THE EVOLUTION OF WEAK INTERACTIONS

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ABSTRACT

A history of the study of weak interactions is presented, together with some personal recollections of the important contributions made by Jack Steinberger.
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1. **INTRODUCTION**

In January 1949, Jack Steinberger submitted a paper [1] to *Physical Review* entitled 'On the range of the electrons in meson decay', in which he established:

'The experiment, therefore, offers some evidence in favor of the hypothesis that the mu meson disintegrates into three light particles.'

This was a pivotal point in the history of weak interactions. Before that paper we had the classical period of the β-decay. After that started the modern period of the universal Fermi interaction.

Let us begin with the era before Jack.

2. **CLASSICAL PERIOD (1889–1949)**

In 1898 Lord Rutherford [2] discovered that the so-called Becquerel ray actually consisted of two distinct types of radiation: one that is readily absorbed which he called α-radiation, and another of a more penetrating character which he called β-radiation. Then, in 1900, the Curies measured the electric charge of the β-particle and found it to be negative [3]. That, at the turn of the century, began the history of the weak interaction. From the very start the road of discovery was tortuous, and the competition intense. A letter written by Rutherford [4, 5] to his mother expressed the spirit of research at that time:

'I have to keep going, as there are always people on my track. I have to publish my present work as rapidly as possible in order to keep in the race. The best sprinters in this road of investigation are Becquerel and the Curies...'

Most of the people at this symposium can well appreciate these words. Rutherford's predicament is very much shared by us to this day.

Soon even more runners appeared: Hahn, Meitner, Wilson, von Baeyer, Chadwick, Bohr, Pauli, Fermi, Ellis, Uhlenbeck, and many others. In preparing this lecture, I was reminded once more of how relatively recent these early developments were. We know that to reach where we are today took nearly a whole century and a large cast of illustrious physicists. Yet probably everyone in this room can touch any of the people who did the work with three handshakes (for some perhaps only two). You shake Jack's hand, Jack shook Fermi's, and Fermi shook all those other hands.

In the mid-1960s, Lise Meitner came to New York and I had lunch with her at a restaurant near Columbia. When K.K. Darrow joined us, Meitner said 'It's wonderful to see young people.' To appreciate this comment, you must realize that Darrow was one of the earliest members of the American Physical Society and at that lunch he was over 70. But Lise Meitner was near 90.

I was quite surprised when Meitner told me that she started her first postdoctoral job in theory with Boltzmann. Now, Boltzmann was a contemporary of Maxwell. That shows us how recent even the classical period of our profession is.

After Boltzmann's unfortunate death in 1906, Meitner had to find another job. She said she was grateful that Planck invited her to Berlin. However, upon arrival, she found that because she was a woman she could only work at Planck's institute in the basement, and only go in and out through the servants' entrance. At that time, Otto Hahn had his laboratory in an old carpenter's shop. Lise Meitner decided to join him and to become an experimentalist. For the next thirty years, their joint work shaped the course of modern physics.

In 1908 they found [6] that the absorption of β-particles through matter followed an exponential law. From that they concluded β-rays are of unique energy. It was Wilson [7], in 1909, who drew an opposite conclusion that the β-rays are heterogeneous in energy.

So β-decay was not of a unique energy, but soon Hahn and von Baeyer [8] found the presence of line spectra, which again confused the issue. This was cleared up by Chadwick [9] in 1914, who established the continuous β-spectrum.

With the advent of quantum theory, Meitner [10], in 1922, raised the question concerning the origin of the continuous spectrum. She reasoned that a nucleus, presumably quantized, should not
emit electrons of varying energy. Could it be that the observed inhomogeneity was introduced after the expulsion of the electron from the nucleus? A series of experiments by Ellis [11] and others quickly established that this is not the case. This then led to Bohr's suggestion [12] that perhaps energy was not conserved in β-decay. Pauli [13] countered this by formulating the neutrino hypothesis. Fermi [14] then followed with his celebrated theory of β-decay. This in turn stimulated further investigation on the spectrum shape of the β-decay, which did not agree with Fermi's theoretical prediction. This led Konopinski and Uhlenbeck [15] to introduce the derivative coupling. The confusion was only cleared up completely after World War II, in 1949, by Wu and Albert [16], the same year that Jack published his first paper, signalling the end of one era and the beginning of a new one.

