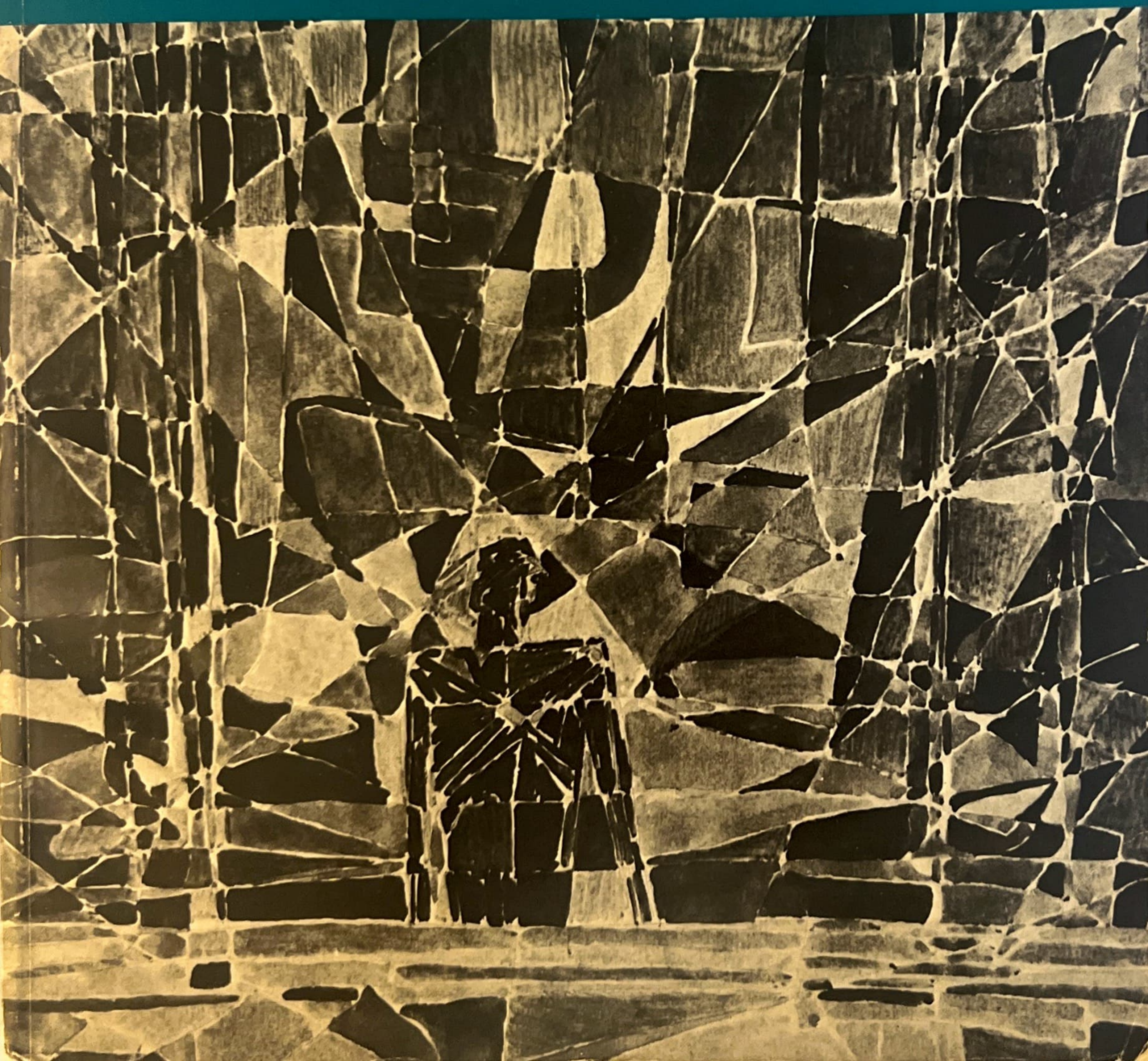
GAMMA γ VOLUME

adventures in experimental physics

bogdan maglich
editor



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GAMMA γ VOLUME

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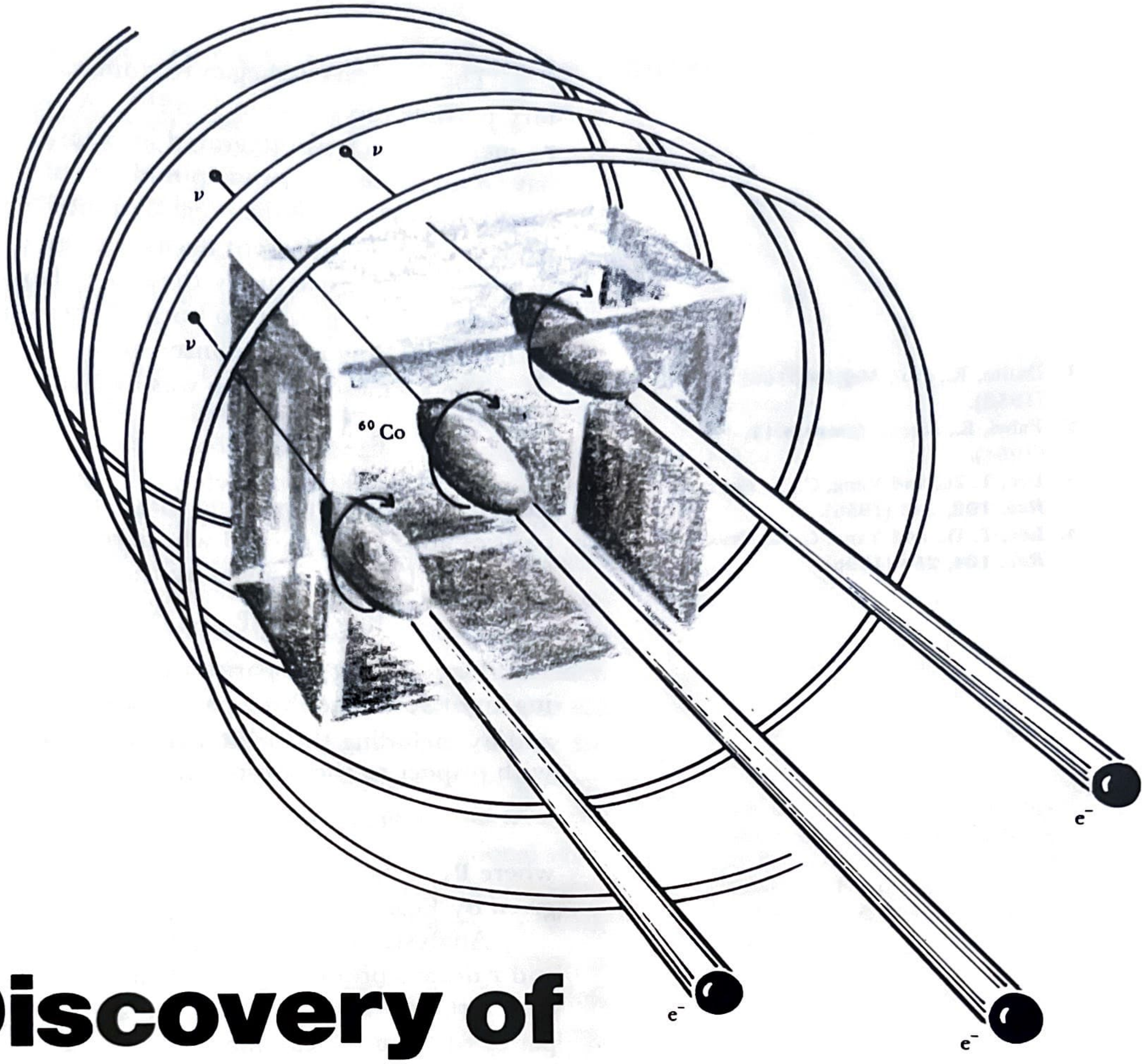
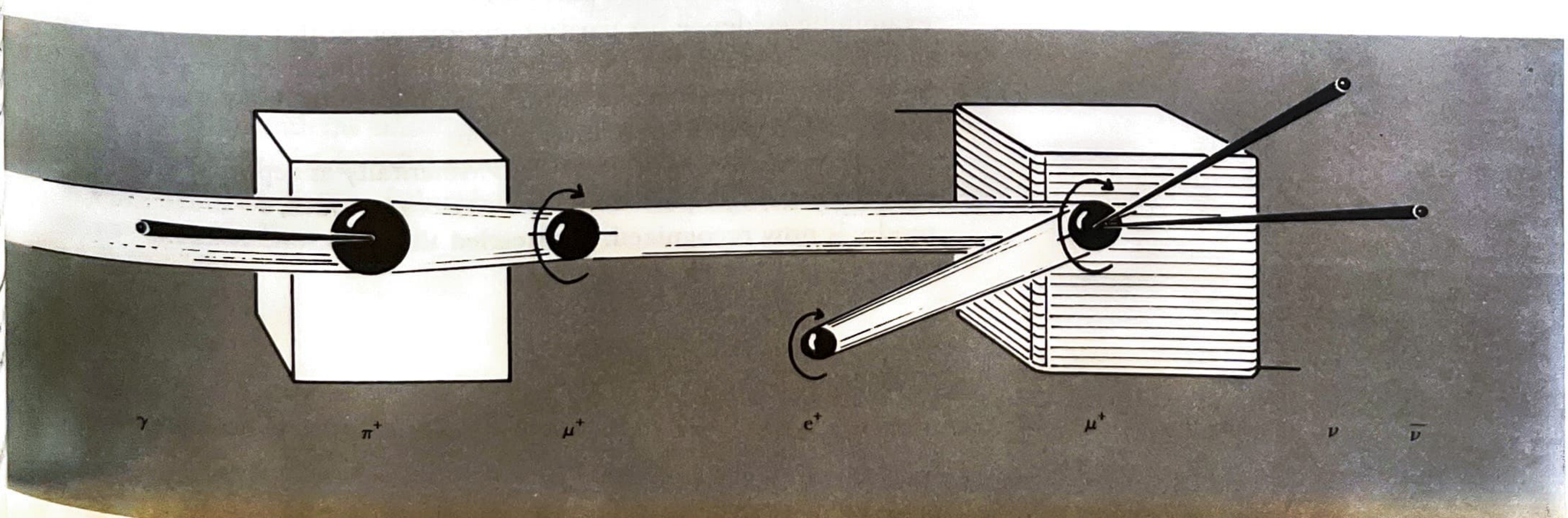


Discovery of Parity Violation in Weak Interactions



EXPERIMENTAL EVIDENCE WHICH CALLED INTO QUESTION THE CONCEPT OF PARITY: THE τ - θ PUZZLE

The τ - θ puzzle began to dominate the attention of elementary particle physicists in 1953[1,2]. A charged particle called the τ - meson, which disintegrated into *three* π - mesons, was found to have a mass, lifetime, and spin identical to another particle called the θ - meson, which disintegrated into only *two* π - mesons. If it were not for the different decay modes, one could believe that the two particles were actually the same. However, a single particle could not disintegrate into both of these final states (2π and 3π) without violating parity conservation.

The π - meson or pion was known to have odd intrinsic parity,

$$P_\pi = -1. \tag{1}$$

The parity of a *system* consisting of two particles, whose intrinsic parities are P_1 and P_2 and whose relative orbital angular momentum is $\ell_{12} = 0, 1, 2, \ldots$, is given by the product:

$$P_{12} = P_1 P_2 (-1)^{\ell_{12}}. \tag{2}$$

One obtains the parity of the three-particle system by treating any two of the three particles as one entity whose parity is P_{12}, and by including the orbital angular momentum of the third particle with respect to the two-particle system, $\ell_{(12)3}$, in the product:

$$P_{123} = P_{12} P_3 (-1)^{\ell_{(12)3}} \tag{3}$$

where P_3 is the intrinsic parity of the third particle and P_{12} is given by Equation (2).

Analysis of the measured angle-energy correlations in the θ and τ decays produced the conclusion that in the θ - decay $\ell_{12} = 0$ and in the τ - decay both $\ell_{12} = 0$ and $\ell_{(12)3} = 0$. This gives the parity of the θ - decay mode:

$$P_{12} = (-1)^2 = +1 \tag{4}$$

For the τ - decay mode:

$$P_{123} = (-1)^3 = -1. \tag{5}$$

The τ - θ puzzle led Lee and Yang to first suggest that physicists were dealing with a new phenomenon: the existence of 'parity doublets'[3] for all strange particles of odd strangeness. Later, after examining evidence for parity conservation both in these decays and in other processes, the two physicists hypothesized that parity might be violated in weak interactions[4], as they could find no evidence that it was conserved in these interactions.

The puzzle was resolved experimentally as reported in this chapter. Only a single particle with two decay modes, θ - mode and τ - mode, is now recognized. It is called the K-meson, or kaon.

1. Dalitz, R., *Phil. Mag.* **44**, 1068 (1953).
2. Fabri, E., *Nuovo Cimento* **11**, 479 (1954).
3. Lee, T. D., and Yang, C. N., *Phys. Rev.* **102**, 290 (1956).
4. Lee, T. D., and Yang, C. N., *Phys. Rev.* **104**, 254 (1956).

EXPLANATORY PHYSICS NOTES FOR NON-SPECIALISTS

THE CONCEPT OF PARITY AND ITS FAILURE IN WEAK INTERACTIONS

"The situation that the physicist found himself in at that time [the τ - θ puzzle] has been likened to a man in a dark room groping for an outlet. He is aware of the fact that in some direction there must be a door which would lead him out of his predicament. But in which direction?"

by
C. N. Yang
(Excerpt from Nobel Lecture, 1957.)

One of the symmetry principles, the symmetry between the left and the right, is as old as human civilization. The question whether Nature exhibits such symmetry was debated at length by philosophers of the past[1]. Of course, in daily life, left and right are quite distinct from each other. Our hearts, for example, are on our left sides. The language that people use both in the orient and the occident, carries even a connotation that right is good and left is evil. However, the laws of physics have always shown complete symmetry between the left and the right, the asymmetry in daily life being attributed to the accidental asymmetry of the environment, or initial conditions in organic life. To illustrate the point, we mention that if there existed a mirror-image man with his heart on his right side, his internal organs reversed compared to ours, and in fact his body molecules, for example sugar molecules, the mirror image of ours, and if he ate the mirror image of the food that we eat, then according to the laws of physics, he should function as well as we do.

The law of right-left symmetry was used in classical physics, but was not of any great practical importance there. One reason for this derives from the fact that right-left symmetry is a discrete symmetry, unlike rotational symmetry which is continuous. Whereas the continuous symmetries always lead to conservation laws in classical mechanics, a discrete symmetry does not. With the introduction of quantum mechanics, however, this difference between the discrete and continuous symmetries disappears. The law of right-left symmetry then leads also to a conservation law: the conservation of parity.

The discovery of this conservation law dates back to 1924 when Laporte[2] found that energy levels in complex atoms can be classified into « gestrichene» and « ungestrichene » types, or in more recent language, even and odd levels. In transitions between these levels during which one photon is emitted or absorbed, Laporte found that the level always changes from even to odd or *vice versa*. Anticipating later developments, we remark that the evenness or oddness of the levels was later referred to as the parity of the levels. Even levels are defined to have parity + 1, odd levels parity − 1. One also defines the photon emitted or absorbed in the usual atomic transitions to have odd parity. Laporte's rule can then be formulated as the statement that in an atomic transition with the emission of a photon, the parity of the initial state is equal to the total parity of the final state, i.e. the product of the parities of the final atomic state and the photon emitted. In other words, parity is conserved, or unchanged, in the transition.

1. See, for example, the interesting discussion on bilateral symmetry by Weyl, H., *Symmetry*, (Princeton University Press, 1952.)
2. Laporte, O., *Z. Physik*, 23, 135 (1924).

In 1927 Wigner[3] took the critical and profound step to prove that the empirical rule of Laporte is a consequence of the reflection invariance, or right-left symmetry, of the electromagnetic forces in the atom. This fundamental idea was rapidly absorbed into the language of physics. Since right-left symmetry was unquestioned also in other interactions, the idea was further taken over into new domains as the subject matter of physics extended into nuclear reactions, β-decay, meson interactions, and strange-particle physics. One became accustomed to the idea of nuclear parities as well as atomic parities, and one discusses and measures the intrinsic parities of the mesons. Throughout these developments the concept of parity and the law of parity conservation proved to be extremely fruitful, and the success had in turn been taken as a support for the validity of right-left symmetry.

3. Wigner, E. P., *Z. Physik*, **43**, 624 (1927). For references to these developments see also: Wigner, E. P., *Proc. Am. Phil. Soc.*, **93**, 521 (1949).
4. Klein, O., *Nature*, **161**, 897 (1948); Tiomno, J., and Wheeler, J. A., *Rev. Mod. Phys.*, **21**, 144 (1949); Lee, T. D., Rosenbluth, M., and Yang, C. N., *Phys. Rev.*, **75**, 905 (1949).

Against such a background the so-called ϑ—τ puzzle developed in the last few years. Before explaining the meaning of this puzzle it is best to go a little bit into a classification of the forces that act between subatomic particles, a classification which the physicists have learned through experience to use in the last 50 years. We list the four classes of interactions below. The strength of these interactions is indicated in the column on the right.

1. Nuclear Forces	1
2. Electromagnetic Forces	10^{-2}
3. Weak Forces (Decay Interactions)	10^{-14}
4. Gravitational Forces	10^{-38}

Four Classes of Interactions in Nature and Their Relative Strengths

Interaction/Strength

1. *Nuclear or Strong* -
   $f^2/\hbar c = 1$
   (responsible for production and scattering of nucleons, pions, hyperons, and kaons).
2. *Electromagnetic* -
   $e^2/\hbar c = \frac{1}{137} \approx 10^{-2}$
3. *Weak* -
   $g^2/\hbar c \approx 10^{-14}$
   (includes all non-electromagnetic decays and absorption of neutrinos by nucleons).
4. *Gravitational* -
   $G^2/\hbar c \approx 10^{-38}$

The strongest interactions are the nuclear interactions which include the forces that bind nuclei together and the interaction between the nuclei and the π mesons. It also includes the interactions that give rise to the observed strange-particle production. The second class of interactions are the electromagnetic interactions of which physicists know a great deal. In fact, the crowning achievement of the physicists of the 19th century was a detailed understanding of the electromagnetic forces. With the advent of quantum mechanics, this understanding of electromagnetic forces gives in principle an accurate, integral and detailed description of practically all the physical and chemical phenomena of our daily experience. The third class of forces, the weak interactions, was first discovered around the beginning of this century in the β-radioactivity of nuclei, a phenomena which especially in the last 25 years has been extensively studied experimentally. With the discovery of π-μ, μ-e decays and μ capture it was noticed independently[4] by Klein, by Tiomno and Wheeler, and by Lee, Rosenbluth and me, that these interactions have roughly the same strengths as β-interactions. They are called weak interactions, and in the last few years their rank has been constantly added to through the discovery of many other weak interactions responsible for the decay of the strange particles. The consistent and striking pattern of their almost uniform strength remains today one of the most tantalizing phenomena – a topic which we shall come back to later. About the last class of forces, the gravitational forces, we need only mention that in atomic and nuclear interactions they are so weak as to be completely negligible in all the observations with existing techniques.

Now to return to the ϑ—τ puzzle. In 1953, Dalitz and Fabri[5] pointed out that in the decay of the ϑ and τ mesons

$$\vartheta \rightarrow \pi + \pi$$
$$\tau \rightarrow \pi + \pi + \pi$$

some information about the spins and parities of the τ and ϑ mesons can be obtained. The argument is very roughly as follows. It has previously been determined that the parity of a π meson is odd (i.e. $= -1$). Let us first neglect the effects due to the relative motion of the π mesons. To conserve parity in the decays, the ϑ meson must have the total parity, or in other words, the product parity, of two π mesons, which is even (i.e. $= +1$). Similarly, the τ meson must have the total parity of three π mesons, which is odd. Actually because of the relative motion of the π mesons the argument was not as simple and unambiguous as we just discussed. To render the argument conclusive and definitive it was necessary to study experimentally the momentum and angular distribution of the π mesons. Such studies were made in many laboratories, and by the spring of 1956 the accumulated experimental data seemed to unambiguously indicate, along the lines of reasoning discussed above, that ϑ and τ do not have the same parity, and consequently are not the same particle. This conclusion, however, was in marked contradiction with other experimental results which also became definite at about the same time. The contradiction was known as the ϑ—τ puzzle and was widely discussed. To recapture the atmosphere of that time allow me to quote a paragraph concerning the conclusion that ϑ and τ are not the same particle from a report entitled «Present Knowledge about the New Particles» which I gave at the International Conference on Theoretical Physics[6] in Seattle, in September 1956.

«However it will not do to jump to hasty conclusions. This is because experimentally the K mesons (i.e. τ and ϑ) seem all to have the same masses and the same lifetimes. The masses are known to an accuracy of, say, from 2 to 10 electron masses, or a fraction of a percent, and the lifetimes are known to an accuracy of, say, 20 percent. Since particles which have different spin and parity values, and which have strong interactions with the nucleons and pions, are not expected to have identical masses and lifetimes, one is forced to keep the question open whether the inference mentioned above that the τ^+ and ϑ^+ are not the same particle is conclusive. *Parenthetically, I might add that the inference would certainly have been regarded as conclusive, and in fact more well-founded than many inferences in physics, had it not been for the anomaly of*

5. Dalitz, R., *Phil. Mag.*, **44**, 1068 (1953); Fabri, E., *Nuovo Cimento*, 11, 479 (1954).
6. Yang, C. N., *Rev. Mod. Phys.*, **29**, 231 (1957).

mass and lifetime degeneracies.»

The situation that the physicist found himself in at that time has been likened to a man in a dark room groping for an outlet. He is aware of the fact that in some direction there must be a door which would lead him out of his predicament. But in which direction?

That direction turned out to lie in the faultiness of the law of parity conservation for the weak interactions. But to uproot an accepted concept one must first demonstrate why the previous evidence in its favor were insufficient. Dr. Lee and I[7] examined this question in detail, and in May 1956 we came to the following conclusions: (*A*) Past experiments on the weak interactions had actually no bearing on the question of parity conservation. (*B*) In the strong interactions, i.e. interactions of classes 1 and 2 discussed above, there were indeed many experiments that established parity conservation to a high degree of accuracy, but not to a sufficiently high degree to be able to reveal the effects of a lack of parity conservation in the weak interactions.

The fact that parity conservation in the weak interactions was believed for so long without experimental support was very startling. But what was more startling was the prospect that a space-time symmetry law which the physicists have learned so well may be violated. This prospect did not appeal to us. Rather we were, so to speak, driven to it through frustration with the various other efforts at understanding the ϑ—τ puzzle that had been made[8].

As we shall mention later there is known in physics a conservation law – the conservation of isotopic spin – that holds for interactions of class 1 but breaks down when weaker interactions are introduced. Such a possibility of an approximate symmetry law was, however, not expected of the symmetries related to space and time. In fact one is tempted to speculate, now that parity conservation is found to be violated in the weak interactions, whether in the description of such phenomena the usual concept of space and time is adequate. At the end of our discussion we shall have the occasion to come back to a closely related topic.

Why was it so that among the multitude of experiments on β-decay, the most exhaustively studied of all the weak interactions, there was no information on the conservation of parity in the weak interactions? The answer derives from a combination of two reasons. First, the fact that the neutrino does not have a measurable mass introduces an ambiguity that rules out[9] indirect information on parity conservation from such simple experiments as the spectrum of β-decay. Second, to study directly parity conservation in β-decay it is not enough to discuss nuclear parities, as one had always done. One must study parity conservation of the *whole* decay process. In other words, one must design an experiment that tests right-left symmetry in the decay. Such experiments were not done before.

Once these points were understood it was easy to point out what were the experiments that would unambiguously test the previously untested assumption of parity conservation in the weak interactions. Dr. Lee and I proposed[7]

7. Lee, T. D., and Yang, C. N., *Phys. Rev.*, 104, 254 (1956).
8. Lee, T. D., and Orear, J., *Phys. Rev.*, 100, 932 (1955); Lee, T. D., and Yang, C. N., *Phys. Rev.*, 102, 290 (1956); a general discussion of these ideas can be found in the *Proceedings of the Rochester Conference*, April 1956, Session VIII, Interscience, New York, 1957.
9. Yang, C. N., and Tiomno, J., *Phys. Rev.*, 79, 495 (1950).

in the summer of 1956 a number of these tests concerning β-decay, π-μ, μ-e and strange-particle decays. The basic principles involved in these experiments are all the same: *One constructs two sets of experimental arrangements which are mirror images of each other, and which contain weak interactions. One then examines whether the two arrangements always give the same results in terms of the readings of their meters* (or counters). If the results are not the same, one would have an unequivocal proof that right-left symmetry, as we usually understand it, breaks down. The idea is illustrated in Fig. 1 which shows the experiment proposed to test parity conservation in β-decay.

