Enrico Fermi, 1901-1954

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The sudden death of Enrico Fermi at the age of 53 has filled physicists all over the world with greatest sadness and consternation. One of the most outstanding and in some respects unique scientific personalities, a wonderful teacher and a marvellous representative of his native country, Italy, has left us.

Fermi was born in Rome on 29 September 1901. He was educated at the High School in Rome and later at the Scuola Normale Superiore of Pisa where he obtained a Doctorate in 1922. He later studied at Göttingen with Born and at Leiden with Ehrenfest. From 1924-26 he was Lecturer in Mathematical Physics at Florence. In 1927 he was elected to a Professorship of Theoretical Physics in Rome and in 1929 became one of the Founder Members of the Royal Academy of Italy.

Fermi’s early work was mostly concerned with theory, often with problems which arose from the advent of the new mechanics of Heisenberg, Dirac and Schrödinger. One group of investigations dealt with spectroscopy: the anomaly of the intensity ratio of the multiplets of the higher alkali metals, the magnetic moments of nuclei, calculations of spectra of ions, the Raman effect in CO₂ and in crystals, the oscillations and rotations of the NH₃ molecule and the hyperfine structure separation.

Into the same period, when he was in Rome, falls his theory of a gas whose particles obey Pauli’s exclusion principle. A preliminary study of the conditions under which degeneration of a gas can take place (18) led him to the correct partition function at about the same time (21) as Dirac developed his theory of an ideal gas. The method of Fermi’s derivation is anything but elegant, being based on an oscillator model, but it has the advantage of showing up what he was doing. This clarity is one of the reasons why Fermi’s papers appeal to the experimental physicists (1). Fermi rapidly recognized the general usefulness of the statistical method and applied it with conspicuous success to the calculation of various atomic properties. This Fermi–Thomas method was later applied by F. Bloch with success to calculate the stopping power of matter for charged particles. Here again one strength of Fermi’s becomes apparent: the skill and instinct with which he obtained approximate results where accurate calculation would be prohibitively complex.

Though Fermi was not a theoretical formalist, he was greatly interested in
and contributed to the development of Dirac’s radiation theory and to quantum electrodynamics at about the same time as Heisenberg and Pauli performed their work on a similar topic. An exposition of this topic of unsurpassable clarity was published by Fermi in the *Review of Modern Physics* of January 1932.

Not unconnected with his interest in field theory was his paper on ‘An attempt at a theory of beta rays’. Here Pauli’s *ad hoc* explanation of the continuous beta-ray spectrum by the neutrino is put into a theory, formally constructed in a manner similar to that of the emission of electro-magnetic radiation. There is a certain arbitrariness in the choice of the interaction Hamiltonian. Fermi apologizes for the particular choice by saying it was the simplest. At the time, his theory was objected to, among other reasons, on the grounds of being at variance with the experiments, particularly in so far as the low energy spectrum of the electrons was concerned. Very characteristically, Fermi pointed out that the experiments were difficult and perhaps their precision not sufficient to be decisive to test his theory! Only shortly before, N. Bohr, in his Faraday Lecture (1932), had stressed the great dilemma of physics in so far as beta decay was concerned. Though Pauli’s proposal of a neutrino was the lesser of two evils (the other meant giving up conservation of energy in the beta decay) and was a relief to all, Fermi’s theory reproduced a considerable number of experimental facts and made the neutrino hypothesis more plausible to our minds (*International Conference on Physics* Vol. 1, p. 67 (1934)). Today Fermi’s theory is essentially accepted over a wide range, though nature has turned out to be more complex than we thought.

Both his work on quantum statistics and on the theory of the beta decay had by this time (1934) established Fermi’s international fame as a theoretical physicist.

In 1933 Fermi’s activity entered a new stage, when after Joliot-Curie’s discovery of the production of artificial radioactivity by *α*-particle bombardment he took up neutron research as an experimenter or a theoretician as the situation required. By that time he had collected round him a number of young Italian physicists all of whom were to obtain international repute in later life. Fermi learnt to construct Geiger counters and was able to use the radon from the 1 gram of radium belonging to the Bureau of Public Health to build a radon-beryllium neutron source. With this he began a systematic search through the elements for artificial radioactivity, starting with hydrogen. He obtained no positive results for the elements up to and including oxygen but found that fluorine was strongly activated. In this work he collaborated with Amaldi, D’Agostino, Pontecorvo, Segré and Rasetti.