3. NEW HORIZON (1949–1953)

When Jack and I began our graduate study of physics at the University of Chicago, in 1946, the pion was not known. Fermi and Teller [17] had just completed their theoretical analysis of the important experiment of Conversi, Pancini and Piccioni [18]. I attended a seminar by Fermi on this work. He cut right through the complex slowing-down process of the mesotron, the capture rate versus the decay rate, and arrived at the conclusion that the mesotron could not possibly be the carrier of strong forces hypothesized by Yukawa. Fermi's lectures were always superb, but that one to me, a young man not yet twenty and fresh from China, was absolutely electrifying. I left the lecture with the impression that, instead of Yukawa's idea, perhaps one should accept Heisenberg's suggestion [19] that the origin of strong forces could be due to higher-order processes of β-interaction. As was known, these were highly singular.

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

$$G(\bar{\psi}_e \gamma_\mu \gamma_\lambda \psi_\mu)(\bar{\psi}_e \gamma_\gamma \gamma_\lambda \gamma_\nu \psi_\nu)$$

was, after all, too simple; to conform to reality, it should be extended to include a Gamow-Teller term. [Fermi told us that his interaction was modelled after the electromagnetic forces between charged particles, and his coupling G was inspired by Newton's constant. His paper was, however, rejected by Nature for being unrealistic. It was published later in Italy, and then in Zeitschrift für Physik [14]. Fermi wrote his γ-matrices explicitly in terms of their matrix elements. His lepton current differs from his hadron current by a γ5 factor; of course the presence of this γ5 factor has no physical significance. Nevertheless, it is curious why Fermi should choose this particular expression, which resembles the V-A interaction, but with parity conservation. Unfortunately, by 1956, when I noticed this, it was too late to ask Fermi.]

A year later, the discovery of the pion through its decay sequence π → μ → e by Lattes, Muirhead, Occhialini and Powell [20] 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 force.

There have been two lucky breaks in my life. The first was the good fortune of having Jack as a fellow-student at Chicago, because it was Jack who told us that the muon decays into an electron and two neutrinos. This made it look very much like any other β-decay, and stimulated Rosenbluth, Yang and myself to launch a systematic investigation. Are there other interactions, besides β-decay, that could be described by Fermi's theory?

We found that if μ-decay and μ-capture were described by a four-fermion interaction similar to β-decay, all their coupling constants appeared to be of the same magnitude. This began the universal Fermi interaction. We then went on to speculate that, in analogy with electromagnetic forces, the basic weak interaction could be carried by a universal coupling through an intermediate heavy boson [21], which I later called W⁺ for weak. Naturally we went to our teacher, Enrico Fermi, and told him of our discoveries. Fermi was extremely encouraging. With his usual deep insight, he immediately recognized the further implications beyond our results. He put forward the problem that if this is to be the universal interaction, then there must be reasons why some pairs of fermions should have such interactions, and some pairs should not. For example, why does
and
\[ p \not\rightarrow e^+ + \gamma \]
\[ p \not\rightarrow e^+ + 2\nu ? \]

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 together to give selection rules. However, at that time (1948), my own reaction to such a scheme was to be quite unimpressed: surely, I thought, 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 (\( \bar{\nu}p \), (\( \bar{\nu}r \)), and (\( \bar{\nu}\nu \)) can have interactions with the intermediate boson. [Little did I expect that soon there would be many other pairs joining these three.]

Most discoveries in physics are made 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 establishing the universal Fermi interaction was a discovery of exactly this nature. This is clear, since the same universal Fermi coupling observations were made independently by at least three other groups, Klein [22], Puppi [23], and Tjonno and Wheeler [24], all 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 presented to me, unfortunately it did not turn out to be the last.

In the early fifties, extensive efforts were made on \( \beta \)-decay experiments. By then, the Konopinski-Uhlenbeck interaction was definitely ruled out. The absence of the Fierz interference term [25] in the spectrum shows that the \( \beta \)-interaction must be either V, A or S, T. These two possibilities were further resolved by a series of \( \beta - \nu \) angular correlation experiments. In an allowed transition, the distribution for the angle \( \theta \) between \( \beta \) and \( \nu \) is given by (neglecting the Fierz term)
\[
[1 + \lambda (P/E)\cos \theta] \cos \theta ,
\]
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 \( ^6\text{He} \) decay by Rustad and Ruby, in 1953, gave [26]
\[
\lambda = +0.34 \pm 0.09 ,
\]
which seemed to establish unquestionably that the \( \beta \)-decay interaction should be S, T with perhaps some unknown admixture of P.

