Enrico FERMI

A record of the Fermi Memorial symposium held April 29, 1955, in Washington, D. C., as a part of the Spring Meeting of the American Physical Society.

By N. Metropolis

IN an extraordinary plenary session at its annual Washington meeting, the American Physical Society honored the memory of Enrico Fermi. Most appropriately the program was arranged and presided over by his friend, Hans Bethe. The history of Fermi's accomplishments was presented by five speakers, each of whom described one period of his unique career. The chairman began the story and by his interpolated remarks created a beautiful synthesis.

Frederick Seitz began with Fermi's formulation of quantum statistics, his first great contribution—at the age of 23. Two years earlier, Bose had proposed a new form of statistics to account for the behavior of light quanta. Einstein's brilliant implementation of Bose's theory followed soon after. Pauli had just introduced his exclusion principle for electrons in an atomic system. It was then that Fermi described the new statistics, a form that is appropriate for the treatment of electrons.

This formulation, complementary to that of Bose, had far-reaching implications. As Seitz remarked, Fermi himself made only one direct application, apart from the original treatment of a free electron gas. This was to the spatial distribution of the electron cloud that surrounds the nucleus of an atom. Admittedly a first approximation to a more realistic picture, the statistical model of the atom provided many semi-quantitative features in a very simple and direct manner.

The most striking successes of Fermi statistics came in the theory of metals. In retrospect, it is easily seen how that whole subject was reactivated by the Fermi touch. A simple concept, yet so fundamental it brought to fruition many diffuse ideas in a variety of fields and gave stimulus and a solid foundation to many new ones. First came the interpretation by Pauli of the weak, temperature-independent paramagnetism of the alkali metals. Then Sommerfeld and his famous school undertook, on a very broad basis, an examination of the theory of metals. The understanding of the anomalous specific heats was one of the principal prizes of those investigations. Quite generally, the new statistics had the effect that the obscure was suddenly brought into sharp focus. The works of Houston and Bloch on the mean free path of conduction electrons and the study of the anomalous Hall effect made by Peierls had their origin in the new statistics. Seitz then summarized briefly the development of: (1) the "theory of holes" in the physics of the solid state by Heisenberg and others, (2) the theory of positrons by Dirac and by Oppenheimer, (3) the statistical nuclear model by Majorana and Weiszäcker. Finally, the role of the new statistics in modern field theories was touched upon. Jordan and Wigner, among others, have demonstrated that the Fermi and the Bose statistics provide the framework for this field formalism. In fact, the application seems such a natural one that this development is not likely a blind digression.

Before presenting the next speaker, Bethe remarked on some less well-known spectroscopic investigations and molecular structure studies that Fermi carried on before coming to his second great contribution. These included: an explanation of the then curious vibra-

Nicholas Metropolis, Los Alamos theoretical physicist and specialist in the design and use of high-speed electronic computing equipment, was a friend of Fermi's.

tional spectrum of CO_2; the derivation of a general formula for the hyperfine structure in atomic spectra; explanation and calculation of the anomalous intensities in the prominent doublets of alkali spectra.

E. J. Konopinski then embarked on an account of Fermi's theory of β-decay, proposed in 1934. A clear, simple description was given of the underlying principle, together with a consideration of the mathematical variants, or types of coupling, admitted by the theory. One must remember that only the naturally occurring β-emitters were known at the time. Later, when positron decay was discovered, its theory was easily incorporated within the original framework. The capture of an orbital electron by a nucleus was predicted and eventually observed. With time, much experimental data on β-decay accumulated and, as Konopinski remarked: "Numerous peculiarities have been observed among the cases of β-decay, and always Fermi's theory was equal to the challenges they presented."

One important consequence of the Fermi theory was to establish β-spectroscopy as a powerful method in the study of nuclear structure.

Konopinski then focused attention on the present state of knowledge of β-theory, presenting evidence to narrow the field of allowed types of interactions. While the arguments are not yet complete, the work has been very impressive.