This experiment was first performed in the latter half of 1956 and finished early this year by Wu, Ambler, Hayward, Hoppes, and Hudson[10]. The actual experimental setup was very involved, because to eliminate disturbing outside influences the experiment had to be done at very low temperatures. The technique of combining β-decay measurement with low temperature apparatus was unknown before and constituted a major difficulty which was successfully solved by these authors. To their courage and their skill, physicists owe the exciting and clarifying developments concerning parity conservation in the past year.

The results of Drs. Wu, Ambler, and their collaborators was that there is a very large difference in the readings of the two meters of Fig. 1. Since the behavior of the other parts of their apparatus observes right-left symmetry, the asymmetry that was found must be attributed to the β-decay of cobalt. Very rapidly after these results were made known, many experiments were performed which further demonstrated the violation of parity conservation in various weak interactions. In his lecture[11] Dr. Lee will discuss these interesting and important developments.

10. Wu, C. S., Ambler, E., Hayward, R. W., Hoppes, D. D., and Hudson, R. P., *Phys. Rev.*, **105**, 1413 (1957). Reproduced p. 119.

11. See p. 138 of this chapter.

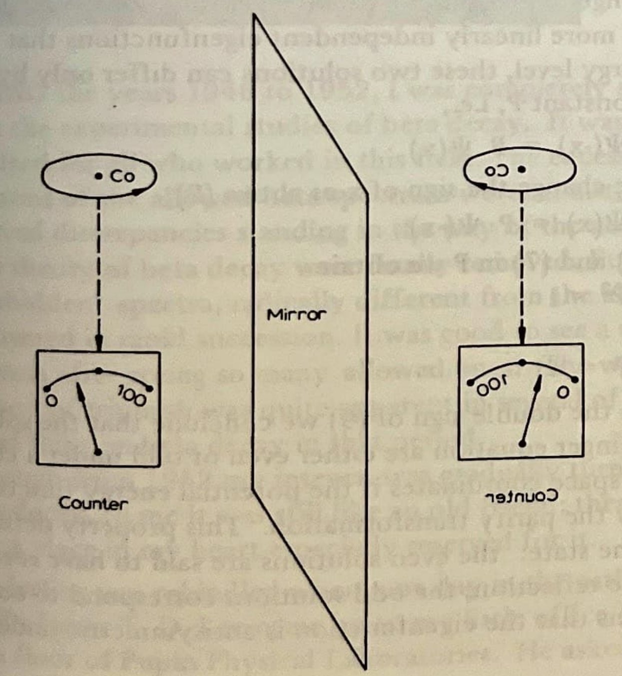


Fig. 1.

EXPLANATORY PHYSICS NOTES FOR NON-SPECIALISTS

Elementary Formal Introduction to the Concept of Parity

The concept of parity can be understood from a simple case: the one-dimensional, time-independent Schrödinger equation

$$-\frac{\hbar^2}{2m}\frac{d^2\,\Psi(x)}{dx^2} + V(x)\,\Psi(x) = E\,\Psi(x) \tag{1}$$

Let us now write the same type of equation for the mirror image position by changing the sign of x:

$$x \rightarrow -x \tag{2}$$

$$-\frac{\hbar^2}{2m}\frac{d^2\,\Psi(-x)}{dx^2} + V(-x)\,\Psi(-x) = E\,\Psi(-x) \tag{3}$$

If the potential energy function V(x) is *symmetric* about the center, x = 0; thus,

$$V(-x) = V(x). \tag{4}$$

Hence, Eq. (3) becomes

$$-\frac{\hbar^2}{2m}\frac{d^2\,\Psi(-x)}{dx^2} + V(x)\,\Psi(-x) = E\,\Psi(-x) \tag{5}$$

By comparing Eqs. (1) and (4) we see that for the same potential functions V, there are two solutions, $\Psi(x)$ and $\Psi(-x)$. The solutions of Schrödinger equations are called eigenfunctions. Unless there are two or more linearly independent eigenfunctions that correspond to this energy level, these two solutions can differ only by a multiplicative constant P, i.e.,

$$\Psi(-x) = P\,\Psi(x) \tag{6}$$

Now, if we change the sign of x as above (2),

$$\Psi(x) = P\,\Psi(-x) \tag{7}$$

Solving (6) and (7) in P we obtain

$$P^2 = 1 \tag{8}$$

or

$$P = \pm 1 \tag{9}$$

From the double sign of (9) we conclude that the solutions of the Schrödinger equation are either even or odd under a change of sign in the space coordinates if the potential energy function is unchanged by the parity transformation. This property defines the parity of the state: the even solutions are said to have *even parity*, under space reflection; the odd solutions correspond to *odd parity*, and it means that the eigenfunction is antisymmetric under space reflection.

DISCOVERY STORY I

Chien-Shiung Wu—
discoverer of parity violation.
Photo: [Courtesy of
The American Institute of Physics].

One Researcher's Personal Account

by
Chien-Shiung Wu

DURING the years 1946 to 1952, I was completely submerged in the experimental studies of beta decay. It was an exciting period indeed for all who worked in this field. The excess low energy electrons of the allowed beta spectrum were, at last, eliminated. The observed discrepancies standing in the way of the acceptance of the Fermi theory of beta decay were finally removed. Furthermore, some 'forbidden' spectra, radically different from the allowed shape, were uncovered in rapid succession. It was good to see a real forbidden spectrum after seeing so many allowed ones! The whole exuberant mood of triumph was quite apparent in several of my review articles and talks on beta decay in that period.

Although from 1952 my interest was gradually turning away from beta decay, to me it was still like an old friend; there would always be a place in my heart especially reserved for it.

This feeling was rekindled when, one day in the early spring of 1956, Professor T. D. Lee came up to my little office on the thirteenth floor of Pupin Physical Laboratories. He asked me a

Nuclear Polarization

If the spins of nuclei are oriented predominantly in one direction, the nuclei are said to be polarized. Static and dynamic methods have been developed which make it possible, in principle, to polarize any nucleus. Nuclei with an odd number of nucleons have a magnetic moment $\mu \neq 0$. Those with an even number of nucleons have a zero moment in the ground state (e.g., ^{152}Sm), but may have $\mu \neq 0$ in an excited state.

The brute force method of achieving a large degree of polarization is to apply a large static magnetic field (H) at low temperatures (T). The polarization (P) will then be given by

$$P = \tanh \frac{\mu H}{kT}$$

where k is Boltzman's constant. This method is impractical if the nuclei are in a region of low thermal conductivity. Adiabatic demagnetization of copper nuclei polarized by this method has achieved a temperature of 20 microdegrees Kelvin.

Another method (the Rose-Gorter method) uses a moderate external field to orient the paramagnetic electrons. This induces a polarization of the nuclei through a hyperfine coupling and produces a local field of several hundred kilogauss. This is the method that was used to polarize the ^{60}Co nuclei in CMN crystals. The field used was less than a kilogauss at a temperature of several millidegrees Kelvin. By measuring the anisotropy in the emitted γ radiation, the nuclear polarization was found to be 60%.

In the dynamic polarization method, a resonant magnetic field whose frequency is usually near the electron resonance is used. In the Overhauser effect, transitions between the spin states of the unpaired electrons are induced. This in turn induces transitions between nuclear spin states, due to a hyperfine coupling between nuclei and electrons. At low temperatures ($< 1°$ K) and high magnetic fields (~ 50 kilogauss) polarizations of 95% may be achieved.

A second dynamic method, the Jeffries-Abragam method, relies on the states of the coupled system to provide the transition. The magnetic field oscillates at a frequency which is the electron spin frequency plus or minus the nuclear magnetic resonance frequency. This method can produce polarizations similar to the Overhauser method.

series of questions concerning the status of the experimental knowledge of beta decay. He explained to me, first, the τ-θ puzzle and how it led to the question: is parity conserved in weak decays? If the answer to the τ-θ puzzle is violation of parity—he went on—then the violation should also be observed in the space distribution of the β-decay of polarized nuclei: one must measure the pseudo-scalar quantity $\langle \sigma \cdot p \rangle$ where p is the electron momentum and σ the spin of the nucleus. If parity is not conserved in the decay, there should be asymmetry in respect to the sign of $\langle \sigma \cdot p \rangle$, now called helicity (see page opposite).

Unfortunately, I could not supply him with any information on the pseudo-scalar quantity. At that time, there was absolutely no experimental data on this point. All the previous experiments on β-decay investigated essentially only the scalar quantity, for example: the shape of the β-spectrum (i.e., energy, which is a scalar).

People not only took it for granted that parity was conserved in all interactions, but this untested notion was also used to discourage others from doing any experiments to test, much less challenge, the validity of this concept. I was told by Dr. M. Morita, who joined our group in October, 1956, that this actually happened at an International Conference in Japan in 1955.

Before Professor Lee left my office, I asked him whether anyone had any ideas about doing this research. He said some people had suggested using polarized nuclei resulting from nuclear reactions or using a polarized slow neutron beam from a reactor. Somehow I had great misgivings about using either of these two approaches. I suggested that the best bet would be to use a ^{60}Co β-source polarized by the demagnetization method by which one could attain a polarization as high as 65%. Professor Lee was very much interested in the possibility of such a strongly polarized ^{60}Co β-source and asked me to lend him a reference book on the method.

I should probably explain here that for several years just prior to 1956, I was very much attracted to the possibilities of the magnetic h.f.s. polarization method. It is based upon the fact that in certain paramagnetic salts there are large magnetic fields ($\sim 10^6$ gauss) at the nuclei of the paramagnetic ions due to the unpaired electrons and, at temperatures of the order of 0.01° K, the nuclear magnetic moments become oriented with respect to these fields. Since the electron magnetism is easily saturated at low temperatures, a field of a few hundred gauss suffices. Nuclear orientation will automatically follow. Because of my familiarity with the capability and limitations of this technique, it was only natural that the first thought which came to my mind was to use the polarized ^{60}Co source. I did have some slight reservations about the possible effect of the

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EXPLANATORY PHYSICS NOTES FOR NON-SPECIALISTS

HELICITY

Helicity is a term used to describe the intrinsic 'left-handed' or 'right-handed' nature of a particle. It is generally believed that Nature does not distinguish left from right; a mirror image of a physical phenomenon should also depict a possible physical occurrence. In 1957, however, it was shown that this 'rule' is violated in weak interactions. Thus, electrons and neutrinos emitted in beta decay possess an intrinsic handedness.

Helicity (H) is defined mathematically as

$$H = \frac{\sigma \cdot p}{|\sigma \cdot p|}$$

where σ is the particle spin and p its momentum. It is the component of the spin along the direction of the particle's momentum.

Consider, as examples, the neutrino and antineutrino. Both have near zero proper mass and thus move at the speed of light. Both have a well-defined momentum vector, which is independent of the frame of reference. For the antineutrino, its spin is along the direction of its motion; thus it has positive helicity. The spin of the neutrino is antiparallel to its momentum vector and it therefore has negative helicity. The neutrino is left-handed, the antineutrino right-handed.

The terms left and right are used in terms of a screw. If one twists the head of a right-hand screw clockwise, the point moves away from one. A left-hand screw would move toward one if this operation were performed.

Pseudoscalars and Axial Vectors

A pseudoscalar is a quantity which, unlike a scalar, changes sign under space inversion. For example, $\langle \sigma \cdot p \rangle$ *is a pseudoscalar since, under space inversion* $\sigma \rightarrow \sigma$ *and* $p \rightarrow -p$; *hence* $\langle \sigma \cdot -p \rangle = -\langle \sigma \cdot p \rangle$. *Axial vectors also behave unusually.* **L** *(angular momentum) is an axial vector since* $L = p \times r$ *and under inversion* $p \rightarrow -p$ *and* $r \rightarrow -r$ *and* $-p \times -r = (p \times r)$. *Thus* **L** *does not change under this transformation. Spin* $\langle \sigma \rangle$ *is also an axial vector.*

POLARIZATION

Polarization of a beam of particles consists of lining up the spins of the particles along a single axis either parallel to the direction of motion along the axis (positive helicity) or antiparallel (negative helicity). The polarization P is defined as:

$$P = \frac{N_+ - N_-}{N_+ + N_-}$$

where N_+ is the number of particles with positive helicity and N_- is the number of particles with negative helicity.

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magnetic polarizing field on the angular distribution of the beta particles. It turned out that this was no concern at all because the low energy beta particles were tightly spiralled along the lines of magnetic field in either parallel or antiparallel directions. In retrospect, it was a very lucky decision as the reader will discover.

My Decision To Go Ahead

Following Professor Lee's visit, I began to think things through. This was a golden opportunity for a beta decay physicist to perform a crucial test, and how could I let it pass? Even if it turned out that the conservation of parity in beta decay was valid, the experimental result would, at least, set an upper limit on its violation and thus stop further speculation that parity is not violated.

As an experimentalist, I was also challenged by two things which had never been tried before and were difficult. One was to put an electron detector inside a cryostat at a liquid helium temperature and make it function; the other was to have a β-source located in a thin surface layer and polarized for a time period long enough to obtain sufficient statistics.

That spring, my husband, Chia-Liu Yuan, and I had planned to attend an International Conference on High-Energy Physics in Geneva and then proceed to the Far East on a lecture tour. Both of us had left China in 1936, exactly twenty years earlier. Our passages were booked on the *HMS Queen Elizabeth* before I suddenly realized that I had to do the experiment immediately, before the rest of the Physics Community recognized the importance of this experiment and did it first. Although I felt that the chances of the parity conservation law being wrong were remote, I urgently wanted to make a clear-cut test. So I asked Chia-Liu to let me stay and go without me. Fortunately, he fully appreciated the importance of the time element and finally agreed to go alone.

Reassigned Spin of ^{60}Co Scare

I have a habit of browsing through my newly arrived books and journals. One day in the spring of 1956, as the new issue of *Nuclear Data* sheets was delivered to my office, I immediately opened to the page on ^{60}Co. To my great consternation, I found that the spin of the ground state of ^{60}Co had been reassigned to 4 instead of the well-known value of 5. As I mentioned before, the reason I chose ^{60}Co was because its beta transition is given by the spin change $I \rightarrow I - 1$ ($5 \rightarrow 4$) and no parity change. Under these

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EXPLANATORY PHYSICS NOTES FOR NON-SPECIALISTS

Interactions, Couplings and Selection Rules in β-Decay

The process involved in the creation of leptons in beta decay can be understood only from a quantum field theory viewpoint.

Fermi suggested that the beta interaction had a Hamiltonian of the simplest form

$$H = C_V [(\overline{\Psi}_p O_V \Psi_n)(\overline{\Psi}_e O_V \Psi_\nu) + (\overline{\Psi}_n O_V \Psi_p)(\overline{\Psi}_\nu O_V \Psi_e)]$$

where C_V is the coupling constant measuring the strength of the interaction, Ψ's are field operators which destroy their subscripted particles or create corresponding antiparticles. The $\overline{\Psi}$'s perform the inverse operation of the Ψ's; i.e., they create particles and destroy antiparticles. The first term in the expression is β^- decay; the second is β^+. Fermi's interaction is vector coupled, as suggested by electrodynamics. It allows only transitions with no spin or parity change and is scalar in space and spin. When the $^6He\ \beta^-$ decay was shown to involve a spin change, the original vector coupling alone was shown to be inadequate.

Gamow and Teller generalized the interaction by pointing out that five possible invariant interactions could be obtained by using the proper combinations of quantum field operators. These five interactions would involve scalar (S), pseudoscalar (PS), vector (V), axial vector (A) and tensor (T) coupling. The most general interaction is a linear combination of these. The Gamow-Teller interaction allows spin changes, but no parity changes, and is scalar in space but vector in spin.

Vector Models of Interaction Types

S	$\mathbf{a} \cdot \mathbf{b}$
PS	$\mathbf{a} \cdot (\mathbf{b} \times \mathbf{c})$
V	$\mathbf{a}$
A	$\mathbf{a} \times \mathbf{b}$
T	$T_{ij} = a_i b_j$

Subscripts represent the components of the vector.

The most general expression, we now know, must include parity nonconserving terms. The Hamiltonian can thus be written:

$$H = \sum_i C_i (\overline{\Psi}_p O_i \Psi_n)(\overline{\Psi}_e O_i \Psi_\nu) + \sum_i C_i' (\overline{\Psi}_p O_i \Psi_n)(\overline{\Psi}_e O_i \gamma_5 \Psi_\nu) + \text{Hermitian Conjugate}$$

where the conjugate terms represent β^+ decay. The first term is parity conserving, the second is parity nonconserving. The C_i and C_i' represent the coupling constants and the O_i are the conventional operators of quantum field theory (see, for example, J. Bjorken and S. Drell, *Relativistic Quantum Fields,* McGraw-Hill, 1965).

Three of the possible couplings, (V, A, T) contain a large part and a small part (proportional to the nucleon velocity). The small terms, along with the PS interaction, have selection rules requiring a parity change. However, S and the large V term obey Fermi selection rules, while the large T and A terms obey Gamow-Teller rules.

These define the 'allowed' transitions (for 'forbidden' transitions, see below).

Fermi interactions have been shown to proceed entirely through vector coupling (no scalar), while Gamow-Teller are axial vector coupled (no tensor coupling). The coupling constants have been measured and are found to be

$$C_S = C_S' = C_T = C_T' = C_{PS} = C_{PS}' = 0$$

$$C_V = C_V' = 1.418 \times 10^{-49} \text{ erg cm}^3$$

$$C_A = C_A' = -1.18\ (C_V)$$

The fact that the C_i's have been shown to be real is of great importance to the question of invariance. While the general theoretical expression allows complex coupling constants, time reversal invariance requires that the coupling constants be real. Under the CPT theorem, time reversal invariance further implies invariance under the combined operation of parity and charge conjugation (CP invariance). Both are thus shown to hold in beta decay.

'Forbidden' Spectra

If the matrix elements for allowed transitions vanish, the lepton wave functions must be expanded in a series. First 'forbidden' transition elements now arise from: (1) first order lepton wave function expansion terms; (2) the previously neglected small terms in V, A and T interactions; (3) any PS interaction present. Higher order "forbidden" transitions arise from higher order lepton expansion terms.

Fermi and Gamow-Teller Selection Rules

The selection rules depend on the exact nature of the interaction as well as whether a particular coupling scheme provides a good approximation to the wave function.

ALLOWED TRANSITIONS

interaction / coupling	FERMI scalar in space, scalar in spin	GAMOW-TELLER scalar in space, vector in spin
General	$\Delta J = 0$ no parity change	$\Delta J = 0, \pm 1$ $J = 0 \rightarrow J = 0$ forbidden no parity change
L-S coupling	$\Delta L = \Delta S = 0$ no change of ℓ-values	$\Delta L = 0$ $\Delta S = 0, \pm 1$ $S = 0 \rightarrow S = 0$ forbidden no change of ℓ- values
j-j coupling	no changes of ℓ - or j - values	no change of ℓ - value at most one j - value changes $j = \ell + ½$ to $\ell - ½$ or vice versa

Forbidden transitions occur for $\Delta J = n$ (parity unfavored) or $\Delta J = n + 1$ (parity favored or "unique") and parity change $(-1)^n$. First order forbidden transitions are exceptional in that $\Delta J = 0$ is possible. [*Encyclopaedic Dictionary of Physics,* J. Thewles, Editor, Pergamon Press 1961].

Cont. from p. 104

conditions the transition is not only allowed but is given by a pure Gamow-Teller type interaction. If the spin change were of the type I → I (4 → 4) then the allowed transition could be given by both Gamow-Teller and Fermi type interactions. Under these mixed interactions, not only would the asymmetry effect be greatly reduced but one would also have to take into account the interference effect between these two types of interactions which had to be evaluated or estimated from other methods.

It was explained in the *Nuclear Data* sheets that the spin I = 4 was assigned to ^{60}Co because the spectrum of the very weak outer beta group (from ground state of ^{60}Co to the first excited state of ^{60}Ni) was in better agreement with 4 → 2 (no)† transition rather than 5 → 2 (no) transition. Apparently they had observed excess electrons in the outer beta group. In order to clarify this situation, I knew I had to carry out a precision measurement of the beta spectrum of ^{60}Co. I suspected that the excess electrons which they observed were actually due to the Compton electrons from the two strong γ-lines (1.17 and 1.33 MeV). With the help of Mrs. Marion Biavati, we electroplated an extremely thin ^{60}Co source on an aluminized formvar film (to avoid charging effect) and carried out a thorough study of the entire beta spectrum under various source conditions. We found the outer beta group could be satisfactorily explained by ΔI = (5 → 2), (no). This detailed study of the beta spectra of ^{60}Co was never written up for publication because soon

† (no) means no parity change.

ASYMMETRY DEPENDENCE ON THE TYPE OF TRANSITION

The strong dependence of the asymmetry on the type of transition is seen from the following beta - gamma circular polarization data, obtained after the demonstration of parity violation in weak interactions.

A. In Allowed Gamow-Teller Beta Transitions:

	Asymmetry Coefficient A
Na^{22} (β^+) $(3^+ \rightarrow 2^+)$	+ 0.35

B. In $(4^+ \rightarrow 4^+)$ Transitions, the Value of A is Greatly Reduced:

Na^{24} (β^-) $(4^+ \rightarrow 4^+)$	∼ + 0.07
Co^{56} (β^+) $(4^+ \rightarrow 4^+)$	∼ 0.00 ± 0.03

afterward we were deeply involved in the great excitement of the discovery of parity nonconservation. Later, we read in *Physical Review* that about the same time, Dobrowalski *et al.* published‡ their determination of the spin of ^{60}Co by paramagnetic resonance hyperfine structure which showed clearly 11 lines, thereby definitely assigning the spin of ^{60}Co equal to 5. What a close call!

‡ *Phys. Rev.* **101, 1001 (1956).**

Interested Dr. Ambler In Joining The Great Venture

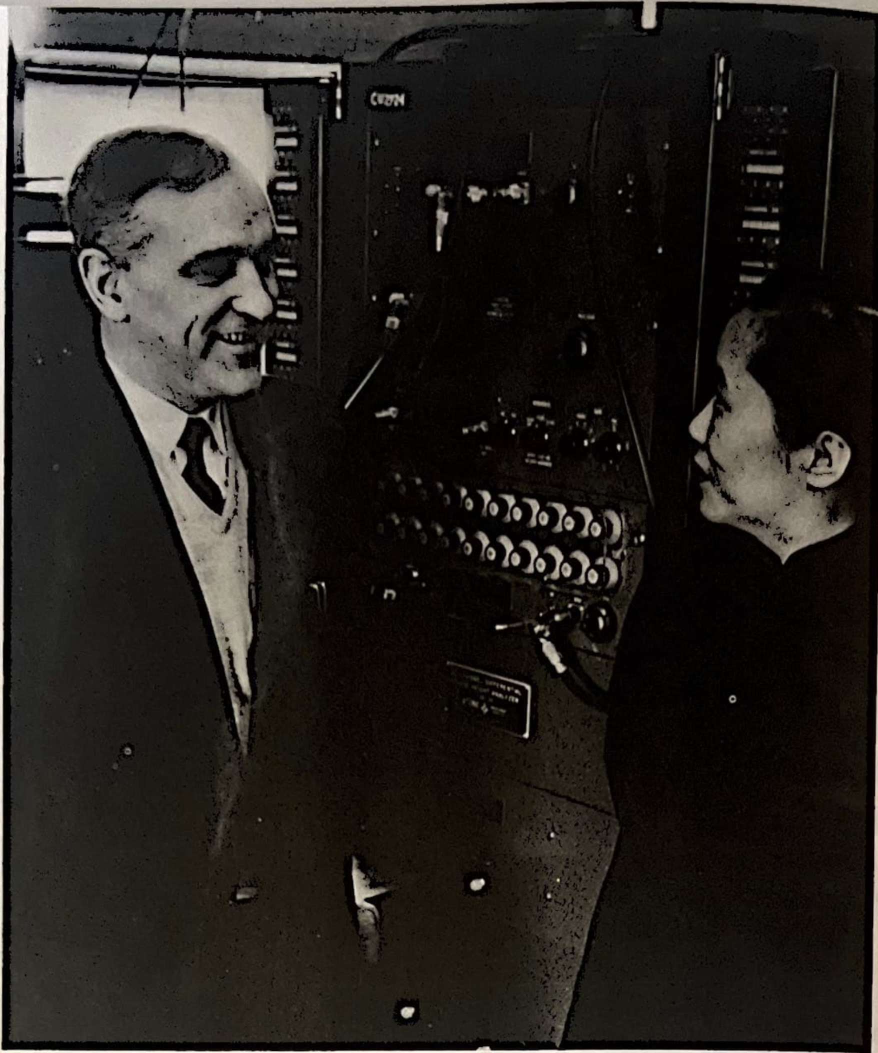
At Pupin Lab, we had a small but fine low temperature research group. They had no elaborate equipment but were familiar with the latest techniques. I benefited greatly from talking to a competent young man, Mr. Bruce Biavati, who was about to receive his Ph.D. degree with Professor Henry Boorse. I learned from him various kinds of ingenious techniques to insure good thermal contact and conduction.