I was quite depressed at that time because, with this new result, the theoretical idea of the 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 simply cannot even be transmitted by a spin-2 boson, since the former is described by an antisymmetric tensor and the latter by a symmetric one.

4. **THE \( \theta - \tau \) PUZZLE (1953–1955)**

We now come to the \( \theta - \tau \) puzzle in the early fifties.

During a recent physics graduate qualifying examination in a well-known American university, one of the questions was on the \( \theta - \tau \) problem. Most of the students were puzzled over what \( \theta \) was; of
course they all knew that $\tau$ is the heavy lepton, the charged member of the third generation. So much for the history of physics.

This reminds me of two men who met on the street. One said, 'John, how are you?' The other looked puzzled. The first said, 'My, you have changed! You used to be tall, and now you are short. You used to be thin, but now you are fat. John, what happened?' The other looked even more puzzled and said, 'But I'm not John.' Whereupon the first exclaimed, 'You have even changed your name!' So it is with the $\tau$ meson. Perhaps I should explain for the young people in this audience.

In the early 1950s, $\theta$ referred to the meson which decays into $2\pi$, whereas $\tau$ referred to the one decaying into $3\pi$:

\[ \theta \to 2\pi \]
\[ \tau \to 3\pi . \]

The spin-parity of $\theta$ is clearly $0^+$, $1^-$, $2^+$, etc. As early as 1953, Dalitz [27] had already pointed out that the spin-parity of $\tau$ can be analysed through his Dalitz plot, and, by 1954, the then-existing data were more consistent with the assignment $0^-$ than $1^-$. Although both mesons were known to have comparable masses (within ~ 20 MeV), there was, at that time, nothing too extraordinary about this situation. Because the phase space of $2\pi$ differs greatly from that of $3\pi$, one expects their lifetimes to be also quite different. However, by 1955, very accurate lifetime measurements became available, and it turned out that $\theta$ and $\tau$ have the same lifetime (within a few per cent, which was the experimental accuracy). This, together with a statistically much more significant Dalitz plot of $\tau$ decay, presented a very puzzling picture indeed. The spin-parity of $\tau$ was determined to be $0^-$; therefore it appeared to be definitely a different particle from $\theta$. Yet, these two particles seemed to have the same lifetime, and also the same mass. This was the $\theta-\tau$ puzzle.

My first efforts were all on the wrong track. In the summer of 1955, Jay Orear and I proposed [28] a scheme to explain the $\theta-\tau$ puzzle within the bounds of conventional theory. We suggested a cascade mechanism, which turned out to be incorrect.

The idea that parity is perhaps not conserved in the decay of $\theta-\tau$ flickered through my mind. After all, strange particles are by definition strange, so why should they respect parity? The problem was that, after you say parity is not conserved in $\theta-\tau$ decay, then what do you do? Because if parity non-conservation exists only in $\theta-\tau$, then we already have all the observable facts, namely the same particle can decay into either $2\pi$ or $3\pi$ with different parity. I discussed this possibility with Yang, but we were not able to make any progress [29]. So we instead wrote papers on parity doublets, which was another wrong try [30].

5. THE BREAKTHROUGH (1956)

In 1956, I had another lucky break, this time because Jack was my colleague at Columbia.

At that time, Steinberger and others were conducting extensive experiments on the production and decay of the hyperons $\Lambda^0$ and $\Sigma^-$:

\[ \pi^- + p \to \begin{cases} \Lambda^0 + K^0 \\ \Sigma^- + K^+ \end{cases} \]  \hspace{1cm} (1)

and

\[ \begin{align*} \\ \Lambda^0 \\ \Sigma^- \end{align*} \to \pi + N \ . \]  \hspace{1cm} (2)

The dihedral angle $\phi$ between the production plane and the decay plane is of importance for the determination of the hyperon spin.

