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MASTER

HISTORY OF WEAK INTERACTIONS*

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While the phenomenon of β-decay was discovered near the end of the last century, the notion that the weak interaction forms a separate field of physical forces evolved rather gradually. This became clear only after the experimental discoveries of other weak reactions such as μ-decay, μ-capture, etc., and the theoretical observation that all these reactions can be described by approximately the same coupling constant, thus giving rise to the notion of a universal weak interaction. Only then did one slowly recognize that the weak interaction force forms an independent field, perhaps on the same footing as the gravitational force, the electromagnetic force, and the strong nuclear and sub-nuclear forces.

In preparing this lecture, I was reminded, once more, of how relatively recent some of these "early developments" were. Indeed, most of the shaping from the single class of nuclear β-decay phenomena to the entire field of activities of what is now called "the weak interaction" took place after I became actively interested in physics, and I certainly consider myself (or rather, I would like to consider myself) to be one whose major scientific career still lies in the future.

1. Early History

The difficulty with weak interactions is that, on the theoretical side, ever since in the early 1930's Pauli introduced the notion of neutrino and Fermi formulated the phenomenological Lagrangian of β-decay interaction, one thinks one has already the complete theory. Almost all subsequent theoretical developments are, to a large extent, merely modifications and elaborations of the same old basic idea. The impressive success of Fermi's formulation enables us to describe, through a single parameter, the Fermi coupling constant G which is related to the nuclear mass m_N by ($\hbar = c = 1$)

$$G \cong 10^{-5}/m_N^2 \ , \qquad (1)$$

the enormous complexities of all known weak reactions, ranging from the assorted allowed and forbidden β-decays of different nuclei to various other weak processes involving leptons, mesons and hyperons. Yet, as we shall see, Fermi's theory is only a phenomenological one; its success also implies that this large body of known weak processes is really quite insensitive to the underlying basic theory, and therefore it is difficult to make further theoretical progress.

On the experimental side, the difficulty has always been the presence of wrong but convincing experimental results. From the very beginning, experiments on β-decay have followed a tortuous path. At first, its spectrum appeared to consist of discrete lines; this was, of course, later proven to be incorrect. Nevertheless, in those early days of quantum jumps, it was much easier to understand, theoretically, discrete lines rather than a continuum spectrum. Even after Fermi's theory, for a very long time, the theoretical prediction did not agree with the experimental results. The correct β-energy spectrum was observed by Lawson and Cork in 1940 and, in the low energy range, only since 1949 by C. S. Wu and her co-workers, more than 50 years after the discovery of the β-ray. Thus, probably we should not feel too bad about the present uncertainties in the $K_{\ell 3}$ spectrum and the confusion over CP violations.

When I began my graduate study of physics at the University of Chicago in 1946, the pion was not known. Fermi and Teller had just completed their theoretical analysis of the important experiment of Conversi, Piccioni and Pancini; they concluded that the mesotron, then discovered in cosmic radiation, could not possibly be the carrier of strong forces hypothesized by Yukawa. This made it somewhat more plausible, at least to me as a young graduate student, that perhaps the β-decay interaction, which becomes exceedingly singular according to the second order perturbation formula, could be the strong

nuclear force. At that time, the β-decay interaction was thought to be reasonably well understood. Fermi's original vector coupling form,

$$G(\psi_n^\dagger \gamma_4 \gamma_\lambda \psi_p)(\psi_e^\dagger \gamma_4 \gamma_\lambda \gamma_5 \psi_\nu) \tag{2}$$

was, after all, too simple; to conform to reality, it should be extended to include a Gamow-Teller term. [The presence of γ_5 has, of course, no physical significance. Nevertheless, it is curious why Fermi should choose this particular expression, which resembles the VA interaction, but with parity conservation.]

A year later, the discovery of the pion through its decay sequence π-μ-e by Powell and his group dramatically confirmed the original idea of Yukawa. The fact that the higher order β-interaction is singular is not a good argument that it should simply become the strong interaction. Thus, it seems reasonable to regard β-decay as belonging to a new class of interactions. With this motivation, several graduate students at Chicago, Rosenbluth, Yang and myself, began in 1948 a more systematic investigation of whether there exist interactions, other than β-decay, that could belong to the same class.

