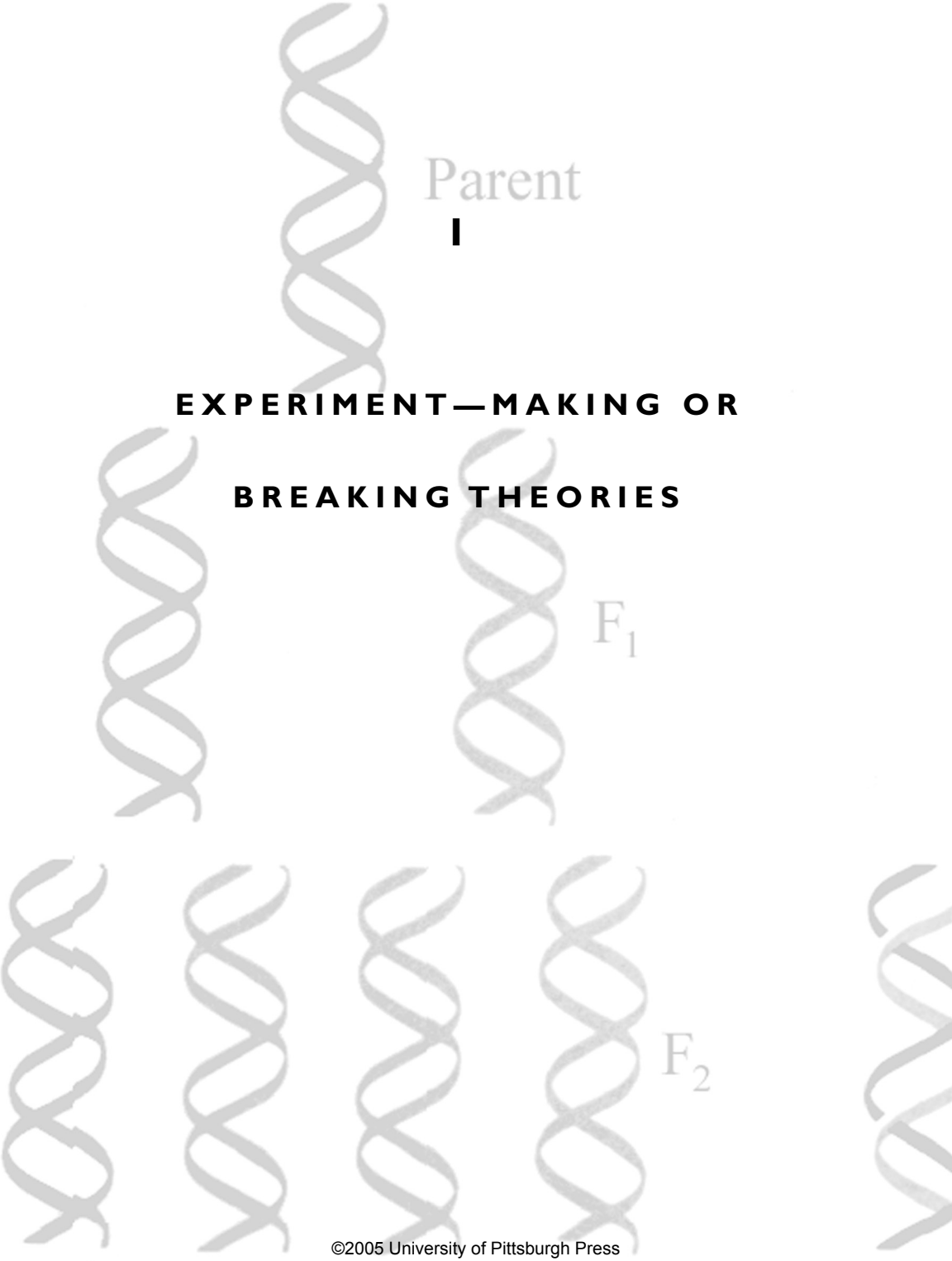

I

EXPERIMENT—MAKING OR BREAKING THEORIES

(2)
The Violation of Parity Conservation

One of the important purposes of experiment is testing theories or hypotheses. One example of a crucial experiment, which decided unequivocally between two competing theories, was the discovery that parity conservation—also known as mirror-reflection symmetry or left-right symmetry—is violated in weak interactions. It is perhaps the clearest case of a crucial experiment in the history of physics. This case is fascinating because experiments done in the late 1920s and early 1930s in retrospect also demonstrate parity nonconservation. The significance of these experiments was not realized by either the experimenters themselves or anyone else in the physics community. It was only after parity nonconservation had been discovered in the 1950s that physicists recognized the significance of the earlier experiments.