The neutron source and the counter were kept at the end of a long corridor to prevent interference with measurements. Mrs Fermi has described how the short-lived elements required fast running to take them from the source to detector and Fermi and Amaldi excelled in this. Of the 63 elements investigated 37 were shown to have an easily detectable activity. The
activation cross-section was found not to depend in any systematic way on the atomic nucleus. Two types of transmutation were discovered leading to emission of protons or \( \alpha \)-particles and a third in which the neutron was captured with emission of a gamma ray. The heavy elements were usually transmuted by neutron capture and led to a single unstable element with a single exponential decay of radioactivity. Uranium and thorium proved, however, to be exceptional for several radio-active elements were produced. In a letter to Nature of 16 June 1934, Fermi described the existence of activities having half-lives of 10 seconds, 40 seconds, 13 minutes and at least two longer period activities. The 13-minute activity was shown not to be isotopic with elements 82, 83, or 88-92. He therefore suggested tentatively that a transuranium nucleus with charge 93 might have been formed. It was not till the discovery of uranium fission by Hahn and Strassman 3½ years later that the reason for the complex radioactivities was discovered. One of the activities of 2·3 days half life was shown by E. McMillan in early 1940 to be due to element 93—neptunium.

During the course of the experiments in 1934, Amaldi and Pontecorvo found that the intensity of the radioactivity induced by neutrons varied with the surroundings of the specimen. The radioactivity of silver was found to be increased 100 times by surrounding it with paraffin. Water produced a similar effect and it was inferred that this was due to the slowing down of the neutrons increasing the activation cross-section—in some cases, 1000 times. These neutrons were shown to make 100 collisions in paraffin before capture. This work was the precursor of the atomic pile. On the advice of Corbino a joint patent for the production of artificial radioactivity by slow neutrons was taken out and was the subject of an award in the United States after the war. The main results appeared in two papers (56) and (57) communicated by Lord Rutherford on 25 July 1934 and 15 February 1935 for publication in the Proceedings of the Royal Society. Paper I is mostly concerned with the activities induced by unmoderated sources; II contains a large assortment of observations such as efficiency of various slowing down materials, scattering and diffusion of the slow neutrons, temperature effects, the large variation of capture cross sections for different elements, emission of gamma rays on neutron capture, separation of radioactive isotopes, and a list of all activities found. A section on theoretical considerations on the properties of slow neutrons yields much of the picture of the neutron capture process, such as the \( 1/\nu \) law. Fermi was quite aware of the theoretical difficulties caused by the existence of finite capture cross sections for fast neutrons, a fact which was only later elucidated by Bohr's postulate of the compound nucleus formation. The theory of the slowing down of neutrons, which was later to become so important in theoretical calculations on atomic piles, is already contained in a simple form in a memorandum published by the Consiglio Nazionale delle Ricerche, Roma, 1934, under the title 'Sul Moto dei neutroni nelle sostante idrogenate'. Here it is also pointed out that the large \( n-p \) low energy scattering is mainly due to the singlet state of the deuteron and that
the neutron-capture cross section of hydrogen is due to a magnetic dipole transition. With this one simple assumption of Fermi’s many incomprehensible features of the deuteron system obtained a natural explanation. As the physics of the deuteron system is of fundamental importance to nuclear physics, just as the hydrogen atom to atomic physics, any contribution in such a field is of particularly high merit.

In the years following, i.e. 1936-7, about ten papers appeared mostly in the Ricerca Scientifica on the diffusion and absorption of neutrons, work mostly carried out in collaboration with Amaldi. The last note in this is dated July, 1937.

In the autumn of 1938 the anti-semitic movement in Italy, which developed after the Italian-German alliance, made Fermi decide to leave Italy to take up a professorship at Columbia University. He was awarded the Nobel Prize in December 1938 and the journey to Stockholm helped his move to the United States.

