LETTERS

Note on the History of the Experimental Proof of Nonconservation of Parity

IN a recent article in this journal Kurti [1] has emphasized the great technical skill shown by the cryogenic workers in carrying out the first experiment to prove the nonconservation of parity and that the difficulty of the experiment does not seem everywhere to have been appreciated. It has been pointed out to me that some mistakes of fact in my Rutherford Memorial Lecture at McGill in 1958 [2] may have had an unfortunate effect in this connection. I wish here to apologize for my mistakes and to make some further comments. In this lecture I discussed the relative part played during the last decade by theoretical prediction and experimental discovery in the field of the elementary particles. I used the fascinating story of the discovery of the nonconservation of parity in weak interactions as an example of the dangers of experimentalists allowing themselves to be deterred from carrying out important experiments, because current theories predicted that nothing interesting would be observed.

In September 1956 [3] Lee and Yang suggested several experiments to test the conservation of parity. The first to be carried out, by Wu of Columbia University in collaboration with Ambler, Hayward, Hoppes, and Hudson, of the National Bureau of Standards, was published in February 1957,[4] and showed that the beta rays from polarized Co^{60} nuclei were emitted asymmetrically with regard to the direction of the polarizing field, so proving that parity was not conserved. Where I went wrong was to write that this crucial experiment took only 48 hours to do and that, as regards its technical feasibility, it could have been performed five or even ten years earlier. This was quite wrong. From a more careful reading of the original literature—previously I had evidently been too much influenced by the voluminous articles in the world press—the following facts emerge. The first proposals how nuclei could be *aligned*, that is in principle, one half oriented parallel and the other half antiparallel to an axis, were made in 1949 and 1951 respectively by Pound [5] and by Bleaney.[6] It was with the latter method that nuclear alignment was convincingly demonstrated in 1951 by Daniels, Grace, and Robinson [7] and also by Gorter et al.[8]

Stupidly I confused alignment with polarization, that is, the orientation of all the nuclear spins in the same direction. This latter and in some ways more difficult experiment was first suggested by Gorter [9] and by Rose,[10] but was not successfully performed until 1953 by Ambler, Grace, Kurti, Durand, Johnson, and Lemmer.[11] So it was only in fact three years and not five, far less ten, which elapsed between the experimental achievement of nuclear polarization and the theoretical suggestion of Lee and Yang that nonconservation of parity would be proved if the beta rays from the polarized nuclei were emitted asymmetrically. Moreover, Dr. Hudson has pointed out to me that, though in 1956 the method of polarizing nuclei was well known, there were still serious experimental difficulties to be overcome connected with the thinness of the beta-ray source and the use of scintillation detectors at 0.01°K. For these reasons the experiment took several months to perform. I never underestimated the great experimental skill needed to carry out this beautiful experiment, but erroneously thought that all the essential techniques had been developed previous to Lee and Yang's suggestion, whereas I realize now that this was not so.

There were, however, other crucial experiments which were in fact performed within a few days. One of these was that of Garwin, Lederman, and Weinrich of Columbia,[12] who demonstrated the polarization of muons from pion decay by observing an asymmetry of the subsequent emission of the electrons. This experiment was carried out essentially with the same experimental arrangement which had been previously used to measure the lifetimes of muons in many different elements. These workers state that this experiment was undertaken as a result of Wu and colleagues' success with the orientation experiment: the two papers were published simultaneously. Many other experiments by many workers, demonstrating the nonconservation of parity in weak interactions, were carried out in the subsequent months, many of them of considerable simplicity.

In retrospect, it is odd that of the crucial experimental tests suggested by Lee and Yang, it was the experimentally most difficult which was done first and that the easier ones came later, stimulated by the former.