After we found that if μ-decay and μ-capture were described by a similar four-Fermion interaction as β-decay, all their coupling constants appeared to be of the same magnitude, we naturally went to our teacher, Enrico Fermi. We told him of our discovery, including also our speculation that, in analogy with electromagnetic forces, the basic weak interaction should be represented by a universal interaction between a hypothetical heavy boson and a pair of Fermion fields. [Since then, this intermediate boson has acquired a definite name, $W^{\pm}$, though otherwise remaining hypothetical.] Fermi, with his usual deep insight, immediately recognized the full significance. He asked: if this is to be a universal interaction, then there must be

reasons why some pairs of Fermions should have such interactions, and some pairs should not. For example, why is it that

$$\mu^- \not\rightarrow e^+ + 2e^- ,$$

and

$$p \not\rightarrow e^+ + 2\nu ,$$

and, in a similar manner, we may also ask why is it that

$$p \not\rightarrow e^+ + \gamma \quad ?$$

A few days later, he told us that he had found the answer; he then proceeded to assign various sets of numbers, $+1$, -1 and 0, to each of these particles. This was the first time to my knowledge that both the laws of baryon number conservation and of lepton number conservation were formulated. However, at that time (1948), my own reaction to such a scheme was totally negative: surely it is not necessary to explain why $p \not\rightarrow e + \gamma$, since everyone knows that the identity of a particle is never changed through the emission and absorption of a photon; as for the weak interaction, why should one bother to introduce a long list of mysterious numbers, when all one needs is to say that only three combinations $(\bar{n}p)$, $(\bar{e}\nu)$ and $(\bar{\mu}\nu)$ can have interactions with the intermediate boson.

Sometimes a discovery is made simply because the time is ripe; if one person does not make it, then surely another person will do it at about the same time. In looking back, what we did in finding the same weak interaction coupling constant for different processes was precisely a discovery of such a nature. This is clear, since very similar observations were made independently by at least three other groups,

Klein, Puppi, and Tiomno and Wheeler, at about the same time. Yet, Fermi's thinking was of a more profound nature. Unfortunately for physics, his proposal was never published. The full significance of these conservation laws was not realized until years later. While this might be the first time that I failed to recognize a great idea in physics when it was first presented to me, unfortunately this did not turn out to be the last time.

In the early fifties, extensive efforts were made on β-decay experiments. By then, the Konopinski-Uhlenbeck interaction was definitely ruled out. The absence of the Fierz interference term in the spectrum shows that the β-interaction must be either V, A or S, T. These two possibilities were further resolved by a series of β-ν angular correlation experiments. In an allowed transition, the distribution for the angle θ between β-ν is given by (neglecting Fierz term)

$$[\, 1 + \lambda \left(\frac{P}{E}\right)_e \cos\theta \,]\; d\cos\theta \tag{3}$$

where the subscript e refers to the momentum and energy of the electron. For a $\Delta J = 1$ transition,

$$\lambda = \begin{cases} +\frac{1}{3} & \text{for } T \\ -\frac{1}{3} & \text{for } A\,. \end{cases}$$

The experiment on He^6 decay by Ruby and Rustard in 1953 gave

$$\lambda = +\ 0.34 \pm 0.09 \tag{4}$$

which seemed to establish unquestionably that the β-decay interaction should be S, T with perhaps some unknown mixture of P.

With this new result, the theoretical idea of intermediate boson seemed to be definitely ruled out. It is bad enough to assume the possibility of two kinds of intermediate bosons of different spin-parity, one for the Fermi coupling and the other for the Gamow-Teller coupling. However, a tensor interaction with no derivative coupling is too much even for a liberal free-thinking theorist. It cannot even be transmitted by a spin-2 boson, since the former is described by an anti-symmetric tensor and the latter by a symmetric one.