Discovery of Parity Nonconservation

In 1956 Tsung Dao Lee and Chen Ning Yang, who would win the Nobel Prize for their suggestion, proposed that parity, or mirror-reflection

For details, see Franklin (1986, chap. 1 and 2).

symmetry or left-right symmetry, is not conserved in weak interactions. (Physicists identify four interactions. In decreasing order of strength they are the strong, or nuclear, interaction, which holds the atomic nucleus together; the electromagnetic interaction, which holds atoms together; the weak interaction, responsible for radioactive decay; and the gravitational interaction.)

Parity conservation was a well-established and strongly believed principle of physics. As students of introductory physics learn, to determine the magnetic force between two currents, first determine the direction of the magnetic field caused by the first current with a right-hand rule and then determine the force exerted on the second current by that field with a second right-hand rule. Exactly the same answer is reached if two left-hand rules are used. This is left-right symmetry, or parity conservation, in electromagnetism and in classical physics.

In 1927 Eugene Wigner proposed the concept of parity conservation in quantum mechanics as a way of explaining some recent results in atomic spectra. It quickly became an established principle. As Hans Frauenfelder and Ernest Henley stated (1975, 359), "Since invariance under space reflection is so appealing (why should a left- and right-handed system be different?), conservation of parity quickly became a sacred cow." An early indication of this came in 1933 when Wolfgang Pauli rejected a theory proposed by Herman Weyl on the grounds that it was not invariant under reflection, or because it did not conserve parity. "However, as the derivation shows, these wave equations are not invariant under reflections (interchanging left and right) and *thus are not applicable to physical reality*" (1933). Pauli, a future Nobel Prize winner for his exclusion principle, a crucial element in the explanation of atomic structure, was notoriously critical and skeptical. He is said to have commented on a paper by another physicist that "it is not even wrong." In this instance, Pauli himself was mistaken.

In the early 1950s, physicists were faced with a problem known as the τ-θ puzzle. According to one set of criteria, that of mass and lifetime, two elementary particles (the τ and the θ) appeared to be the same, whereas by another set of criteria, that of spin and intrinsic parity, they appeared to be different, a very unusual situation in physics. (A good analogy to spin is the rotation of the earth on its axis. Intrinsic

parity refers to the mirror-symmetry properties of the wave function, a mathematical function used to describe the particle.) In 1956 Lee and Yang realized that the problem would be solved, and that the two particles would just be different decay modes of the same particle, if parity were not conserved in the decay of the τ- and θ-particles, a weak interaction (Franklin 1986, chap. 1 and 2). They examined the evidence for parity conservation and found, to their surprise, that although there was strong evidence that parity was conserved in the strong interaction and in the electromagnetic interaction, there was, in fact, no supporting evidence that it was conserved in the weak interaction. It had never been tested. It had just been assumed.

The survey by Lee and Yang was incomplete. They overlooked experiments done in the 1920s and 1930s that, in retrospect, provide evidence for parity nonconservation, although no one at the time realized their implications. They also did not find an amusing early test of parity conservation. In their paper, "Movement of the Lower Jaw of Cattle During Mastication," Pascual Jordan and Ralph Kronig (1927) noted that the chewing motion of cows is not straight up and down, but is either a left-circular or right-circular motion. (These motions reverse in a mirror and are also visible in humans.) The results of their survey of cows in Sjaelland, Denmark, indicated that 55% were right circular and 45% were left circular, a ratio they regarded as consistent with parity conservation.

> This nomenclature is based on the tacit assumption that one and the same cow always maintains its sense of rotation. We could confirm this by a limited number of observations but are aware that more complete data, extending over longer periods of time, are necessary to definitely to settle this point. Statistical investigations on cows distributed over the northern part of Sjaelland, Denmark, led to the result that about fifty-five percent were right-circular, the rest left-circular animals. As one sees, the ratio of the two is approximately unity. The number of observations was, however, scarcely sufficient to make sure if the deviations from unity is real. Naturally these determinations allow no generalisation with regard to cows of different nationality.