Let $\pi$, $\Lambda$, and $\Sigma$ be the momenta of $\pi$, $\Lambda$ in expression (1) and $N$ in expression (2), all, say, in the respective centre-of-mass systems of the reactions. The normal to the production plane is parallel to
and that to the decay plane is parallel to $\vec{\Lambda} \times \vec{N}$. Hence, the dihedral angle $\phi$ is defined through its cosine:

$$
\cos \phi \propto (\vec{\pi} \times \vec{\Lambda}) \cdot (\vec{\Lambda} \times \vec{N}) .
$$

(3)

Its distribution is

$$
\begin{align*}
D(\phi) &= 1 & \text{if the hyperon-spin is } \frac{1}{2} . \\
&= 1 + \alpha \cos^2 \phi & \text{if the hyperon-spin is } \frac{1}{2} .
\end{align*}
$$

(4)

etc. By this definition, $\phi$ varies from 0 to $\pi$. Furthermore, $D(\phi)$ is identical to $D(\pi - \phi)$. At the Rochester Conference in early April, 1956, Jack gave a talk and plotted his data on $D(\phi)$ with $\phi$ varying from 0 to $\pi$. However other physicists, W.D. Walker and R.P. Shutt, plotted $D(\phi) + D(\pi - \phi)$; in this way $\phi$ can only vary from 0 to $\pi/2$. A few days after the conference, Jack came to my office to discuss a letter which he had just received from R. Karplus. In this letter Karplus questioned why Jack did not join the others, since the total number of events was (at that time) quite limited, and a folding of $D(\pi - \phi)$ onto $D(\phi)$ would increase the experimental sensitivity of the spin determination.

The dihedral angle, as defined by expression (3), has nothing to do with parity, since it is a scalar. In the course of the discussion, I suddenly realized that if one changes the definition of $\phi$ to be the angle of rotation around the $\Lambda$ momentum-vector, which is the intersection of these two planes, then the range of $\phi$ can be extended from 0 to $2\pi$; that is, in addition to expression (3), one introduces the pseudoscalar

$$
\sin \phi \propto (\vec{\pi}_\perp \times \vec{N}_\perp) \cdot \vec{\Lambda} .
$$

(5)

where $\vec{\pi}_\perp$ and $\vec{N}_\perp$ refer to the components of $\vec{\pi}$ and $\vec{N}$ perpendicular to $\vec{\Lambda}$, as shown in Fig. 1.

If parity is not conserved in strange particle decays, there could be an asymmetry between events with $\phi$ from 0 to $\pi$ and those with $\phi$ from $\pi$ to $2\pi$. This is the missing key! I was quite excited, and urged Jack to re-analyse his data immediately and test the idea experimentally. This led to the very first experiment on parity non-conservation. Very soon, Jack and his collaborators (Budde,
Chretien, Leitner, Samios and Schwartz) had their results, and the data were published [31] even before the theoretical paper [32] on parity non-conservation. The odds turned out to be 13 to 3 in $\Sigma^-$ decay and 7 to 15 in $\Lambda^0$ decay (see Fig. 2). Of course, because of the limited statistics, no conclusion can be drawn. Nevertheless, except for the high standard of Jack and his group, this might have been claimed as the first indication of parity non-conservation.

However, on the theoretical side there was still the question of parity conservation in ordinary $\beta$-decay. In this connection, about two weeks later, I had the further good fortune of having Yang join me. This led to our discovery that, in spite of the extensive use of parity in nuclear physics and $\beta$-decay, there existed no evidence at all of parity conservation in any weak interaction.
Several months later followed the decisive experiments by Wu, Ambler, Hayward, Hopps and Hudson, at the end of 1956, on $\beta$-decay [33], and by Garwin, Lederman and Weinrich [34] and by Friedman and Telegdi [35] on $\pi-\mu$ decay.

From then on we entered the modern period: $\theta-\tau$ became the $K$ meson, $S-T$ turned out to be $V-A$, and there were all kinds of asymmetries, with the weak interaction becoming a part of the electroweak interaction [36]. The kaon system is still the richest store of symmetry violations: CP-violation [37], T-violation, etc. The well-known series of experiments by the Steinbergers [38] and their collaborators on the $K\bar{K}$ system stands out to this day as an example of incisiveness, elegance, and conceptual simplicity.