Even in the United States, there was only a handful of low temperature laboratories which were equipped to do nuclear orientation experiments. The one nearest Columbia was the National Bureau of Standards in Washington, D. C. Dr. Ernest Ambler, a pioneer in the nuclear orientation field, had moved from Oxford University to the low temperature lab at NBS several years earlier. I decided to contact him by phone to determine whether he would be interested in a collaboration. It was on June 4, 1956 that I called and put the proposition directly to him. He accepted enthusiastically.

On The Long Road to Planning

As soon as the spring semester ended in the last part of May, I started work in earnest in preparing for the experiment. From the beginning of June until the end of July, two solid months were spent on testing our beta particle detectors. What type of scintillator would be best for this use? What shape should the head of the light guide be? How could we bring the long lucite light pipe (4′) with small diameter (1″) out of the cryostat? Could one leave the scintillator or the photo-multiplier inside the helium cryostat? How would the polarizing magnetic field affect the counting rates? If we had known that the observed asymmetry effect would be so large, we would have been spared some of these worries, but the thorough preparation was worth all the effort.

On July 24th, I wrote to Dr. Ambler to inform him that the experimentation on the detection of β-particles at liquid helium temperature was progressing satisfactorily. If no unforeseeable technical problems were to arise, I suggested that he and I should get together and also make proper arrangements with the authorities at the NBS.



Ernest Ambler with Chien-Shiung Wu at the time of the experiment at the National Bureau of Standards in Washington, D. C. —where parity violation was first observed. The equipment in the background was used in the experiment reported by Professor Wu in her Discovery Story. Photo: [Courtesy National Bureau of Standards]

A week later on July 31st, I received a letter from Dr. Ambler including a rough sketch of the cryostat to give me an idea of the dimensions involved. However, he said he would be away on vacation for two weeks beginning August 4th. So during the month of August, we made detailed studies of the magnetic field effects on the β-countings and also the backscattering of beta particles from the cerium magnesium nitrate (CMN crystal) source backing. This correction amounted to approximately 30-35% which diluted the observed asymmetry effect.

In the middle of September, I finally went to Washington, D.C. for my first meeting with Dr. Ambler. He was exactly as I had imagined from our numerous telephone conversations: soft-spoken, capable, efficient, and above all, inspired confidence. He took me to his lab and introduced me to Dr. R. P. Hudson, who was his immediate supervisor at that time. The two of them had been working closely together. Hudson's subsequent decision to join our exciting experiment was indeed welcome.

In the beta particle counting and the gamma ray anisotropy measurements we required a great deal of electronics. Dr. R. W. Hayward of the Radioactivity group of the NBS had offered us (through Mrs. Betty Frota-Pessaa, one of my former students who was working at the NBS) the use of his 10-channel, pulse-height

Members of the National Bureau of Standards staff, codiscoverers of parity violation, at the Bureau of Standards laboratory, 1957. Left to right: R.P. Hudson, E. Ambler, R. W. Hayward, D. D. Hoppes. Ambler holds the container for the crystal sample, while Hudson prepares to replace the dewar, prior to cooling the sample with liquid helium. [Photo: Courtesy National Bureau of Standards.]

analyser and other equipment. The eventual joining of Dr. Hayward and his half-time research assistant, D. D. Hoppes, added further strength to our group; and during the exciting yet exasperating days and nights when we had hardly any sleep, we wished we had more such able collaborators.

By the time of my third trip to Washington, D. C., I had grown two ^{60}Co specimens. One was made by taking a good single crystal of CMN and growing on the upper surface only an additional crystalline layer containing ^{60}Co. The thickness of the radioactive layer used was about 0.002 inches and contained a few microcuries of activity. The other had the ^{60}Co evenly distributed throughout the CMN crystal for the study of the anisotropy of the gamma rays.

Everything worked as well as could be expected. The 4-feet long, 1-inch diameter lucite light pipe gave the ^{137}Cs conversion line (624 KeV) a fine resolution of 17%. This excellent resolution was due mostly to the careful selection of a clear lucite rod, the machining of the lucite head to a logarithmic spiral for maximum light collection and, above all, Marion Biavati's personal attention to its surface polish.

Cont. p. 114

EXPLANATORY PHYSICS NOTES FOR NON-SPECIALISTS

PARITY – THE REFLECTION SYMMETRY

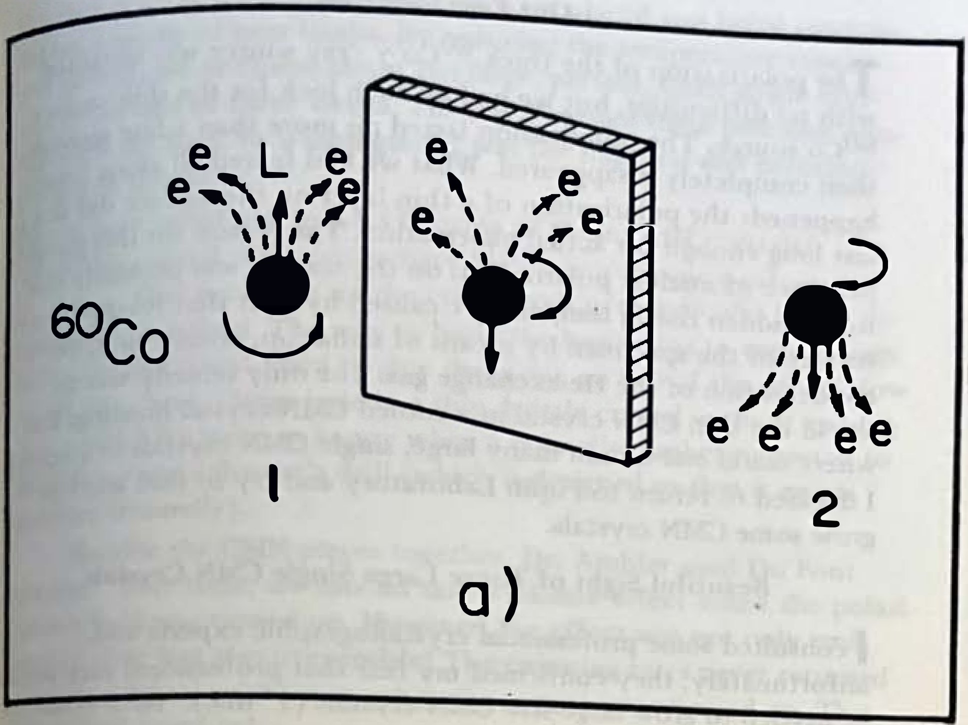
The statement that a physical law is invariant under certain operations means that this law does not change as a result of a change in physical conditions. The basis of this belief in the invariance of physical laws lies in the credence that certain symmetries exist in Nature and these invariant transformations lead to a statement of some conservation law. For example, invariance under linear transformations (homogeneity of space) yields conservation of momentum; invariance under time displacement leads to a conservation of energy; invariance under rotations (isotropy of space) leads to conservation of angular momentum.

The conservation of parity is the conservation law corresponding to invariance under reflections in quantum mechanics. The law requires that the symmetry, or *parity* of a physical system remains unchanged by any physical process. Parity is simply a formal expression of the behavior of the wave function of a particle (or system of particles) when the spacial coordinates x, y, z are inverted through the origin to become -x, -y, -z.

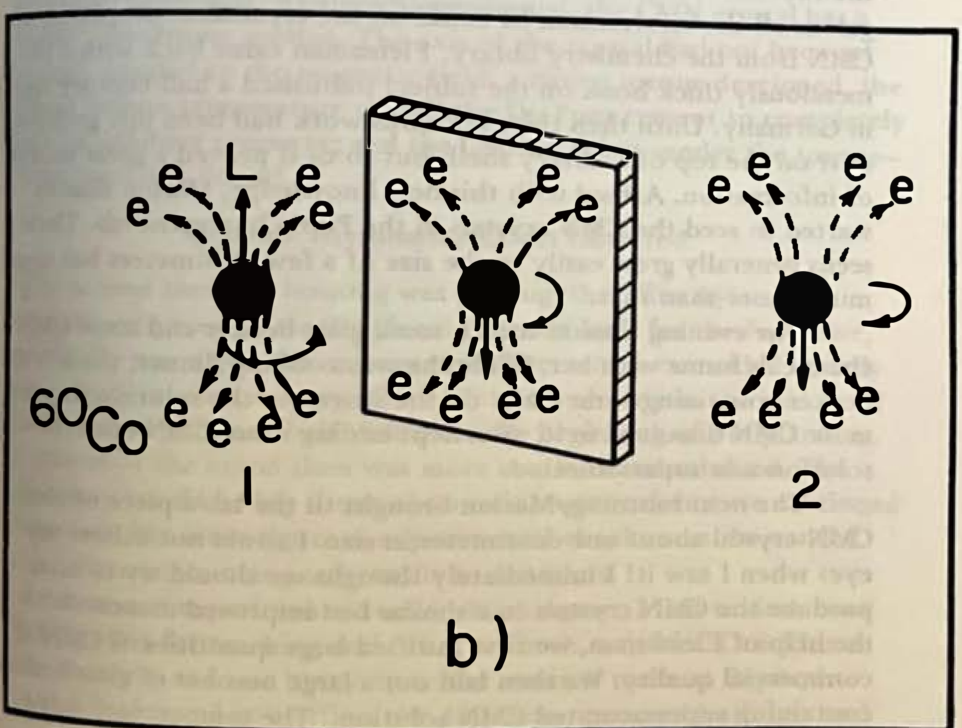
Parity conservation has no classical analogue, so it is difficult to visualize. One can describe this symmetry as requiring any experiment to look identical to its mirror image.

If a crystal of cobalt is kept at a very low temperature and then placed in a strong magnetic field directed upward, the cobalt atoms will line up in the direction of the field. When a ^{60}Co nucleus undergoes β-decay, conservation of parity demands that there be no preferential direction of electron emission. If one were to observe this experiment in a mirror (reflection transformation) the magnetic field would be reversed (see diagram) and the cobalt atoms would line up in the opposite direction. This experiment can then only appear identical if the number of particles emitted upward is equal to the number emitted downward. If there are more particles emitted in either direction, the experiment would appear reversed in a mirror (see diagram). This is, in fact, what happens, i.e., there are more particles emitted in the direction opposite to that of the magnetic field. The experiment, therefore, is not symmetric under reflections, and thus parity is not conserved.

Notice, however, that if we replace all the mirror image particles involved by their antiparticles, the experiment now looks the same as in real life. Thus the combined operation of parity and charge conjugation holds (CP invariance).



L is the direction of the angular momentum vector, e's represent the electrons



Parity Violation

a) The mirror image (center) of particle 1 (left) emits electrons in the same direction as particle 1, but its spin L is in the opposite direction. Configuration 2 (right) is simply configuration 1 under rotation. If parity is to be conserved there should be no difference experimentally between 2 and the mirror image of 1. Parity violation manifests itself by the fact that the electron direction is correlated to L (indicating the presence of the σ · p term).

Parity Conservation

b) The mirror image of particle 1 will only be identical to particle 2 if the number of electrons ejected upward is equivalent to the number ejected downward. This is the case of parity conservation and is shown in the figure at left.

Cont. from p. 111

Our Fear Confirmed

The polarization of the thick ^{60}Co γ - ray source was obtained with no difficulties, but we had no such luck for the thin surface ^{60}Co source. The polarization lasted no more than a few seconds, then completely disappeared. What we had feared all along finally happened: the polarization of a thin layer on the surface did not last long enough for actual observation. The reason for this disappearance of nuclear polarization on the surface was probably due to its sudden rise in temperature caused by heat that reached the surface of the specimen by means of radiation, conduction, or condensation of the He-exchange gas. The only remedy was to shield the thin CMN crystal in a cooled CMN crystal housing. But where could one obtain many large, single CMN crystals in a hurry? I decided to return to Pupin Laboratory and try to find ways to grow some CMN crystals.

Beautiful Sight of Those Large Single CMN Crystals

I consulted some professional crystallographic experts and, unfortunately, they confirmed my fear that professional care would be needed to grow large-size CMN crystals (1″ dia.). Both elaborate equipment and plenty of patience were required and we had neither the funds nor the time. I sent our chemist, Herman Fleishman, to gather all the information he could on the crystalline properties of CMN from the chemistry library. Fleishman came back with a tremendously thick book on the subject published a half century ago in Germany. Until then this enormous work had been just gathering dust on the top of a library shelf, but to us it proved a great source of information. Armed with this new knowledge, Marion Biavati started to seed the CMN crystals in the Pupin basement lab. These seeds generally grew easily to the size of a few millimeters but never much more than that.

One evening Marion took a small glass beaker and some CMN chemicals home with her. While she was cooking dinner, she left the beaker containing some CMN on the stove. As the solution warmed, more CMN dissolved in it. She kept adding more CMN until the solution was supersaturated.

The next morning, Marion brought to the lab a piece of clear CMN crystal about one centimeter in size. I could not believe my eyes when I saw it! I immediately thought we should try to mass produce the CMN crystals in a similar but improved manner. With the help of Fleishman, we first purified large quantities of CMN of commercial quality. We then laid out a large number of glass beakers containing supersaturated CMN solution. The temperature inside the enclosure in which these beakers were placed was being controlled by a group of heat lamps. By reducing the temperature evenly and slowly, we obtained about ten large, perfect, CMN single crystals at the end of three weeks. The day I carried these precious crystals with me back to Washington, I was the happiest and proudest person in the world.

Now, how could one make a housing of these CMN crystals? The CMN crystal is known to have highly anisotropic g-values: $g_\perp > g_\parallel$. One must line up the crystal axis perpendicular to the demagnetization field. The way to build the house was to carve a large hole in each crystal, then to glue them one on top of the other. How could one carve a large hole in a thin, brittle crystal without causing it to crack? We were so happy when a crystallographer suggested to us that we use a dentist's drill (which is designed so that it exerts pressure *inwardly*).

To glue the CMN pieces together, Dr. Ambler used Du Pont cement. This time, we saw an unmistakable effect when the polarization field was turned on. However, the effect was not only eminently clear but also irreversible! The counting rates never returned to their original values even when the source was warmed up. The house had caved in!

When the cryostat was warmed up and opened, we saw exactly what had happened. As already mentioned, the CMN crystal has a highly anisotropic g-value. The axis of the crystal had not been set exactly parallel to the magnetic field, a strong torque developed, the liquid helium temperature caused the Du Pont cement to completely lose its adhesive property; and the CMN housing—under the torque—came tumbling down!

Genuine Asymmetry Effect Observed

The second time the housing was put together, fine nylon threads were used to tie the pieces together and, for the first time, we finally saw a genuine asymmetry effect which coincided exactly with the γ - ray anisotropy effect.That was already in the middle of December, 1956, one half year after the beginning of our planning. I remember the mood then was more cautious and subdued. The discovery would be big if our observation was real, but we cautioned ourselves that more rigorous experimental checks must be carried out before announcing our results to outsiders.

Between experimental runs in Washington, I had to dash back to Columbia for teaching and other research activities. One Thursday morning, as I was hurrying to the seminar room at Pupin, I passed Professor Lee's office; the door was open and both Lee and

Alignment & Polarization

The words *alignment* and *polarization* are sometimes used inexactly. *Alignment* generally means an orientation along an axis without preference of direction, while *polarization* specifies a preferred direction along the axis.

Yang were there. As I stuck my head in to say hello, they inquired about the ^{60}Co experiment. I casually mentioned that it seemed there was a huge asymmetry effect. They were excited and pleased. As I passed their room again, after the seminar, they wanted to know more. I told them the effect was large and reproducible, but it must be regarded as preliminary because some systematic checks were not yet completed. I remember on that occasion, Yang also wanted to know whether anyone had calculated the interference term between the G-T and Fermi interaction. I told him that Dr. M. Morita had carried out these calculations in detail and the interference term might be destructive, depending on the signs between C_A and C_V'. I said I was pleased that the beta transition in ^{60}Co was a pure G-T transition. We know now that the observed asymmetry parameter A in ^{60}Co ($5 \to 4$) is nearly -1, but it is much reduced in mixed transitions such as in neutrons ($½ \to ½$), A is -0.11 and in ^{19}Ne ($½ \to ½$), A is -0.057.

Rigorous Experimental Checks

One week later, after some modifications on the glass dewar were completed, we began to follow through intense experimental checks on the asymmetry effect observed. First, we had to prove that this symmetry effect was not due to the strong magnetic fields of the CMN crystals produced at extremely low temperatures. We also needed to show that this effect was not due to the remnant magnetization in the sample induced by the strong demagnetization field. The most clear-cut control experiment would be one in which a beta activity would be introduced into the CMN crystal, but in which the radioactive nucleus would be known not to be polarized. No asymmetry effect should be detected. To carry out all these experiments would take many weeks.

On Christmas eve, I returned to New York on the last train; the airport was closed because of heavy snow. There I told Professor Lee that the observed asymmetry was reproducible and huge, but we had not exhausted all experimental checks yet. When I started to make a quick rough estimate of the asymmetry parameter A, I found it was nearly -1. Professor Lee realized it immediately and said that this was very good. The result of A = -1 was the first indication that the interference between parity conserving and parity non-conserving terms in the G-T interaction Hamiltonian was close to maximum or, $C_A = C_A'$. This result is just what one should expect for a two-component theory of the neutrino in a pure Gamow-Teller transition. It also implies that, in this case, the charge conjugation is noninvariant. Lee then told me that during the summer of 1956, when he and Yang worked together at Brookhaven National Laboratory, they had not only entertained the idea of the two-component theory of the neutrino, but had also worked out some details of the theory. However, they felt it was too rash to publish it before the violation of the law of parity was experimentally observed. Confronted by the clear evidence of the two-component theory of the neutrino, we discussed possible experiments one could do. One of them was the measurement of electron polarization; the other was the π - μ - e parity experiment. It was not too clear how to actually carry them out. At that time I was still hesitant to have them quote our results but promised to give them our affirmative answer soon.

Law of Parity Overthrown

On January 2nd, I went back to the Bureau to continue with our experimental checks. Some of these checks had not gone as smoothly as we had expected. The period between January 2nd and January 8th was probably the most tense in our whole experimental venture. Our cryostat at the NBS was made of glass and the glass joints were put together with low-temperature vacuum grease which was concocted by melting together glycerine and Palmolive soap. (Later on, we changed to Ivory soap.) The trouble which plagued us repeatedly was the superfluid leak below the lambda point (T=2.3° K). Each time this happened, it took at least 6-8 hours to warm up, regrease and then cool the cryostat down again. To save time, Hoppes slept on the ground near the cryostat in a sleeping bag. Whenever the cryostat reached liquid helium temperature, he would telephone each of us to go to the lab, no matter what time of the night it was.

During the week of January 7th, rumors started to come in fast about the Nevis π - μ - e parity experiments. Very much alarmed and excited, the director and the high administration officials of the NBS came to call on us and wanted to know more about our experiment which was rumored to be as important as the Michelson-Morley one.

We were as vigilant as ever. Even after the muon decay had shown the violation of the law of parity, we still did not relax. We had to be totally convinced ourselves.

After we finished all the experimental checks which we had set out to do, we finally gathered together around 2 o'clock in the morning of January 9th to celebrate the great event. Dr. Hudson smilingly opened his drawer and pulled out a bottle of wine* and put it on the table with a few small paper cups. We finally drank to the overthrow of the law of parity.

I remember vividly several research workers in other sections

Two Component Theory of the Neutrino

The experiments which established parity nonconservation in weak interactions led to the formulation of a simple and appealing theory of the neutrino. Proposed independently by Lee and Yang[1], Landau[2], and Salam[3], this theory requires that the spins of a neutrino and antineutrino can only be aligned antiparallel or parallel, respectively, to their momentum vectors. As a result, the four-component Dirac equation (relativistic generalization of the Schrödinger equation) can be reduced to a two-component equation. The theory also requires a massless neutrino, which is compatible with present experimental data.

The two-component theory of the neutrino violates both charge conjugation and parity invariance separately. However, it does retain invariance under the combined operation CP.

For a more complete discussion, see Explanatory Physics Note: "Phenomenological Aspects of the Two-Component Theory of the Neutrino," p. 144.

1. Lee and Yang, *Phys. Rev.* 105, 167 (1957).
2. Landau, *Nuc. Phys.* 3,127 (1957).
3. Salam, *Nuovo Cim.* 5, 299 (1957).

* *According to R. P. Hudson, the bottle of wine he opened was actually a Chateau Lafite-Rothschild, vintage 1949.*

of the low temperature laboratories stopping by our lab the next morning and being surprised by the silent and relaxed atomosphere. They suddenly turned around to take a look at our waste paper basket and nodded to themselves, "All right, the law of parity in beta decay is dead!"

I hurried back to the Pupin Laboratories on the night of January 10th and on the morning of the 11th, a Saturday, there was a meeting in room 831. Lee, Yang, the Nevis group, and I were all there. The discussion lead by the two brilliant theorists was enthralling. Before that meeting our results had already been written up to be submitted to ***Physical Review.*** What a great shock to the world of physics!

On the afternoon of January 15th, the Department of Physics at Columbia University called a press conference to announce the dramatic overthrow of a basic law of physics, known as the conservation of parity, to the public. The next day, the New York ***Times*** carried a front page headline "Basic Concept in Physics Reported Upset in Tests." The news burst into public view and quickly spread around the world. As Professor O.R. Frisch of Cambridge University described it in a talk at that time, "The obscure phrase 'parity is not conserved' circled the globe like a new gospel."

As usual, following an important discovery, we were asked to give symposia, colloquia, and lectures on our experiments. Finally, the American Physical Society held its annual meeting in New York around the end of January. A post-deadline paper session was assigned to the topic of the nonconservation of parity. Later, K. K. Darrow recorded the event with his lively and witty pen in ***The Bulletin of A. P. S.*** *2* (1956-57):

> **On Saturday afternoon to boot—the largest hall normally at our disposal was occupied by so immense a crowd that some of its members did everything but hang from the chandeliers.**

The sudden liberation of our thinking on the very structure of the physical world was overwhelming. Activities along these lines advanced at an unprecedented pace. First, the nonconservation of parity was also observed in the decay of the muon (see Garwin's and Telegdi's stories pp. 124 and 131). The asymmetry effect of the beta particles from the polarized ^{60}Co was also used to examine the validity of time reversal and it was found sound. Therefore, in weak interactions, the charge conjugation "C" and the parity "P" were both violated and the time reversal "T" was still intact. This suggested the combined "CP" invariance.

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Experimental Test of Parity Conservation in Beta Decay*

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and
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National Bureau of Standards, Washington, D. C.
(Received January 15, 1957)

In a recent paper[1] on the question of parity in weak interactions, Lee and Yang critically surveyed the experimental information concerning this question and reached the conclusion that there is no existing evidence either to support or to refute parity conservation in weak interactions. They proposed a number of experiments on beta decays and hyperon and meson decays which would provide the necessary evidence for parity conservation or nonconservation. In beta decay, one could measure the angular distribution of the electrons coming from beta decays of polarized nuclei. If an asymmetry in the distribution between θ and $180°-\theta$ (where θ is the angle between the orientation of the parent nuclei and the momentum of the electrons) is observed, it provides unequivocal proof that parity is not conserved in beta decay. This asymmetry effect has been observed in the case of oriented Co^{60}.

It has been known for some time that Co^{60} nuclei can be polarized by the Rose-Gorter method in cerium magnesium (cobalt) nitrate, and the degree of polarization detected by measuring the anisotropy of the succeeding gamma rays.[2] To apply this technique to the present problem, two major difficulties had to be overcome. The beta-particle counter should be placed *inside* the demagnetization cryostat, and the radioactive nuclei must be located in a *thin surface* layer and polarized. The schematic diagram of the cryostat is shown in Fig. 1.