The physics community generally, and many leading physicists, did not believe that the Lee and Yang suggestion was correct. Pauli skepti-

cally remarked, "I do not believe the Lord is a weak left-hander, and I am ready to bet a very large sum that the experiments will give symmetric results" (quoted in Bernstein, 1967, 59). There were other bets between physicists. Richard Feynman, one of the leading theoretical physicists of the 20th century and a Nobel Prize winner, bet Norman Ramsey, another winner, $50–$1 that parity would be conserved. Ramsey notes that Feynman believed that the real odds were 1 million to 1, but wouldn't bet that much on anything (personal communication). Felix Bloch, yet another Nobel Prize winner, offered to bet his hat with any other member of the Stanford physics department that parity would be conserved (personal communication from T. D. Lee, 1977).

Lee and Yang (1956) suggested several possible experimental tests of parity conservation in the weak interaction. Of the two most important ones, the first was the β-decay of oriented nuclei. (β-decay is the transformation of one atomic nucleus into a different nucleus, with the emission of an electron and a neutrino. Oriented nuclei are nuclei whose spins all point in the same direction.) The second was the sequential decay $\pi \rightarrow \mu \rightarrow e$. This is the decay of a π meson, an elementary particle, into a μ meson, another particle, and a neutrino. The μ meson subsequently decays into an electron and two neutrinos. These were the first experiments done and provided the crucial evidence for the physics community.

Figure 2.1 helps to explain this. An example is a radioactive nucleus, whose spin points upward and which always emits an electron in the direction opposite to the spin. In the mirror the spin is reversed, whereas the electron's direction of motion is unchanged. Now the electron is emitted in the same direction as the spin. The mirror result is different from the real result. This violates mirror symmetry and shows the nonconservation of parity. Parity conservation would also be violated if, in a collection of oriented nuclei, more electrons were emitted in the direction of the nuclear spin than opposite to the spin, or vice versa. Only if a collection of nuclei emitted equal numbers symmetrically with respect to the spin direction would parity be conserved. For $\pi \rightarrow \mu \rightarrow e$ decay, parity nonconservation implies that the muon (the μ) will be longitudinally polarized, which means that its spin will point either parallel to or antiparallel to its direction of motion. If the muon is

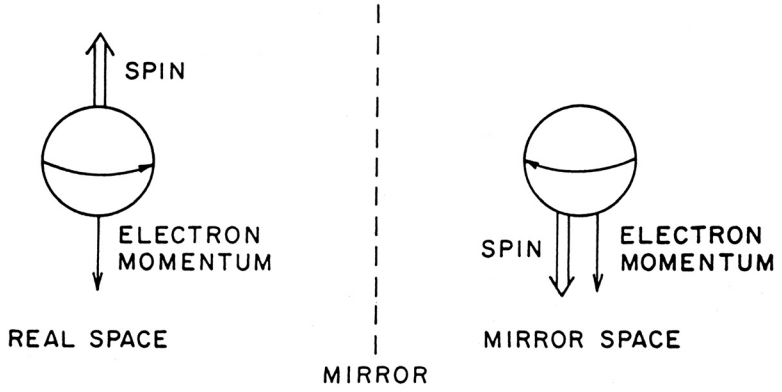

FIGURE 2.1. Spin and momentum in real space and mirror space. In real space the spin and momentum point in opposite directions. In mirror space they point in the same direction. This is an example of parity nonconservation.

stopped, its polarization remains and its subsequent decay will look just like that of an oriented nucleus. In this case an asymmetry would be expected in the distribution of the muon decay electrons emitted along the direction of the muon motion and opposite to that direction.

The first experiment was performed by Chien-Shiung Wu and her collaborators. It consisted of a layer of oriented ^{60}Co nuclei and a single, fixed, electron counter, which was located either along the direction of, or opposite to, the orientation of the nuclei. The direction of the orientation of the nuclei could be changed and any difference in counting rate in the fixed electron counter observed. The results are shown in figure 2.2. With the counter opposite to the nuclear orientation, the ratio of the counts observed when the nuclei were oriented to when they were not was 1.20. With the counter parallel to the orientation, the ratio was 0.80. If parity were conserved, the ratio would have been one. (In statistical terms this was a 13-standard-deviation effect. This meant that it was extremely unlikely that the observed effect was due to a statistical fluctuation in the number of counts.) This was a clear asymmetry. The experimenters concluded, "If an asymmetry 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 β-decay. This