At Columbia Fermi joined forces with H. L. Anderson, Zinn and Szilard, to study the possibility of developing a chain reaction in uranium. About the same time as in France it was shown that neutrons were emitted in the fission process. Uranium was then surrounded with water to moderate the neutron energy, but it was found that ordinary water absorbed too many neutrons to make a chain reaction possible. Szilard and Fermi then decided to try a pile of graphite blocks interposed with lumps of uranium metal. When I (J.D.C.) visited Columbia in November 1940, I saw Fermi carrying out these experiments—at the same time as Halban and Kowarski were carrying out experiments on a heavy water nuclear chain reaction in Cambridge.

By the spring of 1941 a small pile had been built at Columbia, but was too small to become divergent. At the end of 1941 the group went to Chicago and Anderson and Zinn built a larger pile in the squash court of the University of Chicago. Of this period Anderson has said... ‘Fermi possessed a sure way of starting off in the right direction, of setting aside the irrelevancies, of seizing all the essentials and proceeding to the core of the matter. The whole process of wresting from nature her secrets was for Fermi an exciting sport which he entered into with supreme confidence and great zest. No task was too menial if it sped him towards his goal. He thoroughly enjoyed the whole of the enterprise. The piling of the graphite bricks, the running with the short lived activated rhodium foils, and the merry clicking of the Geiger counter which effected the measurement. All was done with great energy and obvious pleasure, but by the end of the day, in accordance with his plan, the results were neatly compiled, their significance assessed, and the progress measured, so that early in the morning on the following day, the next step could begin.

It was a feature of the Fermi approach never to waste time—to keep things as simple as possible, never to construct more elaborately or to measure with more care than was required by the task at hand. In such matters his judgement was unerring. In this way, step by step, the work sped forward until in less than four short years Fermi had reached his goal. A huge pile of graphite
and uranium had arisen in the West stands of the University of Chicago Campus. When, on 2 December 1942, 12 years ago, Enrico Fermi stood before that silent monster he was its acknowledged master. Whatever he commanded it obeyed. When he called for it to come alive and pour forth its neutrons it responded with remarkable alacrity; and when at his command it quieted down again, it had become clear to all who watched that Fermi had indeed unlocked the door to the Atomic Age.’

In 1943 Fermi and his family went to Los Alamos to join Oppenheimer on the atomic bomb project.

One of us (E.B.) had the following impressions of this period:

‘During part of my stay at Los Alamos I was in charge of the experimental group of the division for advanced development, headed by Fermi. It is natural, therefore, that we had a good deal of contact with Fermi who took great interest in what we were doing. An incident which is very characteristic of Fermi occurred when some particularly important and surprising results were obtained. When I told Fermi about it, he said, “Please give me the experimental data and I will calculate the final result and if my calculations agree with you then probably the results obtained are correct.” It was very characteristic of Fermi that he would not accept an experimental result but try to find out in detail how it was obtained. He was always available for any discussion and when he reported about the work of the division he was always very generous and fair in allotting credit where it was due.

‘Besides the profound influence of his research work, I believe that he had very considerable influence on American physics through his lecturing and teaching at the University of Chicago. He was able, practically without preparation, to present any topic in nuclear and atomic physics with clarity and restriction to the essentials, which permitted nearly everybody to follow. This often happens with good lecturers, but frequently one finds that after one has left the lecture hall most of what one thought one understood has vanished. In Fermi’s case the attraction of his exposition rested in the fact that one really understood the problem and did not, therefore, have to rely on memory. He was quite free from oratory but simply had a very penetrating insight into a problem and could formulate his thoughts clearly. The same quality is found in most of his papers; one of them, with Marshall, practically started off the whole interaction of neutrons with solids. Everything is there, just to be exploited.

‘During my last summer at Los Alamos, when Fermi came for a visit, I asked him what he was doing. He said: “Well, I have one big course where I teach children and, you know, I find it much more difficult than giving a highbrow course.” I said to Fermi: “I am really jealous of your students: I wish someone like you would teach me physics under the same circumstances.” The next day he came to my office and said: “I am quite prepared to give you a small private course provided you find half a dozen suitable people to listen.” I gave him a list of a dozen people: he crossed half of them off—those he thought would not be suitable—and then started on a course of
radiation theory and quantum electrodynamics. It was a very great experience and extremely profitable for us. The only trouble was that Fermi did not seem to get tired and instead of one hour he lectured for two; we were too exhausted, but it was obvious that he could easily have gone on for a further period. He just knew the topic so well, in spite of its difficulties, that it cost him no effort to reproduce it.