To detect beta particles, a thin anthracene crystal $\frac{3}{8}$ in. in diameter $\times \frac{1}{16}$ in. thick is located inside the vacuum chamber about 2 cm above the Co^{60} source. The scintillations are transmitted through a glass window and a Lucite light pipe 4 feet long to a photomultiplier (6292) which is located at the top of the cryostat. The Lucite head is machined to a logarithmic spiral shape for maximum light collection. Under this condition, the Cs^{137} conversion line (624 kev) still retains a resolution of 17%. The stability of the beta counter was carefully checked for any magnetic or temperature effects and none were found. To measure the amount of polarization of Co^{60}, two additional NaI gamma scintillation counters were installed, one in the equatorial plane and one near the polar position. The observed gamma-ray anisotropy was

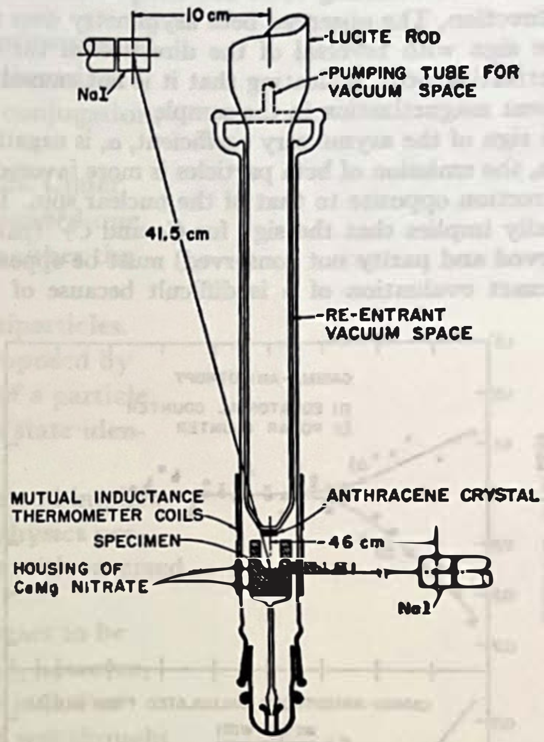

Fig. 1. Schematic drawing of the lower part of the cryostat.

used as a measure of polarization, and, effectively, temperature. The bulk susceptibility was also monitored but this is of secondary significance due to surface heating effects, and the gamma-ray anisotropy alone provides a reliable measure of nuclear polarization. Specimens were made by taking good single crystals of cerium magnesium nitrate and growing on the upper surface only an additional crystalline layer containing Co^{60}. One might point out here that since the allowed beta decay of Co^{60} involves a change of spin of one unit and no change of parity, it can be given only by the Gamow-Teller interaction. This is almost imperative for this experiment. The thickness of the radioactive layer used was about 0.002 inch and contained a few microcuries of activity. Upon demagnetization, the magnet is opened and a vertical solenoid is raised around the lower part of the cryostat. The whole process takes about 20 sec. The beta and gamma counting is then started. The beta pulses are analyzed on a 10-channel pulse-height analyzer with a counting interval of 1 minute, and a recording interval of about 40 seconds. The two gamma counters are biased to accept only the pulses from the photopeaks in order to discriminate against pulses from Compton scattering.

A large beta asymmetry was observed. In Fig. 2 we have plotted the gamma anisotropy and beta asymmetry *vs* time for polarizing field pointing up and pointing down. The time for disappearance of the beta asymmetry coincides well with that of gamma anisotropy. The warm-up time is generally about 6 minutes, and the warm counting rates are independent of the field direction. The observed beta asymmetry does not change sign with reversal of the direction of the demagnetization field, indicating that it is not caused by remanent magnetization in the sample.

The sign of the asymmetry coefficient, α, is negative, that is, the emission of beta particles is more favored in the direction opposite to that of the nuclear spin. This naturally implies that the sign for C_T and C_T' (parity conserved and parity not conserved) must be opposite. The exact evaluation of α is difficult because of the many effects involved. The lower limit of α can be estimated roughly, however, from the observed value of asymmetry corrected for backscattering. At velocity $v/c \approx 0.6$, the value of α is about 0.4. The value of $\langle I_z \rangle / I$ can be calculated from the observed anisotropy of the gamma radiation to be about 0.6. These two quantities give the lower limit of the asymmetry parameter $\beta(\alpha = \beta \langle I_z \rangle / I)$ approximately equal to 0.7. In order to evaluate α accurately, many supplementary experiments must be carried out to determine the various correction factors. It is estimated here only to show the large asymmetry effect. According to Lee and Yang[3] the present experiment indicates not only that conservation of parity is violated but also that invariance under charge conjugation is violated.[4] Furthermore, the invariance under time reversal can also be decided from the momentum dependence of the asymmetry parameter β. This effect will be studied later.

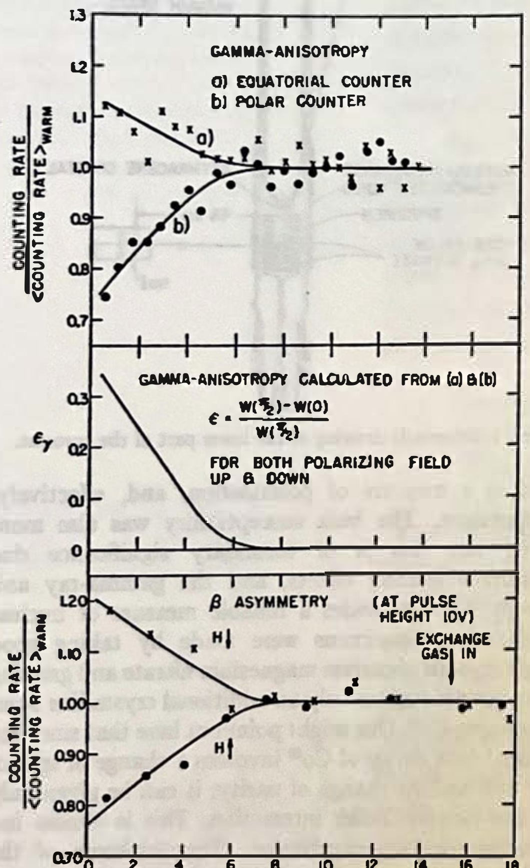

FIG. 2. Gamma anisotropy and beta asymmetry for polarizing field pointing up and pointing down.

The double nitrate cooling salt has a highly anisotropic *g* value. If the symmetry axis of a crystal is not set parallel to the polarizing field, a small magnetic field will be produced perpendicular to the latter. To check whether the beta asymmetry could be caused by such a magnetic field distortion, we allowed a drop of $CoCl_2$ solution to dry on a thin plastic disk and cemented the disk to the bottom of the same housing. In this way the cobalt nuclei should not be cooled sufficiently to produce an appreciable nuclear polarization, whereas the housing will behave as before. The large beta asymmetry was not observed. Furthermore, to investigate possible internal magnetic effects on the paths of the electrons as they find their way to the surface of the crystal, we prepared another source by rubbing $CoCl_2$ solution on the surface of the cooling salt until a reasonable amount of the crystal was dissolved. We then allowed the solution to dry. No beta asymmetry was observed with this specimen.

More rigorous experimental checks are being initiated, but in view of the important implications of these observations, we report them now in the hope that they may stimulate and encourage further experimental investigations on the parity question in either beta or hyperon and meson decays.

The inspiring discussions held with Professor T. D. Lee and Professor C. N. Yang by one of us (C. S. Wu) are gratefully acknowledged.

* Work partially supported by the U. S. Atomic Energy Commission.

[1] T. D. Lee and C. N. Yang, Phys. Rev. 104, 254 (1956).

[2] Ambler, Grace, Halban, Kurti, Durand, and Johnson, Phil. Mag. 44, 216 (1953).

[3] Lee, Oehme, and Yang, Phys. Rev. (to be published).

[4] Their arguments are as follows: From the He[6] recoil experiment and from Eq. (A-4) of reference 1 one concludes that $(|C_A|^2+|C_A'|^2)/(|C_T|^2+|C_T'|^2) \lesssim \frac{1}{3}$. Hence, by comparing Eq. (16) of reference 3 [see also Eq. (A-6) of reference 1], one concludes that the present large asymmetry is possible only if both conservation of parity and invariance under charge conjugation are violated.

EXPLANATORY PHYSICS NOTES FOR NON-SPECIALISTS

CPT Invariance

Symmetries in Nature are expressed in physics as principles of invariance. Three such 'reflection symmetries' are: the invariance of space inversion, or parity P; invariance under charge conjugation, C; and time reversal invariance, T.

P conservation is an inversion of all three space axes. Under this transformation, a right-hand screw becomes a left-handed one. The fundamental physical laws should look the same in either the original or the inverted system.

C invariance is an interchange of particles with antiparticles. It reflects the symmetry of matter and antimatter as proposed by Dirac. Charge conjugation implies that for every state of a particle system, there exists a corresponding antiparticle system state identical in every way.

T invariance involves a change in the temporal axis and implies that for every state of motion of a system, the laws of physics permit a corresponding reversed state where particles move with reversed velocities and spins.

Each of these symmetries was thought for many years to be exact. The experiments described in this chapter showed, however, that P is not conserved in weak interactions and neither is C. For several years following these experiments, CP combined was thought to be the correct invariant, i.e., if all particles are replaced by antiparticles in their mirror image, then a symmetry still remains.

If one applies all three transformations CPT, one then obtains a transformation that is invariant, although it may not be invariant under the separate transformations C,P,T, or any combination of two of these. In beta decay, for example, the T operation and the combined CP operation are invariant. More recent experimentation has produced strong evidence that even the CP symmetry is not exact in weak interactions. The decay of the K^0 meson violates both the combined CP invariance and T invariance, but not the CPT invariance.

If the combined operator CPT were shown not to be an invariant, then the whole theory of elementary particles would have to be drastically revised.

SUBSEQUENT DEVELOPMENTS

Immediately after the news was announced, I received a letter from Wolfgang Pauli in which he pondered on this combined "CP" invariance after learning the results of our experiments:

Zürich
Jan. 19, 1957

Dear C. S. Wu:

Exciting news is coming from the States (Blatt wrote it to me in a letter from Princeton) about an asymmetric angular distribution in your beta-decay experiment with directed nuclear spins, indicating a restriction of the left-right asymmetry of the theory in such a way that the left-right interchange must be coupled with a change of sign of the 'lepton-charge' (means neutrino ⇄ antineutrino, $e^+ \rightleftarrows e^-$). So it is, if I understood Blatt correctly.

When I considered such formal possibilities in my paper in the Bohr-Festival Volume (1955), I did not think that this could have something to do with Nature. I considered it merely as a mathematical play, and, as a matter of fact, I did not believe in it when I read the paper of Yang and Lee. I did not believe Salam either, when I read this proposal to establish a connection between the above restriction in parity and the vanishing of the rest-mass of the neutrino. Salam's proposal has a certain beauty in itself; namely, it is equivalent with the description of the neutrino with a *two*-component spin only.

What prevented me *until now* from accepting this formal possibility is the question why this restriction of mirroring appears only in the 'weak' interactions, not in the 'strong' ones. *Theoretically,* I do not see any interpretation of this fact, which is empirically so well established. Do you know somebody in the States, who has real ideas about that?

Can you write me more about your work—of course only when you have time for it and when you are sufficiently sure about the results—and can you send me a preprint, when there will be one?

That Professor Pauli was perturbed by these dramatic events can be seen from the following lines:

In any case, I congratulate you (to the contrary of myself). This particle neutrino—on which I am not innocent—still persecutes me. On Monday, I have to give a more general lecture on old and new history of the neutrino. I have read much literature this autumn, including your article in K. Siegbahn's Handbook. It is fine, but I disagree with your discussion of the upper limit of the neutrino rest-mass which seems to me too high.

Two months later, the observation of the longitudinal polarization of the beta particles was first reported by H. Frauenfelder's group*. Soon the precision determinations of the longitudinal polarizations of negative and positive beta particles by different techniques were reported from numerous laboratories. The other ingenious parity experiments like the β - γ circular polarization correlation were independently reported by H. Schopper, F. Boehm and A. N. Wapstra**. It was, indeed, a most productive and exciting period. This can be seen from the Proceedings of two important conferences: the Rochester Conference† (May 1957), and the Rehovoth Conference‡ (Sept. 1957).

* Frauenfelder, H., *et al.*, *Phys. Rev.* 106, 386 (1957).

** Appel, H., and Schopper, H., *Zeit. Phys.* 149, 103 (1957); Boehm, F., and Wapstra, A. N., *Phys. Rev.* 106, 1364 (1957), *Phys. Rev.* 107, 1202 (1957), *Phys. Rev.* 107, 1492 (1957), and *Phys. Rev.* 109, 456 (1958).

† Edited by G. Ascoli *et al.*, section VII "Weak Interactions" (Interscience, 1957.)

‡ Edited by H. Lipkin, section VI, p. 319 (North-Holland, 1958.)

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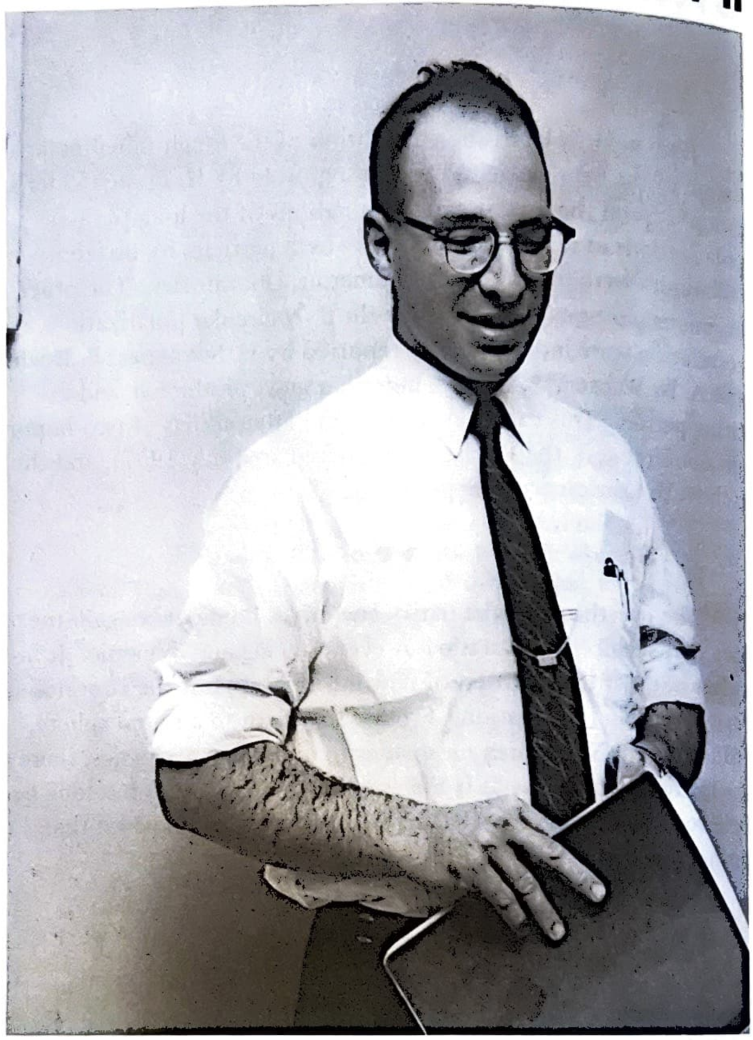
The overthrow of the parity law drives home once again the idea that science is not static but ever growing and dynamic. It involves not just the addition of new information but the continuous revision of old information. From a flat earth to a round sphere, from classical Newtonian mechanics to quantum mechanics, there are many illustrations. It is the courage to doubt what has long been believed and the incessant search for verification and proof that pushes the wheels of science forward.□

Chien-Shiung Wu

New York, March 1973.

DISCOVERY STORY II



Richard L. Garwin,
August, 1961

One Researcher's Personal Account

by
Richard Garwin

MUCH of the physics and direct communication in the Physics Department at Columbia takes place around the lunch table of a Chinese restaurant on Fridays. It was at such a luncheon, toward the end of December 1956, that we learned of the positive results indicating nonconservation of parity in the cobalt-60 experiment of Professor Wu. Ideas of parity violation in decays involving the μ-meson, however, centered on studying the π - μ - e decay chain in photographic emulsion—or else working with the submillimeter range of the μ-meson to separate the μ's from the π's—since it was also predicted, under certain assumptions, that the μ would be polarized along its direction of emission from a pion at rest.

On Friday, January 4, I had been at Poughkeepsie and missed our weekly luncheon. Shortly after I returned home around 8 o'clock, I received a phone call from Leon Lederman. He quickly told me that he had thought of a way to obtain a beam of polarized muons by taking advantage of the well-known fact that muons emitted forward from a beam of pions in flight have higher energy and range than those emitted backwards. In fact, for many years, counting experiments had been done at Nevis* using such beams of separated muons produced in the fringe field of the cyclotron by the forward decay of pions in flight. Thus there was the possibility, if parity was really not conserved, that these muons in our standard beams were already polarized! Leon continued by saying that if we could think of a reliable way to measure the asymmetry of the emitted electrons from these muons, then we could actually *do* the experiment! I suggested that we meet at the cyclotron in fifteen minutes, and Leon quickly agreed.

Once at the cyclotron, we mulled over the possibility of stopping the muons and worried that either the muon spin would not retain its initial direction during the stopping process, or that the muons might be depolarized in the microsecond or two before decay. There was also the problem of detecting an electron asymmetry which might be small even in the case of *fully* polarized muons. What made matters worse was the fact that we had no guarantee that our muon beam was anywhere near fully polarized.

Marcel Weinrich had been doing his thesis with Leon Lederman. His work involved the stopping of positive or negative muons in various materials and the determination of the rate of emission of electrons as a function of delay time (with a view to measuring the lifetime of the negative muon). It was therefore natural that we first considered physically swinging a counter around a block in which the muons were to be stopped. But we immediately noted that false front-to-back asymmetries would be introduced by a nonsymmetrical stopping distribution of muons, by the fringe field of the cyclotron modifying the gain of the photomultiplier tubes, etc.

Since about 1953**, I had been working in nuclear magnetic resonance on He^3 liquid, so that I was quite familiar with the extent to which one could rely completely on classical concepts in dealing with nuclear spins and their interactions with magnetic fields. It therefore occurred to me that the best arrangement by far would be to leave the counters fixed and to move the spins (if, indeed, there was a magnetic moment associated with the muon spin, the magnitude of the muon spin, itself, being uncertain at that time). This would sweep any electron decay asymmetry past a fixed counter telescope.

Our problems were to choose a stopping material which would not depolarize the muons in the slowing-down period of about two microseconds, and to produce a uniform magnetic field throughout the stopping material. The system also had to be compatible with the detection of electrons by an electron telescope.

The machine shop at Nevis was locked by that time on Friday

* *Nevis Laboratories, high energy physics laboratory of Columbia University, at Irvington-on-Hudson, 20 miles N.W. of New York City.*

** *Before joining IBM in New York in 1952, Dick Garwin worked at the University of Chicago in beta-gamma angular correlation and on some cyclotron experiments. He developed first a twofold, and then a multiple-input coincidence-anticoincidence analyzer*[1] *and subsequently arranged for IBM to purchase one of the latter instruments from the University of Chicago that he had made there. This instrument and similar devices, which had been built at Nevis, were major components of the equipment used in 1957 for the* π - μ - *e experiment.*

In 1950, a flexible coincidence-anticoincidence set of 10^{-8} *sec resolution and good stability was a major advance, and the Garwin coincidence circuit was in standard use for over a decade.*

1. *Rev. Sci. Instr.*, 24, 618 (1953).

night, so, using what was on hand, I selected a standard one-inch carbon block for the stopping material. A lathe served to trim a hollow cylindrical lucite shell and to wind upon it a uniform solenoid which we could connect to some nominal counter signal leads in order to control a precession field from the counting room, as well as being able to leave the experiment unattended on the counting floor.

The Appearance and Disappearance of the Effect

As I remember now, the cyclotron closed Saturday morning for the weekend, but by approximately 6 A.M. we had observed a substantial influence of solenoid current on the electron counting rate—a significant effect. Such an effect could only mean that the muon beam was strongly polarized and that the electron asymmetry about the muon spin direction was large. Furthermore, we had selected the magnitude of solenoid current to give the largest effect if the muon spin was one half, and the muon g-value was 2.

There was no initial basis for assurance regarding any of these assumptions, so our experiment required a small amount of hunting for the best g-value, and thus the best magnetic field to produce the largest amplitude of precession curve. As our counting rate was quite low, the counts during alternate 20-minute cycles were recorded manually and differenced to give an indication of the asymmetry. Unfortunately, by 9 A.M., when the cyclotron shut down for the weekend, our effect seemed to have vanished. We verified the connections and the counter calibration. Finding nothing amiss, we went down to the experimental area to turn off the power supplies to the counters. Here, unhappily, we noticed that the lucite coil form had overheated and that the copper wire was lying at the base of the form, instead of being wrapped around its surface.

Over the weekend, we considered better ways to produce the magnetic field, and finally decided to uniformly wind wire in the form of a solenoid of rectangular cross section directly onto the carbon stopping block and to provide a ferromagnetic return path which would not interfere with either the incident beam or the electron telescope.

A 22 Standard Deviation Effect

Monday was maintenance day, consequently the cyclotron did not come on until evening. We set up the new apparatus, checked counters, and about midnight began taking data. At around 3 A.M. when Leon went home, it was still not certain that there was any effect. I continued the run. Three hours later I was on the phone to tell him that the effect was now 22 standard deviations and that there was absolutely no doubt that we had established the nonconservation of parity and charge conjugation in the case of the π - μ decay and the subsequent μ - e decay! Leon returned immediately, inspected the data, and recorded a few more points. Shortly thereafter, we called T. D. Lee and gave him the good news.

The most striking characteristic of the experiment was that our analysis and intuition had succeeded in persuading us of the feasibility of performing such an experiment in the face of three major unknowns: the polarization of the muon beam; the magnitude of the electron decay asymmetry; and the preservation of the muon spin direction in slowing down and stopping before its decay. But the sinusoidal curve of our results left us with no doubts as to the reality and interpretation of the effect. It was exciting to have such a prompt and unambiguous result which at once confirmed all three working hypotheses. We were ready to publish by Tuesday afternoon, by which time I had worked out the theoretical curve for the experimental results and had also found a way of obtaining from the experimental curve the g-value as corrected for the decay of the μ-meson, the angular breadth of the electron telescope, and the effect of the gate width in smearing the curve. It would not, however, have been appropriate for our results to precede in print the work of Professor Wu and her collaborators. There followed intensive exploration of the sensitivity of the asymmetry to both electron energy and stopping material. These results are also reported in our paper (reproduced p. 128).

New Avenue for Adventure Opens

The graphic nature of our muon results left no room for skepticism. As noted in our Letter, we had convincingly demonstrated a tool with applications far beyond the exhibition of nonconservation of parity. Much work followed immediately at Nevis and elsewhere, as energetic and clever physicists raced to exploit this new tool and phenomenon which had been right under our noses for six or seven years. Indeed, nonexponential decays which had sometimes been observed (irreproducibily) for muons stopped in matter had arisen from the precession of polarized muons in the cyclotron fringe field!

As one result of our experiment, sleepy cyclotron laboratories revived; night work became common. Our own work extended to the measurement of some solid state effects, to the precision measurement of the magnetic moment of the muon, and much later to an experiment, in which I participated at CERN, that involved the trapping of polarized muons in a six-meter-long static magnetic field and allowed a direct measurement of the departure of the muon's g-value from 2. Tirelessly, I worked twenty hours a day for weeks, exploring ideas, building multimegawatt rf pulsers, nursing them into operation, and integrating equations of spin motion. All of us were amazed with the amount we could accomplish.□

New York, January, 1973

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Observations of the Failure of Conservation of Parity and Charge Conjugation in Meson Decays: the Magnetic Moment of the Free Muon*

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(Received January 15, 1957)

LEE and Yang[1-3] have proposed that the long held space-time principles of invariance under charge conjugation, time reversal, and space reflection (parity) are violated by the "weak" interactions responsible for decay of nuclei, mesons, and strange particles. Their hypothesis, born out of the $\tau-\theta$ puzzle,[4] was accompanied by the suggestion that confirmation should be sought (among other places) in the study of the successive reactions

$$\pi^+\rightarrow\mu^++\nu, \quad (1)$$

$$\mu^+\rightarrow e^++2\nu. \quad (2)$$

They have pointed out that parity nonconservation implies a polarization of the spin of the muon emitted from stopped pions in (1) along the direction of motion and that furthermore, the angular distribution of electrons in (2) should serve as an analyzer for the muon polarization. They also point out that the longitudinal polarization of the muons offers a natural way of determining the magnetic moment.[5] Confirmation of this proposal in the form of preliminary results on β decay of oriented nuclei by Wu *et al.* reached us before this experiment was begun.[6]

By stopping, in carbon, the μ^+ beam formed by forward decay in flight of π^+ mesons inside the cyclotron, we have performed the meson experiment, which establishes the following facts:

I. A large asymmetry is found for the electrons in (2), establishing that our μ^+ beam is strongly polarized.

II. The angular distribution of the electrons is given by $1+a\cos\theta$, where θ is measured from the velocity vector of the incident μ's. We find $a=-\frac{1}{3}$ with an estimated error of 10%.

III. In reactions (1) and (2), parity is not conserved.

IV. By a theorem of Lee, Oehne, and Yang,[2] the observed asymmetry proves that invariance under charge conjugation is violated.