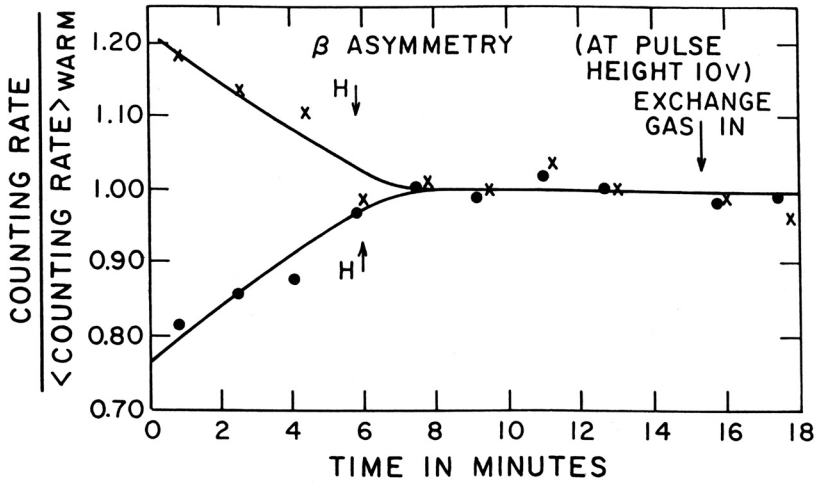

FIGURE 2.2. Relative counting rates for β-particles from the decay of oriented ^{60}Co nuclei for different nuclear orientations (field directions). More electrons are emitted opposite to the nuclear orientation than in the same direction. This demonstrates parity nonconservation. From Wu et al. (1957).

asymmetry has been observed in the case of oriented ^{60}Co" (Wu et al. 1957).

The second experiment, on the sequential decay $\pi \rightarrow \mu \rightarrow e$, was performed with two different experimental techniques by Richard Garwin, Leon Lederman, and Marcel Weinrich (1957) and by Jerome Friedman and Valentine Telegdi (1957). The Garwin experiment found a sinusoidal variation in counting rate, in contrast to the symmetric distribution expected if parity were conserved. Their statistically overwhelming effect (22 standard deviations) led them to conclude that parity was not conserved. In fact, Lederman called Lee at 7 a.m. and announced, "Parity is dead" (quoted in Lee 1971). Friedman and Telegdi performed the same experiment with a different technique. They found a forward-backward asymmetry of 0.091 ± 0.021 (a four-standard-deviation effect). Like Garwin and colleagues, Friedman and Telegdi also concluded that parity was not conserved.

The immediate reaction of the physics community was that parity nonconservation in the weak interaction had been clearly demonstrated. It is fair to say that any physicist, upon seeing these experi-

mental results, believed that parity wasn't conserved. Even Pauli was convinced. He wrote (quoted in Bernstein 1967, 60), "Now, after the first shock is over, I begin to collect myself. Yes, it was very dramatic. On Monday, the twenty-first, at 8 p.m. I was to give a lecture on the neutrino theory. At 5 p.m. I received the three experimental papers. I am shocked not so much by the fact that the Lord prefers the left hand, as by the fact that He still appears to be left-right symmetric when He expresses Himself strongly. In short, the actual problem now seems to be the question: Why are strong interactions right and left symmetric?"

Pauli was fortunate that he had not wagered a very large sum of money that parity would be conserved. Feynman paid Ramsey. Bloch remarked that it was lucky he didn't own a hat (personal communication from T. D. Lee, 1977). The Nobel Prize in physics was awarded to Lee and Yang in 1957, less than a year after their suggestion that parity wasn't conserved.

This is, perhaps, the clearest example of a crucial experiment, one that decides unequivocally between two theories or, in this case, between two classes of theory, in the history of physics. The evidence was beyond a reasonable doubt. Three different experiments were conducted, involving two different processes, the β-decay of oriented nuclei and $\pi \rightarrow \mu \rightarrow e$ decay. The statistical evidence was overwhelming. Friedman and Telegdi found a 4-standard-deviation effect, Wu and collaborators a 13-standard-deviation effect, and Garwin and colleagues a 22-standard-deviation effect. (The probability of a 10-standard-deviation effect is 1.5×10^{-23}. In a lottery with a guaranteed winner that sells 10 million tickets, a buyer of 1 ticket has a better chance of winning the lottery three times in a row than of seeing a 10-standard-deviation effect.) As my former student Mark Corske remarked, "Four standard deviations is strong evidence, 13 standard deviations is absolute truth, and 22 standard deviations is the word of God."

Parity is not conserved in the weak interaction.