In the meantime, progress was also made in other domains of weak interactions, such as the electron spectrum in μ-decay. In such a decay the normalized electron spectrum $N(x)$ depends only on one parameter, called the Michel parameter ρ :

$$N(x) = 6x^2 \left[2 (1 - x) + \frac{4}{9} (-3 + 4x) \rho \right] \tag{5}$$

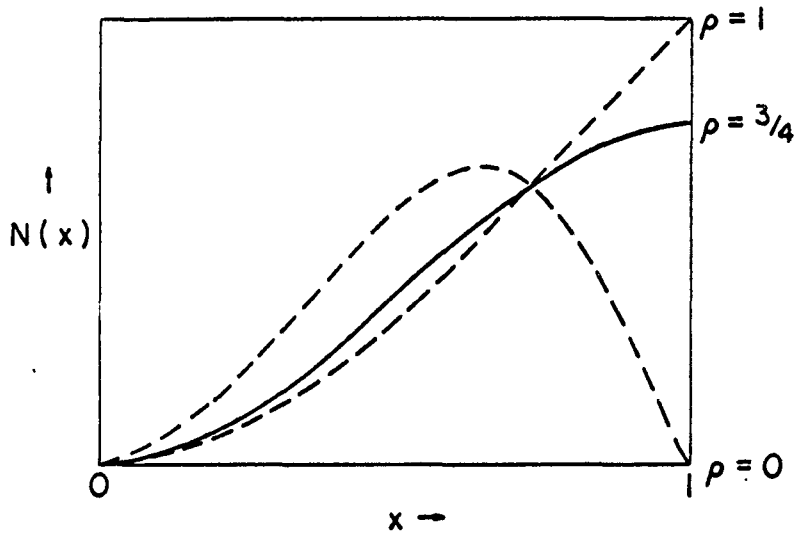
where

$$\int_0^1 N(x)\, dx = 1$$

and x is related to the electron momentum p_e and the muon mass m_μ by

$$x = \frac{p_e}{\frac{1}{2} m_\mu} . \tag{6}$$

As one can see from the following graph, different ρ-values give very different shapes of the electron spectrum