V. The g value (ratio of magnetic moment to spin) for the (free) μ^+ particle is found to be $+2.00\pm0.10$.

VI. The measured g value and the angular distribution in (2) lead to the very strong probability that the spin of the μ^+ is $\frac{1}{2}$.[7]

VII. The energy dependence of the observed asymmetry is not strong.

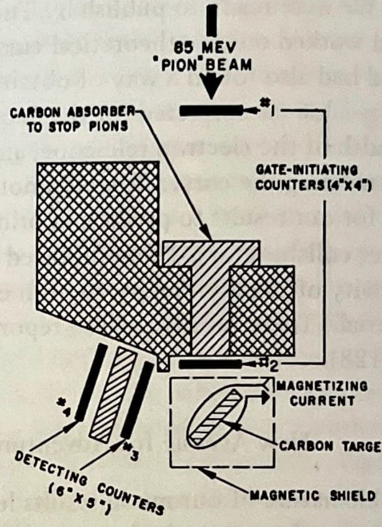

FIG. 1. Experimental arrangement. The magnetizing coil was close wound directly on the carbon to provide a uniform vertical field of 79 gauss per ampere.

VIII. Negative muons stopped in carbon show an asymmetry (also leaked backwards) of $a\sim-1/20$, i.e., about 15% of that for μ^+.

IX. The magnetic moment of the μ^-, bound in carbon, is found to be negative and agrees within limited accuracy with that of the μ^+.[8]

X. Large asymmetries are found for the e^+ from polarized μ^+ beams stopped in polyethylene and calcium. Nuclear emulsion (as a target in Fig. 1) yields an asymmetry of about half that observed in carbon.

The experimental arrangement is shown in Fig. 1. The meson beam is extracted from the Nevis cyclotron in the conventional manner, undergoing about 120° of magnetic deflection in the cyclotron fringing field and about −30° of deflection and mild focusing upon emerging from the 8-ft shielding wall. The positive beam contains about 10% of muons which originate principally in the vicinity of the cyclotron target by pion decay-in-flight. Eight inches of carbon are used in the entrance telescope to separate the muons, the mean range of the "85-"Mev pions being ~5 in. of carbon. This arrangement brings a maximum number of muons to rest in the carbon target. The stopping of a muon is signalled by a fast 1–2 coincidence count. The subsequent beta decay of the muon is detected by the electron telescope 3–4 which normally requires a particle of range >8 g/cm^2(~25-Mev electrons) to register. This arrangement has been used to measure the lifetimes of μ^+ and μ^- mesons in a vast number of elements.[9] Counting rates are normally ~20 electrons/min in the μ^+ beam and ~150 electrons/min in the μ^- beam with background of the order of 1 count/min.

In the present investigation, the 1–2 pulse initiates a gate of duration $T=1.25$ μsec. This gate is delayed by $t_1=0.75$ μsec and placed in coincidence with the electron detector. Thus the system counts electrons of energy >25 Mev which are born between 0.75 and 2.0 μsec after the muon has come to rest in carbon. Consider now the possibility that the muons are created in reaction (1) with large polarization in the direction of motion. If the gyromagnetic ratio is 2.0, these will maintain their polarization throughout the trajectory. Assume now that the processes of slowing down, stopping, and the microsecond of waiting do not depolarize the muons. In this case, the electrons emitted from the target may have an angular asymmetry about the polarization direction, e.g., for spin ½ of the form $1+a\cos\theta$. In the absence of any vertical magnetic field, the counter system will sample this distribution at $\theta=100°$. We now apply a small vertical field in the magnetically shielded enclosure about the target, which causes the muons to precess at a rate of $(\mu/s\hbar)H$ radians per sec. The probability distribution in angle is carried around with the μ-spin. In this manner we can, with a fixed counter system, sample the entire distribution by plotting counts as a function of magnetizing current for a given time delay. A typical run is shown in Fig. 2. As an example of a systematic check, we have

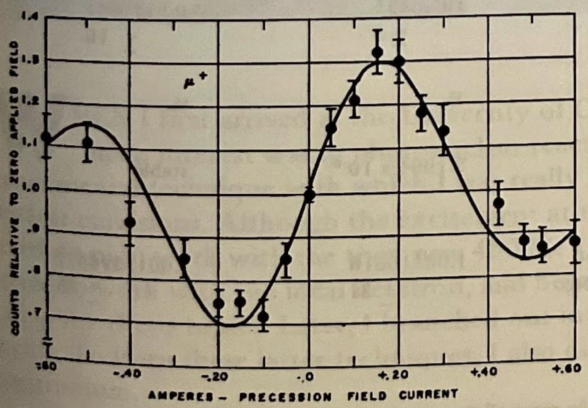

FIG. 2. Variation of gated 3–4 counting rate with magnetizing current. The solid curve is computed from an assumed electron angular distribution $1-\frac{1}{3}\cos\theta$, with counter and gate-width resolution folded in.

reduced the absorber in the telescope to 5 in. so that the end-of-range of the main pion beam occurred at the carbon target. The electron rate rose accordingly by a factor of 10, indicating that now electrons were arising from muons isotropically emitted by pions at rest in the carbon. No variation in counting rate with magnetizing current was then observed, the ratio of the rate for $I=+0.170$ amp to that for $I=-0.150$ amp, for example, being 0.989 ± 0.028. The highest field produced at the target was ~50 gauss which generates a stray field outside of the magnetic shield of $<\frac{1}{10}$ the cyclotron fringing field of 20 gauss. The only conceivable effect of the magnetizing current is the precession of muon spins and we are, therefore, led to conclusions I–IV as necessary consequences of these observations.

The solid curve in Fig. 2 is a theoretical fit to a distribution $1-\frac{1}{3}\cos\theta$, where

(1) the gyromagnetic ratio is taken to be +2.00;[10]

(2) the angular breadth of the electron telescope and the gate-width smearing are folded in, as well as (to first order) the exponential decay rate of muons within the gate;

(3) the small residual cyclotron stray field (*up* for Fig. 2, the positive magnetizing current producing a *down* field) is included. This has the accidental effect of converting the 100° initial angle ($H=0$) to 89° as in Fig. 2. We note that this experiment establishes only a lower limit to the magnitude of a, since the percent polarization at the time of decay is not known. If polarization is complete, $a=-0.33\pm0.03$.

Proof of the 2π symmetry of the distribution and the sign of the moment was obtained by shifting the electron counters to 65° with respect to the incident muon direction. The repetition of a magnetizing run yielded a curve as in Fig. 2 but shifted to the right by 0.075 ampere (5.9 gauss) corresponding to a precession angle of 37°, in agreement with the spatial rotation of the counter system. Thus we are led to conclusions V and VI.

A specific model, the two-component neutrino theory, has been proposed by Lee and Yang[3] in an attempt to introduce parity nonconservation naturally into elementary particle theory. This theory predicts, for our experimental arrangement and on the basis of 1.86 for the integrated spectrum (Fig. 2), a ratio of the order of 2.5 for energies greater than 35 Mev. We have increased the amount of absorber in the electron telescope to exclude electrons of less than ~35 Mev. The resulting peak-to-valley ratio was then observed to be 1.92 ± 0.19.[11]

We have also detected asymmetry in negative muon decay and have verified that the moment is negative and roughly equal to that of the positive muon.[7] The asymmetry in this case is also peaked backwards.

Various other materials were investigated for μ^+ mesons. Nuclear emulsion as a target was found to have a significantly weaker asymmetry (peak-to-valley ratio of 1.40 ± 0.07) and it is interesting to note that this did not increase with reduced delay and gate width. Neither was there any evidence for an altered moment. It seems possible that polarized positive and negative muons will become a powerful tool for exploring magnetic fields in nuclei (even in Pb, 2% of the μ^- decay into electrons[9]), atoms, and interatomic regions.

The authors wish to acknowledge the essential role of Professor Tsung-Dao Lee in clarifying for us the papers of Lee and Yang. We are also indebted to Professor C. S. Wu[6] for reports of her preliminary results in the Co^{60} experiment which played a crucial part in the Columbia discussions immediately preceding this experiment.

* Research supported in part by the joint program of the Office of Naval Research and the U. S. Atomic Energy Commission.

† Also at International Business Machines, Watson Scientific Laboratories, New York, New York.

[1] T. D. Lee and C. N. Yang, Phys. Rev. 104, 254 (1956).
[2] Lee, Oehme, and Yang, Phys. Rev. (to be published).
[3] T. D. Lee and C. N. Yang, Phys. Rev. (to be published).
[4] R. Dalitz, Phil. Mag. 44, 1068 (1953).
[5] T. D. Lee and C. N. Yang (private communication).
[6] Wu, Ambler, Hudson, Hoppes, and Hayward, Phys. Rev. 105, 1413 (1957), preceding Letter.

[7] The Fierz-Pauli theory for spin $\frac{3}{2}$ particles predicts a g value of $\frac{2}{3}$. See F. J. Belinfante, Phys. Rev. 92, 997 (1953).

[8] V. Fitch and J. Rainwater, Phys. Rev. 92, 789 (1953).
[9] M. Weinrich and L. M. Lederman, *Proceedings of the CERN Symposium, Geneva, 1956* (European Organization of Nuclear Research, Geneva, 1956).

[10] The field interval, ΔH, between peak and valley in Fig. 2 gives the magnetic moment directly by $(\mu\Delta H/s\hbar)(t_1+\frac{1}{2}T)\delta=\pi$, where $\delta=1.06$ is a first-order resolution correction which takes into account the finite gate width and muon lifetime. The 5% uncertainty comes principally from lack of knowledge of the magnetic field in carbon. Independent evidence that $g=2$ (to ~10%) comes from the coincidence of the polarization axis with the velocity vector of the stopped μ's. This implies that the spin precession frequency is identical to the μ cyclotron frequency during the 90° net magnetic deflection of the muon beam in transit from the cyclotron to the 1–2 telescope. We have designed a magnetic resonance experiment to determine the magnetic moment to ~0.03%.

[11] *Note added in proof.*—We have now observed an energy dependence of a in the $1+a\cos\theta$ distribution which is somewhat less steep but in rough qualitative agreement with that predicted by the two-component neutrino theory ($\mu\rightarrow e+\nu+\bar{\nu}$) without derivative coupling. The peak-to-valley ratios for electrons traversing 9.3 g/cm², 15.6 g/cm², and 19.8 g/cm² of graphite are observed to be 1.80±0.07, 1.84±0.11, and 2.20±0.10, respectively.

The Muon

A muon has all the properties of an electron, except that it is about 210 times heavier and is unstable against decay into an electron and neutrino—antineutrino pair. Its mass, spin, mean life and magnetic moment are compared below to those of the electron. Errors indicated are in the last digits of each value.

	Muon	*Electron*
Mass (in MeV/c²)	105.6595 ± 3	0.5110041 ± 16
Spin	½	½
Mean Life (seconds)	2.1994×10^{-6}	stable
Magnetic Moment (in Bohr magnetons $\frac{e\hbar}{2m_i c}$ *)	1.00116616 ± 31	1.0011596577 ± 35

** m_i represents the mass of the muon for the muon's magnetic moment; and the mass of the electron for its magnetic moment.*

DISCOVERY STORY III

Valentine Telegdi (left)
and Wolfgang Pauli
at Padua - Venice Conference in 1957.

One Researcher's Personal Account

by
Valentine Telegdi

WHEN I first arrived at the University of Chicago in 1951, my main interest was in photonuclear reactions, and the sole experimental technique with which I was really familiar was that of nuclear emulsions. Although the excitement at the university centered on pion work with the then new 450 MeV cyclotron, I preferred to work with the local betatron, and began by simply elaborating my thesis topic. Later, I branched out into electronic experiments. To learn these latter techniques, I also did a little work on positronium.

By the summer of 1956, I was pretty tired of working with the betatron – Chuck Oxley and I had just spent much effort on the proton Compton effect (at 60 MeV) merely to confirm the Thomson formula! Fortunately, at about the same time, life around the cyclotron had grown less competitive and I considered switching to it. I decided to work on muons. Except for one brave radiochemist, nobody at the Chicago cyclotron had used them for anything. They were merely a known contamination of the pion beams. As a specific topic, I decided to concentrate on muonic X-rays, hoping to resolve the question of the muon spin. With the help of several graduate students, I began building electronics.

Around August 1956, I ran across a preprint on Leona Marshall's desk: Lee and Yang's now classic article on parity violation and the θ - τ puzzle. It instantly struck me as an extraordinarily exciting paper. It was obvious that the π - μ chain could readily be studied in emulsions, and I also felt that the parity-violating effects could not be small if they *were* the explanation of the θ - τ puzzle.

I gave a seminar on the Lee-Yang paper, vainly trying to communicate my excitement to others. I met with little enthusiasm from senior colleagues who tried to tell me that we would be wasting our time. I replied that I would be willing to risk wasting three months on such an exciting possibility.

Two facts had a great impact on my next steps. The first was that I was (unfortunately, perhaps) familiar with the behavior of positrons in solids and I knew that in many amorphous materials they formed positronium. The second was that Professor Allison (then director of our Institute) was working on the capture and loss of electrons by particles slowing down in matter. During the Ph.D. exam of one of Allison's students, Sol Krasner, I asked the candidate what would happen if muons (rather than protons) were slowed down. I believe that I invented the word "muonium" during that exam. Thinking more along this line, I expected that positive muons would probably form muonium in emulsion. If the muon lost and captured an electron repeatedly, it would be depolarized; in any case, triplet muonium should precess in a given external field (e.g., the stray field of a cyclotron) about 100 times faster than a free muon; and I thought it would be necessary to shield the emulsions against even very weak fields. This idea, good or bad, delayed us by about two months.

Jerry Friedman had just gotten his degree on emulsion work related to polarization in pion-nucleon scattering and was working as a postdoc in the emulsion group. I invited him to work with me on this project, since he had invaluable experience in "confusing scanners," i.e., excluding biases due to them. Jointly we started building a magnetically shielded region for the emulsions, and a

Nuclear Emulsion Technique

Emulsions are often used to gather information in nuclear scattering experiments. In this process, a film coated with silver halide crystals (the emulsion) is used to 'photograph' the event. As a charged particle passes through the emulsion, it ionizes the atoms of the silver halide crystals, producing a 'line' which shows the trajectory of the charged particle. The number of ionizations per unit length is directly proportional to the square of the charge and inversely proportional to the square of the velocity. The number will also be proportional to -dE/dx (energy loss per distance for the incident particle). Information may also be gathered from a measurement of the track length, provided the range-energy curve of the particle is well-known.

In some instances, enough energy will be transferred to electrons of the silver halide crystals to enable them to travel several microns across the film. The tracks thus produced are called δ (delta) rays. Their number also measures the ionization. The production of δ-rays depends strongly on conditions which include emulsion grain density, emulsion materials, particle energy and the charge Z of the incident particle.

sensitive flip-coil to check it out. We also decided on our goal: to examine 2000 π-μ-e decays and then quit whether or not an effect was found.

Other interesting things were happening in parallel. I could not understand how a scalar or pseudo-scalar coupling of π-μ-ν could 'polarize' the muon; it seems like a very stupid question now, I'm sure. I turned to R. Oehme, then a postdoc, for advice. He did not say much at first, but the next day told me that it was a matter of phases between coupling constants. Perhaps it had something to do with some work that Pauli (my own teacher in Zürich) had recently published in the Niels Bohr Festschrift*. To make a long story short, Oehme hit upon the CTP theorem and worked out many of its implications in weak interactions. He soon went to Princeton and published a now famous paper jointly with Lee and Yang**. Around this time, Landau's preprint on CP arrived; in it he mentioned that the "formation of mesonium" would probably mask the P-violating effect. I felt quite proud, but even more discouraged about the chances of our experiment.

Jerry and I made the exposure and started scanning in October. (We also provided some of our 'shielded' emulsions to two European physicists who had asked M. Schein for such exposures.) There was a modest group of scanners at our disposal, headed by Elaine Garwin, Dick Garwin's sister-in-law.

On September 3 1956, a sad event occurred: my father died in Milan. I could not go to Europe before the Christmas vacation. Before my departure, we had accumulated a sizeable decay asymmetry from 1300 events, but had not yet reached our full quota of 2000. I wrote Oehme in Princeton about our status. We knew from him that Professor Wu was very active at the National Bureau of Standards, but knew nothing of any muon experiment at Columbia (since it had not yet been designed!).

I swore Jerry to secrecy and left for Milan to assist my mother. On my way back, I stopped at CERN and gave a talk about work in Chicago, but I carefully omitted any reference to our parity experiment. On January 11 (I especially remember the date because it's my birthday), my wife and I returned via New York. From the airport I called Dick Garwin, who had become a close friend during our common years in Chicago; this was purely a personal call, since I had no idea that Dick (on the IBM staff) was involved in any cyclotron work. My call was transferred from one place to another. In the end I was told that Dick was "not out of town, but unavailable." Upon my return to Chicago, I found a note from Oehme hinting at

* *Niels Bohr and the Development of Physics;* essays on the occasion of his seventieth birthday, edited by Pauli, W., Rosenfeld, L., Weisskopf, V; Chapter 3, (McGraw-Hill, 1955.)

** Lee, T. D., Oehme, R., and Yang, C. N., *Phys. Rev.*, 106, 340 (1957). For further information on the CPT theorem, see Explanatory Physics Note "CPT Invariance" p. 121.

Muonium

An unstable atom of muonium (called "mesonium" by Landau) is formed when an electron is captured in a Bohr orbit around a positive muon (μ^+).
See: V. Hughes, *Ann. Rev. Nucl. Sci.*, 16, 445 (1966).

Mesic X-Rays

A slow-moving meson may be captured by the Coulomb field of a nucleus. The captured meson will cascade to the lowest available energy level, giving off photons with energies in the X-ray region of the electromagnetic spectrum. The process is similar to the emission spectrum in normal atoms. The ground state energy of the atom is lower with the captured meson than it was previously with an electron due to an increase in the reduced mass, which results from the large meson mass. The radius of the energy levels will also be reduced in inverse proportion to this factor. That is, for the mesic atom system, where the reduced mass $\bar{\mu}$ is given by:

$$\bar{\mu} = \frac{m_a m}{m_a + m} \quad (1)$$

(m_a is the mass of the nucleus; m is the meson mass.) The energy E and radius r of the n^{th} energy level are give as:

$$E \propto -\frac{\bar{\mu}}{n^2} \quad (2)$$

$$r_n \propto \frac{n^2}{\bar{\mu}} \quad (3)$$

the ^{60}Co result, and there were rumors (propagated by Pless at MIT to Hildebrand here) about an electronic π-μ experiment at Columbia. I again called Garwin, and this time I reached him. I told him that we *did* have an effect, and asked him what his group had found. He, somewhat jocularly, invited me to tell our asymmetry first. Once I had done this, he replied that what we had seen was in good agreement with *their* observations! I told him we would send a Letter to *The Physical Review* right away, to appear simultaneously with theirs. Dick added that there was a closing date (about January 15) at the journal and that we might be late.

The Letter was sent off, and we soon received preprints of the two crucial Columbia experiments. I instantly mailed photostats of the experimental data of all three experiments to my revered teacher, Pauli, in Zürich (he received them just an hour before giving a lecture "Old and New Neutrino Problems!").

A short time after it was submitted, our Letter was rejected. According to the editors, it was "unclear, confusing," etc. As Professor Wentzel, one of my highly distinguished colleagues, was then associate editor of *The Physical Review*, I showed him the letter of rejection and asked whether he concurred with that evaluation. He did not, and readers of these lines can judge for themselves (see p. 136 for a reproduction of this Letter). Professor Wentzel, along with Joe Mayer, tried to contact the editor who was always "unavailable." In despair, I called up Eugene Wigner. He did come to the telephone, and said (in Hungarian): "What do you want me to do? I am only the president of the A.P.S."

Meanwhile, a cyclotron run had been scheduled for our mesic X-ray run. Aware of the Columbia experiment, we instead repeated and extended their work, using a time-converter. By the time of the New York A.P.S. Meeting on January 25, we had some very pretty precession curves.

Jerry and I went to that meeting. Wu, Lederman, I and Yang (in that order) spoke at the post-deadline session—to an audience of at least a thousand people. The excitement was unbelievable and unforgettable. I must add that Jerry and I were still disturbed by the way *The Physical Review* had dealt with us. Since we felt that doing something relevant gets you into trouble, we seriously discussed quitting physics.

During the meeting, several older physicists came over to quiz me about our experiment. One of them, Martin Deutsch, asked me to have dinner with him. From his remarks during that meal, it became clear to me that there had been a rumor not only that we had obtained our answer after the group at Columbia (a difficult proposition, since scanning takes time), but that we had somehow 'concocted' the whole result. Martin, of course, did not believe this and merely wanted details; he certainly must have done his share to convince others of our integrity.

I was, at that time, very impressed by the fact that many renowned physicists (like Deutsch) stood up for Jerry and myself; so we remained in physics. However, I considered the impartial publication of learned journals the main job of the A.P.S., and since I felt that I had suffered some injustice, I left the Society in 1957. I am not sure that I did the right thing by not rejoining, nor do I recommend this approach to others.

I also ran into the editor of *The Physical Review* at that Meeting. He insisted that our Letter had not been published in the same issue as the two Columbia papers "for purely technical reasons," and that it would be published in the next issue. I said that he should add a footnote indicating the presumed reasons for the delay—and that's the way it was printed. I believe that it is the only *Physical Review* Letter ever published with such a footnote.

I might add, as an aside, that not long afterward I had a different problem with *The Physical Review.* I had learned only in the summer of 1955 (from Murray Gell-Mann on his honeymoon trip to Copenhagen) the difference between V and A. By July 1957, I was ready to sign a paper as "V.-A. Telegdi." That Letter was not rejected—but the editor altered my name!

Following the New York Meeting, I started working with an Argonne group on the β-decay of polarized neutrons. That experiment was most exciting, and did a lot to clear up β-decay. Later that year, Lee and Yang got the Prize, and in their Nobel lectures gave appropriate credit to all three groups.

Despite the unpleasant publication problems and rumors surrounding our experiment, my boundless admiration for Dick Garwin and my deep friendship with him remained undiminished; nor were they affected by our subsequent competition in muon experiments. In fact, we worked jointly on the first conclusive muon (g-2) experiment at CERN in 1960.

The change in our thinking which accompanied the discovery of parity violation has enabled us to do many quick and simple experiments. Perhaps particle physics will bring other excitements of comparable intensity again, but I believe that the technical simplicity of those days is lost forever. Few young men today can gamble 'only three months' for such a big stake. □

Chicago, February 1973.

Nuclear Emulsion Evidence for Parity Nonconservation in the Decay Chain $\pi^+ - \mu^+ - e^+$*†

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Enrico Fermi Institute for Nuclear Studies, University of Chicago, Chicago, Illinois
(Received January 17, 1957)

Lee and Yang[1] recently re-examined the problem as to whether parity is conserved in nature and emphasized the fact that one actually lacks experimental evidence in support of this most natural hypothesis in the case of weak interactions (such as β decay). Violation of parity conservation can be inferred essentially only by measuring the probability distribution of some *pseudoscalar* quantity, e.g., of the projection of a polar vector along an axial vector, and measurements of this kind had not been reported. Lee and Yang suggested several experiments in which a spin direction is available as a suitable axial vector; in particular, they pointed out that the initial direction of motion of the muon in the process $\pi\rightarrow\mu+\nu$ can serve for this purpose, as the muon will be produced with its spin axis along its initial line of motion if the Hamiltonian responsible for this process does not have the customary invariance properties. If parity is further not conserved in the process $\mu\rightarrow e+2\nu$, then a forward-backward asymmetry in the distribution of angles $W(\theta)$ between this initial direction of motion and the momentum, $\mathbf{p}_e$, of the decay electron is predicted.

It is easy to observe the pertinent correlation by bringing π^+ mesons to rest in a nuclear emulsion in which the μ^+ meson also stops. One has only to bear in mind two facts: (1) even weak magnetic fields, such as the fringing field of a cyclotron, can obliterate a real effect, as the precession frequency of a Dirac μ meson is $(2.8/207)\times10^6$ sec^{-1}/gauss; (2) μ^+ can form "muonium," i.e., (μ^+e^-), and the formation of this atom can be an additional source of depolarization, both through its internal hyperfine splitting and the precession of its total magnetic moment around the external field. In the absence of specific experiments on muonium formation, one can perhaps be guided by analogous data on positronium in solids.[2,3]

With these facts in mind, we exposed (in early October, 1956) nuclear emulsion pellicles (1 mm thick) to a π^+ beam of the University of Chicago synchrocyclotron. The pellicles were contained inside three concentric tubular magnetic shields and subject to $\leq4\times10^{-3}$ gauss. Over 1300 complete $\pi-\mu-e$ decays have been recorded to date, and the space angle θ defined above has been calculated for each. From these preliminary data we find[4]

$$\left\{\int_{90^\circ}^{180^\circ}|W(\theta)|d\Omega-\int_0^{90^\circ}|W(\theta)|d\Omega\right\}\bigg/\int_0^{180^\circ}W(\theta)d\Omega=0.062\pm0.027,$$

or

$$W(\theta)=1-0.12\cos\theta,$$

i.e., the presence of an excess in the backward hemisphere to a 95% confidence level. This effect agrees in sign and magnitude with one observed in a recent analogous experiment[5] performed electronically at Columbia University.