Overlooking Parity Nonconservation

The experimental results reported in the 1920s and 1930s that, at least in retrospect, showed the nonconservation of parity in weak interac-

tions were performed by Richard Cox and his collaborators (1928) and by his student, Carl Chase (1929, 1930a, 1930b). The anomalous nature of these experimental results was fairly well known, although the exact nature of the anomaly was not clear. One thing is certain: the relationship of the results to the principle of parity conservation was not recognized or understood by any contemporary physicists, including the authors themselves.

These early experiments were part of the attempt to demonstrate the vector nature of electron waves. Louis De Broglie suggested in 1923 that just as light exhibits both particle and wave characteristics (light shows interference, a wave phenomenon, whereas in the photoelectric effect light behaves as a particle), so should those things that are normally considered particles, such as electrons or protons, exhibit wave characteristics. The wave nature of electrons was confirmed in 1927 in an experiment on the diffraction of electrons by crystals performed by Clinton Davisson and Lester Germer (1927). They had shown that in such experiments the electrons exhibited interference effects that were characteristic of waves. This idea of electron waves was then combined with the concept of electron spin by Charles Darwin (grandson of the Charles Darwin of evolutionary theory) to form the idea of a vector electron (1927). Cox and his collaborators thought that an experiment in which electrons were twice scattered from metal targets would provide evidence for the vector electron. In analogy with experiments on light and x-rays, the first scattering would polarize the electrons, resulting, for example, in more electrons with spin pointing in the positive x-direction than in the negative x-direction. The second scattering would detect that polarization. (If the electrons were polarized, the second scattering would result in an asymmetric result. For example, fewer electrons would be scattered in the forward direction than in the backward direction.)

Although the general nature of the effect to be observed in this experiment was known from the optical analogies, a detailed calculation of the effects expected was not carried out until the work of Nevill Mott in 1929. Mott calculated, on the basis of Paul Dirac's electron theory, that in the double scattering of electrons from heavy nuclei at large angles there would be a difference in the number of electrons scattered in the forward and backward directions (a 0°–180° asymmetry). If, on

the other hand, the electron beam was initially longitudinally polarized, its spin either parallel to or opposite to the electron momentum, the number of electrons scattered at 90° and at 270° would be different, a left-right asymmetry. This latter possibility, which would indicate parity nonconservation, was not considered by Mott. The very existence of a longitudinal polarization for electrons from β-decay is also evidence for parity nonconservation. This is made clear by figure 2.1. In this case the spin is regarded as the spin of the electron itself, rather than that of the nucleus. Assuming that the electron spin is opposite to its momentum, a one-dimensional mirror reflection will reverse the spin direction, but the direction of the momentum will remain unchanged. The mirror image will have the spin in the same direction as the momentum, a clear difference. If the mirror image differs from the real object, parity is not conserved.

Cox and his colleagues described their experiment as follows: "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. . . . The angles at which most of the observations have been made are indicated 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*" (Cox, McIlwraith, and Kurrelmeyer 1928; emphasis added). Their targets consisted of gold plugs, and a milligram of radium, a radioactive element, was used as the source of electrons. The scattered electrons were then detected by platinum-point Geiger counters. These Geiger counters had a short lifetime, and the points often had to be replaced. In addition, their behavior was inconsistent. Not all of the experimental runs showed an asymmetry. Cox and his collaborators stated, "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 weighted average of their experimental results gave the ratio of the number of events at 90° to the number at 270° as 0.91 ± 0.01. This left-right asymmetry was a startling and unexpected result.

The experimenters then examined the possible sources of error in their experiment. They rejected all of these as unlikely and concluded,

"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 authors offered no theoretical explanation of their results, but they did suggest that the discrepancies in their results might be attributable to a velocity-dependent inefficiency of their Geiger counters. (Some of the counters used detected only the slower electrons, whereas the polarization effect was largest for faster electrons.)

Cox's experiments were continued by Carl Chase, a graduate student working under Cox's supervision. His early results, obtained with a Geiger counter as a detector, gave "no indication of polarization . . . of the kind suspected by Cox, McIlwraith, and Kurrelmeyer" (Chase 1929). By this time Mott's 1929 calculation had appeared, and Chase remarked that he had observed a small asymmetry between the counts at 0° and 180°, the forward-backward asymmetry predicted by Mott, but he attributed the effect to a difference in the paths that the electrons traveled in his apparatus.