6. MODERN PERIOD

The modern period is familiar to all of us in this room. At present, there seems to be a divergence in the viewpoints of theorists and experimentalists. The experimentalists are full of problems, looking for solutions—money problems, managerial problems, scheduling problems, etc. On the other hand, the theorists think they already have the ultimate solution and that there is no problem. Superstrings may well be the theory of everything (TOE), but how about calculating the Higgs mass, quark–lepton masses, CP violation, Cabibbo angle, etc. Therefore, instead, I would like to go over our experience and try to extract not the laws of physics, but the laws of physicists.

We all know that to do high-energy physics requires accelerators. When each new accelerator is proposed, theorists are employed like high priests to justify and to bless such costly ventures. Therefore it pays to look at the track record of theorists in the past, to see how good their predictions were before experimental results. The next graph (Fig. 3) gives almost all the important discoveries made in particle physics for more than three decades.

It is of interest to note that, with the exception of the antinucleon and the intermediate bosons $W^\pm$ and $Z^0$, none of these landmark discoveries was the original reason given for the construction of the relevant accelerator.

When Lawrence built his 184 in. cyclotron, the energy was thought to be below pion production. Therefore, after the cyclotron was turned on, even though pions were produced abundantly, for a long time nobody noticed them.

![Diagram of landmark discoveries](image)

Fig. 3 Landmark discoveries in high-energy physics over the past three decades
The progress of particle physics is closely tied to the discovery of resonances, which started with the (3,3) level found at the Chicago cyclotron (the second item on the chart). Yet even the great Enrico Fermi, when he proposed the machine, did not envisage this at all. After its unexpected discovery, for almost a year Fermi expressed doubts whether it was a genuine resonance. A similar story can be told about the next landmark discovery. When the Cosmotron was constructed at Brookhaven, some of the leading theorists thought that the most important high-energy problem was to understand the angular distribution of proton–proton collisions, which remains mysteriously flat even at a few hundred MeV, although at that energy the dynamics of the collision is quite complicated; many different levels (s, p, d, f, g, ...) are all involved. Why should they conspire to make a flat angular distribution? But as it turned out, when the energy increases the angular distribution of proton–proton collisions no longer remains flat and becomes quite uninteresting. Instead, it was production and decay dynamics of the strange particles, Λ, Σ, ..., that put the Cosmotron on the map.

We could go on and on, and the same pattern would repeat itself throughout this list. This leads to my first law of physicists:

‘Without experimentalists, theorists tend to drift.’

There is no reason for us to believe that it will change, nor should we expect too much from our present theorists for the prediction of the future.

Looking at the chart in Fig. 3, you will notice that the density of great discoveries per unit time is quite uniform and averages out to about one in two years. Let us hope that this long-standing record of constant rate of discovery can be maintained. In order to achieve that, we must have good experiments.

We now come to my second law of physicists:

‘Without theorists, experimentalists tend to falter.’

A good example is the history of the Michel parameter in μ-decay. The electron distribution in μ-decay is given by

$$\frac{dN_e}{dx} = 6x^2 \left[ 2 - \frac{4}{3} \rho \right] - [ 2 - \frac{16}{9} \rho ] x,$$

where

$$x = (\text{momentum of } e)/(\sqrt{2}m_\mu),$$

and $\rho$ is the well-known Michel parameter, which can be any real number between 0 and 1, and measures the height of the end point at $x = 1$, as shown in Fig. 4.

![ELECTRON DISTRIBUTION](image)

**Fig. 4** The $\rho$ parameter in μ-decay
It is instructive to plot the experimental value of $\varrho$ against the year when the measurement was made. As shown in Fig. 5, historically it began with $\varrho = 0$ and then slowly drifted upwards; only after the theoretical prediction in 1957 did it gradually become $\varrho = \frac{3}{4}$. Yet, it is remarkable that at no time did the 'new' experimental value lie outside the error bars of the preceding one.

But there are exceptions. Let us take another look at the 1949 paper by Jack on the electron spectrum in $\mu$-decay, shown in Fig. 6. His result fits beautifully the correct $\varrho = \frac{3}{4}$ value. Yet Jack, with typical scientific modesty, said the limits of error of his spectrum were 'unknown, but large'. (This is why it is not included in Fig. 5.) I think this illustrates the integrity and dedication underlying Jack Steinberger’s superb ability.

It is indeed a privilege for me to know Jack and a pleasure to be able to pay homage to this great physicist.
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See also
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