In connecting our result with basic theoretical principles, one has to remember (a) that the asymmetry observed here is only a lower limit owing to the possibility of muonium formation[6] and other conceivable depolarization effects; (b) that existence of an asymmetry implies the joint violation of parity conservation and charge conjugation invariance rather than of parity conservation alone.[7]

In view of the intrinsic importance of the subject, we consider it worthwhile to present our data at this preliminary stage.

We would like to thank the Columbia workers, in particular R. L. Garwin, for communicating their unpublished results to us. We are grateful to R. Oehme for illuminating theoretical discussions and to R. Levi-Setti for criticism of the experimental techniques.

* This work was supported by a joint program of the Office of Naval Research and the U. S. Atomic Energy Commission.

† For technical reasons, this Letter could not be published in the same issue as that of Garwin, Lederman, and Weinrich, Phys. Rev. 105, 1415 (1957).

[1] T. D. Lee and C. N. Yang, Phys. Rev. 104, 254 (1956).

[2] S. Berko and F. Hereford, Revs. Modern Phys. 28, 299 (1956).

[3] Telegdi, Sens, Yovanovitch, and Warshaw, Phys. Rev. 104, 867 (1956).

[4] *Note added in proof.*—From 2000 events, we get for this ratio 0.091 ± 0.022.

[5] Garwin, Lederman, and Weinrich (private communication from R. L. Garwin, January 13, 1957).

[6] The Columbia workers find that the observed asymmetry appears to depend on the material stopping the μ^+ mesons. This is consistent with (μ^+e^-) formation and suggests the use of μ^- mesons for absolute measurements, in low-Z materials with no nuclear magnetic moment.

[7] R. Oehme (private communication); Lee, Oehme, and Yang, Phys. Rev. (to be published).

Reprinted from *The Physical Review*, Vol. 105, No. 5, 1681-1682, March 1, 1957.

A REPLY FROM THE EDITOR OF THE PHYSICAL REVIEW

by
S. Goudsmit

Dr. Telegdi severly criticizes the handling of his Letter about Parity Nonconservation by me as editor of *The Physical Review.* He resigned from the American Physical Society because his paper was published two weeks later than the two Letters on the same subject from the Columbia University and Bureau of Standards groups. The history of this adventure in experimental physics is therefore not complete without hearing the editorial side of the story.

I could tell about deadlines and scheduling, about five to six weeks delays in the publication of the journal and about similar technicalities. That would miss the point. In principle, we could probably have pulled out the two earlier Letters and postponed their appearance so as to coincide with that by Dr. Telegdi. We did not do so.

The obstacle encountered by Dr. Telegdi was of a different nature. A careful study of his Letter shows clearly that at the time he submitted it the work was unfinished. He had obtained an asymmetry of 0.062 ± 0.027 with 1300 events, that is $(690 - 610)/1300$ with a standard deviation of 35 in the difference. The effect is only a little larger than two standard deviations. This should be compared with the overwhelming and compelling evidence presented in the two other Letters as seen especially clearly in their graphs. An effect of less than three standard deviations is quite insufficient in such an important and subtle experiment. A very similar more recent example is the Columbia - Stony Brook work of 1966 on the decay of the η-meson, where an asymmetry of 0.072 ± 0.028 in 1441 events was reported. This was widely heralded as proof of violation of charge-conjugation invariance. Later experiments showed that there is no asymmetry in the η-decay.

Several weeks after submission, Dr. Telegdi added a note in proof to his Letter, stating that a total of 2000 events had now given an asymmetry of 0.091 ± 0.022, that is $(1091-909)/2000$. This is quite an improvement. Note, however, that the 700 additional cases must have shown a surprisingly large asymmetry, namely $(401 - 299)/700$ or 0.146 ± 0.039, as compared to the original 0.062 ± 0.027. These large statistical errors (27%-43%) definitely prove the preliminary nature of the initial result submitted for publication.

The experimental method chosen by Dr. Telegdi, the use of emulsions, is a slow process. According to his narrative the work was slowed down further by very unfortunate circumstances beyond his control. Thus the timing of events seems regrettable to Dr. Telegdi, since he began his experiments before the other groups started theirs. However, I do not believe that what happened detracted at all from the value of his work.

EXPLANATORY PHYSICS NOTES FOR NON-SPECIALISTS

PHENOMENOLOGICAL DISCUSSION OF PARITY VIOLATION*

By
T. D. Lee

**Excerpt from Nobel Lecture 1957*

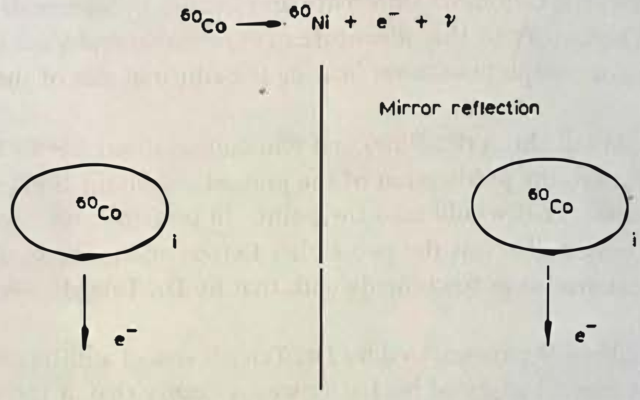


Fig. 1

(1) *β-decay*

The first experiment that conclusively established the nonconservation of parity was that on β-angular distribution from polarized ^{60}Co nuclei[1] (see Fig. 1). The ^{60}Co nuclei are polarized by a magnetic field at very low temperatures. Indeed in this experiment, the circular direction of the electric current in the solenoid that produces the polarizing magnetic field together with the preferential direction of the β-ray emitted, differentiates in a most direct way a right-handed system from a left-handed system. Thus the nonconservation of parity or the non-invariance under a mirror reflection can be established without reference to any theory.

Furthermore from the large amount of angular asymmetry observed it can also be established[2] that the β-decay interaction is not invariant under a charge conjugation operation. That this can be concluded without performing the extremely difficult (in fact, almost impossible) experiment using anti-^{60}Co is based on certain theoretical deductions under the general framework of local field theory. In the following we shall try to sketch this type of reasoning[3].

Let us consider the β-decay process, say

$$n \rightarrow p + e^- + \nu \tag{1}$$

in which each particle is described by a quantized wave equation. In particular the neutrino is described by the Dirac equation[4]

$$\sum_{\mu=1}^{4} \gamma_\mu \frac{\partial}{\partial x_\mu} \psi_\nu = 0 \tag{2}$$

1. Wu, C. S., Ambler, E., Hayward, R. W., Hoppes, D. D., and Hudson, R. P., *Phys. Rev.*, 105, 1413 (1957), (Reproduced p. 119.)
2. Lee, T. D., Oehme, R., and Yang, C. N., *Phys. Rev.*, 106, 340 (1957); Ioffe, B. L., Okun, L. B., and Rudik, A. P., *J.E.T.P. (U. S. S. R.)*, 32, 396 (1957).
3. If the neutrino is described by a two-component theory, then the result of the large angular asymmetry in ^{60}Co decay establishes in a trivial way the noninvariance property of β-decay under the charge conjugation operation. However, this noninvariance property can also be proved under a much wider framework. In this section we take as an example the case of a four-component theory of neutrino to illustrate such a proof. See Explanatory Physics Notes: "Phenomenological Aspects of the Two-Component Theory of the Neutrino," p. 144.
4. For notations and definitions of γ matrices, see, e.g., W. Pauli, *Handbuch der Physik*, Julius Springer Verlag, Berlin, 1933, Vol. 24.

where $\gamma_1, \gamma_2, \gamma_3, \gamma_4$, are the four (4×4) anti-commuting Dirac matrices and $x_1, x_2, x_3, x_4 = ict$ are the four space-time coordinates. For each given momentum there exists two spin states for the neutrino and two spin states for the anti-neutrino. These may be denoted by $\nu_R, \nu_L, \bar{\nu}_R, \bar{\nu}_L$. If we define the helicity H to be

$$H \equiv \vec{\sigma} \cdot \hat{p} \tag{3}$$

with $\vec{\sigma}$ as the spin operator and $\hat{p}$ the unit vector along the momentum direction, then these four states have, respectively, helicities equal to $+1$, -1, -1 and $+1$ (Fig. 2). Mathematically, this decomposition of states corresponds to a separation of ψ_ν into a right-handed part ψ_R and a left-handed part ψ_L with

$$\psi_\nu = \psi_R + \psi_L \tag{4}$$

where

$$\psi_R = \tfrac{1}{2}(1 - \gamma_5)\, \psi_\nu \tag{5}$$

$$\psi_L = \tfrac{1}{2}(1 + \gamma_5)\, \psi_\nu \tag{6}$$

and

$$\gamma_5 = \gamma_1 \gamma_2 \gamma_3 \gamma_4$$

It is easy to see that both ψ_R and ψ_L separately satisfy the Dirac equation [Eq. (2)]. With this decomposition the β process of a nucleus A can be represented schematically as

$$\mathrm{A} \rightarrow \mathrm{B} + e^- + \begin{cases} C_i^R \nu_R & (H = +1) \quad (7) \\ C_i^L \nu_L & (H = -1) \quad (8) \end{cases}$$

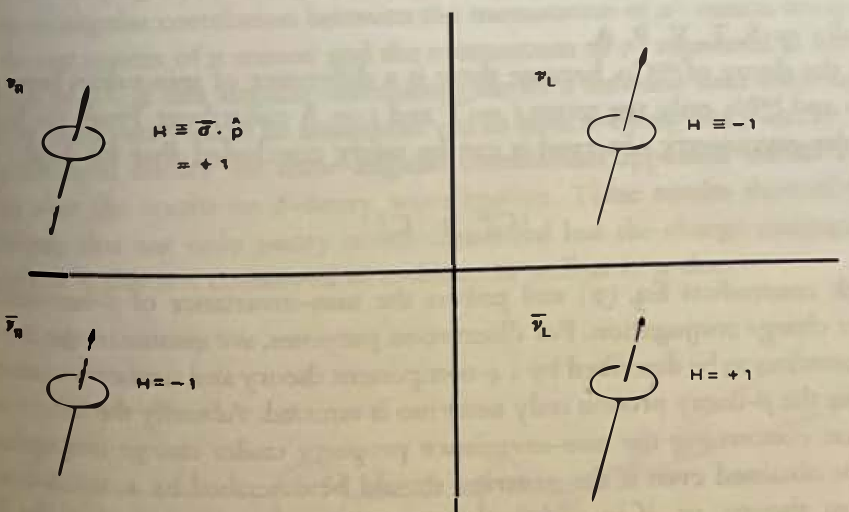


Fig. 2

with C_i^R and C_i^L as the various probability amplitudes for emission of ν_R and ν_L respectively. The suffix i represents the various possible channels for such emissions. If the theory is invariant under proper Lorentz transformation, then there are five such channels: namely scalar S, tensor T, vector V, pseudo-scalar P and axial-vector term A. According to the general rules of quantum field theory with any interaction term representing the decay of a particle, there exists a corresponding hermitian conjugate term which represents decay of the antiparticle. Thus, the decay of the anti-nucleus $\bar{A}$ can be schematically represented by

$$\bar{A} \rightarrow \bar{B} + e^+ + \begin{cases} C_i^{R*}\,\bar{\nu}_R & (H = -1) & (7') \\ C_i^{L*}\,\bar{\nu}_L & (H = +1) & (8') \end{cases}$$

with C_i^{R*} and C_i^{L*} as the corresponding amplitudes for emission of $\bar{\nu}_R$ and $\bar{\nu}_L$. Under the charge conjugation operator we change a particle to its antiparticle but we do not change its spatial or spin wave functions. Consequently it must have the same helicity. Thus, if the β-decay process is invariant under the charge conjugation operator, then we should expect process (7) to proceed with the same amplitude as process (8′). The condition for invariance under charge conjugation is, then

$$C_i^R = C_i^{L*} \qquad (9)$$

for all i = S, T, V, P, A.

In the decay of ^{60}Co, because there is a difference of spin values between ^{60}Co and ^{60}Ni, only the terms i = T and i = A contribute. From the large angular-asymmetry observed it can be safely concluded that for both i = T, A

$$|C_i^R| \neq |C_i^L|$$

which contradicts Eq. (9) and proves the non-invariance of β-interaction under charge conjugation. For illustration purposes, we assume in the above the neutrino to be described by a 4-component theory and further we assume that in the β-decay process only neutrino is emitted. Actually the same conclusion concerning the non-invariance property under charge conjugation can be obtained even if the neutrino should be described by a, say, 8-component theory, or, if in addition to neutrino, anti-neutrino may also be emitted.

Recently many more experiments[5] have been performed on the longitudinal polarization of electrons and positrons, the β-γ correlation together with the circular polarization of the γ radiation and the β angular distribu-

5. For a summary of these experiments see, e.g., *Proceedings of the Seventh Annual Rochester Conference*, (Interscience, New York, 1957.) Updated in References 10 and 11.

tion with various polarized nuclei other than ^{60}Co. The results of all these experiments confirm the main conclusions of the first ^{60}Co experiment, that both the parity operator and the charge conjugation operator are not conserved in β-decay processes.

Another interesting question is whether the β-decay interaction is invariant under the product operation of (charge conjugation $\times$ mirror reflection). Under such an operation we should compare the decay of A with that of $\bar{A}$ but with opposite helicities. Thus if β-decay is invariant under the joint operation of (charge conjugation $\times$ minor reflection) we should expect process (7) to proceed with the same amplitude as process (7′) and similarly for processes (8) and (8′). The corresponding conditions are then

$$C_i^R = C_i^{R*}$$

$$\text{and} \quad C_i^L = C_i^{L*} \qquad (10)$$

(2) π-μ-e decay

The $\pi^{\pm}$ meson decays into a $\mu^{\pm}$ meson and a neutrino. The $\mu^{\pm}$ meson, in turn, decays into an $e^{\pm}$ and two neutrinoes (or anti-neutrinoes). If parity is not conserved in π-decay, the μ meson emitted could be longitudinally polarized. If in the subsequent μ-decay parity is also not conserved, the electron (or positron) emitted from such a μ meson at rest would in general exhibit a forward and backward angular asymmetry with respect to the polarization of μ meson (Fig. 3). Consequently in the π-μ-e decay sequence we may observe an angular correlation between the momentum of $\mu^{\pm}$ meson measured in the rest system of π meson and the momentum of $e^{\pm}$ measured in the rest system of $\mu^{\pm}$. If this angular correlation shows a forward backward asymmetry, then parity must be nonconserved in both π-decay and μ-decay. The experimental results[6] on these angular correlations appeared within a few days after the results on β-decay were known. These results showed conclusively that not only parity is not conserved but the charge conjugation operator is also not conserved in π-decay as well as in μ-decay.

$$\pi^+ \longrightarrow \mu^+ + \bar{\nu}$$

$$\mu^+ \longrightarrow e^+ + \nu + \bar{\nu}$$

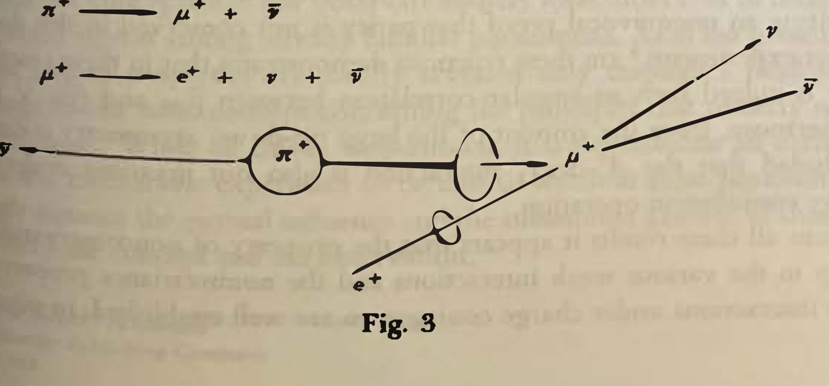


Fig. 3

6. Garwin, R. L., Lederman, L. M., and Weinrich, M., *Phys. Rev.*, 105, 1415 (1957); Friedman, J. I., and Telegdi, V. L., *Phys. Rev.*, 105, 1681 (1957); (Reproduced pp. 128 and 136 respectively.)

Later, direct measurements[7] on the longitudinal polarization of the positron from μ^+-decay was done establishing the same conclusion concerning μ-decay.

(3) K-μ-e *decay*
In this case we have instead of the π meson the heavier K meson which decays into a μ meson and a neutrino (Fig. 4). Experiment[8] on the angular correlation between the μ^+ momentum from the decay of K^+ meson and the positron momentum from the $\mu^{\pm}$-decay establishes that in K-decay the parity as well as the charge conjugation operator is not conserved.

(4) Λ^0-*decay*
The Λ^0 particle can be produced by colliding an energetic π^- on proton. The

$$K^+ \longrightarrow \mu^+ + \bar{\nu}$$

$$\mu^+ \longrightarrow e^+ + \nu + \bar{\nu}$$

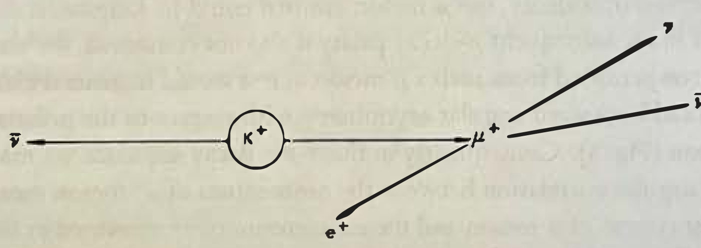


Fig. 4

Λ^0 subsequently decays into a proton plus a π^- (Fig. 5). The observation of an assymetrical distribution with respect to the sign of the product $\vec{p}_{out} (\vec{p}_{in} \times \vec{p}_{\Lambda})$ formed from the momentum of the incoming pion $\vec{p}_{in}$, the momentum of the lambda particle, $\vec{p}_{\Lambda}$, and that of the decay pion $\vec{p}_{out}$ would constitute an unequivocal proof that parity is not conserved in this decay. Recent experiments[9] on these reactions demonstrates that in these reactions there is indeed such an angular correlation between $\vec{p}_{out}$ and $(\vec{p}_{in} \times \vec{p}_{\Lambda})$. Furthermore, from the amount of the large up-down asymmetry it can be concluded that the Λ^0-decay interaction is also not invariant under the charge conjugation operation.

From all these results it appears that the property of nonconservation of parity in the various weak interactions and the noninvariance property of these interactions under charge conjugation are well established. In connec-

7. Culligan, G., Frank, S. G. F., Holt, J. R., Kluyver, J. C., and Massam, T., *Nature*, 180, 751 (1957).
8. Coombes, C. A., Cork, B., Galbraith, W., Lambertson, G. R., and Wenzel, W. A., *Phys. Rev.*, 108, 1348 (1957).
9. Crawford, J., *et al.*, *Phys. Rev.*, 108, 1102 (1957); Eisler, F., *et al.*, *Phys. Rev.*, 108, 1353 (1957); Leipuner, L. B., and Adair, R. K., *Phys. Rev.*, 109, 1358 (1958).

Recommended Literature

A. Lee, T. D., and Wu, C. S., Weak Interactions, *Annual Review of Nuclear Science*, 15, 381 (1965).
B. Schopper, H., "Weak Interactions and Nuclear Beta-Decay," (North-Holland, 1966.)
C. Wu, C. S., and Moszkowski, S. A., *Beta Decay*, (Interscience, 1966)

tion with these properties we find an entirely new and rich domain of natural phenomena which, in turn, gives us new tools to probe further into the structure of our physical world.

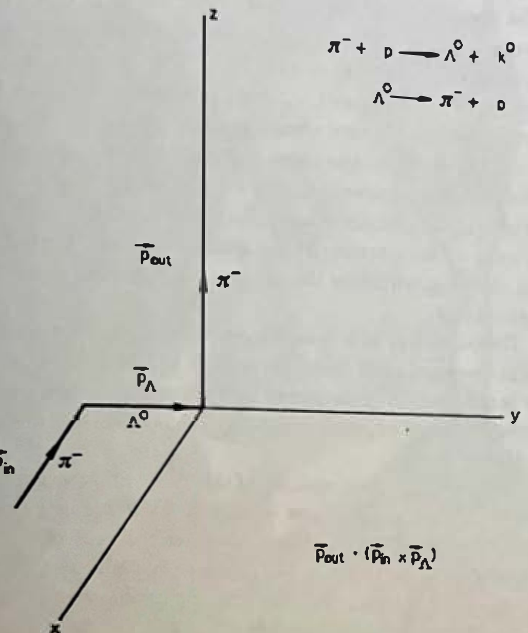


Fig. 5

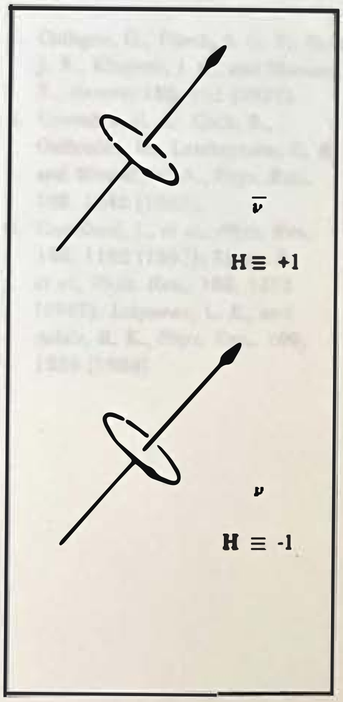
•●•

The progress of science has always been the result of a close interplay between our concepts of the universe and our observations on nature. The former can only evolve out of the latter and yet the latter is also conditioned greatly by the former. Thus in our exploration of nature, the interplay between our concepts and our observations may sometimes lead to totally unexpected aspects among already familiar phenomena. As in the present case, these hidden properties are usually revealed only through a fundamental change in our basic concept concerning the principles that underly natural phenomena. While all this is well-known, it is nevertheless an extremely rich and memorable experience to be able to watch at close proximity in a single instance the mutual influence and the subsequent growth of these two factors – the concept and the observation.

EXPLANATORY PHYSICS NOTES FOR NON-SPECIALISTS

Phenomenological Aspects of the Two-component Theory of the Neutrino*

Before the discovery of nonconservation of parity, it was customary to describe the neutrino by a four-component theory in which for each definite momentum there are the two spin states of the neutrino ν_R and ν_L, plus the two spin states of the antineutrino $\bar{\nu}_R$ and $\bar{\nu}_L$. In the two-component theory, however, two of these states, ν_R and $\bar{\nu}_R$, simply do not exist in Nature. The spin of the neutrino is then always antiparallel to its momentum, $H \equiv -1$, while the spin of the antineutrino is always parallel to its momentum, $H \equiv +1$. Thus in the two-component theory we have only half of the degrees of freedom as in the four-component theory. Graphically, we may represent the spin and the velocity of the neutrino by the spiral motion of a left-handed screw and that of the antineutrino by the motion of a right-handed screw, as shown in the figure at left.



The possibility of a two-component relativistic theory of a spin ½ particle was first discussed by H. Weyl[1] as early as 1929. However, in the past, because parity is not manifestly conserved in the Weyl formalism, it was always rejected[2]. With the discovery of parity violation, such an objection becomes completely invalid[3].

To appreciate the simplicity of this two-component theory in the present situation, it is best if we assume further the existence of a conservation law for leptons[4]. This law is in close analogy with the corresponding conservation law for baryons. We assign to each lepton a leptonic number L equal to +1 or -1 and to any other particle the leptonic number zero. The leptonic number for a lepton must be the negative of that for its antiparticle. The law of conservation of leptons then states that in all physical processes the algebraic sum of leptonic numbers must be conserved.

Some simple consequences follow immediately if we assume that this law is valid and that the neutrino is described by the two-component theory.

1. The mass of the neutrino and the antineutrino must be zero. It is important to note that the two-component theory, by itself, does not require the mass of the particle to be zero. The two-component theory, together with the lepton conservation law, *always* requires the physical mass of the particle to be zero, even with the inclusion of all interactions. If either of the two types of neutrinos, ν_μ or ν_e, has a small but nonzero mass, then its helicity could deviate from -1. Such small deviation cannot be ruled out by the existing accuracy in the present helicity measurements.
2. The theory is not invariant under the parity operator P which by definition inverts all spatial coordinates but does not change a particle into its antiparticle state. Under such an operation one inverts the momentum of a particle but not its spin direction. Since in this theory these two are always parallel for a neutrino, the parity operator P applied to a neutrino state leads to a non-existing state. Consequently, the theory is not invariant under the parity operation.
3. Similarly, one can show the theory is not invariant under the charge conjugation operation which changes a particle into its antiparticle but not its spin direction or its momentum.