Chase continued his work and found a substantial velocity dependence in the efficiency of the Geiger counters, as suggested earlier by Cox and his collaborators. Chase then redesigned and modified his experimental apparatus, using an electroscope rather than a Geiger counter to detect the scattered electrons, to avoid the difficulties involved with the use of those counters. His new experiment gave a ratio of 0.973 ± 0.004 (counts at 90°)/(counts at 270°). He concluded, "The following can be said of the of the present experiments: the asymmetry between the counts at 90° and 270° is always observed, which was in no sense true before. Not only every single run, but even all readings in every run, with few exceptions show the effect" (1930b). In this second experiment, Chase also obtained 0°–180° asymmetry of 0.985 ± 0.004. This time he believed that his result was not an artifact produced by his apparatus, and he did attribute it to a Mott scattering effect.

During the 1950s, after the initial experiments that demonstrated parity nonconservation, experiments on the double scattering of elec-

trons were again performed with electrons from β-decay sources, an important point because only electrons from β-decay are initially longitudinally polarized. These later experiments obtained results quite similar to those of Cox and Chase and demonstrated the nonconservation of parity. As Cox remarked later, "It appears now in retrospect, that our experiments and those of Chase were the first to show evidence for parity nonconservation in weak interactions" (1973).

That was not, however, the reaction of the 1930s physics community. Although the results of Cox and Chase were occasionally mentioned as an anomaly in the literature on electron scattering, absolutely no recognition was accorded either by the authors or by anyone else to their significance for the question of parity nonconservation. Bernard Kurrelmeyer, a collaborator of Cox, stated, "As to our understanding of parity, it was nearly nil. Even the term had not been coined in 1927, and remember, this experiment was planned in 1925 and none of us were theoreticians" (personal communication, 1977). Cox, in discussing the reaction of the physics community, stated, "I should say that the experiments were widely ignored," and he added, "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 50s could our results be comprehended" (1973).

There is an interesting and quite puzzling problem associated with the experimental results of Cox and of Chase. In 1959, Lee Grodzins recognized the relevance of those early results to the question of parity conservation. He concluded that these two experiments did indeed show a left-right (90°–270°) asymmetry and thus could have provided evidence for parity nonconservation. In a later publication, Grodzins pointed out that his earlier analysis was incorrect because both experiments had found fewer counts at 90° than at 270°, whereas contemporary theory predicts, and modern experiments demonstrate, more counts at 90°, and thus that both Chase and Cox had found an effect with the wrong sign. My own analysis, along with comparison between the results of experiments in the 1950s and those of Cox and Chase, confirmed that the sign of the asymmetry obtained by Chase and Cox was, in fact, wrong. Grodzins concluded that although the published sign of the asymmetry was incorrect, that Cox and Chase had carried

out correct experiments: "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. . . . 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" (1973).

Cox was initially unaware of Grodzins's later analysis. His own later recollections of the problem differ:

> I was quite surprised many years later when Lee Grodzins credited McIlwraith, Kurrelmeyer and myself with having been the first to observe parity violation. I was equally surprised; and naturally disappointed when he wrote in a later article that the asymmetry in the double scattering of β-rays, as described in our paper, was in the direction opposite to that predicted by the theory and that predicted by Yang and Lee. . . . I did not know, before the articles were printed, of the contradiction between the asymmetry predicted by the theory and that reported by Mcllwraith, Kurrelmeyer, and myself, and by Chase. Grodzins in his article expressed the opinion that we (or I should say I, since I think our paper as published was mainly written by me) made a slip between the experimental observations and its published description. He supposes that the asymmetry we found was actually in the sense the theory predicts but that, in describing the experiment, I accidentally reversed it. At first sight, at least, this seems unlikely. But the alternative explanation, which assumes a persistent instrumental asymmetry, also seems unlikely when I consider how often we removed the Geiger counter to change electrodes (as was necessary in the early short-lived type of counter which Mcllwraith, Kurrelmeyer and I used) and when I remember also other changes which Chase made in the very different equipment with which he replaced ours. I have thought about the matter off and on for a long time without coming to any conclusion either way. (Personal communication, 1977)

Although Cox was being cautious, his argument against a persistent instrumental asymmetry, in both his reminiscence above and the published paper, is convincing. In addition, the experiments of both Cox

and Chase showed the velocity dependence of the polarization that is predicted by modern theory and that has been observed in later experiments. Despite the sign problem it does seem that those early experiments were the first to show evidence for parity nonconservation in weak interactions. (In a letter to me, Professor Cox indicated that he now agreed with my analysis that he had done a correct experiment but had made an error in the coordinate systems.)