Perhaps the most influential aspect of this work of Fermi is that his particular form of β-interaction established a pattern which may be appropriate for the study of other types of interactions. Mention was made of the relevance of this concept to the new particles of nuclear physics, in particular to the phenomenon associated with μ-mesons. Such considerations have led Yang and Tiomno to the notion of a "Universal Fermi Interaction," an hypothesis that implies the need for two new conservation principles. The first of these is the "conservation of heaviness"; in brief, the number of nucleonic particles is not changed in a nuclear reaction. The second, suggested by Konopinski and Hahmoud, is that a pair of particles, obeying Fermi statistics, must be created for every pair destroyed. These additional laws are needed to preclude processes that do not occur.

To appreciate the impact produced by Fermi's theory of β-decay on the world of physicists, one may refer to the remark by Konopinski: "It is amazing what varieties of observed phenomena (and what thicknesses of *The Physical Review*) have their roots in his one paper on the subject."

Bethe then remarked that Fermi, with his paper on β-decay, brought to a close his purely theoretical studies and became an experimentalist, a role he pursued with undiminished intensity. Emilio Segrè sketched with warmth and color the evolution of the famous Italian school, which in short order became the scientific Mecca for nuclear physicists. Here we get a picture of the man and his methods seen by a close friend and collaborator. Warm, understanding, and untiring, Fermi organized a small group of co-workers that included Segrè, Rasetti, Amaldi, d'Agostino, and later Pontecorvo. One could not but be impressed by the directness of their methods, the general spirit of camaraderie, the minimum of red tape and the circumvention of what there was of the latter.

First came the experiments with neutrons from radon-beryllium in which an extensive series of elements was bombarded and the induced radioactivity observed. Thus the signal disclosing Fermi's entrance upon the field of experimental physics proved as telling as that which had announced his arrival in theoretical physics just fourteen years earlier. After describing the then perplexing results of uranium bombardment, Segrè went on with the story of first experiments with neutrons slowed down in paraffin. Here again a new and broad horizon was unfolding, foreshadowing the historic developments that soon were to follow.

The now classical experiments with boron and cadmium, the work with a velocity selector, the study of the slowing-down process of neutrons in hydrogenated media were next on their program. All this in less than two years!

The scientific climate there, however, was not left undisturbed by the political developments in Italy and that magnificent era, when so much was accomplished that was so significant, endured for only a short time. Who could have known then how very important it was that Fermi elected to come to this country?

With the talk by Walter H. Zinn, the scene shifted to the United States where three subsequent periods of time were covered. The first dealt with Fermi's work at Columbia University from early in 1939 to May 1942. With a small group of co-workers, he demonstrated the existence and determined the number of prompt neutrons associated with the uranium fission process, results which brought a chain-reacting system closer to reality. First thoughts for such a system were to use uranium and graphite. Preliminary experiments were started by Fermi at Columbia and Wigner at Princeton, and by the summer of 1941 the first serious experiments were started, namely the construction of the so-called exponential pile. Once again, Zinn's description recalled the characteristic qualities of mind and hands in Fermi's patient preparations, his care in making the precise measurements required, his response to the first disappointing results on the all-important reproduction factor k. The low value of k, compared to the expected value, stopped Fermi only momentarily. With that steady, sustained drive he possessed, work was immediately started to trace the impurities that were poisoning the pile.

Following America's involvement in World War II, however, this type of scientific endeavor went completely underground. The second phase described by Zinn began in May 1942 with Fermi at the "Metallurgical Laboratory" of the University of Chicago, continuing the exponential pile experiments. Zinn, who was a participant, described the course which led to the self-sustaining pile, first brought to criticality on December 2, 1942.

There followed the almost anticlimactic phase of the construction of the production pile at Hanford. But even it was not without excitement: xenon-135 helped to make life "interesting".

Zinn then scanned the list of applications afforded by the intense beam of neutrons emerging from the pile. It had become abundantly clear in the war-time period that the pile had taken its place among the very useful experimental tools.