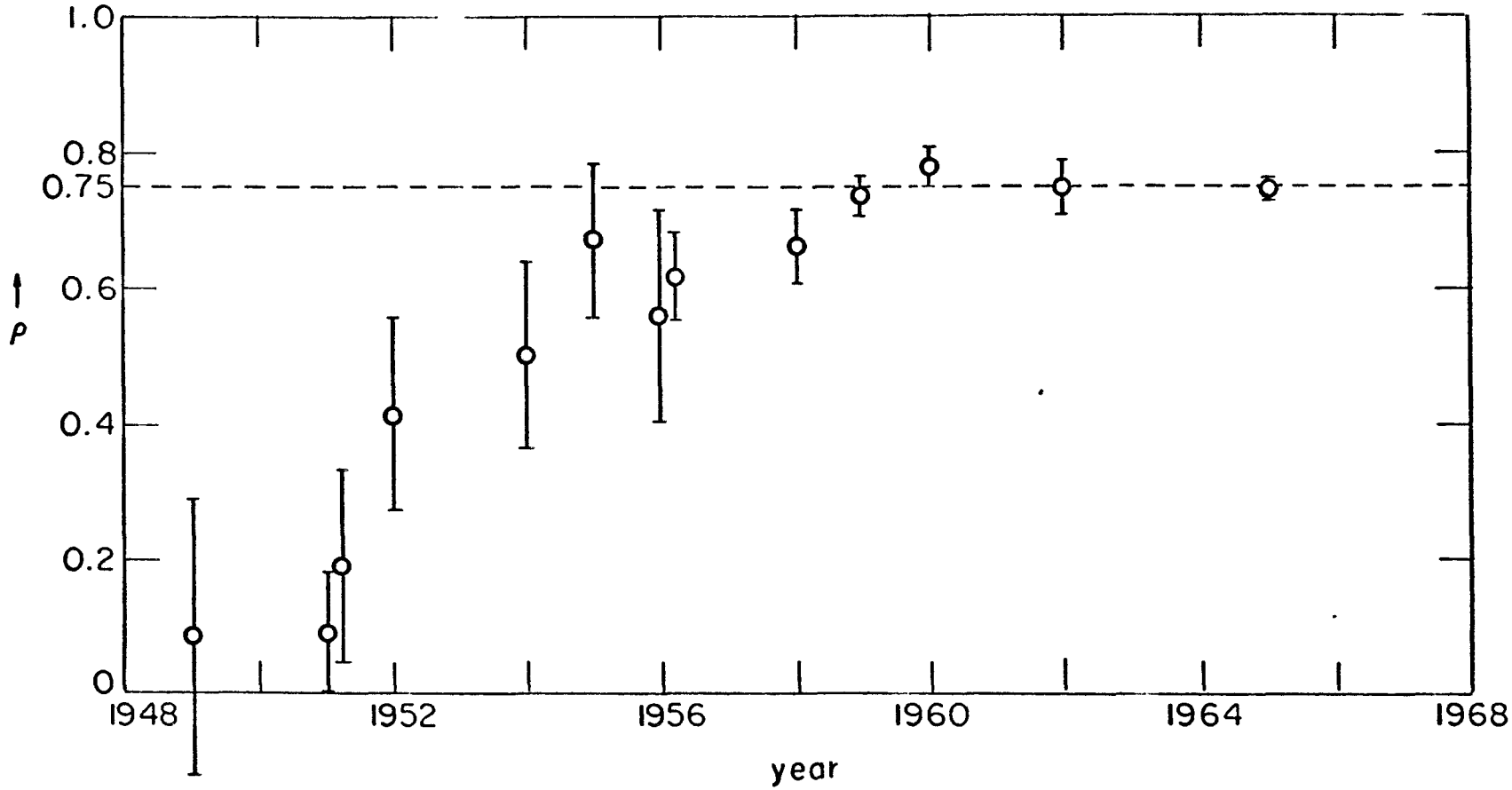


Figure 2

Yet, as shown by the next figure, the experimental value in 1951 was

$$\rho = 0.1 \pm 0.1$$

which is to be compared with the presently known value of

$$\rho = 0.752 \pm 0.003 \quad . \tag{7}$$

It is strange to observe this time dependence of the ρ-value in Figure 2, especially to note that at any given time, each experiment is always within the error of the preceding one.

Our understanding of weak interactions could have remained in such a misrepresented situation for a much longer period, had it not been for the appearance of an entirely different class of weak processes due to the then newly discovered strange particles.

2. Transition Period

The decays of the strange particles extended weak interactions to include, in addition to the already existing purely leptonic process μ-decay, and the strangeness conserving $(\Delta S = 0)$ semi-leptonic processes μ-capture and β-decay, two entirely new classes of processes: the strangeness non-conserving semi-leptonic and non-leptonic transitions. Among these new processes, the most puzzling ones were the charged θ and τ mesons. Both mesons were defined according to their decay modes:

$$\theta^+ \rightarrow \pi^+ \pi^o$$

and

$$\tau^+ \rightarrow \pi^+ \pi^+ \pi^-$$

or

$$\tau'^+ \rightarrow \pi^+ \pi^o \pi^o \ .$$

The spin-parity of θ^+ is clearly 0+, 1-, 2+, etc. As early as 1954, Dalitz already pointed out that the spin-parity of τ^+ can be analysed through his Dalitz plot, and the then existing data was more consistent with the assignment 0- than 1- . Although both mesons were known to have comparable masses (within ~20 Mev), there was nothing extraordinary about it. However, by 1955, very accurate lifetime measurements became available. This, together with a statistically much more significant Dalitz plot of τ^+-decay, presented a very puzzling picture indeed. Except for very high spin assignments $J \geq 3$ [which was not preferred partly on esthetic grounds and partly because it would lead to angular correlations between the production and decay directions, while none was observed], the spin-parity of τ^+ was determined to be definitely 0- ; therefore, it appeared to be definitely a different particle from 0+ .