As a person, Fermi seemed simplicity itself. He was extraordinarily vigorous and loved games and sport. On such occasions his ambitious nature became apparent. He played tennis with considerable ferocity and when climbing mountains acted rather as a guide. One might have called him a benevolent dictator. I remember once at the top of a mountain Fermi got up and said: “Well, it is two minutes to two, let’s all leave at two o’clock”; and of course, everybody got up faithfully and obediently. This leadership and self-assurance gave Fermi the name of “The Pope” whose pronouncements were infallible in physics. He once said: “I can calculate anything in physics within a factor 2 on a few sheets: to get the numerical factor in front of the formula right may well take a physicist a year to calculate, but I am not interested in that.” His leadership could go so far that it was a danger to the independence of the person working with him. I recollect once, at a party at his house when my wife cut the bread, Fermi came along and said he had a different philosophy on bread-cutting and took the knife out of my wife’s hand and proceeded with the job because he was convinced that his own method was superior. But all this did not offend at all, but rather charmed everybody into liking Fermi. He had very few interests outside physics and when he once heard me play on Teller’s piano he confessed that his interest in music was restricted to simple tunes.

After the war Fermi naturally made full use of the availability of high neutron fluxes from the Argonne reactors. Again the happy ability to turn to experiments or attack a problem theoretically bore fruit lavishly. The bare statement of titles of his papers is impressive: ‘Production of low energy neutrons by filtering through graphite’ (72), ‘Transmission of slow neutrons through microcrystalline materials’ (74); ‘Interference phenomena of slow neutrons’ (76). ‘Phase of scattering of thermal neutrons by aluminium and strontium’ (75); ‘Spin dependence of scattering of slow neutrons by Be, Al, Bi’ (79); ‘by deuterium’ (82); ‘On the interaction between neutrons and electrons’ (81); ‘A thermal neutron velocity selector and its application to the measurement of the cross section of boron’ (78). In this large amount of work he was essentially only supported by Leona Marshall, though in some cases other collaborators helped, such as W. J. Sturm, R. G. Sachs, H. L. Anderson. Some papers are astonishingly comprehensive; the one on interference phenomena starts with the theory introducing the scattering lengths, the basic convenient datum easily extracted from experiments, Bragg reflexions, filtered neutrons, diffraction by gaseous molecules, total reflexion by mirrors, concluding with a good table of scattering lengths deduced from his results. Only the cream of neutron optics had been skimmed off but it was
rich, and much in the direction of refinement could be done and was achieved at the Argonne, Oak Ridge and Brookhaven Laboratories. But Fermi's basic work, together with his teaching of the subject at the University of Chicago, gave immense impetus to the development in this field. The effect of this genial atmosphere and the high standard set by Fermi can still be felt in many places remote from Chicago.

When the cyclotron began to operate Fermi participated in the work of the cyclotron group. The scattering of pions with protons and deuterons was studied in detail and great efforts were made to obtain the phase shifts. Fermi's theoretical work this time was concerned with a statistical, strong interaction model for the multiple production of pions at very high particle energies (85) suitable to evaluate the observations of the Brookhaven cosmotron.

In parallel with the above research, Fermi had taken a considerable interest in W. A. Hiltner's observation of the polarization of star light and its interpretation by L. Davis and J. L. Greenstein as magnetic dichroism of interstellar material caused by weak magnetic fields of huge extension. He showed that wandering magnetic fields can, on the average, accelerate particles to cosmic ray energies provided their initial energy is above a certain limit. It seems at present that Fermi's basic idea is correct and that a considerable step forward has been made in a field which has been extremely puzzling. It is worth noting that Fermi and Chandrasekhar were able to confirm by two independent estimates the strength of field in the spiral arms of our galaxy at $7 \cdot 2 \times 10^{-6}$ and $6 \times 10^{-6}$ gauss.

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