*Updated from T. D. Lee's Nobel Lecture, 1957. For the complete review of the Two-Component Theory of the Neutrino, the reader is referred to Lee, T. D., and Wu, C. S., *Ann. Rev. Nucl. Sci.*, 15, 391 (1965).

1. H. Weyl, *Z. Physik*, 56 (1929) 330.
2. See, for example: W. Pauli, *Handbuch der Physik*, Julius Springer Verlag, Berlin, 1933, Vol. 24, pp. 226-227.
3. The possible use of a two-component theory for expressing the nonconservation property of parity in neutrino processes was independently proposed and discussed by T. D. Lee and C. N. Yang, *Phys. Rev.*, 105 (1957) 1671; A. Salam, *Nuovo Cimento*, 5 (1957) 299; and L. Landau, *Nucl. Phys.*, 3 (1957) 127.
4. The possible existence of a conservation law for leptons has been discussed before the discovery of nonconservation of parity. Cf. E. Konopinski and H. M. Mahmoud, *Phys. Rev.*, 92 (1953) 1045.

STORY OF THE EXPERIMENT ON DOUBLE SCATTERING OF ELECTRONS

Was Parity Violation Observed Three Decades Before Its Actual Discovery?

Richard T. Cox at the time of his beta ray scattering experiments (about 1928).

One Researcher's Personal Account

by Richard Cox

To physicists beginning their vocation in the mid-twenties, the demonstration by Davisson and Germer*, in 1926, that electrons, like X-rays, could be diffracted by crystals suddenly gave substance to what had seemed rather ghostly in the wave mechanics by which de Broglie and Schrödinger had described the atomic model of Bohr and Sommerfeld. At the same time, it gave incentive to other experiments: were these electron waves longitudinal or transverse, or did they exhibit the characteristics of both?

It was with scarcely more theoretical guidance than this that Charles G. McIlwraith, Bernhard Kurrelmeyer and I carried out an experiment during the year 1927 and the first months of 1928. We described the results in an article entitled "Apparent Evidence of Polarization in a Beam of β-Rays," published in the *Proceedings of the National Academy of Science*, July, 1928. (Reproduced p. 150.)

* *Phys. Rev.* 30, 705 (1926). This experiment verified de Broglie's hypothesis that electrons should exhibit wave, as well as particle, characteristics.

Our work was done on the University Heights campus of New York University, in Butler Hall, a former residence overlooking the Harlem River which had been converted years earlier for the use of the Department of Physics. After receiving my Ph.D. from Johns Hopkins in 1924, I joined the department at NYU as the youngest of its four faculty members. McIlwraith came for graduate study in 1925, with an A.B. from Reed College in Portland, Oregon. He was one of many students who followed their teacher, Professor A. A. Knowlton, into the profession of physics and he accepted my suggestion to make this research the subject of his doctoral thesis. Bernhard Kurrelmeyer had been my friend and fellow student at Hopkins, where his father and my stepfather were members of the faculty. We had received our doctorates the same year. From 1925 to 1927, Kurrelmeyer was at Harvard on a National Research Council Fellowship. In the spring of 1927, he received permission to complete the term of his fellowship at NYU—and he quickly accepted the invitation of McIlwraith and myself to work with us.

The department in which we were working was not committed to any one field of research, and its equipment was as meager in one field as in any other. But at least within the limits set by our means, we were free to let our curiosity direct our research. Our immediate incentive was the experiment of Davisson and Germer. Our model was the experiment of Barkla, in which he found that X-rays could be polarized by scattering. Our anticipation of a positive result was heightened by Uhlenbeck and Goudsmit's introduction of electron spin to quantum theory**.

** Uhlenbeck, G., and Goudsmit, S., Naturwissenschaften 13, 953 (1925).

The reason for our choosing a beam of β-particles rather than one of electrons from a hot filament for our experiment, I don't remember after a passage of forty-five years. I am sure that we didn't expect them to behave any differently from other electrons of the same energy. We had not heard of the conservation of parity, much less of its nonconservation or of the helicity of β-particles. I suppose that the possibility of counting β's by means of a Geiger counter was appealing. Otherwise we were only looking for the simplest way to determine whether a beam of electrons could be polarized.

We bought a milligram of radium and designed a small steel cylinder, with windows for the ingress and egress of β-particles, gold targets to scatter them twice at right angles, and an airtight joint for turning the upper part (to which the radium and the upper target were fastened) about the line joining the midpoints of the two target faces. The apparatus was carefully constructed by Mr. Hermann Beck, the kindly, old Alsatian machinist for the Department of

Physics, who had once been an instrument maker for Kohlrausch. He could quote Goethe for our edification and Alexander von Humboldt† for our instruction.

To evacuate the cylinder, we used a pump which had been presented to the university some years before when a nearby electric lamp factory had gone out of business. It was an efficient, but very noisy pump because the transmission belt from its motor was a sprocket-chain. Our timing was done with a ship's chronometer which had once belonged to a member of the faculty and had been donated to the university by his heirs; it served us very well. Once, when it needed attention, McIlwraith took it to the firm where it had been purchased. This company was still in business in lower Manhattan. An elderly gentleman there consulted a worn ledger and said: "Yes, we sold this to Professor Draper on..." It was some date in the eighteen-eighties!

The minutes which were ticked off by the chronometer and the amplified discharges of the counter were recorded as pips on an inked line traced on a fast-moving paper tape. This contrivance, though strictly amateurish, was entirely serviceable.

The Geiger counters we used were not the improved tubular type developed by Geiger and Müller in 1928, but an earlier form in which the discharge occurred between a 'point' (in our experiments a minute knob fused on the end of a fine, platinum wire) and a metal foil. With different counters there must have been a great deal of variation in the size of these points and some random variation in their placement. Consequently, there must have been corresponding variations in the size and location of the sensitive area. A point had to be replaced after an hour or two of use, and different points gave different counts.

In counting (as well as I can remember) some 10^5 discharges, with a score or more points, we found one asymmetry recurring with enough regularity that we believed it real and significant, although we had been surprised when we first noticed it. It could not be attributed to any asymmetry in the design of the apparatus, and it did not appear to be a likely consequence of any accidental asymmetry. Unfortunately, its magnitude could vary greatly when we changed counter points. With some it was large, with others it was small enough to be within the probable error; and with still others it reversed, though never by so much that the reversal could not be ascribed to a random fluctuation. We decided at length that the probable cause was an actual polarization of the β-particles. We published this as the most likely inference and acknowledged our doubts with the word "Apparent" in the title. To account for the

† **von Humboldt, Alexander (Baron Friedrich Heinrich)** *(1769 - 1859)* *German naturalist, traveler and statesman—who considered his service to the state to be merely an apprenticeship to the service of science. Travels led him across Europe and throughout the Americas. The Baron laid the foundations for the study of physical geography and meteorology and was the first to delineate isothermal lines and investigate the variation of temperature with altitude. Crowning achievement, the writing and publication of a treatise on the physical world, the Kosmos, at the age of 77.*

observed discrepancies, we proposed tentatively that some of the counter points were sensitive only to the slower β-particles, and that the asymmetry appeared only as the faster β-particles were counted.

One conclusion seemed beyond doubt, namely that McIlwraith had vividly demonstrated his ingenuity, skill and devotion to experimental physics. He was awarded his Ph.D. from New York University in June, 1928. His interest and insight in instrumentation and experimentation lead him first to the National Bureau of Standards, then to the U. S. Coast and Geodetic Survey and, later, into a variety of agencies and achievements in research above and below sea level. Kurrelmeyer, on the expiration of his fellowship, joined first the faculty at Columbia and later at Brooklyn College, where he continues as professor emeritus, well-remembered by former students now at many institutions, who learned from his teaching and his example.

In the fall of 1928, Carl T. Chase came to New York University as a graduate student in physics. He had received his B.S. from Princeton University in 1924, his M.S. from the California Institute of Technology in 1926, spent a year in astronomy at Harvard and taught a year at Dartmouth. I interested him in working on our problem, although I could not spend as much of my own time on it as previously. He took up the work with a great deal of interest and, working mostly alone, published his findings in three papers.

Chase first made some highly desirable and well-contrived improvements in the scattering chamber. With these improvements, he measured and then greatly reduced the counting of electrons ejected from the walls by γ - rays. Chase also enhanced the proportion of β-rays to γ - rays by replacing our milligram of radium with Ra E (from old radon tubes) in sufficient quantity to provide a large increase in the emission of β-rays. As a result of these changes, Chase was able to reduce the voltage on the Geiger counter to a value which gave the points a much longer life and still left him with enough counts for valid statistical reasoning. Under these conditions, he found that the asymmetry which McIlwraith, Kurrelmeyer and I had observed virtually disappeared. These observations were the subject of his first paper.

In Chase's second experiment, he found (as McIlwraith, Kurrelmeyer and I had conjectured) that counters such as all of us had been using were selective with respect to the speed of the electrons counted. Most importantly, he found that the selectivity depended on the voltage. With the voltage lowered to prolong the life of the points, as in Chase's first experiment, only the slower electrons were counted and the asymmetric scattering disappeared. With the voltage high enough to count the faster electrons, the asymmetric scattering reappeared but the points did not last long.

In his third paper, Chase described experiments in which the Geiger counter was replaced by a sensitive electroscope. A small but consistent asymmetry was observed.

Thus, at the end of these three experiments, Chase had evidence both to confirm the real existence of the asymmetry which McIlwraith, Kurrelmeyer and I had observed and to explain its puzzling appearance and disappearance in accordance with the replacement of the counter-points.

Only one other attempt was made by any of us to demonstrate polarization in a beam of β-rays. In analogy with the polarization of light by a tourmaline crystal, Frank E. Myers and I, at McIlwraith's suggestion, counted the β-particles passing through two thin iron foils magnetized in different directions. No change in the count was observed when the angle between the directions of magnetization was changed.

Chase and Myers remained at New York University after receiving their Ph.D.'s and, with the aid of a succession of graduate students, carried on other experiments on the polarization of fast electrons. But these experiments, though well worthwhile and well-executed, could not show the helicity of β-particles, since the electrons they used were emitted from hot filaments and accelerated by high voltage.

This later work by Chase, Myers and others left me quite puzzled. I found it difficult to reconcile our observations of β-particles with the prevailing theory and observations on artificially accelerated electrons. I never published a retraction of our findings, but I probably expressed some doubts about the reality of our effect in conversations with friends.

To the best of my knowledge, neither our experiments, nor those by Chase, have ever been replicated‡. Our observations were left unconfirmed until a number of experiments in 1957 conclusively demonstrated parity nonconservation in weak interactions. During the nearly thirty years which passed between our experiments and those of Wu, Garwin, and Telegdi, many doubts were expressed about our observation. These doubts can be easily understood when one considers the theoretical models which prevailed before Lee and Yang. Our work was, prior to 1957, generally unaccepted, disbelieved and poorly understood. Only by viewing it from the new theoretical framework, and experimental observations of the late 50's could our results be comprehended. It appears now, in retrospect, that our experiments and those of Chase, were the first to show evidence for parity nonconservation in weak interactions. ☐

Baltimore, May 1973.

‡ For a report on a replication of these experiments, see: "A Comment on the History of Double Scattering of Beta Rays," p. 154.

APPARENT EVIDENCE OF POLARIZATION IN A BEAM OF β-RAYS

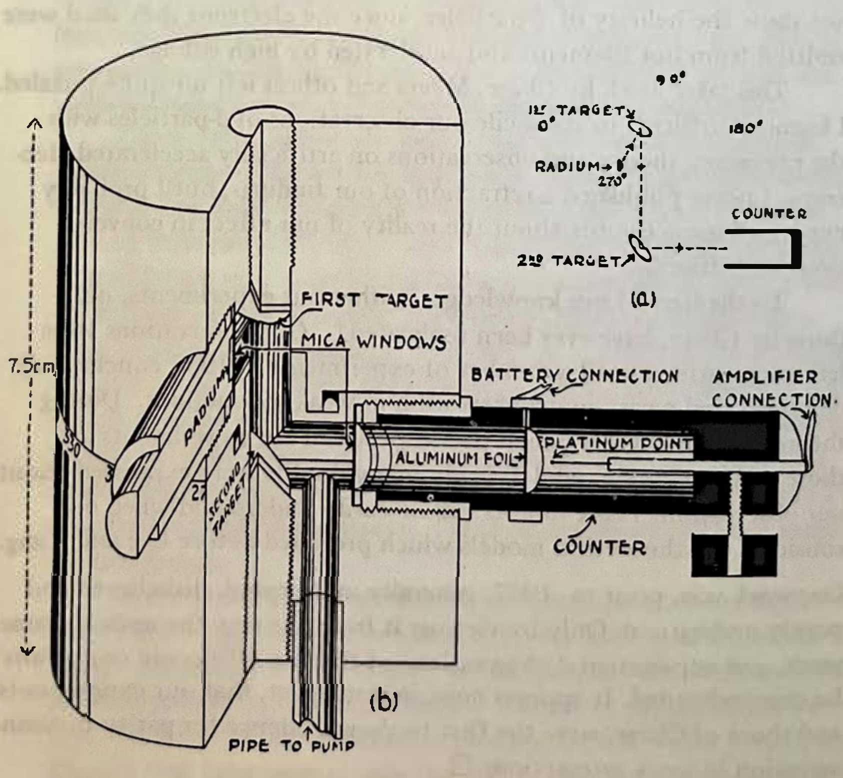
By R. T. Cox, C. G. McIlwraith and B. Kurrelmeyer*

New York University and Columbia University†

Communicated June 6, 1928

The already classic experiment of Davisson and Germer[1] in which the diffraction of electrons by a crystal shows the immediate experimental reality of the phase-waves of de Broglie and Schrödinger suggested that it might be of interest to carry out with a beam of electrons experiments analogous to optical experiments in polarization. It was anticipated that the electron spin, postulated by A. H. Compton[2] to explain the systematic curvature of the fog-tracks of β-rays, and recently so happily introduced in the theory of spectra by Uhlenbeck and Goudsmit[3] might appear in such an experiment as the analogue of a transverse vector in the optical experiments. This idea has lately been theoretically developed by C. G. Darwin.[4] Experiments in this line were undertaken over a year ago. Since they are soon to be interrupted, it seems advisable to make a preliminary report of the results obtained thus far, although they are somewhat inconclusive in spite of a great accumulation of data.

Since the equivalent wave-length in the wave mechanics of even slowly moving electrons is of the order of that of x-rays, it seemed preferable to attempt an experiment analogous to optical polarization by scattering or absorption rather than by reflection or double refraction. We have been chiefly occupied by an experiment analogous to that in which Barkla demonstrated the polarization of x-rays by double scattering. In our experiment β-particles, twice scattered at right angles, enter a Geiger counter. The relative numbers entering are noted, as the angle between



the initial and final segments of the path is varied. For reasons to be mentioned later the angles at which most of the observations have been made are those indicated in figure 1(*a*) as 270° and 90°. The difference between the configurations of the three segments of path at these two angles is the same as the difference between right- and left-handed rectangular axes.

The apparatus in its latest form is shown in detail in figure 1(*b*), a quarter-section being supposed removed. Its largest part is a steel cylinder with a cylindrical passage along the axis and two others at right angles to it. The axial passage is closed at either end with a screw plug. Each plug ends in a gold scattering target set opposite one of the radial passages and at 45° to the axes of the two passages at the junction of which it is placed. A milligram of radium, which is the source of the β-particles, is set in one of the radial passages and the counter in the other. The three passages thus form the three segments of the path of the β-particles. To obtain the required variation in angle between the initial and final segments, the cylinder is made in two parts, so that the upper part, carrying the radium and the first target, can be rotated with respect to the lower about their common axis. The parts fit together at a ground joint with a mercury seal; the two radial passages are closed with mica windows, thin enough to allow the entrance and exit of the β-particles but thick enough to sustain atmospheric pressure; the plugs in the axial passage are sealed in with wax. The cylinder is thus made nearly air-tight and the cavity is kept evacuated by continuous pumping to a pressure at which the mean free path of a β-particle is large in comparison with the length of its path through the cavity. The counting chamber is a hollow ebonite cylinder. It is closed with an aluminum foil, which is kept at a positive potential of about 2000 volts by connection to a storage battery. The ionization which occu swhen a β-particle passes through the foil into the chamber produces a discharge between the foil and a platinum point a few millimeters behind it. The point is a platinum wire two mils in diameter ending in a fu sedknob about six mils in diameter. This point is connected through a high resistance to ground and directly to a four-stage amplifier. The amplifier operates a sensitive relay, which in turn actuates a recording pen on a moving tape. Discharges are produced not only by β-particles but also by photo-electrons ejected from the apparatus by the γ-rays of the radium. The high penetration of these rays makes it impossible to shield against them without interposing so much material that the path of the β-particles would be too much lengthened. Their effect is considerably reduced by making the counting chamber of ebonite, from which comparatively few photo-electrons are ejected. Their number, however, could not be neglected, but there is no reason to expect that it would vary between the two settings at which most of the counts were made.

Although the platinum points described were found the best of several types and materials that were tried, they are far from satisfactory. They usually give inconsistent results after an hour or two of use and have to be replaced. Moreover, the counts obtained with two different points do not agree. For this reason and on account of the uncertainty of the effect of the γ-rays, it seemed inadvisable to attempt counts all around the circle. Attention was given instead to taking counts to test an early observation that fewer β-particles were recorded with the radium at 90° than at 270°. Data are taken as follows. A count is taken at one setting for a definite time (five or ten minutes) and then at the other setting for an equal time. This is continued usually as long as the counter gives self-consistent results. To offset the effect of small gradual changes in the characteristics of the counter and in the voltage of the battery, the count at one setting is compared with the counts immediately preceding and following it at the other setting. Thus, for example, from eleven counts five values are obtained for the ratio of the count at one setting to the count at the other. The mean of these values is taken and the probable error computed in the usual way. It is these means and probable errors that are given in the following table. It must be admitted that the probable error in many cases is reckoned from too few values to give it great validity. The table shows the results of nineteen sets of data.

Count at 90° / Count at 270°	..	..	..	..	0.76	0.90	0.94	0.87	0.98	1.03
Count at 90° / Count at 0°	0.78	0.62	0.65	..	..	..	..	..	..	..
Count at 0° / Count at 270°	..	..	..	0.87	..	..	..	..	..	..
Probable error	0.01	0.02	0.01	0.02	0.01	0.07	0.01	0.02	0.01	0.03

Count at 90° / Count at 270°		1.03	0.91	0.95	0.99	1.01	1.06	1.05	0.55	0.91
Probable error		0.02	0.02	0.05	0.03	0.04	0.05	0.02	0.05	0.03

It will be noted that of these results a large part indicate a marked asymmetry in the sense already mentioned. The rest show no asymmetry beyond the order of the probable error. The wide divergence among the results calls for some explanation, and a suggestion to this end will be offered later. Meanwhile a few remarks may be made on the qualitative evidence of asymmetry. Since the apparatus is symmetrical in design as between the two settings at 90° and 270°, the source of the asymmetry must be looked for in an accidental asymmetry in construction or in some asymmetry in the electron itself. The following possibilities may be suggested in the former case. The radium and the point in the counter were doubtless not exactly centered. But they were removed and replaced repeatedly in the course of the observations, and it seems unlikely that their accidental dislocations could be so preponderantly in one direction as are the observations. Any effect due to this cause could be offset by turning the counter and the rod that carries the radium through 180°. The apparatus with which these data were obtained was not designed to make this convenient, but in the latest apparatus these rotations can be made without disturbing anything else. The results thus far obtained with this apparatus do not lead us to believe that this factor is effective. There was doubtless some asymmetry in the targets as regards their orientation and surface condition. These also were several times removed and replaced after their surfaces had been freshly filed bright. A magnetic field inside the cavity due either to the slight penetration of the earth's field through the steel walls or to an accidental magnetization of the cylinder itself would introduce an asymmetrical factor. It seems highly unlikely, however, that any deflection so caused could be great enough to produce effects of the magnitude observed. It seems possible, on the other hand, that a spinning electron might be oriented by even a weak field by a kind of space quantization and that this orientation might combine with the scattering to produce the observed asymmetry. This explanation, of course, assumes a polarity in the electron as definite as that required to explain the observations as due to double scattering. Of the same sort is the supposition that the beam of β-particles undergoes a polarization in passing through the mica windows, similar to the polarization of light in passing through a tourmaline crystal. This effect was in fact looked for carefully in an experiment auxiliary to the present investigation but it was not found.

It should be remarked of several of these suggested explanations of the observations that their acceptance would offer greater difficulties in accounting for the discrepancies among the different results than would the acceptance of the hypothesis that we have here a true polarization due to the double scattering of asymmetrical electrons. This latter hypothesis seems the most tenable at the present time. The discrepancies observed we ascribe tentatively to a selective action in the platinum points, whereby some points register only the slower β-particles. Observations in apparent agreement with this assumption have recently been made by N. Riehl.[5] It is necessary to suppose further that the polarization is also selective, the effect being manifest only in the faster β-particles. In support of this it may be remarked that a few observations we have just made seem to show that asymmetry is more consistently observed when a piece of celluloid or cellophane is placed in front of the counter to stop the slower β-particles. Perhaps the simplest assumption here is that only β-particles which are scattered without loss of energy show polarization.

We have made no attempt at a theoretical treatment of double scattering beyond a consideration of the question whether the results here reported are of an asymmetry of higher order than what might be expected of a spinning electron. The following suggestion is then offered not at all as a theory of the phenomenon but merely as a remark on the geometry of the experiment. If it be supposed that the spin vector of a moving electron is always at right angles to its velocity vector, and that when the electron is scattered at right angles its new velocity vector has the direction of the vector product of its former velocity and spin vectors and its new spin vector has the direction of its former velocity vector, then the observations here described will be qualitatively accounted for.

In closing we wish to acknowledge gratefully our indebtedness, for help and advice in the construction of Geiger counters, to Dr. C. W. Hewlett of the General Electric Company, to Mr. A. E. Loomis of the Loomis Laboratory and to Dr. L. F. Curtiss of the Bureau of Standards. We are under obligation also to Mr. Hermann Beck for his interest and care in the construction of the apparatus used.

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A COMMENT ON THE HISTORY OF DOUBLE SCATTERING OF BETA RAYS

BOTH THE ANALYSIS AND AN (UNPUBLISHED) EXPERIMENT, PERFORMED AT M.I.T. IN 1960, INDICATE THAT THE SIGN OF THE ASYMMETRY REPORTED BY COX AND CHASE WAS WRONG

by
Lee Grodzins

It has been more than a decade since I've done an experiment on parity violation. I have long since swept my files clean of the scores of reprints and pounds of notes and calculations that had amassed.

My memory may undoubtedly distort the history of that time as may my attempts to correct for personal bias. It is, therefore, with a feeling of some uneasiness that, at the request of the editor of *Adventures,* I set forth my recollections of a confused footnote to the history of early experiments on double scattering of beta particles—a confusion which I helped to confound.

In the late spring of 1958, while still a staff member at Brookhaven National Labs, I was asked by Otto Frisch to write a review article on the measurement of parity violation. Maurice Goldhaber, Andrew Sunyar and I had completed several experiments which measured the extent of parity violation in beta decay interactions and had found a way to measure the longitudinal polarization of the neutrino. Frisch allotted me little time in which to complete the article and even with extended deadlines it was a racking labor which I recall with little pleasure.

As the research for the writing unfolded, I read the early papers on the scattering of beta decay electrons and positrons. My own thesis, completed in 1954, was on the single and multiple scattering of 2.5 MeV positrons from various thin foils. Thus, I was only too aware that a relatively minor modification of the experimental approach would have led to observable parity violating effects. My readings of the early papers had, therefore, a definite direction. Had anyone seen effects which, in hindsight, could be interpreted as evidence of parity violation?

A description of my thesis approach and the modifications which I alluded to may be useful. I had used a 90° bending magnet to select a nearly monochromatic beam of positrons from a radioactive source. The selected beam was then scattered by a gold or aluminum foil and the angular distribution of scattered positrons measured. I noted in my thesis that as many positrons scattered through a given angle to the left as to the right of the beam axis. I stated, therefore, that the instrumental asymmetry in my experiment was negligible. After the historic paper by Lee and Yang was published, I knew that symmetry was expected since the magnetic selector had left the polarization of the positrons essentially unchanged and the scattering distribution should be symmetric for longitudinal positrons. If, however, I had used an electrostatic rather than a magnetic selector prior to the scattering, the longitudinal polarization would be transformed to transverse and the scattering would have resulted in an observable left-right asymmetry.