Yet, within the experimental accuracy (~ a few percent) these two particles have exactly the same lifetime, and also comparable masses. This was known as the $\theta - \tau$ puzzle.

My first reaction was to invent schemes within the conventional theories to explain this puzzle. Together with Orear, we proposed in the summer of 1955 a cascade mechanism: Accepting the preferred spin parity assignments, 0- for τ^+ and 0+ for θ^+, it seems reasonable to assume that θ^+ should have a much shorter lifetime than τ^+ because of phase space considerations; now if one assumes τ^+ is, say, ~ 5 Mev heavier than θ^+, the 0-0 forbidden transition

$$\tau^+ \rightarrow \theta^+ + 2\gamma$$

can become comparable to the 3π decay mode of τ^+, resulting in the "apparent" θ-lifetime being simply the lifetime of τ^+, provided that such lifetime measurements are (as they were) determined by the attenuation of the 2π decay mode. The approximate mass degeneracy was then incorporated into a parity-exchange symmetry.

However, when Alvarez's group failed to find such a 5 Mev γ in their bubble chamber, it finally became clear to me, in early 1956, that the solution to the θ-τ puzzle must lie in something much deeper; perhaps, parity is not conserved, and θ and τ are indeed the same particle. The immediate reaction to this simple proposal was "so what". Unless it could be shown that parity conservation also extended to other processes, one would <u>never</u> know for <u>sure</u> whether in θ-τ decays parity conservation is, or is not, violated. Thus, it was necessary to investigate how possible parity non-conservation effects could be observed in other weak reactions.

At that time, the true meaning of parity operation P was not clear, at least not to me. Of course, I knew its mathematical property: P should be represented by a unitary operator in the Hilbert space, and under P, one has, e.g., for a spin-$\frac{1}{2}$ Fermion field

$$P\,\psi(r,\,t)\,P^{-1} \;=\; e^{i\phi}\,\gamma_4\,\psi(-\,r,\,t)$$

etc. I assumed that β-decay should be described by a more general Lagrangian consisting of ten coupling constants, the usual five C_i $(i = S, P, V, A, T)$ and the additional five parity violating constants C_i'. I then borrowed from C. S. Wu the authoritative book on β-decay edited by K. Siegbahn, and proceeded with C. N. Yang to calculate systematically all possible parity violating effects. We started with the allowed spectrum, and found exactly the same expression after replacing the usual

$$C_i^*\,C_j \quad \text{by} \quad C_i^*\,C_j + C_i'^*\,C_j' \;. \tag{8}$$

Next came the forbidden spectrum, and the same thing happened. We then computed Coulomb effects, β-ν correlations, β-γ correlations and β-γ-γ correlations; while some of these calculations were rather complicated, in the end all interference terms between C_i and C_j' invariably were cancelled, and the same old expressions appeared, provided we made the same simple replacement (8). After we went through the entire Siegbahn book, rederived all these old formulas with this new interaction, it became obvious to us that not only was there, at that time, not a single evidence for parity conservation in β-decay, but that we must have been very stupid! There had to be an extremely simple reason why all those complicated interference terms $C_i^*\,C_j'$ cancelled each other. Once we stopped calculating and started to think, in a rather short time, it dawned on us that the reason for this lack of evidence was the

simple fact that nobody had made any attempt to observe a pseudo-scalar as distinct from an otherwise seemingly right-left symmetrical arrangement.

From then on, the theoretical problem came under control, and it resulted in, in 1956, the papers on parity non-conservation, two-component theory and the general question of possible C, P, T non-invariance, including possible CP violations in neutral K-decays. It also led, in January 1957, to the important first experiment on P and C asymmetries in β-decay by Wu, Ambler, Hayward, Hoppes and Hudson, which was quickly followed by the observation of the same symmetry breaking in π and μ decays by Garwin, Lederman and Weinrich, and by Friedman and Telegdi. The experiment of Garwin et al. was done in a particularly elegant way.