Regarding the third part of the atomic energy phase of Fermi's career, namely, the period at Los Alamos, Zinn remembers, "I went to his laboratory and the situation appeared normal. There was a pile, the water boiler. Fermi sat at a table on which the recorders from counters were making their familiar noise. A yellow pencil was between his lips and a slide rule in his hand. Another experiment was underway."

With Herbert L. Anderson (Fermi's first and last co-worker in America) as narrator, the last chapter unfolded. On several occasions during his career Fermi had qualitatively changed the direction of his researches. Finally, he determined to find out what he could about mesons. With the new synchrocyclotron at Chicago, he and his very competent group were able to produce intense beams of either positive or negative mesons with which they made a careful study of the scattering in hydrogen at a variety of meson energies. The results of these experiments are of fundamental importance in any discussion of nuclear interactions.

Fermi immersed himself in every phase of the work, sometimes designing and calibrating equipment, sometimes assembling scintillation counters, sometimes tracing the orbits of the pions—even the soldering iron did not escape!

Anderson described the measurements of total cross sections for the scattering of pions in hydrogen by so-called transmission experiments. A very clear exposition was given of the multiplicity of events that can occur with the negative pions, in which connection an amusing story was related about Fermi's deduction of the ratios of the scattering intensities for the three types of pions. Anderson had shown him, in the midst of some data-taking, a preprint of Brueckner's work on the subject. After only a few moments of mediation, Fermi produced his interpretation of their new data.

The angular distribution measurements were given brief attention, then Anderson went on to considerations of the corresponding phase shift analysis. In the latter discussion he indicated how another principle, the condition of causality, helps in sorting out the physically admissible solutions from the many that are found mathematically.

This review of thirty years of Fermi's life, observed Chairman Bethe, is in reality the story of thirty years of modern physics. In his concluding remarks, Bethe recalled how often physicists would go to Fermi with their problems and how he always had time to listen. Sometimes he would give the answer straightaway; more often he would simply reformulate the problem in so clear a fashion that the answer would be all but crystallized, thus giving his listener the pleasure of taking the last easy step.

PHYSICS

By ***Enrico Fermi***

The following is a verbatim transcript of Enrico Fer last address before the American Physical Society, delive informally and without notes at Columbia Universi McMillin Theater on Saturday morning, January 30, 1 His retiring presidential address was delivered one earlier. The present speech, transcribed from a tape reco ing, is left deliberately in an unpolished and unedited fo Such informality would no doubt have been frowned u by Fermi, who was very particular about his publis writings. For those who knew Fermi or heard him sp however, the verbatim transcript may serve (as no for document could ever serve) to bring back for a mom the very sound of his voice. The paper was presented part of the session "Physics at Columbia University" dur the Society's 1954 annual meeting.

Mr. Chairman, Dean Pegram, fellow members, ladies and gentlemen:

IT seems fitting to remember, on this 200th anniversary of Columbia University, the key role that the University played in the early experimentation and the organization of the early work that led to the development of atomic energy.

I had the good fortune to be associated with the Pupin Laboratories through the period of time when at least the first phase of this development took place. I had had some difficulties in Italy and I will always be very grateful to Columbia University for having offered me a position in the Department of Physics at the most opportune moment. And in addition this offer gave me, as I said, the rare opportunity of witnessing the series of events to which I have referred.

In fact I remember very vividly the first month, January, 1939, that I started working at the Pupin Laboratories because things began happening very fast. In that period, Niels Bohr was on a lecture engagement in Princeton and I remember one afternoon Willis Lamb came back very excited and said that Bohr had leaked out great news. The great news that had leaked out was the discovery of fission and at least an outline of its interpretation; the discovery as you well remember goes back to the work of Hahn and Strassmann and at least the first idea for the interpretation came through the work of Lise Meitner and Frisch who were at that time in Sweden.

Then, somewhat later that same month, there was a meeting in Washington organized by the Carnegie In-