Another and pragmatically equivalent method for transforming longitudinal to transverse polarization is to scatter the beta particles through 90° by a thick, low Z scatterer. Several experiments carried out in 1957 and 1958 had shown that such double scattering experiments using a thick first scatterer and a second thin foil resulted in asymmetries which were unambiguous evidence of parity violation.

My literature search uncovered no evidence for parity violating effects until I came across the papers of Cox and his students. Indeed, I could not find any experiments which *should* have observed an effect. In the intervening 25 years, investigators of electron scattering had used electron gun sources which *by design* produced unpolarized electron beams. My experiment was one of the first two on the scattering of positrons derived from beta sources. The other was carried out by Harry Lipkin who also used a magnet to select a monochromatic beam of positrons for scattering.

In my review paper[1] I summarized the Cox and Chase papers by quoting their clearly stated results and conclusions. Cox states in his first paper:

> '. . . fewer beta particles were recorded with the radium at 90° than at 270°'[2]. A table is presented of nineteen sets of data. The average of all of their data, considering the statistical errors only, is $N_{270}/N_{90} = 1 \cdot 10 \pm 0 \cdot 01$. The authors realize the evident instrumental errors which cause large fluctuations in the data and ascribe them to malfunctioning of the Geiger counter. They then continue,'A few remarks should be made on the qualitative evidence of asymmetry. Since the apparatus is symmetrical in design as between the two settings at 90° and 270°, the source of the asymmetry must be looked for in an accidental asymmetry in construction or in some asymmetry in the electron itself. Many tests were made on the former point as well as on the effect of the mica windows through which the beta rays passed.' They also discuss the effect of a weak magnetic field which may have been present. They conclude that instrumental asymmetries do not account for the effect and furthermore, 'it should be remarked of several of these suggested explanations of the observations that their acceptance would offer greater difficulties in accounting for the discrepancies among the different results than would the acceptance of the hypothesis that we have here a true polarization due to the double scattering of asymmetrical electrons. This latter hypothesis seems the most tenable at the present time. It is necessary to suppose further that the polarization is also selective, *the effect being manifest only in the faster beta particles.* In support of this it may be remarked that a few observations we have just made seem to show that asymmetry is more consistently observed when a piece of celluloid or cellophane is placed in front of the counter to stop the slower beta particles. Perhaps the simplest assumption here is that

1. "Measurement of Helicity" Grodzins, L., *Progress in Nuclear Physics,* Vol 7, (Pergamon Press, 1959.)
2. Cox, R. T., McIlwraith, C. G., and Kurrelmeyer, B., *Proceedings of the National Academy* 14, 544, (1928) - reproduced p. 150.

3. Chase, C. T., *Phys. Rev.* 34, 1069 (1929).
4. Chase, C. T., *Phys. Rev.* 36, 984 (1930 a).
5. Chase, C. T., *Phys. Rev.* 36, 1060 (1930 b).

only beta particles which are scattered without loss of energy show polarization.'

The problem was then taken up by Chase, a student of Cox. He improved the apparatus in several essential ways. Path lengths were lengthened, mixed radium was replaced by RaE, lead shields were placed further to reduce gamma-ray counts in the counter, provision was made to block the beta rays conveniently and obtain the background rate, and, most important of all, the counter sensitivity was investigated (1930a)[3]. Chase showed that the Geiger counters were unreliable for counting high-energy betas, and he replaced the counters with an electroscope (1930b)[4]. This latter change made it very difficult to determine the true errors in counting but did give reproducible data. Chase was able to show that the negative results he obtained in one experiment (1929)[5] were due to the low voltage on the Geiger counter which made it insensitive to energetic betas, so that 'with the Geiger counter used as it had been before, at lower voltages, the results were negative as before, but with high voltages on the counter, high enough to ruin the point within an hour or two, the effect was very likely to appear. *Making no changes except in the voltage on the counter, the effect could be accentuated or suppressed.*' With the improved apparatus, using an electroscope, he concluded, 'the asymmetry between the counts at 90° and 270° is always observed . . . not only in every single run, but even all the readings in every run, with few exceptions, show the effect.' While some difference in counting rate was observed between the 0° and 180° positions, in agreement with the theory of Mott published while the investigation was being conducted, the evident asymmetry in the geometry of the apparatus in these two positions was realized by the experimenters and Chase concludes: 'The position of the point at 180° on the curve must therefore be regarded as the least reliable of the set.' The effect obtained in this last experiment was $N_{270}/N_{90} = 1 \cdot 03$.

The objections to these experiments were realized partially by the investigators and in greater degree by subsequent writers. The scatterers were thick so that scattering was multiple and plural and should not show any Mott polarization. The beta rays were uncollimated so that wall scattering was present and the scattering angles were ill-defined. The velocity of the beta rays was defined loosely by absorbers and was essentially uncontrollable. For all of these reasons subsequent experiments were done with thin scatterers, better collimation, and electron sources from high-voltage accelerators (where, of course, the electrons would not be helical and where, indeed, no asymmetry was observed). Experimenters checked their apparatus by showing that no asymmetry existed between 90° and 270°. Nevertheless, from the point of view of present understanding, these original experiments contained three essential features:

1. The second target was of a high Z material.
2. The geometry was symmetric between 90° and 270°.
3. Relativistic beta rays were used.

It appears in retrospect that these early investigations were the first experiments showing evidence for the nonconservation of parity in weak interactions!

The pages written for the review paper were expanded and published as a separate historical note[6] in the *Proceedings of the National Academy* where Cox had first published his findings in 1928. The *Proceedings* paper appeared in early 1959; the review paper was not printed until the summer of 1960. So much for deadlines.

When I transferred from Brookhaven to the faculty of MIT in early 1959, I still did not know that the Cox and Chase work gave the wrong sign; $N_{270°}$ should have been less than $N_{90°}$ for negative helicity of beta particles. Of course I had checked the sign of the asymmetry against what was to be expected. I remember Otto Frisch asking me about this point and I assured him that it had been done by myself and others. I wasn't to find out for almost a year that my analysis was wrong. Thus, when I decided to duplicate the Cox experiment at MIT using modern detection techniques, it was simply a fun project of pedagogic interest.

In the fall term of 1959, Sidney Altman, a senior physics major, was enthusiastic about completing his undergraduate thesis requirement by duplicating these historic experiments. We had the apparatus built, and he carried out a series of experiments which were written up as a senior thesis[7].

Sidney's thesis was submitted on May 23 1960, months before the review article appeared. In the meantime, though, I had learned of the error in my analysis in a letter from Dr. Waldmann of the Max Planck Institute, who wrote me in early 1960 after reading a preprint of the review paper. In the best teacher-student tradition, I didn't tell Sidney. He conducted the experiment without bias. Of course, by the time he wrote up the work, he knew full well of the comedy of errors—for his results were the opposite to those of Cox and Chase—in their terms he got $N_{90°}$ greater than $N_{270°}$.

Sidney's thesis is a model: logical, analytical, thorough. His lengthy introduction is followed by a novel calculation of the intensity of the asymmetry expected from an unpolarized source. The apparatus is described in terms of design problems and instrumental uncertainties. He includes an analysis of instrumental

6. *Proceedings of the National Academy* 45, 399 (1959).
7. Altman, Sidney; Senior Thesis, Massachusetts Institute of Technology, 1960, (unpublished). It is believed that this was the last of Altman's research in physics. He did his graduate work in microbiology and is now on the biology faculty at Yale University.

asymmetries, energy losses during flight, multiple scattering, collimation, etc. In the section on procedure, Altman writes that:

> The experimental procedure was very straightforward. Integrated counting rates were recorded for specific time intervals at 15° intervals around a circle; that is, rotating the upper target and source with respect to the detector position. Depending upon the Z of the first and second target, the counting rate was higher with high Z and lower with low Z as the Coulomb scattering cross section predicts. The total counting time was fixed so that the statistical error would be small. In some cases much more data were taken at the 90° and 270° positions since that is where we expected the greatest asymmetry. Background readings were taken by reversing the top target. Calibrations were made by removing the bottom target and inserting the bismuth source on a stand so that it faced the crystal detector directly.

His discussion on results is lengthy with many graphs and tables. He writes:

> We are mainly interested in data taken at a baseline setting at 0.95 MeV since the helicity, and therefore the asymmetry in counting rates at 90° and 270°, will be greater at higher energies. At the 0.37 MeV setting we should see a much reduced asymmetry or no asymmetry at all. Since the asymmetry is also dependent on Z^2 we should observe this, as the Z of the bottom target is changed.

The angular distribution for a Cu-Cu scattering combination is shown in Fig. 1. The difference in counting rates between 0° and 180° is not due to Mott scattering effects but, as Altman explains

No Asymmetry With Low Energy Electrons

Our present understanding of beta decay enables us to understand why the asymmetry effect disappeared at low energies. The two component theory of the neutrino predicts (and it has been confirmed experimentally) that the polarization of electrons, P, from beta decay is: $P = \pm v/c$. At zero velocity, $P = 0$, since there is no momentum vector to which one can refer. As $v \to c$, $P \to 1$, since the mass of the electron becomes unimportant and it behaves like a neutrino. One can see that for low velocities, the electrons are not highly polarized and hence no asymmetry would be observed.

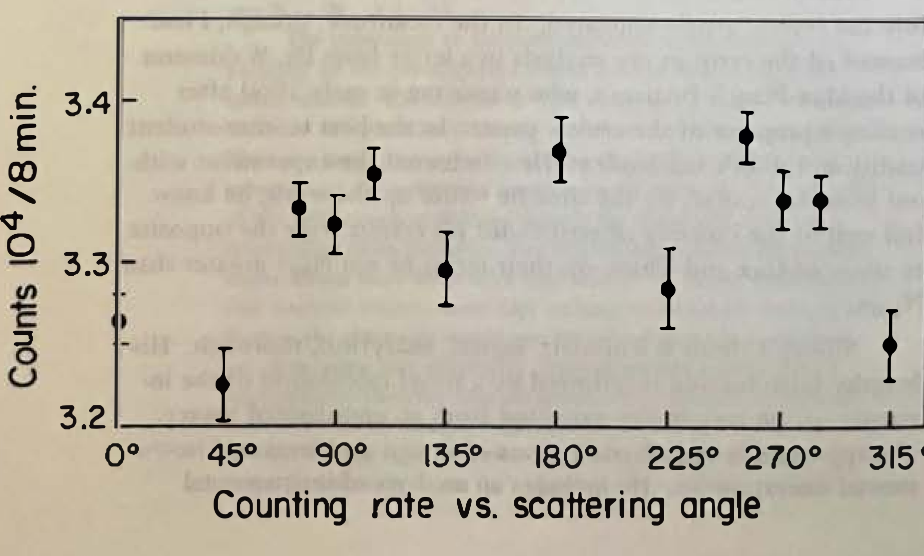

Fig. 1
Counting rate as a function of scattering angle with both targets made of copper. The energy of the β-particles is 0.95 MeV (April 26, 1960).

in a separate section, is due to a geometrical effect. Except for this set, there is no obvious asymmetry between any two data points 180° apart. Altman goes on to say:

Now, Pb was used as the second target, Z = 82 in this case. In graph 9 (not shown) the baseline of 0.37 MeV shows an asymmetry of approximately 2 per cent at the 90° - 270° position that increases slightly when we look only at higher energies (see Fig. 2). Alikhanov[5] had reported that effect of plural scattering (depolarizing) in the first target was negligible. It was decided to make the first target Pb to see if this was so. Graphs 11 - 13 (not shown) show that difference in counting rates at all positions is now negligible, certainly less than 1 per cent at 90° - 270° position for all energies. At a baseline of 1.20 MeV there is practically no difference at all. This indicates that changing the Z of the first target by making it higher does have some effect in depolarizing the electron beam.

In confirmation of this, the first target was now changed to Al, Z = 11. We now expected a larger asymmetry than with Cu as the first target since Al would depolarize the beam less. Graphs 14 and 15 (not shown) bear this prediction out. These are the results of runs made with concentration about the 90° - 270° points. The statistical accuracy for the other points is not very good. Even so, there is about a 2 per cent difference at the 0.37 MeV baseline setting which rises to 3 per cent at the 0.95 MeV setting. For the first time also, one can see the depression in counting rate around 90° and heightening around 270°. These preliminary results also indicate that the differences observed by Chase have the wrong sign.

5. Alikhanov, A. I., *Nuclear Physics* 6, 588 (1958).

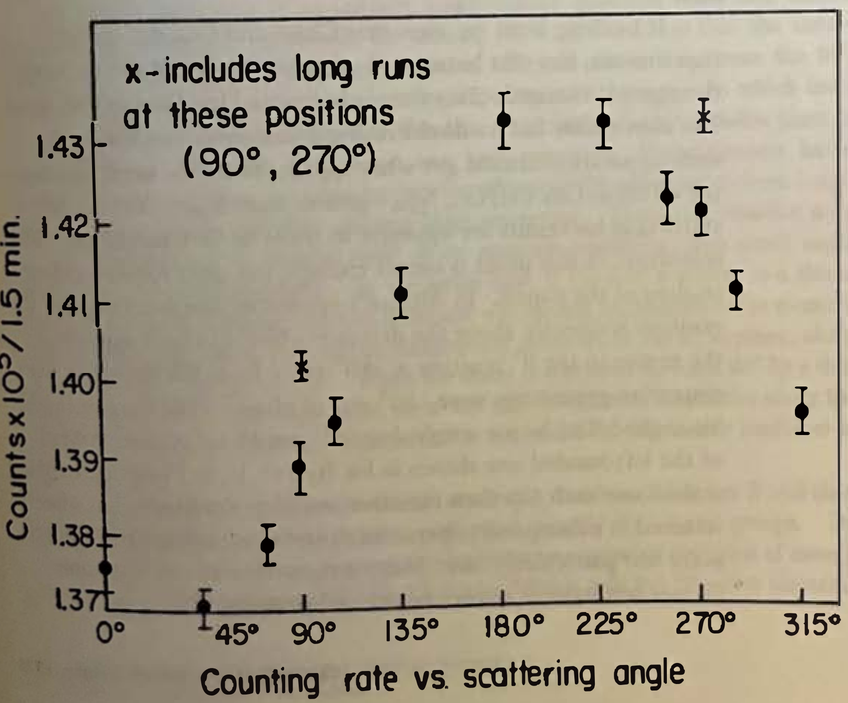

Fig. 2
Counting rate as a function of scattering angle where the first target is lead; the second is copper, as in Fig. 1. The energy of the β's is again 0.95 MeV. Data include longer runs at 90° and 270° (April 30, 1960).

Altman, after a discussion of instrumental asymmetries in light of the results, displays the data to reduce those asymmetries. He then considers further tests which should be undertaken with improved equipment. He concludes with the following paragraphs:

> **This experiment can only be considered as groundwork for more exhaustive and more conclusive tests. Preliminary results indicate a 3 per cent asymmetry in counting rates at 90° and 270° ($\frac{270°}{90°}$ = 1.032) scattering angles for a thick first target of Z = 13 and thick second target of Z = 82. This asymmetry seems to vanish when the Z of the second target is 29. Raising the Z of the first target has an appreciable effect in depolarizing the electron beam, seemingly in contradiction with the results reported by Alikhanov. The asymmetry observed is greatest at energies above 0.95 MeV and decreases for energies below 0.95 MeV. The sign of the asymmetry reported by Cox appears to be wrong.**

Unfortunately, no further tests were made. None of my students in the succeeding years wanted to continue the work and my own energies were channeled into investigations using the Mössbauer effect. I recall ambivalent feelings about the entire affair. On one hand I wanted to tell the story, on the other hand I wanted to wait until the story was right; there seemed little point in publishing unfinished work. As projects do, this one kept getting pushed further and further back until it no longer troubled my thinking, though it never dropped completely from my consciousness.

It has long been my view that Chase and Cox did correct experiments, but that between the investigation and the write-up the sign got changed. This view is buttressed by Altman's write-up. The alert reader has no doubt realized that, with essentially the same apparatus, Altman got what appears to be the same sign for the effect as Cox did; i.e., $N_{270°}$ greater than $N_{90°}$. Yet Altman states that his results are opposite to those of Cox and Chase. The resolution of this point is simple enough, but does require a close reading of the papers. In Altman's apparatus, the source in the 0° position is directly above the detector while in Cox's apparatus the source in the 0° position is 180° away from the detector; the respective geometries were 180° out of phase[6]. Did Cox mislabel his angles? Did he use a right-handed coordinate system instead of the left-handed one shown in his figure? If, as I suspect, he did make some such slip then the error would undoubtedly have been retained in subsequent papers. Such errors are neither difficult to make nor particularly rare. Many a researcher and at least one former historian of science have erred similarly.□

6. See Fig. 1, p. 150 for Cox's apparatus.

Cambridge, Massachusetts, October 1973

EXPLANATORY PHYSICS NOTES

DOUBLE SCATTERING OF ELECTRONS AND POLARIZATION OF BETA RAYS*

Lee and Yang, in their historic paper[1] suggesting the possibility of nonconservation of parity in weak interactions, pointed out that an experimental test of parity violation must involve the observation of a pseudoscalar. The pseudoscalars they considered in beta decay of nuclei were $(\bar{J}_i \cdot \bar{p}_e)$ and $(\bar{J}_f \cdot \bar{p}_e)$, where $\bar{J}_i$ and $\bar{J}_f$ are the initial and final spins of the nucleus and $\bar{p}_e$ is the momentum of the beta ray. In that paper they concluded that none of the then existing experiments measured either of these quantities. It was later realized[2] that another observable pseudoscalar would be the longitudinal polarization of electrons emitted from unoriented nuclei; i.e., $(\bar{\sigma}_e \cdot \bar{p}_e)$, $\bar{\sigma}_e$ being the spin of the electron. This prediction has been confirmed by a number of experiments.[3, 4]

In 1929, Mott[5] showed that the spin orbit coupling in Coulomb scattering could be used as a polarizer or analyzer of electron spin. The spin orbit coupling is largest when the energy of the electrons is relativistic, the Z of the scatterer is high, and large angle scattering occurs; most calculations consider single scattering through 90° by nuclei of $Z \simeq 80$. Mott double scattering is illustrated in Figure 1*A*, where an unpolarized beam of relativistic electrons is first polarized and then analyzed by being scattered singly twice by very thin high Z scatterers; $\hat{n}_1$ is a unit vector in the direction from the source to scatterer 1, $\hat{n}_2$ is a unit vector in the direction of scatterer 2, $\hat{n}_3$ is a unit vector in the direction of the detector. The spin orbit term in the cross section is of the form $\bar{\sigma} \cdot (\hat{n}_1 \times \hat{n}_2)$, so that only a spin perpendicular to the plane of scattering is affected. The single scattered beam at $\hat{n}_2$ will be partially polarized along the $+X$ direction. In the second scattering the spin orbit part of the cross section, $\bar{\sigma}_{+X} \cdot (\hat{n}_2 \times \hat{n}_3)$, will add to the Coulomb term, while $\sigma_{-X} \cdot (\hat{n}_2 \times \hat{n}_3)$ will subtract, so that a greater counting rate will be observed when the source is at the 180° position compared to that observed at the 0° position. No difference in counting rate should be observed when the 90° and 270° positions are compared.

The double scattering of an initially longitudinally polarized beam also results in strong asymmetries. However, the term $\bar{\sigma}_{n_1} (\hat{n}_1 \times \hat{n}_2)$ is zero, so that the asymmetry at 0° aud 180° is small and the main effect is observed between the 90° and 270° positions.[6] A double scattering experiment on beta rays in which both the first and second scatterers are so thin that only single scattering takes place is very difficult. No such experiment has been reported. Experimenters have, however, measured beta helicities by first transforming the polarization from longitudinal to transverse and then utilizing Mott scattering. The transformation may be accomplished by an electric field[3] or by multiple scattering since small angle scattering is spin independent. In the latter case, if the first scatterer is a thick, preferably low *Z*, material, the spin direction will remain unchanged as the momentum turns and the beam at $\hat{n}_2$ will be transversely polarized in the n_1, n_2 plane, along the *X* direction in Figure 1, *B*. When the beam is scattered through 90° by a thin high *Z* scatterer, the large spin-orbit term will lead to different intensities along the *Y* axis, or for a fixed detector, a difference in intensities for the source positions at 90° and 270°.

The experiment just described—double scattering first by a thick low Z and then by a thin high *Z* scatterer—has been performed recently by several groups. De Shalit *et al.*[7] measured 6 to 8 per cent asymmetries by scattering electrons of energy greater than 300 kev through 65° by 2.5 mg/cm² thick gold foil (the first scatterer

* from the Proceedings of the National Academy of Sciences
Vol. 45, No. 3, pp. 399–405. March, 1959.

was thick aluminum). Alihanov *et al.*[8] observed asymmetries as large as 40 per cent (and never less than 18 per cent) when 150 to 400 kev beta rays were back scattered through 115° by gold foils ranging from 0.34 to 1.97 mg/cm². They also showed that the asymmetry is essentially independent of the Z of the first scatterer. Heintze[9] obtained 6–12 per cent effects by backscattering ~1 mev betas through 135° by Au and Pt foils.[10]

Lee Grodzins

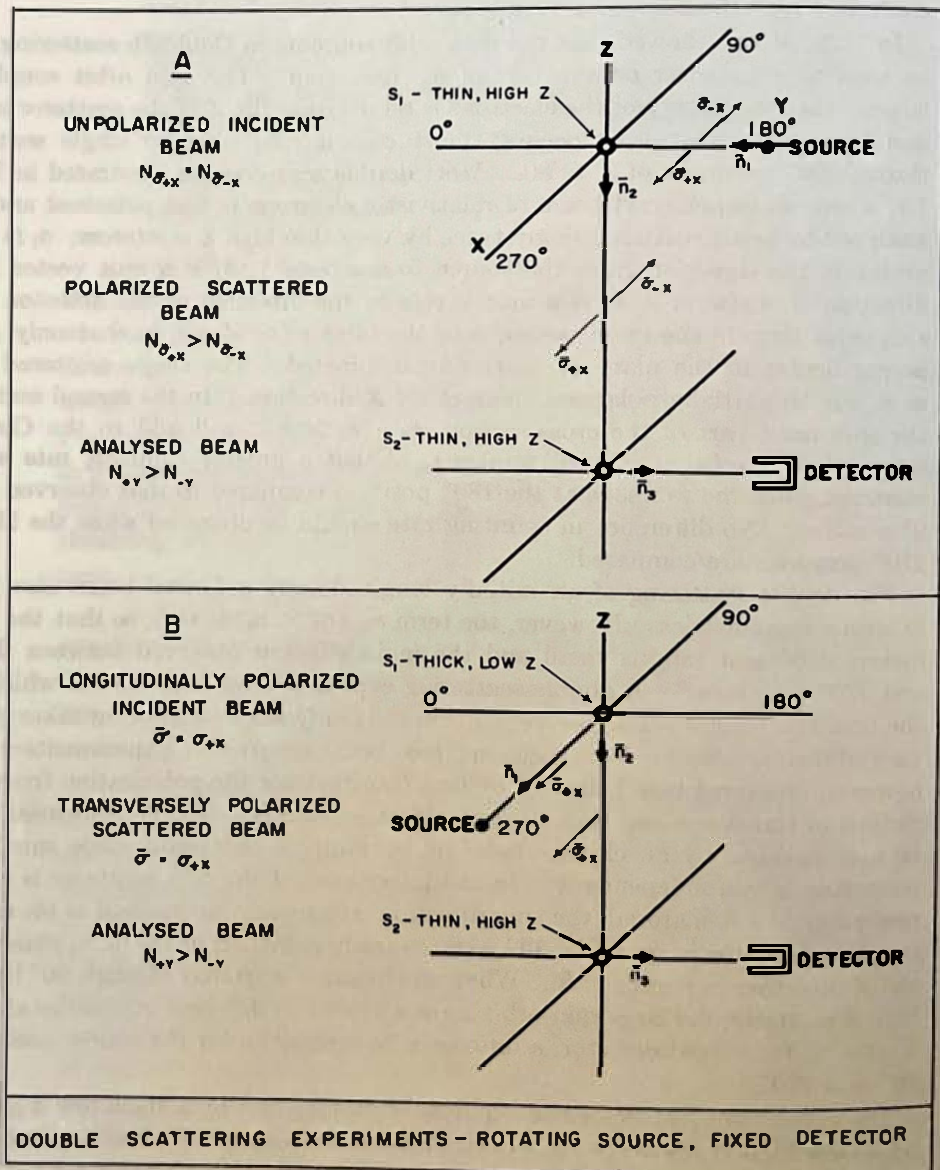

FIG. 1.—*A*, double scattering of an unpolarized beam of electrons. *B*, double scattering of a longitudinally polarized beam of electrons.

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