Why were these experiments almost completely ignored by the physics community? The standard explanation is that the experiments were redone with electrons from heated metals, rather than from β-decay sources, which do not show the effect, so that they were dismissed: "As a cure the beta decay electrons were replaced with those from a hot filament, the effect disappeared and everybody was satisfied" (Frauenfelder and Henley 1975, 392). Although there is an element of truth to this explanation, it is by no means complete. No theoretical context was available at the time that suggested that these experiments were relevant to the question of parity nonconservation. Parity conservation itself had been suggested only in 1927. In addition, there were similar experiments, performed with the same type of apparatus, which, at the time, seemed to be far more important.

Cox's own recollections provide a useful starting point: "As to the reaction of other physicists to the experiment of McIlwraith, Kurrelmeyer, and myself, (and also to that of Chase on the same subject) I should say that the experiments were widely ignored. . . . Our reported results neither confirmed nor disproved any theory which was a subject of acute interest at the time" (personal communication, 1977).

At the time, no specific theoretical context existed into which to place these early experiments, in contrast to the situation in 1957 when the explicit theoretical predictions of Lee and Yang were published. Cox supports this view (1973): "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 50s, could our results be comprehended."

It is understandable that these early experiments were overlooked because of the lack of theoretical predictions. What is still puzzling is why the perceived anomaly in the results did not act as a stimulus for further work, both experimental and theoretical, in the same way as the θ-τ puzzle did in the 1950s and why these results were ultimately ignored. I suggest that they became lost in the struggle of scientists to corroborate the predictions of Mott that there should be forward-backward (0°–180°) asymmetry in the double scattering of electrons (1929). That result, which tested an important, well-supported, and accepted theory, seemed to be far more important. Mott's calculation was based on Dirac's relativistic electron theory, so that any apparent refutation of Mott's theory also cast doubt on Dirac's theory, which was strongly believed on other grounds. (Dirac's was the only theory at the time that predicted the existence of the positron, a positively charged electron. The observation of the positron in 1932 by Carl Anderson provided strong support for Dirac's theory [Anderson 1933].)

Experiments on the double scattering of electrons began in the mid-1920s, and the general problem of electron scattering from nuclei, as well as the discrepancy between the experimental results and the specific predictions by Mott, were of concern until the 1940s. Difficulties with the consistency of experimental results and subtle and unforeseen effects in electron scattering were present throughout.

With the exception of the result of Cox and his collaborators, none of the experiments performed before 1929 showed any evidence of electron polarization. Change came in 1929 with the publication of Mott's theoretical calculation of the double scattering of electrons. Mott's calculation was based directly on Dirac's relativistic electron theory and made specific theoretical predictions concerning the asymmetry to be observed in the double-scattering experiment. Mott predicted that there would be a forward-backward (0°–180°) asymmetry in the double scattering of initially unpolarized electrons. He specified the specific conditions under which this asymmetry should be observed, namely, single, large-angle scattering from nuclei with a large charge. In later work he also provided precise numerical values expected for the asymmetry. But he noted that his theory did not predict any asymmetry between the left and right directions. "It was in this plane [left-right] that asymmetry was looked for by Cox and Kurrelmeyer, and the

asymmetry found by them must be due to some other cause" (1929). Mott was not questioning the correctness of the experimental results of Cox and colleagues and Chase, he was merely noting that his theory did not explain them.

Subsequent experimental work in the 1930s took on a different character following Mott's researches, because there were then explicit theoretical predictions, based on an accepted theory, with which to compare the experimental results. The experimental situation was confused at best, but no attempts were made to replicate the Cox-Chase results. All of the experiments were designed to test Mott's theory and to search for a forward-backward asymmetry. Some experimenters found the predicted results, others did similar experiments and obtained null results, and some experimenters found positive results at one time but not at others. In general, the trend in experimental results was in disagreement with Mott's calculation. This discrepancy between theory and experiment led not only to further experimental work, but also to many unsuccessful attempts by theoretical physicists to provide reasons for the absence of the predicted polarization effects.