My rereading of the literature and my correspondence with Dr. Kurti and Dr. Hudson have confirmed me even more strongly in the thesis expounded in my Rutherford Memorial Lecture, of the danger of allowing theory to act as a brake on experiment. Kurti told me that he had the impression that before the paper by Lee and Yang, theorists considered that the search for a forward-backward asymmetry in beta emission was a trivial and uninteresting experiment, and that the suggestion that it might be worth doing was met at best by an indulgent smile. Dr. Hudson has expressed to me very vividly in a letter the atmosphere of the time and the dominance of theoretical considerations over experimentalists' minds, by writing that, since elaborate analyses of beta decay by the then accepted theory had predicted no asymmetric emission, it was not until Lee and Yang's paper that it became "less than ludicrous" to try such a difficult

experiment. In point of fact, of course, it had been realized for some time that an observed asymmetry of beta emission would disprove parity conservation without involving any detailed theory of beta emission.

It is, of course, perfectly true that in vast fields of physics existing theory is a very good guide to the choice of what experiment to perform. However, it is just in those rapidly developing fields, where one is working on the frontiers of knowledge, that theory sometimes becomes a doubtful guide: for if it were a reliable guide, one would not be at the frontier but well inside fully consolidated territory. On the frontiers of knowledge a technically possible experiment suggested by theory should of course be performed; however, an attractive and technically possible experiment should not be omitted because existing theory suggests that it will be uninteresting. In most cases theory will prove right; in rare cases it will prove wrong and these are the great discoveries.

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Biology and Magnetic Fields

THERE has been an increased interest in the interaction between magnetic fields and biological materials in the past few years. This subject appears to be a specialized section of biomedical electronics, but with no organized means of communication except via formally published papers. Several individuals interested in this field met recently and found the exchange of information and ideas to be of great mutual benefit. We would propose therefore the establishment of an informal group of people working in the field, as well as those in allied fields who have an interest in this topic. We would envision perhaps a short newsletter and perhaps a list of current or proposed projects, observations, new ideas, etc. We suggest that it not be limited to those actively engaged in a project of this type, but that it include all interested parties, engineering or biological, who would be willing to render advice and discussion to groups in their immediate area. We have found that communication between the various disciplines is of incalculable value, and as broad a representation as possible from both the biological and engineering fields is desirable.

We interpret the term "biological effects of magnetic fields" to include alterations in the behavior, physiological state, biochemical processes, growth responses, reaction to injury, etc., induced by exposure of living organisms to magnetic fields. The more basic aspects of magnetic field interaction with cellular or subcellular entities (NMR, Hall effects, etc.) would be included in so far as they had a bearing on the function of the organism as a whole.

If any interested readers will write to either of the undersigned with their suggestions, opinions, current projects, ideas, etc., we will attempt to get out the first communication to all correspondents. It will be possible to forward, to those interested, an extensive bibliography on this subject recently prepared by Dr. Otto Wendel of the Albany Medical College. Should interest and numbers warrant it, further organization of the group within the confines of The Biophysical Society or American Physical Society would be contemplated.

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A Translator's Rebuttal

MY translation of L. A. Chernov's *Wave Propagation in a Random Medium* was reviewed by Prof. Philip Morse in the December 1960 issue of *Physics Today*. I am very grateful to Prof. Morse for his favorable review. However, I must take exception to a remark made in the first paragraph of the review, where Prof. Morse first enunciates a dictum, dubbed "Condon's rule", to the effect that "a technical volume is not worth reviewing if its author has not considered it important enough to provide with an index" and then asserts that "the present reviewer has reluctantly decided to break the rule just this once, since the *sins of the translator* should not be visited on the author, and besides, this is an important book." (My italics.)

The clear implication of the statement quoted is that I, as sinning translator, deleted an index that appeared in the Russian original. This is simply not true! There is no index in the Russian original, which I offer to the inspection of all interested parties. Moreover, I assure the reviewer that I would not have shirked the trivial task of translating the index and supplying the correct page references, had Chernov's book contained an index in the first place.

It seems to me that a corollary of this misunderstanding is to exhibit that Condon's rule is false. For, despite the fact that Chernov did not see fit to equip his book with an index, has not Prof. Morse gone on to say that "this is an important book" and "the monograph is a readable and well-organized review of a difficult subject of timely interest"? I suggest that Condon's rule does not apply to books of rather small mass!

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