs were obtained in cosmic rays by the Manchester-Jungfrau-Joch cloud chamber group. In both pictures the V^0 's were seen to decay in the cloud chamber and identified as K^0 's and the K^+ was identified by its ionization. These were the first examples of production of two neutral kaons with opposite strangeness, as well as that of a positive and a neutral kaon (Il Nuovo Cimento 5 (1957) 1388). Two examples of associated production of (K^+K^-) pairs had been previously reported by two emulsions groups (Il Nuovo Cimento 2 (1955) 666 ; 2 (1955) 828).

Y. YAMAGUCHI. - In 1951, we discussed on these V-particles. It was true that there were few examples of associated production of V's seen in cloud chambers. If you would analyze them statistically as usual, you might find that V-particles production would be dominantly of single production. Nevertheless, since production and decays of V's must be controlled by different interactions — otherwise we could not understand them at all —, I firmly insisted upon the idea of pair production of V's. Some cosmic ray theorists (and experimentalists) including S. Hayakawa, however, objected naturally me saying that there were no evidences for pair production from their statistical analyses of cloud chamber photos.

In early 1953, Hayakawa and Nishijima wrote a review article (in Japanese) on strange particles (V-particles) in the monthly journal of the Physical Society of Japan, saying that the pair production of V's has no experimental evidences. Under such a situation, the idea of pair production was hardly acceptable to high energy community.

I may remind you that at that time there was another hot controversy : whether is meson production at high energy nucleon-nucleus collisions multiple production or plural production ? (multiple production : mesons are produced in nucleon-nucleon collisions in the form of multiple production. plural production : meson is singly produced at nucleon-nucleon collision, while cascade processes taking place in nucleon-nucleus collisions will lead "multiple" production of meson for nucleon-nucleus collisions.) At that time it was very difficult to select experimentally these two alternatives for meson-production ! And there were a lot of cosmic-ray experiments and hot discussions on this issue.

At present, it might be very difficult to understand why such a "trivial" issue was so hotly discussed and pursued !

I may conclude that, cosmic-rays brought us a lot of interesting and valuable findings for particle physics, but also sometimes misleading impression because of inherent poor statistics on information obtained by cosmic-rays.

C.N. YANG. - In reference to Murray's interesting account of the history of the concept of strangeness, I remember that in the summer of 1953, I did not like Murray's idea at all. In fact, I convinced everybody at bull sessions at Brookhaven in the early summer of 1953 that Murray's proposal was all wrong. I had two objections. I did not feel that a boson should have half integral isospin, and I had believed that there is only one neutral K. But just to keep the records straight, I was not the referee that Murray mentioned :

J. TIOMNO. - It may be convenient for myself to make at this point an observation related to what Yang has said in that. Although, the paper on isospin with the classification of the isodoublet for K impressed me very much, I also had this prejudice and then I developed a treatment on a doublet scheme where all baryons (at least those which were known at that time), were isofermion, being fermions. Correspondingly pions and kaons, being bosons, would be isobosons. In 1957, I was really much convinced that this would be useful when I developed the scheme with $O(7)$ invariance, proposing for the first

time (Nuovo Cimento, 6, 69) the unification of the baryon octet and unification of the seven mesons, π and K . At the Rochester conference in 1957, I submitted this paper, at the same time as Gell-Mann was using the isodoublets for the $\Lambda - \Sigma$ from a different approach (global symmetry in π interactions). Then, as I had mentioned that there was a similarity among the papers by Schwinger, Gell-Mann, and mine, Murray said that someone should point out that they were not quite the same thing - clearly they were not the same thing. Also I like to mention that when Yang was in Rio, he too was thinking on this question and we were studying the possibility of getting a sub-group of $O(7)$ in order to eliminate some unsatisfactory selection rules. We did not work enough to find what Neeman found a few years later, that if you just include the complete set of Γ^A and Γ^{AB} operators you get $SU(3)$ in the octet representation.

M. GELL-MANN.- I think that we can learn from many of these stories a double principle, which is that a good theoretical idea in science often needs to be stripped of unnecessary baggage with which it is accompanied at the beginning, and that then it may need to be taken much more seriously than it was by its original proponent. I said this at the Einstein centenary celebration in Jerusalem and pointed out that in 1905, in the same volume of the Annalen der Physik, Einstein published three articles: one on special relativity, one on the photo-electric effect, and one on Brownian motion. In the Brownian motion article he took seriously the notion of the physical existence of a molecule; in the article on the photo-electric effect, he took seriously the possibility of the physical existence of a quantum; and in the article on special relativity he took seriously the physical importance of the symmetry group of the electromagnetic equation.