By far the most positive evidence in favor of Mott's calculation was provided by Emil Rupp. In a series of papers during the early 1930s, Rupp reported results in general agreement with those predicted by Mott (Rupp 1929, 1930, 1931, 1932a, 1932b, 1932c, 1934; Rupp and Szilard 1931). These results, which differed from the primarily negative results found by almost everyone else, served to confuse the issue of whether the polarization effects predicted by Mott had been observed. It was soon revealed that Rupp's results were fraudulent. In 1935, Rupp published a formal withdrawal of several of his results. This paper contained a note from a psychiatrist stating that for the past several years Rupp had suffered from a mental illness and could not distinguish between fantasy and reality. There are reports that after Rupp's withdrawal was published, his locked laboratory was revealed to contain either no equipment for performing electron-scattering experiments or only apparatus for forging data. The anecdotes differ. (For more details of Rupp's career, see French 1999.)

In 1937 H. Richter published what he regarded as the definitive experiment on the double scattering of electrons. He claimed to have satisfied the conditions of Mott's calculation exactly and found no

effect. He concluded, "Despite all the favorable conditions of the experiment, however, no sign of the Mott effect could be observed. *With this experimental finding, Mott's theory of the double scattering of electrons from the atomic nucleus can no longer be maintained.*"

There was a definite discrepancy between Mott's theory and the experimental results, and that discrepancy continued despite various theoretical attempts to remove it. As Morris Rose and Hans Bethe concluded (1939), "Unfortunately, none of the effects considered produces any appreciable depolarization of the electrons and the discrepancy between theory and experiment remains—perhaps more glaring than before."

Ironically, the solution was provided by the experimental work of Cox, Chase, and their collaborators in the early 1940s. They found that an experimental artifact had precluded the observation of the predicted effects. This became known as the reflection-transmission effect. In a double-scattering experiment, two different types of experimental apparatus are used: one in which the electrons pass through the thin-foil targets, a transmission experiment, and a second in which the electrons are scattered from the front surface of the foil, a reflection experiment. To minimize the effects of multiple scattering, an important background effect, all of the experiments performed in the 1930s were reflection experiments.

The work of Cox and collaborators in the 1940s showed that in such reflection experiments "plural scattering," in which a large-angle scattering is made up of a few smaller-angle scatterings, will mask the effect of single scattering. Because the plural-scattering electrons are unpolarized, the effect predicted by Mott will not be observed. In a transmission experiment, plural scattering is far less important and the predicted effect can be seen. When this was realized, the experiments were redesigned and the discrepancy between theory and experiment removed. At this point, however, not even Cox and his collaborators remembered their earlier left-right asymmetry result, and the double-scattering experiments on that asymmetry were not repeated until the 1950s, after the discovery of parity nonconservation.

Scientists are not omniscient. They do not always realize all of the implications of either experimental results or theoretical calculations. Clearly, the experiment of Cox, McIlwraith, and Kurrelmeyer and those

of Chase show, at least in retrospect, the nonconservation of parity. In this episode a strongly believed scientific hypothesis, parity conservation, was overthrown, a decision based on overwhelming experimental evidence. Things are not always so clear and unambiguous in the practice of science.

Experiments that, in retrospect, showed parity nonconservation were not understood by either the experimenters themselves or anyone else in the physics community. At least part of the reason for the failure to recognize the importance of the experiments of Cox and his collaborators and of Chase was the lack of a theoretical context in which to place the work. Such a context existed in 1956 because of the work of Lee and Yang. Cox and his collaborators did come tantalizingly close to recognizing the implications of their work: "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*" (Cox, McIlwraith, and Kurrelmeyer 1928).

In the difficult investigation of Mott scattering of electrons, which seemed to be a more important problem at the time, these experimental results were also neglected. The fallibility of science is quite clear in this episode. Experiments gave conflicting answers about Mott scattering during the 1930s, but ultimately a consensus was reached that experiment disagreed with theory. The failure of the experiments to agree with Mott's predictions cast doubt on Dirac's theory, which had other substantial evidential support. This episode also illustrates one way in which the physics community reacts to a seemingly clear discrepancy between experimental results and a well-corroborated theory. Dirac's relativistic electron theory, on which Mott's calculation had been based, was not rejected or regarded as refuted, even after many repetitions had seemed to establish the discrepancy beyond any doubt. The tenacity and perseverance of the physics community led to many repetitions of the experiment, under similar and under slightly different conditions. Various theoretical suggestions were made to try to solve the problem, all of which were unsuccessful. The discrepancy was finally resolved by an experimental demonstration, followed by a theoretical explanation, of why the earlier experimental results were wrong.

Experimental evidence and reasoned and critical discussion played an essential role in this episode. It was good—albeit fallible—science.