An outburst of new experiments quickly followed, quite often with mutually contradictory results. The question of what should be the new β-decay interaction was a pressing one, but in the summer of 1957 it was clear that no theory could possibly agree with all then-existing experimental results. Some of these experiments had to be wrong. Privately, I thought, if anything, the old He^6 experiment must be right. First, it was done after years of patient experimental effort, not like one of those new quickies; secondly, it came from that great Institution, Columbia University. Nevertheless, I kept my opinion reasonably quiet, and when Feynman passed through New York that summer asking me about my opinion of the experimental situation, I suggested that the best way was to flip a coin.

At the end of that summer, a manuscript by Gell-Mann and Feynman arrived on my desk in Pupin. On its margin, Feynman had scribbled "I have flipped my coin, and this is the answer." After I read their paper, I knew I should have borrowed his coin.

The fact that if the He^6 experiment were wrong, one could then have V-A interaction which would be theoretically more attractive, e.g., one could have lepton conservation, small π_{e2} decay rate, etc., was known to me. The same reasoning also led, at about the same time, Marshak and Sudarshan, and Sakurai to their independent suggestion of V-A interaction. However, I was particularly impressed by the added statement of Feynman and Gell-Mann that the hadronic weak vector current V_μ^{wk} and the corresponding isovector part of the electromagnetic current $e J_\mu$ could form an isotriplet

$$V_\mu^{wk}, \quad (J_\mu)_{I=1}, \quad v_\mu^{wk\dagger}, \tag{9}$$

which is very appealing and seems to carry a much deeper significance. From that, it follows

$$\frac{\partial V_\mu}{\partial x_\mu} = 0 . \tag{10}$$

Since there is no zero mass particle with strong interactions, (10) implies that, neglecting radiative corrections

$$(G_V)_{obs} = (G_V)_{bare} \tag{11}$$

which makes it possible to compare the weak coupling constant for hadrons with that for leptons. The V-A interaction was established mainly through the neutrino helicity measurement of Goldhaber, Grodzins and Sunyar. The later experiment on weak magnetism effects in $\beta^\pm$ decays of B^{12} and N^{12} by Wu and others, together with experiments on the lifetime of

$$\pi^+ \rightarrow \pi^o + e^+ + \nu_e$$

give strong evidence for the correctness of the Gell-Mann-Feynman isotriplet hypothesis.

The following decade from 1957 to the present was a period of great clarification. For example, as shown in the next Figure, the experimental ρ-value is in excellent agreement with the theory. Through the work of Gell-Mann, Cabibbo and others we now have a very simple interaction form, at least for the semi-leptonic processes. During the same period, one witnessed the innovation of high energy neutrino experiments which led to the discovery of two neutrinos

$$\nu_e \neq \nu_\mu \tag{12}$$

by Lederman, Schwartz and Steinberger. Except for CP, one could have said that, finally, we have the correct theory of weak interactions.

3. Present Status

Independently of the detailed dynamics, one expects the following general properties of the weak interaction to hold to all orders of weak interactions:

Lorentz Invariance
Charge Conservation
Baryon Number Conservation
Lepton Number Conservation
C P T Symmetry
Two Component Theory of Neutrino
μ-e Symmetry
$U_2 \times U_2$ Symmetry of Leptons.

Apart from these general properties, we know that the phenomenological theory of Fermi with V-A interaction holds at least at low 4-momentum transfer. Among the important unresolved problems, I may list:

1. Origin of CP Asymmetry
2. Breakdown of Fermi Theory at High Energy

   (Violation of Unitarity Limit at Momentum $\sim$300 Bev/c)
3. Possible Existence of Intermediate Boson

Besides these, there are also many questions connected with the detailed dynamics and selection rules such as the $|\Delta \vec{I}| = \frac{1}{2}$ rule, validity of the local chiral $SU_3 \times SU_3$ current algebra, etc. The presence of these fundamental and crucial questions, and the prospect of using in the near future the ISR at CERN for possible production of $W^{\pm}$, and the very high energy neutrinos and muons from the NAL 500 GeV machine to search for the answers to these questions, promise a most exciting future for the physicists working in this field.